SCIENCE

A WEEKLY JOURNAL

DEVOTED TO THE ADVANCEMENT OF SCIENCE.

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CONTENTS AND INDEX.

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The Names of Contributors are Printed in Small Capitals.

ERRATA:—p. 144, col. 2, line 34: for these, read three. p. 153, col. 2, line 59: for Maupertius, read Maupertuis. p. 212, col. 1, line 11: for plan, read phase. p. 213, col. 1, line 13: for cooking, read working. p. 334, col. 1, line 23: for Styles, read Stiles. p. 457, col. 2, line 23: for cinipidæ, read Cynipidæ.

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TO OUR READERS.

AFTER a brief period of suspension this journal again appears, greeting its readers with the compliments of the season. The interest in its future which has been shown in various quarters during the past few months, convinces its editorial staff that there is room for a journal devoted to the promotion of intercourse among those interested in the study of nature. The separation of our investigators around many widely separated centres, and the consequent lack of communication between them, increases the necessity of such a journal, as well as the difficulty of adapting it to the wants of all classes of subscribers. The experience of centuries shows that great success in advancing scientific knowledge cannot be expected even from the most gifted men, so long as they remain isolated. The attrition of like minds is almost as necessary to intellectual production as companionship is to conversation. In saying this I am not unmindful that such men as Copernicus, Kepler and Leibnitz were little stimulated by the companionship of other minds while thinking out their great works. But if the age for discoveries of the kind which these men made is not past, it is certain that work of the kind they did can be repeated only once in many generations. What other and less fortunate investigators have to do is to develop ideas, investigate facts, and discover laws. The commencement of this work of development on a large scale, and with brilliant success, was coeval with the formation of the Royal Society of London and the Academy of Sciences of France. When these bodies came together their members began to talk and to think. How imperfectly they thought, and how little they knew the way to learn, is shown more fully by the history of their debates and by the questions discussed at their meetings than by anything contained in the ponderous volumes of their transactions.

At the present day one of the aspects of American science which most strikes us is the comparative deficiency of the social element. We have indeed numerous local scientific societies, many of which are meeting with marked success. But these bodies cannot supply the want of national coöperation and communication. The field of each is necessarily limited, and its activities confined to its own neighborhood. We need a broader sympathy and easier communication between widely separated men in every part of the country. Our journal aims to supply the want of such a medium, and asks the aid of all concerned in making its efforts successful. It will have little space for technicalities which interest only the specialist of each class, and will occupy itself mostly with those broader aspects of thought and culture which are of interest not only to scientific investigators, but to educated men of every profession. A specialist of one department may know little more of the work of a specialist in another department than does the general reader. Hence, by appealing to the interests of the latter, we do not neglect those of the scientific profession. At the same time, it is intended that the journal shall be much more than a medium for the popularization of science. Underlying the process of specialization which is so prominent a feature of all the knowledge of our time there is now to be seen a tendency toward unification, a development of principles which connect a constantly increasing number of special branches. The meeting of all students of nature in a single field thus becomes more and more feasible, and in promoting intercourse among all such students Science hopes to find a field for its energies, in which it may invite the support of all who sympathize with its aim. S. Newcomb.

Washington.

SCRIPTORIBUS ET LECTORIBUS, SALUTEM.

Everybody interested in Science knows what it ought to be, bright, varied, accurate, fresh, comprehensive, adapted to many men of many minds; a newspaper, in fact, planned for those who wish to follow a readable record of what is in progress throughout the world, in many departments of knowledge. It is not the place for 'memoirs,' but for 'pointers;' not for that which is so technical that none but a specialist can read it; not for controversies, nor for the advancement of personal interests, nor for the riding of hobbies. It should not be maintained for the dominant advantage of any profession, institution or place. Wordiness is inappropriate; so, on the other hand, are figures and symbols, unless they are indispensable. Reviews, summaries, preliminary announcements, descriptions, extracts, correspondence, reports of meetings, biographies, should all find a place; but they must be put in the right sort of phrases and paragraphs. 'There's the rub.' Who is to collect, prepare, revise and set forth these accounts of what is going on in the wide domains of investigation? Money helps to secure such articles, but the work must be done 'for love and not for money.' Altruism is called for, the willingness, if not the desire, on the part of scientific workers, even in the very highest classes, to contribute prompt, brief, readable, trustworthy reports of what is going on, with fitting comments.

Scientific men have rarely the editorial instincts or aptitudes, like those of the editors of *Nature*, the *Popular Science Monthly*, the *Journal of Science*. Caution, close attention to details, precise expressions, are indeed theirs, but readiness to collect and impart news, and ability to make use of the phraseology of common life, are often wanting. There are noteworthy exceptions among men of the first rank. Dr. Asa Gray, the botanist, could say what he had to say in a clear and interesting manner, and Clerk Maxwell, the mathematical physicist, could write paragraphs and verses racy enough for *Punch*. No better writers of instructive and agreeable English can be wished for than Darwin, Tyndall, Huxley and Spencer. SCIENCE hopes to be so fortunate as to discover and awaken the desired talent among the American students of nature. Its experience is worth something. Its managers know the rocks and shoals that must be avoided. They will welcome aid, suggestions, contributions, news, from every quarter. They ask co-operation. They believe that the art of writing can be acquired. One of the fundamental canons of success is to write so clearly that the rapid reader can perceive what is meant.

Such will be the aims of the new management of SCIENCE.

Finally,—

> "If to do were as easy as to know what were good to do, chapels had been churches and poor men's cottages princes' palaces. It is a good divine that follows his own instructions: I can easier teach twenty what were good to be done, than to be one of the twenty to follow mine own teaching."

D. C. GILMAN.

JOHNS HOPKINS UNIVERSITY.

*THE CHARACTER AND AIMS OF SCIENTIFIC INVESTIGATION.**

THE influence of this Association is in the highest and best sense of the word *educational*. Its discussions are aimed to present the correct methods of scientific investigation and to be guided by the true spirit of scientific inquiry. Permit me to explain this statement a little, for in it lies more than anywhere else the right to existence of our organization and the best effects it can exert upon its own members or upon a community where it convenes.

The goal which we endeavor to attain is *scientific truth*, the one test of which is that it will bear untrammelled and unlimited investigation. Such truth must be not only verified, but always verifiable. It must welcome every test; it must recoil from no criticism, higher or lower, from no analysis and no skepticism. It challenges them all. It asks for no aid from faith; it appeals to no authority; it relies on the dictum of no master.

The evidence, and the only evidence, to which it appeals or which it admits, is that which is in the power of every one to judge —that which is furnished directly by the senses. It deals with the actual world about us, its objective realities and present activities, and does not relegate the inquirer to dusty precedents or the mouldy maxims of commentators. The only conditions which it enjoins are that the imperfections of the senses shall be corrected as far as possible, and that their observations shall be interpreted by the laws of logical induction.

Its aims are distinctly beneficent. Its spirit is that of charity and human kindness. From its peaceful victories it returns laden with richer spoils than ever did warrior of old. Through its discoveries the hungry are fed and the naked are clothed by an improved agriculture and an increased food supply; the dark hours are deprived of their gloom through methods of ampler illumination; man is brought into friendly contact with man through means of rapid transportation; sickness is diminished and pain relieved by the conquests of chemistry and biology; the winter wind is shorn of its sharpness by the geologist's discovery of a mineral fuel; and so on, in a thousand ways, the comfort of our daily lives and the pleasurable employment of

*From the introductory address of Dr. Daniel G. Brinton, President of the American Association for the Advancement of Science, at the annual meeting in Brooklyn, August, 1894.

our faculties are increased by the administrations of science.

Scientific truth has likewise this trait of its own; it is absolutely open to the world; it is as free as air, as visible as light. There is no such thing about it as an inner secret, a mysterious gnosis, shared by the favored few, the select illuminati, concealed from the vulgar horde, or masked to them under ambiguous terms. Wherever you find mystery, concealment, occultism, you may be sure that the spirit of science does not dwell and, what is more, that it would be an unwelcome intruder. Such pretensions belong to pseudo-science, to science falsely so called, shutting itself out of the light because it is afraid of the light.

Again, that spirit of science which we cultivate and represent is at once modest in its own claims and liberal to the claims of others. The first lesson which every sound student learns is to follow his facts and not to lead them. New facts teach him new conclusions. His opinions of to-day must be modified by the learning of the morrow. He is at all times ready and willing to abandon a position when further investigation shows that it is probably incorrectly taken. He is in this the reverse of the opinionated man, the hobby rider and the dogmatist. The despair of a scientific assemblage is the member with a pet theory, with a fixed idea, which he is bound to obtrude and defend in the face of facts. Yet even toward him we are called upon to exercise our toleration and our charity; for the history of learning has repeatedly shown that from just such wayward enthusiasts solid knowledge has derived some of its richest contributions. So supreme, after all, is energy, that error itself, pursued with fervid devotion, yields a more bountiful harvest than truth languidly cultivated.

But, perhaps, the picture I have thus drawn of the spirit of scientific inquiry excites in the minds of some a certain antipathy, or, at least, a sense of dissatisfaction and incompleteness. To such this description may sound narrow and materialistic; the results of scientific study thus rehearsed may appear vague, indefinite, incompetent to satisfy the loftier yearnings of the soul of man for something utterly true, immutably real.

Vain, indeed, were the life work of our Association; bereft, indeed, were we of just claim on your consideration, did we appear before you with such a thankless and futile confession of the ultimate aim of our labor. But it is far, very far, otherwise.

All this prying into the objective, external aspect of things; this minute, painstaking study of phenomena; this reiterated revision and rejection of results, are with the single aim of discovering those absolute laws of motion and life and mind which are ubiquitous and eternal; which bear unimpeachable witness to the unity and the simplicity of the plan of the universe, and which reveal with sun-clear distinctness that unchangeable order which presides over all natural processes.

This is the mission of science—noble, inspiring, consolatory; lifting the mind above the gross contacts of life; presenting aims which are at once practical, humanitarian and spiritually elevating.

Daniel G. Brinton.
University of Pennsylvania.

*AMERICA'S RELATION TO THE ADVANCE OF SCIENCE.**

"In art and science there is no such thing as nationalism: these, like all things great and good belong, to the entire world, and are promoted only by free interchange of ideas among contemporaries, with constant reference to the heritage of the past." So wrote

*From *What has been done in America for Science*—an Address delivered before the Philosophical Society of Washington, November 24, 1894, by G. Brown Goode, retiring President.

Goethe in his *Sprüche in Prosa.* In the present address I have spoken, not of "*American* Science," but of what has been done in America for science. I have summarized the work accomplished in the study of the physical conditions and biological statistics of two great continents. I have shown that our countrymen have made important contributions to exact knowledge in every one of its departments from astronomy to anthropology, and that, contrary to general belief, these have been chiefly in pure science rather than in the application of science. Most of our American advances in economic science, with the exception of those in the field of electricity, have consisted in multifarious adaptation and bold application of principles and methods first made known in Europe. Except in ingenious mechanical inventions, Americans have done little in connection with applied science that is strikingly new or great.

It is not, however, by determinate contributions to the aggregate of human knowledge that America has aided most largely the advance of science. It has been in a manner vastly more subtle and far-reaching, through the action of an intellectual leaven which has imbued the thought of all mankind.

America has always afforded to scientific workers a most sympathetic and appreciative audience—even at periods in her history when she has been producing the least at home. When Auguste Comte was young he intended, it is said, to seek a career on this side of the sea, but was dissuaded by a friend, who assured him that if Lagrange himself were to come to the United States he could only earn his livelihood by turning land surveyor. This was absurdly false, for in that very year Laplace's *Mécanique Céleste* was being translated, for the first and only time into English, by Nathaniel Bowditch, whose service to science, which was more important through his commentaries than his translation, was fully appreciated even during his own lifetime, and who has ever since been esteemed one of the most distinguished of our countrymen.

European science has always been more warmly appreciated by our people than contemporary European literature, and men like Lyell, Huxley Wallace and Tyndall, when they have come among us, have received the most enthusiastic welcome, and their books have been consumed in much larger editions than at home, and not without becoming royalties to their authors.

Many others have come to us, not in prosperity but through necessity, and were none the less heartily welcomed—Gallatin, Hassler, Priestly, Cooper, Bernard, Duponceau, Cupont de Nemours, Nicollet, Rau and others.

Humboldt wrote in 1807 :

" During five years passed in the Spanish colonies of America a few French emigrants we found at Nueva Valencia, in Guatemala, were the only ones we saw. Beyond the Atlantic the United States of America afford the only asylum to misfortune. A government, strong because it is free, confiding because it is just, has nothing to fear from giving refuge to the proscribed."*

Priestly, who had been forced to withdraw from the Royal Society, called America ' The Land of the future,' and Richard Price, in the midst of the Revolution, one of the most popular men in England, in declining the invitation of Congress to remove to this country wrote : " The United States is now the hope and likely soon to become the refuge of mankind."

There is even more to be said concerning the influence of our people upon the thought and practice of the Old World.

The liberal policy of our State and National governments toward many branches of scientific work is well understood abroad, and has had an influence, especially in en-

Personal Narrative, Vol. ii., Chapter I.

couraging the publication of dignified and well illustrated reports upon the results of scientific exploration and research.

An illustration of the popular appreciation of knowledge in this country is to be found in the growth of libraries, and in the increasing volume of the stream of books, new and old, which pass constantly to the westward across the Atlantic.

Augustine Birrell, M. P., in an address at Dumfermline, Scotland, has presented some most astounding statistics in regard to books and libraries. He said that in the public libraries of Europe there are twenty-one million printed volumes; in those of Australia, one million more, while those of America contain fifty millions—more than twice as many as in all the rest of the world.*

The mere possession of books does not in itself count for much, but the eagerness to acquire the means of research, not books only, but all other instruments and appliances for intellectual progress, is very significant. It should be remarked also that this tendency, so far as the public at large is concerned, has not become very evident until within the last third of the present century.

There is a relationship still more fundamental between America and the advance of science, to which only a passing allusion is proper here.

I refer to the reflex action of democratic institutions upon those of the Old World—to the influence of human freedom, practically demonstrated upon American soil, upon the freedom of the people in our parent lands.

It was one thing for men like Priestley to fly hither for personal liberty. It was quite another for Coleridge and Southey to plan for the founding of a pantisocracy on the banks of the Susquehanna, and then to remain at home with Wordsworth and promote human freedom by their writings, or for Price to denounce the oppression of the American colonies as an outrage against liberty, and thus to secure from the people of London, who presented him with the freedom of their city, an assurance of sympathy among their English kinsmen, which encouraged the colonists to declare their independence. If, at the time of the Great Exodus, the men who organized the Royal Society of London had carried out their purpose of removing in a body to Connecticut, there to found an academy of sciences, the higher learning would have been retarded, not advanced.

It is almost impossible for us to understand the manner in which even now freedom of thought and action is burdened in the Old World by the weight of feudal traditions and by the existence of class distinctions and privileges. Americans surely do not understand, but that quick-witted race of Orientals, the Japanese, have done so from the very time when they applied themselves seriously to the task of making their own what is best in the civilization of the circum-Atlantic peoples. To England they went for ideas about a navy and for lighthouses, to Germany for a system of government, for military instruction and for medicine, and to France for a code of laws. In matters of education, however, they have chosen from the very start to be guided by Americans;* their keen perception teaching them that, whatever may be its defects in detail, the American educational plan is that which in some form or other is certain

* *Pall Mall Gazette*, September, 1891. The estimate is perhaps somewhat extreme, though the official return of public libraries in the United States (excluding the other American republics and the colonies) show nearly 32,000,000 books in public libraries of over one thousand volumes.

* Their postal system, their telegraphic code and their meteorological service are also purely American in origin, as well as such foreign agricultural methods as they may have adopted.

to be adopted by every free people, and to work mighty changes in traditional, social and governmental systems. Not less significant, perhaps, in the same connection is the present attitude of Pope Leo XIII. and his counsellors in regard to educational movements in the United States.

The condition of affairs in Germany up to quite a recent day, as shown in Virchow's address to the Congress of German Naturalists in 1872, seems almost incredible.

Describing the organization of that society, fifty years before, he said:

"Not perhaps at the dead of night, but still beneath the veil of secrecy, a handful of savants assembled for the first time at Leipsic, at the invitation of Oken. In fact, in 1822, no considerable body of men could come together in Germany in answer to a public invitation, without the permission of the civil authority. They could not discuss among themselves scientific questions, no matter how unconnected with the political and national questions of the day. Add to this the other fact that, if I am not mistaken, it was only in 1861, at the Congress of Naturalists at Spires, that the names of the Austrian members could be made public, and then we can appreciate the tremendous change that has been brought about in the Vaterland."

In England personal liberty, though not consciously retarded by law, is severely trammeled by the nature of existing social organizations. Distinction in science and letters is, even to-day, practically, subordinated to social distinction. As an illustration one need only notice the position of the President of the Royal Society upon any list in which the names of influential Britons are arrayed in order of social precedence. It is next to impossible for a man of moderate means, however learned, to become president of one of the great English scientific societies, and the honor most highly esteemed by the masses in England, as well as throughout Europe, that of a decoration, is rarely given, except to men who are prosperous in some material way.

"I know in London," writes Leland, "a very great man of science, *nemini secundus*, who has never been knighted, although the tradesman who makes for him his implements and instruments has received the title and the *accolade*." *

The changes which the last four centuries have wrought are by no means to be all attributed to the influence of the inhabitants of the New World, but in a large degree, no doubt, to the social and political modifications which the discovery of America rendered possible in the Old World.

It is, after all, very difficult to realize the exact relation of this discovery to the intellectual history of mankind, and it may be impossible, unless we were endowed with the gift of omniscience.

A few months ago, standing within the great red fortress of the Alhambra, looking down on the plain of Granada, still green with the orchards and vineyards planted by the former Moorish rulers of Spain, I understood, as I had never done before, that the final expulsion of the Orientals from Europe was almost simultaneous with the discovery of America. Six months before he sailed westward, Columbus stood with Ferdinand and Isabella upon that very tower, and saw the last cavalcade of exiled Moors disappear over the mountains toward Africa. For many centuries the military strength of our European ancestors had been chiefly devoted to repelling the invasions of these restless men of the East. Feudal government held universal domain, and all the learning of Europe was hoarded up within monastic walls.

"The discovery of the New World not only offered new productions to the curiosity of man. It also extended the then existing work of knowledge respecting physical geography, the varieties of the human species, and the migrations of nations. It is impossible to read the narratives of the early Spanish travelers, especially that of Acosta, without perceiving the influence which the aspect of a great continent, the study of extraordinary appearance of nature, and intercourse with men of different races must have exercised on the progress of knowledge in Europe. The germ of a great number of physical truths is found in the works of the sixteenth century; and

* Memoirs, 1893, p. 127.

that germ would have fructified had it not been crushed by ignorance and superstition."

So wrote Humboldt at the end of the last century. He must have felt, although he did not say so then, that ignorance and superstition were also to be dissipated in the new and expanded intellectual atmosphere. The passage already quoted from his writings shows this clearly.

The establishment of the supremacy of Western civilization, and the finding of a New World were, after all, less important than the discovery which the men of both hemispheres made on this side of the sea—that they might become free and their own masters. It was the opening of a new period in human history. Men were awaking from the slumber of ages. Europe began to emerge from abject intellectual slavery. In political life the traditions of the age of despots were broken, and the development of free institutions begun. In religion a reformation was inaugurated, wider in scope than the movement led by Luther, which is commonly associated with that name. In art, soulless and awkward formalities were replaced by enthusiastic culture of the ideals of classical days, which in time grew broader, more spontaneous and more inspired. In the field of letters, scholastic traditions were cast aside, and each nation in Europe developed a new language and a new literature. In science, similar scholastic and traditional usages were discarded. The students who compiled uncritically and generalized upon the worthless results of their own antiquarian researches, gave place to the restless, skeptical, critical inquirers of modern times.

We have just ended our celebration of the discovery of America, the end of the Dark Ages, the birth of individual freedom and of proper government. We celebrated at the same time the beginning of a new epoch. The Mediæval Renascence was limited to Europe; ours will embrace all the nations of the earth. It may be that this should be considered the outgrowth and fulfillment of that which marked the end of the Middle Ages, but whether we are at the beginning of a new movement, or at the culmination of an old one, the last forty years have undoubtedly witnessed greater changes in the spirit of men's thoughts than the four centuries which had gone before.

The earlier Renascence gave to man the right and liberty to think and act as he, in his own judgment, saw fit. The Renascence of to-day is leading men to think, not only with personal freedom, but accurately and rightly. Far be it from me to say that I believe that mankind in general are very much nearer to accurate and just standards of judgment than they were four hundred years ago, but the spirit of to-day favors untrammeled and searching investigation of every question in which man is concerned, a critical comparison of the results of such investigation, and a frank intolerance of all illogical or unsound theory and application.

This is the spirit of science—the spirit of unprejudiced search for truth—and this, emphatically, is the spirit of thinking men of to-day in America, in every department of activity.

Who can say what is to be the part of America in the future intellectual life of the world? It cannot be less important than in the past, and in all likelihood the influence of America will be more comprehensive and deep-seated as the years go by. Is it not possible that it may hereafter become the chief of the conservative forces in civilization rather than, as in the past, be exerted mainly in the direction of change and reform?

Brain of the New World, what a task is thine,
To formulate the Modern—out of the peerless grandeur of the Modern,
Out of thyself. * * *

Thou mental moral orb, thou new, indeed new, spiritual world,
The Present holds thee not—for such vast growth as thine,
For such unparalleled flight as thine, such brood as thine,
The Future only holds thee and can hold thee.*

G. BROWN GOODE.

U. S. NATIONAL MUSEUM.

*Whitman, *Leaves of Grass.*

LEGAL UNITS OF ELECTRIC MEASURE.

IT will, doubtless, be interesting to all physicists, as well as to many in other departments of science, to know of the legalization by Act of Congress, within the last six months, of units of electrical measure. It is not necessary in these columns to go into an exposition of the necessity for such action on the part of the Government, nor to refer to the enormous amount of capital invested in the manufacture of instruments, devices and machinery, the sole object of which is the conversion of some form of energy into electricity and the reconversion of electricity into some form of energy. The measurement of the enormous quantities of electricity that have within the last decade been produced and thus converted has, up to a recent date, in all cases depended upon the conventional acceptance of units of measure which have for many years been in use among scientific men, and which originated in the necessity for such units of measure in scientific investigations. It is always worth while to note, however, that the great simplicity and perfection of electrical measurement is due to the fact that the science of electricity preceded the art of its utilization. In this respect electrical engineering has a very decided advantage over all other branches of engineering, for in all others the art preceded the science, and the science, therefore, was obliged to build itself upon the crude and mostly unphilosophical principles that developed in the art.

The fundamental units of electrical measure, namely, the ohm, the ampere and the volt, have been in use among scientific men, to the exclusion of all others, for more than a decade, related as they are to the fundamental units—length, mass and time, which are admirably adapted for use as the basis of all electrical metrology. It has, however, long been recognized that much inconvenience was caused in electrical discussion by the lack of a few additional units, the use of which would greatly facilitate mathematical calculations and numerical statements. The literature of the subject has abounded, during the past ten years, with suggestions as to these additional and desirable units of measure, and various writers have, from time to time, adopted such as seemed to be necessary for their own use, even giving them such values and such names as were best in their judgment. It was evident, therefore, that to prevent confusion in electrical nomenclature it was desirable to have an international agreement as to these units, their value, their number and their names; the demands for this have grown very extensive in the last few years, the result having now been reached in the passage, by Congress, of a law which seems to define and settle these questions as far as the United States Government is concerned.

All readers of this journal are, doubtless, familiar with the fact that as early as 1881 an electrical convention, or congress, was held in Paris for the purpose of trying to agree upon definitions of the fundamental units of electrical measure and their material representations, in cases where material representations were possible. After much discussion, and not without very considerable opposition, there was proposed at that time a material representation of the ohm which was known to be somewhat in error. The real ohm must always be that defined by the Committee of the British Association for the Advancement of Science, and any material representation which may be adopted should only be considered as an approximation to this. It was first agreed that this theoretical ohm should be represented by the resistance offered to an unvarying current of electricity by a column of mercury one square millimetre in cross section, and one hundred and six centimetres in length, at a definite temperature. Even at the time of the acceptance of this ohm it was well known that the length of this column was nearly three millimetres too small to correctly represent the ohm of the British Association Committee. This result had been established by investigations by Rowland in this country, and by other experimentalists in Europe. In consequence of the inaccuracy of this first material representation of the ohm it did not meet with much favor, although it was quickly taken up among practical men, and resistance coils in great numbers were wound in accordance with this definition, being generally, but incorrectly, known as the 'Legal Ohm.' I do not know that this unit was ever adopted by any government, or even by any municipal corporation.

During the last ten years there has been a continual agitation of this question, resulting in the determination to go over the whole subject again, with a view to defining the fundamental units and adding such other units as might be desirable and necessary, at an International Congress to be held at Chicago in 1893, in connection with the World's Fair. The inception and organization of this Congress was largely due to the American Institute of Electrical Engineers and to local societies in the city of Chicago. Its history is so well known that it is only necessary to refer to it very briefly. In order to avoid errors which are likely to arise in the consideration of a very important subject by a very large assemblage, it was agreed that the question of units should be referred to a body which was within, and formed a part of, the general International Congress, and which was known as the Chamber of Delegates. In this Chamber of Delegates the number of representatives from the different nations was limited; five each were allotted to the United States, Great Britain, France and Germany, three to Italy, and to the other nations a smaller number. Most of the principal delegations were full on the assembling of the Chamber, and the total number of persons was about thirty. Daily sessions were held during the week of the International Congress, and many hours aside from these sessions were occupied by special committees in the discussion and development of the various subjects which came before the Chamber to be acted upon.

In reference to the personnel of this Chamber, it may be well to say that the delegates from foreign countries were appointed by their respective governments and presented regular authenticated commissions, and that the representatives of the United States received their authority from the Secretary of State in a commission which he prepared after the names of the five persons selected had been recommended to him by a vote of about sixty or seventy of the leading electricians of the country, who had been invited to join in this ballot by the Chairman of the Executive Committee for the organization of an International Congress. The five names receiving the greatest number of votes were recommended to the Secretary of State for appointment as representatives of the United States. A list of the delegates present and taking an active part in the deliberation of the Chamber is given herewith:

Representing the United States.

Professor H. A. Rowland, Johns Hopkins University, Baltimore, Md.

Dr. T. C. Mendenhall, Superintendent United States Coast and Geodetic Survey, and of Standard Weights and Measures, Washington, D. C.

Professor H. S. Carhart, University of Michigan, Ann Arbor, Mich.

Professor Elihu Thomson, Lynn, Mass.

Dr. E. L. Nichols, Cornell University, Ithaca, N. Y.

Representing Great Britain.

W. H. Preece, F. R. S., Engineer in Chief and Electrician, Post-office, England; President of the Institution of Electrical Engineers, London.

W. E. Ayrton, City and Guilds of London Central Institution, Exhibition Road, London.

Professor Silvanus P. Thompson, D. Sc., F. R. S., Principal of the City and Guilds Technical College, Finsbury, London.

Alex. Siemens, 12 Queen Anne's Gate, Westminster, London, S. W.

Representing France.

E. Mascart, Membre de l'Institut, 176 rue de l'Université, Paris.

T. Violle, Professeur au Conservatoire des Arts et Metiers, 89 Boulevard St. Michel, Paris.

De la Touanne, Telegraph Engineer of the French Government, 13 rue Soufflot, Paris.

Edouard Hospitalier, Professor a l'École de physique et de chimie industrielle de la ville de Paris; Vice-President de la Societe internationale des Electriciens, 6 rue de Clichy, Paris.

Dr. S. Leduc, 5 quai Fosse, Nantes.

Representing Italy.

Comm. Galileo Ferraris, Professor of Technical Physics and Electro-technics in the R. Museo Industriale, Turin, Via Venti Settembre, 46.

Representing Germany.

H. E. Hermann von Helmholtz, Präsident der Physikalisch-technischen Reichsanstalt, Professor, a. d. Universität, Berlin, Charlottenburg bei Berlin.

Dr. Emil Budde, Berlin N. W. Klopstockstrasse 53.

A. Schrader, Regierungsrath, Mitglied des Kaiserl. Patentamts, Berlin.

Dr. Ernst Voit, Professor an der technischen Hochschule, München, Schwanthalerstrasse, 73-3.

Dr. Otto Lummer, Mitglied der Physikalisch-technischen Reichsanstalt, Charlottenburg, Berlin.

Representing Mexico.

Augustin W. Chavez, City of Mexico.

Representing Austria.

Dr. Johann Sahulka, Technische Hochschule, Wien.

Representing Switzerland.

A. Palaz, Professeur, Lausanne.
René Thury, ingenieur, Florissant, Genève.

Representing Sweden.

M. Wennman, Byrachef i Rougle Telegrafstyrelsen, Stockholm.

Representing British North America.

Ormond Higman, Electrician, Standards Branch, Inland Revenue Department, Ottawa.

As a result of the deliberation of this Chamber, it was agreed to recommend to the several governments represented by the various delegations the adoption of eight units of electrical measure, namely: the ohm, the ampere, the volt, the coulomb, the farad, the joule, the watt and the henry. The Chamber also prescribed definitions for these several units, but as they are essentially the same as those adopted by Congress, and which will be found in detail below, it is not necessary to refer to them here.

Shortly after the adjournment of the Congress a report of its proceedings was made to the Secretary of State by the United States delegates, and this report was distributed by the Department of State among the various nations represented, and also among those not represented, with the request that they should coöperate with the United States in the legalization of the units of electrical measure thus carefully selected and defined. In order to secure action on the part of our own Government, a bill was prepared and introduced into the House of Representatives by Mr. Charles W. Stone, of Pennsylvania, early in 1894, defining these units substantially in agreement with the definitions adopted by the Chamber of Delegates at Chicago, and declaring them to be the legal units of electrical measure for the whole of the United States. Through the active interest of Mr. Stone, and by the assistance of the American Institute of Electrical Engineers or a few individual members thereof who interested themselves in the passage of the measure, this bill became a law by the approval of the President on the 12th of July last.

The differences between the definitions adopted by the International Congress at Chicago and those found in this law are very slight, and consist entirely of verbal changes that were thought to be desirable and necessary by the Senate Committee to which this bill was referred after its passage by the House of Representatives. It may be well to remark that a subcommittee of the Chamber of Delegates, consisting of von Helmholtz, Professor Ayrton and Professor Carhart, had been appointed to prepare specifications for the better realization of the adopted material representation of the volt. The continued illness of von Helmholtz, from the time of his leaving this country, at the close of this Congress, up to the day of his lamented death, about a year later, prevented the completion of the labors of this committee at an earlier date; however, correspondence had been begun, and many points had been defined and settled among its members. The specifications for the better representation of the ampere to which the Chamber of Delegates had agreed will be found in the report of the American delegates to the Secretary of State. As this subcommittee had not yet been able to formulate a report, and as it was necessary for Congress to make some reference to these specifications in the Act adopting the units, it was agreed that the matter should be referred to the National Academy of Science, as is provided in the last section of the Act. This Act, as it finally became a law. is as follows:

(Public No. 105.)

An Act to define and establish the units of electrical measure.

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That from and after the passage of this Act the legal units of electrical measure in the United States shall be as follows:

First. The unit of resistance shall be what is known as the international ohm, which is substantially equal to one thousand million units of resistance of the centimetre-gramme-second system of electro-magnetic units, and represented by the resistance offered to an unvarying electric current by a column of mercury at the temperature of melting ice fourteen and four thousand five hundred and twenty-one ten thousandths grammes in mass, of a constant cross sectional area, and of the length of one hundred and six and three tenths centimetres.

Second. The unit of current shall be what is known as the international ampere, which is one-tenth of the unit of current of the centimetre-gramme-second system of electro-magnetic units, and is the practical equivalent of the unvarying current, which, when passed through a solution of nitrate of silver in water in accordance with standard specifications, deposits silver at the rate of one thousand one hundred and eighteen millionths of a gramme per second.

Third. The unit of electro-motive force shall be what is known as the international volt, which is the electro-motive force that, steadily applied to a conductor whose resistance is one international ohm, will produce a current of an international ampere, and is practically equivalent to one thousand fourteen hundred and thirty-fourths of the electro-motive force between the poles or electrodes of the voltaic cell known as Clark's cell, at a temperature of fifteen degrees centigrade, and prepared in the manner described in the standard specifications.

Fourth. The unit of quantity shall be what is known as the international coulomb, which is the quantity of electricity transferred by a current of one international ampere in one second.

Fifth. The unit of capacity shall be what is known as the international farad, which is the capacity of a condenser charged to a potential of one international volt by one international coulomb of electricity.

Sixth. The unit of work shall be the joule, which is equal to ten million units of work in the centimetre-gramme-second system, and which is practically equivalent to the energy expended in one second by an international ampere in an international ohm.

Seventh. The unit of power shall be the watt, which is equal to ten million units of power in the centimetre-gramme-second system, and which is practically equivalent to the work done at the rate of one joule per second.

Eighth. The unit of induction shall be the henry, which is the induction in a circuit when the electromotive force induced in this circuit is one international volt while the inducing current varies at the rate of one ampere per second.

Sec. 2. That it shall be the duty of the National Academy of Sciences to prescribe and publish, as soon as possible after the passage of this Act, such specifications of details as shall be necessary for the practical application of the definitions of the ampere and the volt hereinbefore given, and such specifications shall be the standard specifications herein mentioned.

Approved July 12, 1894.

It will be desirable to add some remarks upon the steps which have been taken in the same direction by the English Government since the adjournment of the International Congress. All who are familiar with the legislation in the United States on the subject of Weights and Measures will recognize the passage of the Act given above as the first general legislation establishing units of measure for the whole country, on the part of the American Congress.

Although the Constitution provides that Congress shall have the power to establish systems of weights and measures, it is well known that Congress has never exercised this power except in the Act of 1866, which involves the semi-establishment of such a system by making the use of the Metric System permissive throughout the United States. Aside from this, systems of weights and measures in this country have been uniformly and universally the result of State legislation until the passage of the above Act defining units of electrical measure.

In England a committee has for some time been in existence whose object was the recommendation of suitable units of electrical measure, that they might be legalized, as is the practice in Great Britain, by means of an 'Order in Council' signed by the Queen. Among the members of this committee are such well known names as Lord Kelvin, Preece, Glazebrook and Ayrton. This committee made a report on the 2d of August, 1894, and this report was approved by the Queen on the 23d of the same month, so that in this country we were a little more than a month in advance of Great Britain in the legalization of units of electrical measure. The English committee, however, did not feel prepared to go as far as we have gone in the recommendation for the adoption of the whole list of eight units approved at Chicago. Some members of this committee have explained this in personal conference by the statement that the three primary units, the ohm, the ampere and the volt, were found to be not difficult of material representation, while most of the others were very decidedly so, and, as most of the others are derived from these three, it was thought best, at the present time, to restrict authoritative adoption to the ohm, the ampere and the volt. In defining these units the English committee has also departed slightly from the definitions as adopted at Chicago, the changes being mostly verbal, but, in one or two instances, of such a character as to quite alter the fundamental relation of the materialized unit to its theoretical representative. In order that this may be clearly seen, it may be well to quote the definitions of these three units, as found in the 'Order in Council' of August 23d. The following is quoted directly from said 'Order':

"And whereas it has been made to appear to the Board of Trade that new denominations of standards are required for use in trade based upon the following units of electrical measurement, viz.:

"*First.* The Ohm, which has the value of 10^9 in terms of the centimetre and the second of time and is represented by the resistance offered to an unvarying electric current by a column of mercury at the temperature of melting ice 14.4521 grammes in a mass of a constant cross sectional area and of a length of 106.3 centimetres.

"*Second.* The Ampere, which has the value $\frac{1}{10}$ in terms of the centimetre, the gramme and the second of time, and which is represented by the unvarying electric current which, when passed through a solution of nitrate of silver in water, in accordance with the specification appended hereto and marked A, deposits silver at the rate of 0.001118 of a gramme per second.

"*Third.* The Volt, which has the value of 10^8 in terms of the centimetre, the gramme and the second of time, being the electrical pressure that if steadily applied to a conductor whose resistance is one ohm will produce a current of one ampere, and which is represented by .6974 $(\frac{1000}{1434})$ of the electrical pressure at a temperature of fifteen degrees C. between the poles of the voltaic cell known as Clark's cell, set up in accordance with the specification appended hereto and marked B."

The specifications referred to in the above as marked A are those that were adopted at the Chicago Congress, together with some additional suggestions as to the methods of procedure.

The specification marked B refers to the method of preparation of Clark's cell, including a detailed statement as to materials and as to the method of setting up the cells. These specifications are made so as to include several different kinds of cells, so that the Lord Rayleigh modification of the Clark cell, and also a modification devised and used by the Germans, may be used at will. There is certainly a decided advantage in this. Attached to the 'Order in Council' is a schedule which is declared to set forth the several denominations of electrical standards as approved by the Queen. In this schedule the standard of electrical resistance is described as being the resistance between the copper terminals of a particular coil of wire under standard conditions. The standard of current is described as being the current which when passed through the coils forming a part of a particular instrument under specific conditions gives rise to forces which are exactly balanced by the force of gravity at Westminster upon a particular mass of matter forming a part of said instrument. The standard of electro-motive force, or, as it is termed in the 'Order in Council,' 'electrical pressure,' which is denominated as one volt, is described as being $\frac{1}{100}$ part of the pressure which when applied between the terminals of a particular instrument causes the rotation of a certain portion of said instrument to the extent which is measured by the coincidence of a certain wire with the image in the eyepiece of the telescope and with certain fiducial marks.

A careful examination of the above definitions, together with the schedule following, and a comparison of the same with the units as defined by Act of Congress, which are essentially those of the Chicago Chamber of Delegates, will give rise to many interesting and important reflections to which space cannot now be given. It may be suggested, however, that there is room for uncertainty under the provisions of the English regulations as to what is the standard of resistance, or of current, or of electro-motive force. Of course this will all turn upon what would be the action of the English authorities in case of a suspected error in the material representation of these standards as provided for in the schedule. The 'Order in Council' makes no provision for a course of procedure in such an event, and it is but natural to assume that standards of a very complicated character, and so composite in material as those thus adopted, must be continually liable to changes, and the reintroduction of errors of considerable magnitude.

The actual material representations of these three electrical units, it will be observed, are by this 'Order' removed at a considerable distance from the fundamental definitions adopted by the English committee, as well as by the Chicago Chamber of Delegates, thus, although the ohm is defined primarily by reference to the C. G. S. system of units, and secondarily by reference to the column of mercury, in actual practice it is neither the one nor the other of these, but is the resistance of a solid metallic conductor.

The ampere, while defined primarily in terms of the C. G. S. system, and secondarily in reference to the silver voltameter, is in practice determined by the dynamic action of one current upon another. In the same way, the volt is not in practice referred to the C. G. S. system of units, nor is it determined by comparison with the Clark cell, but by the measurement of the rotation effect upon a part of a certain instrument when the electro-motive force is applied between certain points in that instrument.

One cannot refrain from the opinion that, from an absolutely metrological standpoint, the regulations of the 'Order in Council' should be condemned rather than approved; however, personal conference with the representatives of the English Board of Trade and Standardizing Laboratory reveals the fact that the material representations of electrical units, thus provided, are to be considered as but tentative in character, adopted on account of greater convenience in actual practice, and to be continually revised and corrected by reference to the fundamental definitions, which are essentially the same as those approved by the representatives of Great Britain at the Chicago Congress, and where they do differ from those are, it will be generally admitted, I think, on the whole, more sound.

It is very important for the United States that, when the time shall come, as it must before long, for the preparation of material representations of as many of the electrical units that have been legalized as can conveniently be represented, the greatest effort shall be made to see that there be no hasty action, and that, as far as possible, already well established principles of metrology shall be strictly applied.

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THE HUMANITIES.

THE study of the history of mankind is logically developed into five great branches, viz.: industries, pleasures, languages, institutions and opinions. These are the *Humanities.* Into all of these realms modern scientific research penetrates and seeks to discover their origin and development from the beginning of primeval human life to the present time. In following the course of humanity from the earliest savagery to the highest enlightenment it is found that man has traveled by five parallel roads from the starting place of ignorance toward the goal of wisdom. Now he travels on one road, now on another, parceling out his activities and dividing his time between all. On wings of thought he passes from way to way. When he travels by one road he seems to have one end in view, by another road another end in view, and yet as often as he may change his goal and the road by which he travels he is pursuing the route to wisdom. He may travel by false charts, or he may lose his way, and yet the end in view may remain the same. He engages in the arts of industry and the purpose is welfare; he engages in the arts of pleasure and the purpose is happiness; he engages in the arts of speech and the purpose is expression; he engages in institutional arts and the purpose is justice; he engages in the arts of learning and the purpose is knowledge. In the way by labor, the way by pleasure, the way by speech, the way by institutions and the way by learning—in all ways—he runs to the goal of wisdom.

In all the research prosecuted during the present century, and especially during the later decades, one great generalization is reached from the multitudinous facts gathered from the world; this is the intellectual unity of the human race. The history of the lower animals, from primeval geologic time to the present, exhibits a constant differentiation of species, genera, orders and higher groups. The evolution of animal life is the unfolding of new forms. In the study of mankind this evolution is replaced by an involution which tends toward unification. In his early history biotic forms and varieties were developed, with more or less differentiation of functions. Some men were high of stature, others were low of stature; some men were blondes, others were brunettes; some men had long skulls, others short skulls; some men had their eyes placed obliquely, others horizontally; some men had round hair, others had flat hair. The tendency in the beginning was toward the differentiation of varieties, which, had man continued in his lowly estate on a plane with the lower animals, might have resulted in the differentiation of species not interfertile with one another; but with mankind interfertility was preserved.

Man was endowed with superior intellect. He had outrun the lower animals in the race of culture and began to develop the five great activities: industry, pleasure, speech, government and learning. With these evolving powers the evolution of varieties was checked. The evolution of activities superseded the evolution of biotic varieties, and man's course of development was by involution and seriation; men became more and more interdependent, and this is involution. Some men made more progress in the five great activities than others, but all progress resulted in serial development. So some peoples have a higher culture than others. All of the human activities are interrelated and ever become more and more interrelated. Not only are the activities interrelated, but the peoples themselves become more and more interrelated through them in the progress of activital development.

Let us now take a hasty view of mankind in his early estate, moving along the highways of progress toward the present time. Early man was scattered over all the earth in kinship tribes, each one knit together by bonds of kindred blood and cords of marriage ties. All tribal society was thus organized. These little tribes, in vast numbers, each contained but a few individuals who inhabited the Eden between the walls of ice. Their arts of welfare sprung from conditions of local environment. Where the waters were abundant they became fishermen; where the beasts of the wold and prairie were plenty they became hunters, where the fruits of the forest and plain were rich they became gleaners, and where all of these sources of supply existed their food industries were diversified. In frigid lands they built their houses of snow and ice; in forest lands they built their homes of shards of trees, boughs and bark; in the savannahs they built their homes of reeds and mats; in arid lands of naked rock and cliff they built their homes of stone—everywhere they adapted the materials of the local environment to their use. Where the beasts were plenty they made their clothing of pelts; where animals yielded wool they made their clothing of woolen fibers; where fibrous plants were abundant they made their clothing of vegetable tapestries, and they decorated homes and clothing with the pigments and stains which they found where they lived. So man started on the way of welfare.

The children of these little tribes had their youthful sports. They kept playhouse as their mothers kept house; they played with dolls as their mothers played with babies; they played at hunting as their fathers were hunters; they played at fishing as their fathers were fishermen; they played at fruit gathering as their fathers and mothers gathered fruit; and they played at war as their heroes made war, and thus mimetic sports were developed. The elders engaged in running races, in wrestling matches and various games of athletic prowess and skill, and thus their athletic sports began. They engaged in games of chance and staked their little stores of wealth and sought to divine their chances and developed simple methods of divination, and thus their intellectual games began. With sports of mimicry, sports of athletic skill and sports of chance and divination, the highway of pleasure was entered.

They began to express their ideas by gesture speech and oral speech in imitation of the sights and sounds of the world, and especially of the characteristics of one another; thus gesture speech and oral speech began, and the tribes entered upon the highway of speech.

In the biotic constitution of man the seeds of government are planted, for there must be husbands and wives, parents and children, and there must be authority and obedience. As the kinship tribes were developed authority and obedience grew with the group, and a system of terms was developed by which kinship through streams of blood and marriage relations was clearly exhibited, and to the elder was given the right to command, and to the younger the duty to obey—a system of perfect equality, for every individual grew in authority as he grew in years, and must command some and obey others. Thus began forms of government, and the tribes entered upon the highway of institutions.

Every child learns by experience. The accumulation of experience from infancy to old age is great even with primal man, but by speech the experience of the elder is taught to the younger. In the stream of generations there are elder and younger in every tribe, and the experience of ancestors is handed down. Thus primal man entered upon the highway of learning.

Let us see where the human race began. A multitude of kinship tribes spread over the habitable earth, each tribe on the highways of progress, with simple arts suited to local environment, with simple pleasures suited to home environment, with simple speech developed from the gestures and vocal sounds of men and the lower animals and the scenes of nature found in the environment, with simple governments developed out of biotic life conforming to the environment of kinship and age and the needs of daily life, and with simple knowledge gathered by the individual through experience and transmitted one to another by speech and handed down from generation to generation in an ever-growing stream of wisdom, all taught by the environment.

In this picture we have primal men in multitudes of distinct tribes under the differentiating forces of environment by which they may be developed into species, but for one overpowering factor—superior human intellect. There can be but one kind of mind. Two and two are four with every people; the moon is round, gibbous or crescent wherever it shines for man; the sun shines in every eye; the child grows in every experience. Thus the four great mental activities of number, form, cause and becoming are the same in every land, and the mind of every man is a unity of these four powers, and every mind is like every other mind in their possession. They differ only in extent of experience acquired directly by self or indirectly from others. While the mind is the same with all men the will is the same. All desire to gain good and to avoid evil, so all wills develop on a common plan. By mind and will, by mentality and volition, man progresses on the five highways of life, so that all men are impelled to the same goal of wisdom. Pursuit of the common end has proved to be more powerful in producing involution than the forces of environment in producing differentiation or classific evolution. It now becomes necessary to make a hasty sketch of human evolution.

The kinship tribes first developed by man gradually underwent a change. Tribe coalesced with tribe, and when tribes became too large by union or by natural multiplication they divided. In the consolidation of tribes the plan of union by kinship remained. Two or more tribes allied their fortunes by intermarriage, each furnishing wives to the other; so the chains of affinity were forged, and out of this affinity spring new bonds of consanguinity. In succeeding generations fathers and mothers belong to different clans, and each tribe is made up of individuals, every member of which is kin to both primal tribes. Kinship through affinity and kinship, through consanguinity, was maintained in knowledge by a device of naming, so that the name not only expressed kinship by clan, but also kinship by tribe as composed of clans, and at the same time expressed relative age by which authority was claimed and yielded and primeval equality maintained. In the coalescing of tribes in this manner a new generation became heirs to the activities of the coalescing tribes. They inherited industries, pleasures, languages, institutions and opinions of the ancestral tribes. So tribes coalesced with tribes and divided and coalesced again, until tribal society was lost in the confusion of ancestries. Then nations were born, based not on kinship bonds but on territorial boundaries. The first nation and every other nation since has in its very organization lost its ancestral identity by multiplied admixture of streams of blood. To speak of a nation as of one blood or as derived from one primeval tribe with its primitive industries, pleasures, speech, institutions and opinions is absurd. To search for the origin of a nation in one primeval tribe having some one or all of the primeval activities is a search for the impossible.

It is thus that the study of the human race has led to the discovery of its unity. It is found that we cannot classify men as biotic kinds with differing forms, functions and genealogies, as the lower animals are classified. An early tendency to such differentiation is discovered, but it is farther learned that this tendency has been partially obliterated and greatly obscured in the later history of mankind. By these discoveries many interesting facts have been recorded of variations in human forms, functions and genealogies. The study is one of interest and proves to be valuable. Thus the old science of ethnology remains as the study of biotic varieties of mankind, and is pursued with more vigor than ever and becoming of more and more importance.

In the study of ethnology as the science of biotic races the attempt was early made to supplement biotic characteristics with cultural characteristics from the domain of arts, or, as they are here called, *humanities*. This has led to the development of a new science pertaining to human activities as herein classified, and to which the term *demology* is sometimes given, while even the term ethnology is made to include both the biotic and the activital history of mankind. It may be well to keep the term ethnology to the limits of its primitive use and to adopt the term demology for the new science of human activities.

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ZOÖLOGICAL NOMENCLATURE.

THE EARLIEST GENERIC NAME OF THE GROUND SQUIRRELS COMMONLY PLACED IN THE GENUS SPERMOPHILUS.

THE eccentric Rafinesque, who imposed such a multitude of new names upon animals and plants, seems to have been first to name the group of ground squirrels for which the later name *Spermophilus* of Cuvier (1825) has been in common use for more than half a century. In 1817 Rafinesque published a paper entitled 'Descriptions of new genera of North American Quadrupeds,' in which the 'Burrowing Squirrel' of Lewis & Clark was made the type of a new genus and species, *Anisonyx brachiura*.* This animal had been named *Arctomys columbianus* by Ord two years previously; † and was afterward erroneously referred to the genus *Cynomys*—likewise proposed by Rafinesque for one of Lewis & Clark's animals. Several years ago I showed that the animal in question is a true ground squirrel or spermophile, ‡ but refrained from reinstating Rafinesque's genus *Anisonyx* because it was then believed that a still earlier name would be found. A somewhat exhaustive search through the literature, however, has failed to bring to light anything earlier; hence it seems necessary to publicly reintroduce *Anisonyx* as the proper generic name for the group of mammals now commonly referred to *Spermophilus*.

THE EARLIEST AVAILABLE NAME FOR THE MOUNTAIN GOAT.

It has been customary of late to refer the Mountain Goat to the genus *Mazama* of Rafinesque.§ But *Mazama* was based primarily on the *Temamazame* of Mexico, which Rafinesque called *M. tema*, and which has been since shown to be a deer.‖ The next species mentioned by Rafinesque is our Mountain Goat, which he named *M. dorsata*. But under this species he makes the following unequivocal statement which seems to have been overlooked: "This species, with the following [*M. sericea*, which is really the same animal] and the *Mazama puda* [of Chili], will form a particular subgenus (or perhaps genus) which I shall call *Oreamnos*, distinguished by the horns slightly curved backwards or outwards, often rough or annulated, and long hair, besides living in mountains." (Am. Monthly Mag., II., 1817, 44). In view of these facts there seems to be no escape from the adoption of the name *Oreamnos* as the earliest available generic name for the Mountain Goat, which is the type and only known species of the genus, the '*M. puda*' being a South American deer. The full name for the species is *Oreamnos montanus* (Ord) 1815, and the type locality is the Cascade Range, near the Columbia River, in Oregon or Washington.

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* Am. Monthly Magazine, II., 1817, 45.

† Guthrie's Geography, 2d Am. Ed., II., 1815, 292 and 303–304.

‡ Mammals of Idaho, N. Am. Fauna, No. 5, July, 1891, 39–42.

§ Am. Monthly Mag., II., 1817, p. 44.

‖ Biologia Centrali-Americana, Mammalia, 1880, p. 113.

THE NEED OF A CHANGE OF BASE IN THE STUDY OF NORTH AMERICAN ORTHOPTERA.

Some twenty years ago one of the very acutest and most industrious of modern entomologists, the late Carl Stål, of Stockholm, began the publication of a Recensio Orthopterorum. In it and in kindred papers he had within five years laid the foundation of an entirely new system in nearly every family of Orthoptera, offering novel and taxonomically important but easily overlooked points of structure for subdivisions of a high order. A great deal of work has been done since then (the number of species has perhaps doubled), and it has been mainly upon the lines laid down by him, but in greater detail.

Most American students of Orthoptera, however, have been very poorly acquainted with these modern studies, and the result is that, with a distressing wealth of undetermined species, new forms have been described and referred to genera of ancient name, a procedure which in many cases has given little or a wrong impression of the real affinities of the insects in question, and it has now become impossible to correlate American and European work. Something, indeed much, has been done by European entomologists, but their autoptic acquaintance with our fauna is relatively poor; and while there are ample materials here, there appears a remarkable paucity of students inclined to serious work in this direction. Lists we have in number, but in them almost invariably figure Acridium, Caloptenus, Oedipoda, Stenobothrus, Mantis, etc., genera which in their now restricted application do not or hardly exist in North America.

There has been some excuse for this, since the broad scope of Stål's work, embracing the Orthoptera of the globe, rendered work upon exclusively American material difficult to one without means of reference to extra-American insects, collections of which are uncommon in this country, though easily obtainable by any one with means. Still, it is strange that no one having access to the museums in our larger cities or universities has undertaken to apply the modern system of classification to one or another of the families or subfamilies of American Orthoptera. He would have earned merited applause from all students in this field.

One attempt, indeed, was made to collate what could be known of the Acrididæ, but it was before Stål began his work, and it may almost be classed as a hindrance. Now, however, the field is open, for Brunner von Wattenwyl, whose collection of Orthoptera is the richest in the world, published a year ago a *Révision du Système des Orthoptères*, through which, by means of the tables given by him of an exceedingly simple character (sometimes in practice one finds them too limited), one may quickly group his collection in a natural order, and by means of the literature to which reference is briefly made, determine the generic position or affinities of whatever he has before him. The way for a revision of any group is therefore clearer than ever before, and our entomologists will have none but themselves to blame if they do not hereafter better coördinate their work with that of the European writers.

Samuel H. Scudder.

Cambridge.

SCIENTIFIC LITERATURE.

An Elementary Treatise on Theoretical Mechanics.—Part I., Kinematics; Part II., Introduction to Dynamics; Part III., Kinetics. —By Alexander Ziwet, Assistant Professor of Mathematics in the University of Michigan.—8vo.—Macmillan & Co., London and New York, 1893–94. Pp. viii+181, viii+183, viii+236.

Since Lagrange set the model for analytical mechanics in his *Mécanique Analytique*, a little more than a century ago, there has been no serious lack of good elementary works devoted to that science. Most conspicuous of the latter is Poisson's *Mécanique* (1811, 2d ed., 1833), which was undoubtedly more widely read and followed than any other work during the first half of this century. It is only recently, however, that the great advantage of the analytical over the geometrical method in mechanics has come to be generally recognized by authors and educators. The influence of Newton has long held English writers to the geometrical form of the *Principia*. To this, nevertheless, there are a few noteworthy exceptions, the most important of which in the present half century is probably Price, whose volumes on analytical mechanics (*Infinitesimal Calculus*, Vols. III. and IV., 1862) have done excellent service.

Along with the remarkable growth of science in general during the past thirty years a great impetus has been given to mechanics. This is traceable chiefly to two sources, namely: first, the development of the Faraday-Maxwell view of electricity and magnetism; and, second, the thought-inspiring qualities of the great work of Thomson and Tait on *Natural Philosophy*.

The latter treatise and the *Electricity and Magnetism* of Maxwell have stimulated a wonderful activity in the study of mechanical ideas; and, as a result, a number of high-class elementary books on pure mechanics have appeared during the past decade. The work of Professor Ziwet is one of the best of this class. It is up to date and distinctively in touch with the progressive spirit of the age. In accordance with the modern order of presentation, Part I. is devoted to kinematics, Part II. to statics as a special case of dynamics, and Part III. to kinetics. No one acquainted with the magnitude of theoretical mechanics would expect to find a complete treatise even in the space of 600 octavo pages. It goes without saying, in fact, that he who would now do battle in the fields of mechanics should be armed with a battery of treatises. But it must be admitted that the work of Professor Ziwet covers the ground exceedingly well, giving a fairly good idea of nearly every important principle and process from the composition of vectors to the kinetics of variable systems. The mode of treatment, though distinctly analytical, is tempered by the introduction of geometrical illustrations and analogues where they serve to give clearness and fixity of ideas. A noteworthy feature of the work is the large number of references to the literature of the science. These references alone make the work one of the best that can fall into the hands of the enterprising student. The typography and press work are worthy of the distinguished publishers under whose auspices the volumes appear. A few misprints and a few inaccuracies of expression are visible in the work; but these are inevitable in a first edition of such a treatise. A speedy demand for a second edition will, we hope, enable the author not only to remove these trifling defects, but also to add an index, which will much enhance the value of the work for purposes of reference.

R. S. W.

From the Greeks to Darwin.—An outline of the development of the evolution idea.— By HENRY FAIRFIELD OSBORN. — Columbia University Biological Series 1. — New York and London, Macmillan & Co., 1894. Pp. 259. $2.00.

This is a timely book. For it is time that both the special student and the general public should know that the doctrine of evolution has cropped out on the surface of human thought from the period of the Greek philosophers, and that it did not originate with Darwin, and that natural selection is not a synonym of evolution.

The author divides his work into six sections, entitled respectively: The anticipation and interpretation of nature; Among the Greeks; The theologians and natural philosophers; The evolutionists of the eighteenth century; From Lamarck to St. Hilaire; Darwin.

It is clearly shown that evolution has reached its present completeness as a result of a slow growth during the past twenty-four centuries, and that Darwin owes more to the Greeks than has been hitherto recognized by any of us. The Greek philosophers in biology, as in geology, anticipated, at least in some slight degree, modern scientific philosophy. The doctrine of continuity in the organic and inorganic world, anticipations of the monistic philosophy, and of the evolution of life, were taught by Thales and Anaximander, while Aristotle spoke of some of the factors of transformation, and even clearly stated the principle of the survival of the fittest, though he afterwards rejected it.

The father of evolution was Empedocles, who believed in spontaneous generation, that plants came first, that animal life long after budded forth from the plants, and in his poetry Osborn finds the germ of the theory of the survival of the fittest or of natural selection. Democritus perceived the principle of adaptation of single organs to certain purposes, while Anaxagoras attributed adaptations in nature to intelligent design and was thus the founder of Teleology. But as Aristotle was the father of natural history so was he the first scientific evolutionist, being the earliest to conceive of the chain of being from polyps to man, a view afterwards generally held until Lamarck replaced it by his truer simile of a branching tree. The great Greek naturalist and anatomist understood the principle of adaptation of organs in its modern sense, discovered the law of the physiological division of labor, and conceived of life as the function of the organism; was not a vitalist; understood the doctrine of heredity, atavism or reversion; and finally, with all his errors and misconceptions, had vague notions of the unity of type, of nature, of gradations in nature, while the core of his views on evolution was the doctrine of an 'internal perfecting tendency,' which crops out in modern science in the writings of Owen, and even Koelliker, as well as others, including Weismann.

Passing to the evolutionists of the present century, Oken's place is, it seems to us, properly assigned; due credit is given to Buffon, who saw the force of isolation, and full credit to Erasmus Darwin, though sufficient stress is perhaps not laid on the fact that he was not a working zoölogist and had no followers. Osborn effectually disposes of the strong suspicion of Dr. Krause that Lamarck was familiar with the 'Zoonomia,' and made use of it in the development of his theory. He clearly brings out the fact, as stated by Martins, that Laplace supported Lamarck in the doctrine of the inheritance of acquired habits, as applied to the origin of the mental faculties of man, both of these authors anticipating Spencer, the doctrine being an old one, and expressed by De Maillet.

The statement of Lamarck's views is full and carefully drawn up, and his preëminence as the founder of modern evolution, though he had no immediate followers, owing to his Cuvierian environment, clearly stated. This being the case, and in view of the fact that the number of Lamarckian evolutionists is now so great and constantly increasing, we should have wished that he had devoted still more space to one of the greatest naturalists of pre-Darwinian times, giving more quotations from his works.

Osborn controverts, and with success, we think, Huxley's dictum that Treviranus should be placed in the same rank as an evolutionist with Lamarck. We certainly do not hear of Treviranians. The statement of the views of Owen is fair, and yet we should scarcely use the word 'hostility' in stating his attitude towards Darwinism or natural selection. Owen refused to attack the *Vestiges of Creation* when that book appeared, but rather sympathized with the general views of its author. As Osborn states, "Owen was an evolutionist in a limited degree," somewhat in the manner of Buffon, and perhaps a shade more from his wide knowledge of paleontology, but it is to be borne in mind that neither was Koelliker nor were others, Darwinians as such, and there are many still who accept the general doctrine of evolution, but do not regard natural selection as an adequate or efficient cause, or at least consider it as only one of many factors.

While mentioning Darwin and Wallace as the leading selectionists no reference is made to the botanist Hooker, who, in his *Flora antarctica* arrived at the doctrine of transformation independently of Darwin, and became one of his two strongest supporters. Also Bates should have been mentioned.

The book should be widely read, not only by science teachers, by biological students, but we hope that historians, students of social science, and theologians will acquaint themselves with this clear, candid and catholic statement of the origin and early history of a theory which not only explains the origin of life-forms, but has transformed the methods of the historian, placed philosophy on a higher plane, and immeasurably widened our views of nature and of the Infinite Power working in and through the universe. A. S. PACKARD.

BROWN UNIVERSITY.

Materials for the Study of Variation.—WILLIAM BATESON.—London and New York, Macmillan & Co., 1894. xv + 597 $6.50.

Over thirty years ago Mr. Darwin outlined the great problems for investigation in natural history, and, one after another, these lines of investigation have been studied by naturalists. Embryology, paleontology and systematic classification early attracted the attention of many naturalists, and these branches of investigation have been very thoroughly studied in the last quarter of a century. Geographical distribution was made a special subject of research by Mr. Wallace and others. These various lines of study, while, of course, they have not been exhausted, have certainly been studied to such an extent that most of the valuable lessons which they teach have been learned. In recent years also another factor of the evolution problem, namely, that of heredity, has been the subject of eager research by various naturalists. It is somewhat strange that the problem of variation has been so universally neglected except by Darwin's *Animals and Plants*. It is upon variations in animals that the whole of the theories of Darwin and all evolutionary doctrines are based, but while the last thirty years has seen much speculation as to variations, both concerning their causes and distribution, while many illustrative instances have been accumulated, while nearly all the modern theories of evolution are based directly upon certain conceptions of variation, there has been no systematic attempt to study this fundamental problem. Speculative zoölogy has always a greater attractiveness to most minds than the more laborious and less entertaining work of collecting facts. The last twenty-five years has seen an abundance of publications upon evolution from theoretical grounds, and while variations themselves have been discussed on both sides of the Atlantic, these discussions have been almost universally based upon a few stock illustrations, and must be recognized as without any proper foundation in facts. Natural science is certainly indebted to Mr. Bateson for having taken up at last this branch of research which lies at the very foundation of the origin of species. Mr. Bateson's book has a very modest title, and the author simply claims to have brought together materials out of which a theory of the origin of species may in the future be built. But this is the only systematic attempt yet made to study variations themselves. The present volume is only the first instalment, and we are promised more in the future. A book of nearly 600 pages, filled with numerous illustrations, describing in more or less detail variations of all kinds, in all types of animals, will certainly find its way into the library of every naturalist who has any interest in speculative thought.

A review of this character is hardly a fitting place to discuss the subjects presented in this work. In reading over its pages there are, however, three or four striking conclusions of so much general theoretical importance that they may be selected as the teachings of this first volume. Most prominent among them stands the deduction of the author that variations are discontinuous. It is the theory of Darwin, and, in general, of his followers, that species were produced by natural selection acting upon slight continuous variations. The difficulties of this thought were plain to Mr. Darwin, and have become more plain and more forcible as the years have passed. While the followers of Darwin's views have tried to shut their eyes to them and have tried to explain away the objections that have arisen, it has been plain to every thinking naturalist that the natural selection of minute accidental variations is entirely inadequate to accomplish the great end of producing species. The most important result of Mr. Bateson's study of variations is that the variations that occur in animals are not minute and continuous, or, rather, that they are frequently discontinuous. By this term the author means that variations may be sudden and extreme in character, such as the sudden development of a new tooth in a single generation, or the appearance of a new leg, or some other very prominent characteristic which appears at once without the numerous intermediate stages which Mr. Darwin's theory assumes. While Mr. Bateson does not claim that this view is demonstrated by the facts now collected, he does insist that all of his data point in that direction. The extreme significance of this conclusion upon the question of the origin of species is plain at once. A second conclusion which one reaches in the perusal of these instances is that variations are not haphazard, but,while, of course, they cannot be predicted with certainty, they do fall under certain definite laws. Mr. Bateson has found it possible to group the variations that occur in animals under very definite classes, so definite that, in many cases, at least, it is impossible to question that they are regulated by some organic law. Of course, Mr. Darwin recognized that variations had their causes, but, nevertheless, he was inclined to believe that they were 'par hazard.' According to the conclusions of Mr. Bateson, however, they are of a more or less definite nature. Incidentally also Mr. Bateson points out that the study of variation gives us a new conception of homology, and almost deprives us of the belief in the long recognized law of reversion. It is somewhat surprising to be called upon to abandon the law of reversion, and perhaps the author does not deny that it may be a factor in development, but he does claim most of the instances so explained have nothing to do with this principle. It is not possible here to dwell further upon the many suggestive facts which are brought out by this study.

In criticism one may say that the English is extremely poor. The subject, of course, is a difficult one, and the author is obliged to use a new terminology and to explain his principles as he progresses. This in itself renders the book somewhat obscure, but we must add to this the fact that in many cases his sentences are very involved and cumbersome, and altogether the work is difficult reading. We may also somewhat regret that the author does not weave into the work a few more suggestions as to the significance of some of the facts that he has treated. The great part of this work reads like a museum catalogue, and museum catalogues are much more intelligible if one understands the basis of classification. Mr. Bateson, however, distinctly states that he does not consider the evidence as yet sufficient to warrant conclusions except in regard to some few general subjects. One may also question if most of his material does not savor too strongly of abnormal, and, indeed, almost pathological variations, to fairly serve as a basis for a theory of the origin of species. But, in spite of one or two such minor criticisms, the book of Mr. Bateson is an extremely valuable addition to zoölogical literature, and when it is completed by subsequent volumes upon variations of different nature it is hardly possible to doubt that it will be one of the few valuable and lasting additions to the literature on the general subject of the evolution of organic nature.

H. W. Conn.

Wesleyan University.

Grundriss der Ethnologischen Jurisprudenz.—ALBERT HERMANN POST.—Two Vols.—Oldenburg and Leipzig, 1895.

Ethnologische Studien zur Ersten Entwicklung der Strafe.—S. R. STEINMETZ.—Two Vols.—Leiden and Leipzig, 1894.

In these two carefully prepared and thoroughly reasoned works we have for the first time an unbiased application of the facts furnished by ethnology to an analysis of the evolution of jurisprudence. The study of them will prove of the greatest profit to the advocate, the anthropologist and the philosophic student of the growth of society.

Dr. Steinmetz, in his over 900 large octavo pages devoted to the subject, pursues the idea of punishment through all the forms under which it appears in early conditions, such as personal revenge, blood feuds, compounding of offences, family, totemic and social punishment, the vengeance of the gods, and religious chastisement. The foundation for this historic analysis is laid in the earlier pages of the first volume by an able excursus on the psychological motives which underlie the thirst for vengeance and the passion for cruelty. This furnishes a philosophic basis on which the author constructs his conclusions by an inductive study of all the forms of punishment and penalty found in primitive and early peoples. With this he is contented, and with a temperance worthy of high commendation, he refrains from committing his work to one or another 'school' by applying it to the defence of some pet doctrine of popular sociology, which would at once limit its usefulness. He rather says: "Here are the psychic motives; and here are the results to which under various conditions they have given rise. Let the facts present their own inferences."

This impartial spirit also thoroughly pervades the more comprehensive study of Dr. Post. It is considerably over a thousand pages in length and is an exhaustive analysis of the whole notion of rights, of the person, the family, the clan and the state, as they apply to both persons and things. In the second volume he traverses in his investigation of penalties much of the ground occupied by Dr. Steinmetz, and a comparison of their methods and results is quite interesting. The author's reading is immense, and the care with which he cites his authorities is most praiseworthy. While fully aware of the distinctly philosophic nature of his subject,—for a people's abstract conceptions of ethics are embodied in their concrete forms of laws,—he withstands the temptation to theorize on these points and keeps himself strictly within the limits of objective and inductive inquiry.

Of both these works it may be said that they represent the purest scientific method, and that they stand in the front rank of the contributions to Ethnology in its true sense which have appeared of late years.

D. G. B.

Flora of Nebraska.—Edited by members of the Botanical Seminar of the University of Nebraska.—Introduction and Part 1., *Protophyta-Phycophyta;* Part 2, *Coleochætaceæ, Characeæ.*—Lincoln, Nebraska, Published by the Seminar, 1894. 4to, pp. 123, pl. 36.

The beautiful work here noticed must long hold first place in the published results of the exploration and study of a local flora. It is hard to find words in which to express our gratification at its appearance, and we have tried in vain to find any point which is fairly open to adverse criticism. Beginning with a synopsis of the larger groups, including families, and an introduction contributed by Professor Bessey, in the details of which there is room for much difference of opinion, there follow concise descriptions of the classes, orders, families, genera, species and varieties of Protophyta and Phycophyta found within the State, contributed by Mr. DeAlton Saunders, and of the Coleochætaceæ and Characeæ by Mr. Albert F. Woods. The descriptions are well drawn, the typography excellent and the plates accurate and well executed. We tender our cordial congratulations to all concerned in the production of the book and to all who may have opportunity to use it.

N. L. B.

NOTES.

THE SCIENTIFIC SOCIETIES.

The programs of the mid-winter meetings of the several scientific societies promise large attendance and many important papers. The American Society of Naturalists meets at Johns Hopkins University, Baltimore, and in conjunction with it the American Morphological Society and the American Physiological Society. At the same place and time the American Society of Geologists meets. During the same week the Anatomists meet at Columbia College, New York; the American Psychological Association meets at Princeton; the American Folklore Society meets at Washington, and the annual meeting of the American Mathematical Society is held at Columbia College. These meetings will be fully reported in Science.

PHYSICS.

Actual trial trips with flying machines have recently been made by Mr. Maxim and Prof. Langley. Mr. Maxim's machine was fastened to rails to prevent its rising, and sailed 500 feet at the rate of 45 miles per hour. Prof. Langley's æroplane was allowed to fly over the water at Quantico, Md., on December 8th. Both Mr. Maxim and Prof. Langley use light steam engines in preference to storage batteries.

The Société Internationale des Électriciens established a central laboratory at Paris about seven years ago. The principal object of the laboratory was the preservation of electrical standards, and to afford practical electricians an opportunity for testing their various instruments. It is evident that such a laboratory offers special advantages for the investigation of questions belonging to the science and industry of electricity. These facilities have been to some extent utilized; but, in order to increase the usefulness of the institution, the Society has added to it a School of Applied Electricity. This school, which will be opened on December 3d, has been constructed on a plot of land granted by the city of Paris, the funds for the building having been raised by private subscription. Purely practical instruction will be given at the school. There will be two chief courses, one dealing with the industrial applications of electricity, and the other with electrometry. It is hoped that the school will be a training ground for higher work in the Central Laboratory, to which it is attached.—*Nature.*

ANTHROPOLOGY.

Dr. Charles L. Dana's address on *Degeneration and its Stigmata*, delivered at the Anniversary Meeting of the New York Academy of Medicine, Nov. 28, 1894, has been printed in the *Medical Record*, of Dec. 15th. Dr. Dana traces with much skill the historic development of the scientific method that discovers mental traits and especially mental degenerations from their physical manifestations.

The charges made against the management of the Elmira Reformatory have been dismissed by Governor Flower. The majority of the commissioners who examined the charges report that the institution stands preëminent among the reformatories of the world and that its success in the reformation of criminals has been extraordinary. This confirms the views of the leading criminologists and reformers.

EDUCATIONAL.

DR. J. K. TALMAGE has been called to the professorship of geology recently established in the University of Utah.

AMERICA has accomplished much for the advancement of Anthropology, but the work has been largely done by the Government institutions and by individuals. Columbia College offers this year courses in Anthropology (Dr. Farrand and Dr. Ripley), and the University of California must now be added to the institutions proposing courses in this subject.

THE Universities of Oxford and of Cambridge have recently taken action of considerable interest to Americans proposing to study abroad. The comparatively few Americans who have been in residence at Oxford or Cambridge would undoubtedly agree in recommending this course to others as highly as studying at a German university. But hitherto degrees could only be obtained by undergoing very irksome examinations. Oxford will now confer the degrees Litt. B. and Sc. B. on evidence of 'a good general education,' and research work evincing 'a high standard of merit.' Three years' residence is required, but this condition may be modified. The grace adopted at Cambridge is as follows: "That a syndicate be appointed to consider: (1) the means of giving further help and encouragement to persons who desire to pursue courses of advanced study or research within the University; (2) what classes of students should be admitted to such courses; (3) what academic recognition, whether by degrees or otherwise, should be given to such students, and upon what conditions; that the syndicate be empowered to consult and confer with such persons and bodies as they may think fit; and that they report to the Senate before the end of the Lent Term, 1895."

THE fourth edition of *Minerva* (1894–1895) presents as frontispiece an etched portrait of Lord Kelvin by Herkomer. The book now extends to 930 pages, an increase of 69 pages over the preceding edition, many new institutions having been included. The American universities and colleges added in this edition are Bryn Mawr, Cincinnati, Colgate, Massachusetts Institute of Technology, Nebraska, Ohio Wesleyan, Vermont, Wellesley, Western Reserve, making the total number thirty-nine. In attendance of students the order of the great universities is Paris, Berlin, Madrid, Vienna, Naples, Moscow, Budapest, Munich, Athens, Oxford, Harvard. But in many of these institutions attendance on popular lectures seems to be included.

A WORK with the range of *Minerva*, giving the courses as well as the instructors in institutions of learning, would be of much use, but a difficult undertaking. The need has, however, been supplied for the different institutions of Paris by *Le livret de l'étudiant de Paris* (Delalain Frère 1894–95), prepared under the direction of the general council of the faculties.

FORTHCOMING BOOKS.

DR. DANIEL G. BRINTON, Professor of American Archæology in the University of Pennsylvania, has in press a *Primer of Mayan Hieroglyphics*, to be published by Ginn & Co., Boston, in which he aims to explain the elements of the mysterious writing on the monuments of Central America.

GINN & Co. also announce a series of handbooks on the *History of Religions*, edited by Prof. Morris Jastrow, Jr., of the University of Pennsylvania. *The Religions of India*, by Prof. E. W. Hopkins, of Bryn Mawr, will form the first volume.

MACMILLAN & Co. announce *The Principles of Sociology*, by Prof. Franklin H. Giddings, of Columbia College; *Monism, The Confession* *of Faith of a Man of Science*, by Dr. Ernst Haeckel; *Life at the Zoo*, by C. J. Cornish; a new edition of S. Thompson's *Electricity and Magnetism* and *Mental Development in the Child and in the Race*, by J. Mark Baldwin.

SOCIETIES AND ACADEMIES.

THE NEW YORK ACADEMY OF SCIENCES.

J. A. Matthews, *Notes on Carborundum.*

Bashford Dean, *On the collections of Fossil Fishes at Berea, New London and Delaware, Ohio.*

L. McI. Luquer, *The Relative effects of Frost and Sulphate of Soda Efflorescence as shown by Tests of Building Stone.*

J. F. Kemp, *Secretary.*

THE TORREY BOTANICAL CLUB.

B. D. Halsted, *Abnormalities in Plants Due to Fungus Parasites.*

Henry H. Rusby, *Secretary.*

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Ludwig Gutmann, *On the Production of Rotary Magnetic Fields by a Single Alternating Current.*

In the absence of the author, the paper will be presented in abstract by Dr. M. I. Pupin.

A meeting of Western members will be held the same evening at Chicago, where the paper will be read by the author.

Ralph W. Pope, *Secretary.*

SCIENTIFIC JOURNALS.

THE AMERICAN NATURALIST.

Quarternary Time Divisible in Three Periods, the Lafayette, Glacial, and Recent: Warren Upham.

The Homologies of the Uredinea (The Rusts): Charles E. Bessey.

On the Evolution of the Art of Working in Stone; a preliminary paper by J. D. McGuire: Charles H. Read.

Zoölogy in the High School: Clarence M. Weed.

Editorials; Recent Books and Pamphlets; Recent Literature.

General Notes:—Petrography; Geology and Paleontology; Botany; Zoölogy; Entomology; Archeology and Ethnology.

Proceedings of Scientific Societies.

THE PHYSICAL REVIEW.

Frontispiece: portrait of Professor von Helmholtz.

Studies of the Lime Light: Edward L. Nichols and Mary L. Crehore.

A Study of the Residual Charges of Condensers and their Dependence upon Temperature: Frederick Bedell and Carl Kinsley.

A General Theory of the Glow-Lamp II.: H. S. Weber.

Minor Contributions; Notes; New Books.

BULLETIN OF THE AMERICAN MATHEMATICAL SOCIETY.

On the Group of Holoedric Transformation of a Given Group into Itself: E. Hastings Moore.

On the Non-Primitive Substitution Groups of Degree Ten: G. A. Miller.

Briefer Notices; Notes; New Publications.

NEW BOOKS.

Popular Lectures and Addresses. Vol. II., *Geology and General Physics.* Sir W. Thompson. London and New York. Macmillan & Co. 1894. Pp. x., 599.

Light. Elementary text-book; theoretical and practical. R. T. Glazebrook. Cambridge University Press. New York. Macmillan & Co. 1894. Pp. vii., 213. $1.00.

Manual of Physico-Chemical Measurements. By Wilhelm Ostwald. Trans. by James Walker. London and New York. Macmillan & Co. 1894. Pp. xii., 255. $2.25.

Electricity One Hundred Years Ago and To-day. Edwin J. Houston. New York. W. J. Johnson & Co. 1894. Pp. 199. $1.00.

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Subscriptions (five dollars annually) and advertisements should be sent to the Publisher of SCIENCE, 41 East 49th St., New York.

*ON THE MAGNITUDE OF THE SOLAR SYSTEM.**

NATURE may be studied in two widely different ways. On the one hand we may employ a powerful microscope which will render visible the minutest forms and limit our field of view to an infinitesimal fraction of an inch situated within a foot of our own noses; or on the other hand, we may occupy some commanding position and from thence, aided by a telescope, we may obtain a comprehensive view of an extensive region. The first method is that of the specialist, the second is that of the philosopher, but both are necessary for an adequate understanding of nature. The one has brought us knowledge wherewith to defend ourselves against bacteria and microbes which are among the most deadly enemies of mankind, and the other has made us acquainted with the great laws of matter and force upon which rests the whole fabric of science. All nature is one, but for convenience of classification we have divided our knowledge into a number of sciences which we usually regard as quite distinct from each other. Along certain lines, or more properly, in certain regions, these sciences necessarily abut on each other, and just there lies the weakness of the specialist. He is like a wayfarer who always finds obstacles in crossing the boundaries between two countries, while to the traveler who gazes over them from a commanding eminence the case is quite different. If the boundary is an ocean shore there is no mistaking it; if a broad river or a chain of mountains it is still distinct; but if only a line of posts traced over hill and dale, then it becomes lost in the natural features of the landscape, and the essential unity of the

*Part of the Address delivered before the American Association for the Advancement of Science at its Brooklyn meeting, August 16, 1894, by the retiring President, Professor Harkness, and reprinted with his permission.

whole region is apparent. In that case the border land is wholly a human conception of which nature takes no cognizance, and so it is with the scientific border land to which I propose to invite your attention this evening.

To the popular mind there are no two sciences further apart than astronomy and geology. The one treats of the structure and mineral constitution of our earth, the causes of its physical features and its history, while the other treats of the celestial bodies, their magnitudes, motions, distances, periods of revolution, eclipses, order, and of the causes of their various phenomena. And yet many, perhaps I may even say most of the apparent motions of the heavenly bodies are merely reflections of the motions of the earth, and in studying them we are really studying it. Furthermore, precession, nutation and the phenomena of the tides depend largely upon the internal structure of the earth, and there astronomy and geology merge into each other. Nevertheless the methods of the two sciences are widely different, most astronomical problems being discussed quantitatively by means of rigid mathematical formulæ, while in the vast majority of cases the geological ones are discussed only qualitatively, each author contenting himself with a mere statement of what he thinks. With precise data the methods of astronomy lead to very exact results, for mathematics is a mill which grinds exceeding fine; but, after all, what comes out of a mill depends wholly upon what is put into it, and if the data are uncertain, as is the case in most cosmological problems, there is little to choose between the mathematics of the astronomer and the guesses of the geologist.

If we examine the addresses delivered by former presidents of this Association, and of the sister—perhaps it would be nearer the truth to say the parent—Association on the other side of the Atlantic, we shall find that they have generally dealt either with the recent advances in some broad field of science, or else with the development of some special subject. This evening I propose to adopt the latter course, and I shall invite your attention to the present condition of our knowledge respecting the magnitude of the solar system, but in so doing it will be necessary to introduce some considerations derived from laboratory experiments upon the luminiferous ether, others derived from experiments upon ponderable matter, and still others relating both to the surface phenomena and to the internal structure of the earth, and thus we shall deal largely with the border land where astronomy, physics and geology merge into each other.

The relative distances of the various bodies which compose the solar system can be determined to a considerable degree of approximation with very crude instruments as soon as the true plan of the system becomes known, and that plan was taught by Pythagoras more than five hundred years before Christ. It must have been known to the Egyptians and Chaldeans still earlier, if Pythagoras really acquired his knowledge of astronomy from them as is affirmed by some of the ancient writers, but on that point there is no certainty. In public Pythagoras seemingly accepted the current belief of his time, which made the earth the center of the universe, but to his own chosen disciples he communicated the true doctrine that the sun occupies the center of the solar system, and that the earth is only one of the planets revolving around it. Like all the world's greatest sages, he seems to have taught only orally. A century elapsed before his doctrines were reduced to writing by Philolaus of Crotona, and it was still later before they were taught in public for the first time by Hicetas, or, as he is sometimes called, Nicetas, of Syracuse. Then the familiar cry of impiety was raised, and the Pythagorean system was eventually suppressed by that now called the Ptolemaic, which held the field until it was overthrown by Copernicus, almost two thousand years later. Pliny tells us that Pythagoras believed the distances to the sun and moon to be respectively 252,000 and 12,600 stadia, or taking the stadium at 625 feet, 29,837 and 1,492 English miles; but there is no record of the method by which these numbers were ascertained.

After the relative distances of the various planets are known, it only remains to determine the scale of the system, for which purpose the distance between any two planets suffices. We know little about the early history of the subject, but it is clear that the primitive astronomers must have found the quantities to be measured too small for detection with their instruments, and even in modern times the problem has proved to be an extremely difficult one. Aristarchus of Samos, who flourished about 270 B. C., seems to have been the first to attack it in a scientific manner. Stated in modern language, his reasoning was that when the moon is exactly half full, the earth and sun as seen from its center must make a right angle with each other, and by measuring the angle between the sun and moon, as seen from the earth at that instant, all the angles of the triangle joining the earth, sun and moon would become known, and thus the ratio of the distance of the sun to the distance of the moon would be determined. Although perfectly correct in theory, the difficulty of deciding visually upon the exact instant when the moon is half full is so great that it cannot be accurately done even with the most powerful telescopes. Of course Aristarchus had no telescope, and he does not explain how he effected the observation, but his conclusion was that at the instant in question the distance between the centers of the sun and moon, as seen from the earth, is less than a right angle by $\frac{1}{30}$ part of the same. We should now express this by saying that the angle is 87 degrees, but Aristarchus knew nothing of trigonometry, and in order to solve his triangle, he had recourse to an ingenious, but long and cumbersome geometrical process which has come down to us, and affords conclusive proof of the condition of Greek mathematics at that time. His conclusion was that the sun is nineteen times further from the earth than the moon, and if we combine that result with the modern value of the moon's parallax, viz.: 3,422.38 seconds, we obtain for the solar parallax 180 seconds, which is more than twenty times too great.

The only other method of determining the solar parallax known to the ancients was that devised by Hipparchus about 150 B. C. It was based on measuring the rate of decrease of the diameter of the earth's shadow cone by noting the duration of lunar eclipses, and as the result deduced from it happened to be nearly the same as that found by Aristarchus, substantially his value of the parallax remained in vogue for nearly two thousand years, and the discovery of the telescope was required to reveal its erroneous character. Doubtless this persistency was due to the extreme minuteness of the true parallax, which we now know is far too small to have been visible upon the ancient instruments, and thus the supposed measures of it were really nothing but measures of their inaccuracy.

The telescope was first pointed to the heavens by Galileo in 1609, but it needed a micrometer to convert it into an accurate measuring instrument, and that did not come into being until 1639, when it was invented by Wm. Gascoigne. After his death in 1644, his original instrument passed to Richard Townley who attached it to a fourteen foot telescope at his residence in Townley, Lancashire, England, where it was used by Flamsteed in observing the diurnal parallax of Mars during its opposition in 1672.

A description of Gascoigne's micrometer was published in the Philosophical Transactions in 1667, and a little before that a similar instrument had been invented by Auzout in France, but observatories were fewer then than now, and so far as I know J. D. Cassini was the only person beside Flamsteed who attempted to determine the solar parallax from that opposition of Mars. Foreseeing the importance of the opportunity, he had Richer dispatched to Cayenne some months previously, and when the opposition came he effected two determinations of the parallax; one being by the diurnal method, from his own observations in Paris, and the other by the meridian method from observations in France by himself, Römer and Picard, combined with those of Richer at Cayenne. This was the transition from the ancient instruments with open sights to telescopes armed with micrometers, and the result must have been little short of stunning to the seventeenth century astronomers, for it caused the hoary and gigantic parallax of about 180 seconds to shrink incontinently to ten seconds, and thus expanded their conception of the solar system to something like its true dimensions. More than fifty years previously Kepler had argued from his ideas of the celestial harmonies that the solar parallax could not exceed 60 seconds, and a little later Horrocks had shown on more scientific grounds that it was probably as small as 14 seconds, but the final death-blow to the ancient values ranging as high as two or three minutes came from these observations of Mars by Flamsteed, Cassini and Richer.

Of course the results obtained in 1672 produced a keen desire on the part of astronomers for further evidence respecting the true value of the parallax, and as Mars comes into a favorable position for such investigations only at intervals of about sixteen years, they had recourse to observations of Mercury and Venus. In 1677 Halley observed the diurnal parallax of Mercury, and also a transit of that planet across the sun's disk, at St. Helena, and in 1681 J. D. Cassini and Picard observed Venus when she was on the same parallel with the sun, but although the observations of Venus gave better results than those of Mercury, neither of them was conclusive, and we now know that such methods are inaccurate even with the powerful instruments of the present day. Nevertheless, Halley's attempt by means of the transit of Mercury ultimately bore fruit in the shape of his celebrated paper of 1716, wherein he showed the peculiar advantages of transits of Venus for determining the solar parallax. The idea of utilizing such transits for this purpose seems to have been vaguely conceived by James Gregory, or perhaps even by Horrocks, but Halley was the first to work it out completely, and long after his death his paper was mainly instrumental in inducing the governments of Europe to undertake the observations of the transits of Venus in 1761 and 1769, from which our first accurate knowledge of the sun's distance was obtained.

Those who are not familiar with practical astronomy may wonder why the solar parallax can be got from Mars and Venus, but not from Mercury, or the sun itself. The explanation depends on two facts. Firstly, the nearest approach of these bodies to the earth is for Mars 33,870,000 miles, for Venus 23,654,000 miles, for Mercury 47,935,000 miles and for the sun 91,239,000 miles. Consequently, for us Mars and Venus have very much larger parallaxes than Mercury or the sun, and of course the larger the parallax the easier it is to measure. Secondly, even the largest of these parallaxes must be determined within far less than one-tenth of a second of the truth, and while that degree of accuracy is possible in measuring short arcs, it is quite unattainable in long ones. Hence one of the most essential conditions for the successful measurement of parallaxes is that we shall be able to compare the place of the near body with that of a more distant one situated in the same region of the sky. In the case of Mars that can always be done by making use of a neighboring star, but when Venus is near the earth she is also so close to the sun that stars are not available, and consequently her parallax can be satisfactorily measured only when her position can be accurately referred to that of the sun, or, in other words, only during her transits across the sun's disk. But even when the two bodies to be compared are sufficiently near each other, we are still embarrassed by the fact that it is more difficult to measure the distance between the limb of a planet and a star or the limb of the sun than it is to measure the distance between two stars, and since the discovery of so many asteroids, that circumstance has led to their use for determinations of the solar parallax. Some of these bodies approach within 75,230,000 miles of the earth's orbit, and as they look precisely like stars, the increased accuracy of pointing on them fully makes up for their greater distance, as compared with Mars or Venus.

After the Copernican system of the world and the Newtonian theory of gravitation were accepted it soon became evident that trigonometrical measurements of the solar parallax might be supplemented by determinations based on the theory of gravitation, and the first attempts in that direction were made by Machin 1729 and T. Mayer in 1753. The measurement of the velocity of light between points on the earth's surface, first effected by Fizeau in 1849, opened up still other possibilities, and thus for determining the solar parallax we now have at our command no less than three entirely distinct classes of methods which are known respectively as the trigonometrical, the gravitational and the photo-tachymetrical. We have already given a summary sketch of the trigonometrical methods, as applied by the ancient astronomers to the dichotomy and shadow cone of the moon, and by the moderns to Venus, Mars and the asteroids, and we shall next glance briefly at the gravitational and photo-tachymetrical methods.

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The theory of probability and uniform experience alike show that the limit of accuracy attainable with any instrument is soon reached; and yet we all know the fascination which continually lures us on in our efforts to get better results out of the familiar telescopes and circles which have constituted the standard equipment of observatories for nearly a century. Possibly these instruments may be capable of indicating somewhat smaller quantities than we have hitherto succeeded in measuring with them, but their limit cannot be far off because they already show the disturbing effects of slight inequalities of temperature and other uncontrollable causes. So far as these effects are accidental they eliminate themselves from every long series of observations, but there always remains a residuum of constant error, perhaps quite unsuspected, which gives us no end of trouble. Encke's value of the solar parallax affords a fine illustration of this. From the transits of Venus in 1761 and 1769 he found 8·58 seconds in 1824, which he subsequently corrected to 8·57 seconds, and for thirty years that value was universally accepted. The first objection to it came from Hansen in 1854, a second followed from Le Verrier in 1858, both based upon facts connected with the lunar theory, and eventually it became evident that Encke's parallax was about one-quarter of a second too small. Now please observe that Encke's value was obtained trigonometrically, and its inaccuracy was never suspected until it was revealed by gravitational methods which were themselves in error about one-tenth of a second and required subsequent correction in other ways. Here then was a lesson to astronomers who are all more or less specialists, but it merely enforced the perfectly well known principle that the constant errors of any one method are accidental errors with respect to all other methods, and therefore the readiest way of eliminating them is by combining the results from as many different methods as possible. However, the abler the specialist the more certain he is to be blind to all methods but his own, and astronomers have profited so little by the Encke-Hansen-Le Verrier incident of thirty-five years ago that to-day they are mostly divided into two great parties, one of whom holds that the parallax can be best determined from a combination of the constant of aberration with the velocity of light, and the other believes only in the results of heliometer measurements upon asteroids. By all means continue the heliometer measurements, and do everything possible to clear up the mystery which now surrounds the constant of aberration, but why ignore the work of predecessors who were quite as able as ourselves? If it were desired to determine some one angle of a triangulation net with special exactness, what would be thought of a man who attempted to do so by repeated measurements of the angle in question while he persistently neglected to adjust the net? And yet, until recently astronomers have been doing precisely that kind of thing with the solar parallax. I do not think there is any exaggeration in saying that the trustworthy observations now on record for the determination of the numerous quantities which are functions of the parallax could not be duplicated by the most industrious astronomer working continuously for a thousand years. How then can we suppose that the result properly deducible from them can be materially affected by anything that any of us can do in a lifetime, unless we are fortunate enough to invent methods of measurement vastly superior to any hitherto imagined? Probably the existing observations for the determination of most of these quantities are as exact as any that can ever be made with our present instruments, and if they were freed from constant errors they would certainly give results very near the truth. To that end we have only to form a system of simultaneous equations between all the observed quantities, and then deduce the most probable values of these quantities by the method of least squares. Perhaps some of you may think that the value so obtained for the solar parallax would depend largely upon the relative weights assigned to the various quantities, but such is not the case. With almost any possible system of weights the solar parallax will come out very nearly $8{\cdot}809'' \pm 0{\cdot}0057''$, whence we have for the mean distance between the earth and sun 92,797,000 miles with a probable error of only 59,700 miles; and for the diameter of the solar system, measured to its outermost member, the planet Neptune, 5,578,400,000 miles.

William Harkness.

Washington.

THE BALTIMORE MEETING OF THE AMERICAN SOCIETY OF NATURALISTS.

The thirteenth annual meeting of *The American Society of Naturalists* was held at Baltimore during the Christmas vacation. Considering that Baltimore is the southern limit where meetings may be held by the Society, the attendance was large, forty to fifty members being present.

The first session was called to order by the President, Professor Charles S. Minot of the Harvard Medical School, at 2 p. m. on Thursday, December 27th.

A quorum being present, the Society at once proceeded to the transaction of business. The committee appointed in 1893 to obtain, if possible, the removal of the duty on scientific instruments reported that although they had succeeded in obtaining the coöperation of most of the leading scientific men, yet the inception of the movement had been so delayed that the Gorman Bill was already being considered by the Senate before the petitions could be presented to the House.

The following resolution recommended by the committee was then adopted: "Inasmuch as the repeal of the present iniquitous duty on scientific instruments is imperatively needed by the interests of the country, we recommend that a committee be appointed to present our just demands to the President, to the Chairman of the Committee on Finance of the Senate and the Chairman of the Committee of Ways and Means of the House of Representatives, and to take such other steps as may be practicable to secure the immediate repeal of the duty."

The report of the committee on the revision of the Constitution and By-Laws was unanimously adopted. By the new constitution *The American Society of Naturalists* encourages the formation of other societies of similar name and object in other parts of the country and invites other societies whose chief object is the encouragement of the study of Natural History to become affiliated with it. The affiliated societies shall have a common place and time of meeting with the American Society of Naturalists, the expenses of which are to be paid from a common treasury supplied from a common fee. The records of the secretaries of the different societies are also to be published at common expense.

The discussion upon *Environment in its Influence upon the Successive Stages of Development and as a Cause of Variation*, took place in the Physical Lecture Room, Thursday afternoon. It was opened by four papers and followed by remarks by Professors Cope and Hyatt, Dr. Dall, Dr. C. V. Riley and others.

Professor Osborn, of Columbia College, in opening the discussion, observed that naturalists were reacting from the discussion of *theories* towards the renewed inductive and experimental study of the factors of Evolution. This was due to the feeling that the prolonged discussion led by Spencer and Weismann had assumed a largely deductive character and would not lead to any permanent results. The inductive reaction had taken two directions: first towards the exact study of Variation, and second towards experimental Evolution. As regards Variation we should not expect to form any laws so long as variations were considered *en masse* without regard to the past and present history of the organisms studied. That organisms vary with their environment is a truism. What we need is a clearer conception and interpretation of this relation as a basis for experimental study in the laboratory and in the field. The first misconception to be removed is that which has sprung up from the misuse of the terms Heredity and Variability. Nägeli pointed out many years ago as Weismann and Hurst have insisted more recently that Heredity includes one phenomenon seen from two sides which may conveniently be termed Repetition and Variation. A large number of the variations recorded by Bateson, for example, are simple repetitions of ancestral structure, and every new variation is to be regarded as the expression of hereditary forces working under new conditions. The first object of investigation is to decide the *time of origin* of a variation, first in race history, second in individual history. Variations which arise as practical repetitions of past experience may conveniently be termed '*palingenic*,' while those which are new to the organism may be termed '*cenogenic*.' As regards individual history the most important question is to determine whether a variation is merely '*ontogenic*,' that is springing up in the course of individual development from some disturbance of the hereditary mechanism, or '*phylogenic*' and constant as distinguished by Nägeli. From recent study of palingenic variation we must recast our conception of Heredity especially in view of the remarkable researches of Cunningham upon the color, and of Agassiz, Giard and Filhol upon the symmetry of the flat fishes (Pleuronectidæ). These characters of enormous antiquity, summoned as it were from the vasty deep, reveal the law that repetition or variation in ontogeny depend largely upon repetition or variation in environment, that for many of the most fundamental characters, development and environment are inseparable and all theories which tend to separate the two are untenable. As regards cenogenic variations or those which are new in the experience of the organism, the distinction between ontogenic variations, or what are commonly called acquired characters, and phylogenic variations is also of pressing importance. The organism may be compared to a clock, keeping regular time upon a base; if the base is tilted slightly the clock may continue to tick but it may not keep the same time; if after the lapse of a long period the base is restored to its original position the clock will tick in correct time as before. This thought shows that the conditions which have been demanded as crucial tests of the permanent phylogenic influence of environment upon organisms will be very difficult to fulfill in experiment—when the repetition of a mesozoic environment is found to produce a repetition of a mesozoic structure. Experiment should now be directed separately upon each of the four stages of development (germ cell, fertilization, embryonic, larval and adult) and then withdrawn, and putting together the results of all the work which has been recently done of this kind we find three classes of variation phenomena coming to the surface; first 'palingenic variations,' second 'saltations,' third 'ontogenic adaptions' (Haeckel); fourth a class of 'phylogenic variations' which have been termed 'mutations' by some paleontologists. We are so far from a solution of the working causes of these four classes of variation that it seems best to consider that we are on the threshold of the Evolution Problem, to take an entirely agnostic or doubtful position as to all the prevalent theories, and press forward in strictly inductive search for laws which may not be forthcoming until the next century.

Professor Edmund B. Wilson, of Columbia College, followed with a discussion of the influence of the environment on the early stages of embryonic development. That a change of external conditions, such as temperature, chemical nature of the medium and the like, causes changes in the rate or form of development has long been a familiar fact, but we have only recently come to perceive clearly how significant are the changes thus brought about and how vital is the part played by the environment in all development, whether pathological or normal. For if a changed mode of development is the 'result' of a change of environment, the normal development must in exactly the same sense be the 'result' of the normal environment, *i. e.*, in both cases we are dealing with a definite physiological response of the idioplasm to external conditions. The facts both of normal and of experimental embryology demonstrate the justness of this point of view. The experiments of Pflüger, Driesch, Roux and others show, for instance, that the forms of cleavage may be profoundly altered by mechanical means, and indicate that some of the normal fundamental cleavage-forms are the direct result of mechanical conditions, such as the shape of the egg, pressure of the membranes, surface tensions between the blastomeres, and the like. Temperature has a profound effect not only on the rate of development, but also on its form. Thus Driesch showed that the eggs of sea-urchins when incubated at a temperature slightly above the normal undergo remarkable changes. The form of cleavage may be considerably altered (without affecting the end result of development), and the gastrulation may be profoundly affected. In some cases 'exogastrulæ' are formed, the archenteron being turned out instead of in, and these undergo all the normal differentiations of the *Pluteus*, though they ultimately perish since the alimentary canal is turned inside out and the larvæ are incapable of taking food. Other physical agents such as gravity have been shown to have a profound effect on development, determining the position of roots and branches in hydroids (Loeb), or even the polar differentiation of the egg as in the frog (Pflüger, Born, etc.).

The most remarkable and significant examples of environmental influence are, however, found in the effect of change in the chemical environment. In the case of sea-urchins Ponchet and Chabry found that in sea-water deprived of calcareous matter the *Pluteus* larva is unable to develop its spicular skeleton, and Herbst showed that the same result was produced by a very slight excess of potassium chloride in the water even though the normal amount of calcareous matter were present. In both cases the larvæ not only fail to develop spicules, but are unable to produce the characteristic ciliated arms. Thus arises a larva having a simple ciliated belt and very similar to a young *Tórnaria*. This is a very instructive case; for it shows in the first place that a definite character (formation of the skeleton) has a fundamental though very subtle relation to the external environment, and in the second place, that this relation indirectly extends to other characters (ciliated arms) that follow upon the development of the first character. Such cases pave the way to a rational conception of epigenesis, by showing the multiplication of effects in ontogeny and the complicated results that may follow from a single and apparently insignificant condition of the environment.

Even more striking results are those obtained (by Herbst) by the addition of a minute percentage of lithium chloride to the sea-water. The primary result is to cause exogastrulation (like the effect of raised temperature). Beyond this, however, the entoderm area (*i. e.*, archenteric region) often becomes abnormally large. The entoderm may then be reduced to a mere knob consisting of only a few cells, or may even disappear altogether so that a blastula is formed that *consists entirely of entoderm!* This extraordinary result, if it can be accepted, shows that even so fundamental a process as the differentiation of the germ-layers stands in a vital relation to the chemical environment. It is a revelation of the importance of environmental influences in development and it shows that we must readjust our conceptions not only of development but also of inheritance, of which development is an expression. Our attention has been focussed too closely upon the formal morphological aspect of development which we have regarded too largely as the result of a pre-organized germ-plasm operating like a machine. Embryological development must be thoroughly reexamined from a physiological point of view, full weight being given to the essential part played by the environment. This point of view in no way sets aside the necessity of assuming a specifically organized germ-plasm for every species as the basis of inheritance. It simplifies the problem, however, and opens the way for further investigation, which is practically barred by the artificial and formal theories of development advocated by Roux and Weismann.

The third paper read was by Professor

W. K. Brooks, of the Johns Hopkins University. The subject was: *An Intrinsic Error in the Theories of Galton and Weismann.* It will be published in full later. The principal point taken was against the theory of variation springing from a mixture of ancestral characters. It was shown that many lines of descent may arise from a very small number of parents and represent a slender thread, consisting of very few strands, many individuals of the same species having an identical remote ancestry. In other words, sexual environment instead of being unlimited is very narrow, and as we pass backwards the number of ancestors increases rapidly for a number of generations, and then decreases instead of increasing indefinitely. The causes of variation are therefore to be sought rather in modern conditions of organisms than in the remote past.

Dr. C. Hart Merriam, of the United States Department of Agriculture, contributed an exhibition and discussion of a beautiful series of mammal and bird types exhibiting protective coloring and a number of dynamic variations. The origin of protective colors is to be sought in fortuitous variation preserved by selection. The theory of the direct action of environment in modifying color as in the bleached types of the desert regions is not borne out by observations and is disproved in the case of nocturnal types. A second and distinct class of facts comes under the head of Dynamic Variation, and to this class we refer to modifications of the beak, of the feet and limbs as due primarily to the habits and activities of the animals themselves.

At the close of the afternoon session, Professor E. B. Wilson, of Columbia College, exhibited by means of the stereopticon, lantern slides, prepared from photographs taken from sections, illustrating the cytological changes during maturation, fecundation, and segmentation. The different effects of the various killing, fixing and staining agents upon the ultimate details of cell-structure, were admirably brought out.

At eight o'clock the Society had the pleasure of listening to Professor William Libbey, of Princeton, who told of his experiences during *Two Months in Greenland.* The lecture was illustrated by a large number of magnificent views of Polar Scenery.

After the lecture the members were entertained by the authorities of the Johns Hopkins University and the citizens of Baltimore at a most pleasant assembly in McCoy Hall.

The Society reassembled at nine o'clock on Friday morning, Dec. 28th.

Officers for the year 1895 were chosen as follows:

President—Professor E. D. Cope, University of Pennsylvania.

Vice Presidents—Professors Wm. Libbey, Jr., Princeton University; W. G. Farlow, Harvard University; C. O. Whitman, Chicago University.

Secretary—Professor H. C. Bumpus, Brown University.

Treasurer—Doctor E. G. Gardiner, Boston, Mass.

Committee-at-Large—Professors E. B. Wilson, Columbia College; W. H. Howell, Johns Hopkins University.

The following persons were elected to membership in the Society:

William Ashmead, U. S. Dept. Agriculture, Washington; Severance Burrage, Mass. Inst. Tech., Boston; W. E. Castle, Harvard University; H. E. Chapin, University of Ohio, Athens, Ohio; J. E. Humphrey, Johns Hopkins University; M. M. Metcalf, Woman's College, Baltimore; H. C. Porter, University of Pennsylvania; W. H. C. Pynchon, Trinity College, Hartford; Charles Schuchert, U. S. National Museum; Norman Wyld, late of Bristol, England.

The report of the Treasurer showing a balance of somewhat over $200 was accepted by the Society.

The Society, on motion of Professor Bumpus, appropriated a sum not to exceed $130 to equip the American table at the Naples Station with proper microtomes, and a committee of three was appointed to attend to this matter.

Professor J. S. Kingsley detailed a 'bibliographical project' originating with Professor G. W. Field of Brown University. This proposes to put into the hands of workers in zoölogy a bibliography of current literature, in such a form as to be readily accessible, the latter to be readily combined with the earlier, and to present the matter both as to subjects and as to authors. By a vote of the Society, a committee of five was appointed to consider this 'project' and to report in print both in SCIENCE and in *The American Naturalist.*

President Gilman, in a very pleasant and cordial way, then welcomed the members of the visiting societies to Baltimore, speaking on behalf of the authorities of the Johns Hopkins University and of the citizens of Baltimore.

President Minot chose for the subject of his address '*The Work of the Naturalist in the World.*' The object of the naturalist is to discover the truth about nature and to publish the results of his work to the world. The conditions of success are readily to be observed. First and foremost is truth. The naturalist's first business is to get at the truth, and the obstacles which stand most prominently in his way are: (1) the limitations of his own abilities, and (2) the limitations of accessories for carrying on his work. The naturalist must observe, experiment and reason, and his training must necessarily be along these lines. Experimentation is necessarily more difficult than observation, for in the former case the naturalist asks why, not how. The great work of the future, as is already being shown, is to be done by the experimenters. Our notion of causation is still in a very rudimentary condition.

Again, the reasoning faculty is one of our weakest points. The naturalist must learn to carefully distinguish between discussion and controversy, and while being led and taught to indulge freely in the former with all the intelligence at his command, he must also be taught to avoid the latter.

The naturalist is naturally exposed to many evils, such as this matter of controversy, which tend to cause him to depart from his proper mission, viz., of getting at the truth. He is especially likely to be led astray by impatience to get results. Preliminary communications are a very great as well as a very prevalent evil. The opinion of the speaker was very pronouncedly adverse to this form of publication. The greed for priority leads many even fine workers far astray.

The tendency to speculate is a third evil, and this has perhaps reached its culmination in the doctrines of Weismann. Another evil is the one which leads us to accept too readily simple and well finished conceptions. Herbert Spencer furnishes us with an illustrious example of the effects of this.

In the matter of publication, four classes may be distinguished: (1.) *Original Memoirs;* (2.) *Handbooks;* (3.) *Text Books;* (4.) *Bibliographies.* The last three are important both in form and in the matter. The first are like digestive organs. It is their function to assimilate crude facts and render them digestible. Advice to prune and digest such matter for publication is much needed. Details not bearing directly upon the subject should be carefully excluded. Most original papers could be 'boiled down' to one half, and some even to one tenth of the amount that is really published. The English write best and this may be owing to the example of Huxley. The Germans and Americans who copy after them come next, and the French are the greatest sinners in the matter of verbiage.

The effect of the work of the naturalist upon his own character is especially shown in his optimism. Literary men seem much inclined to grow pessimistic. This point is well illustrated by a comparison of the recently published letters of Asa Gray and of James Russell Lowell. Lowell's letters show increasing pessimistic views toward the end of his life, while those of Gray remain uniformly optimistic. Something of this was undoubtedly due to the different temperaments of the two men, but much was also due to the different nature of their work. Gray could always see new things unfolding before him.

One drawback in the naturalist's life is his comparative loneliness and isolation. Seldom has he in his own neighborhood another interested in the same particular line as himself. Reunions of naturalist societies, such as those at the time meeting in Baltimore, counteract this to a considerable extent, but there is need of even greater affiliation.

The influence of the naturalist upon mankind in the way of teaching them competence had not been considered sufficiently. In political questions competency comes in, and the solution of much of our present trouble lies not so much in restricting the right to vote as it does in restricting the right to become a candidate. We, as naturalists and as citizens, should uphold competence. Our schools, even the best of them, judging by their results, do not educate properly. The naturalist should see to it that our schools educate, with science in its proper place. It is the duty of the naturalist to advance the development of the university. The schools use elementary knowledge to advance the mind in acquisitiveness, and the college uses advanced knowledge in the same way, but the university attempts to advance the mind in independent work, to develop and discipline originality.

To carry on its proper work the university needs a large endowment, at least $10,000,000. It is not possible to teach zoölogy unless the proper instruments and books are provided. The university, above all, needs proper professors. The qualifications of a professor in a university should be: (1) the ability to carry on original researches himself, and (2) to train others to carry out original work.

The annual discussion on '*Laboratory Teaching of Large Classes*' followed Professor Minot's address. Professor Alpheus Hyatt, of the Boston Society of Natural History, introduced the subject somewhat as follows:

Teaching has two objects in view: (1) to train the faculties of individuals, and (2) to increase the store of information. The importance in laboratory teaching of bringing the pupil into contact with the organisms themselves is absolutely necessary. The term, 'large classes,' is relative. It may mean twenty, thirty, forty, up to several hundred. In teaching large classes, there must be taken into account the matter of division into sections, rooms, assistants, apparatus, etc. The first point to be insisted upon is the matter of personal contact between the pupils and the instructors. In experience with Boston teachers, the classes numbered five hundred. Tables were provided for the whole number, and on these tables were placed the trays of specimens on which the exercise was to be given. The specimens were thus arranged before the exercises by assistants. The lecturer then proceeded to demonstrate the various points upon his own specimens, and the pupils followed him by working out the same points on the specimens in the tray. The specimens kept the lecturer down to his subject and also kept the pupils at work. Of course the field was necessarily limited.

The initial expense for providing the material was small, being about $10 for geology, $15 for botany, and $25 for zoölogy. Diagrams and crayon sketches, magnifying glasses, and various methods of a simple kind were made use of. These methods were afterwards used with smaller sections with even more satisfactory results. Examinations were given to test the pupils' proficiency, not only in knowledge of the subject but also of methods of study. For this purpose test objects were given the pupils to examine and describe. At the close of his paper, Professor Hyatt exhibited some specimens of these examinations.

Professor H. C. Bumpus, of Brown University, spoke upon the subject from the zoölogical point of view. The value of laboratory work depends largely upon good material, which should be supplied in abundance and in excellent condition. At the present time there is no excuse for supplying poor or scanty material, since abundance of excellent material can be obtained at small cost. The importance of having the best dissections and best drawings obtainable in the laboratory itself cannot be overestimated. It does not induce the laziness and attempts at shirking that seem to be the fear of so many teachers. If the student desires to copy a fine dissection he is to be encouraged to do so, and any teacher can readily detect the sketches copied from a chart or diagram. The speaker said also that he had found it an excellent plan in certain difficult cases to supply blanks on which the outlines of important structures were laid down, the details to be added by the pupil. A printed outline of the order of work, directions for manipulation, and questions to be answered from the specimens are a great help. The need of competent assistants is obvious.

The botanical side of the question was considered in a paper by Professor W. F. Ganong, of Smith College. The experience given was obtained in managing classes of about 200 men at Harvard, and the plan given was worked out under the guidance of Professor G. L. Goodale. The conditions under which the instruction was undertaken were: (1) The classes were too large for individual teaching by the instructor; (2) laboratory hours must be adjusted to other academic work, to insufficient accommodations, and sometimes even to yet other considerations; (3) many students of diverse attainments must be taught how to work and to think scientifically, and must be kept progressing together through the stages of a logically graded course, and (4) large quantities of special material must be provided for at unfavorable seasons.

In conducting such classes competent assistants were necessary, each to have not more than twenty men under him, and these were to remain under his special charge throughout the course. Such assistants may be readily recruited in any large university where there are special students doing advanced work. The assistants met the instructor to talk over plans and details of coming work. Uniformity of plan was insisted upon, but details of method were left to the assistant. The instructor did not devote himself to any one section, but visited each one as often as was possible. Weekly guides were printed for the use of the student, indicating the points to be studied, their relative importance, and any necessary information given. They were intended to supply just enough data to enable the student to progress to correct conclusions.

The materials required were arranged in the course, so that in the winter such things as could be grown easily or procured out of doors, as seeds, seedlings and buds, came first, and then followed the succession of opening buds, leaves, flowers and fruit made accessible by the advance of spring. In other words, the time of giving the course and the grouping of the subjects was so arranged that the material for each subject was in proper condition when it came before the class. Some of the weekly guides accompanied the paper, for examination.

Discussions were presented by Professors H. W. Conn, Marcella O'Grady, E. S. Morse and C. S. Minot, and the additional fact was brought out that a good synoptic collection was a desirable feature of the laboratory equipment, in order that the pupil might not have too narrow a view of each group of organisms, such as he is likely to carry away from the study of a single type.

After passing a vote of thanks to the authorities of the University, the citizens of Baltimore and the University Club for the hospitality extended to it, the Society adjourned.

The annual dinner of the affiliated Societies took place at 'The Stafford' at 7:30 on Friday evening. No set toasts were given, but informal speeches formed a very pleasurable close to this reunion.

W. A. Setchell, *Secretary.*

Yale University.

THE PRINCETON MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

The third annual meeting of *The American Psychological Association* was held at Princeton College on Thursday and Friday, December 27th and 28th, under the presidency of Professor William James, of Harvard University. Psychology is the youngest and likewise one of the most vigorous of the sciences. Although the Association is small, consisting of those only who are actively engaged in psychological investigation, and the members are widely scattered, there were sixteen papers read, exclusive of those presented in the absence of their authors. Indeed, the only drawback to the pleasure of the meeting was the fact that the program was so crowded that there was not sufficient time for discussion and social intercourse. The short intervals between the meetings were, however, pleasantly filled, owing to the hospitality of President Patton and Professor Baldwin, and the excellent accommodations of the Princeton Inn.

The Association was welcomed to Princeton by President Patton in a fitting address in which he alluded to the importance of such meetings, not only for the advancement of science, but also for the cultivation of inter-university friendliness, to the death and life-work of President McCosh, and to the prominent place always given to philosophy and psychology at Princeton.

The papers presented covered a wide range of psychological topics. Experimental psychology proper was not so fully represented as in the Philadelphia and New York meetings, owing to the detention of several members, but all the communications were strictly scientific in method.

The first paper, *Minor Studies and Apparatus*, by Professor Sanford, was, indeed, of purely experimental character, coming from Clark University, where President Hall has given such a prominent place to experimental psychology. Professor Sanford first showed charts demonstrating that the retinal fields for color are relatively smaller in the case of children than in the case of adults. In the second study he reported experiments on the accuracy with which an observer can distinguish by different senses which of two stimuli is first presented. A flash of light is perceived relatively earlier than a sound—contrary to results formerly published by Exner. In a third study primary memory was investigated. In a fourth study questions were asked students concerning the confusion of related ideas, for example:—How do you distinguish your right from your left hand? How do you call up a forgotten name? How do you collect the attention? What were your favorite games when a child? What is the earliest thing you can remember and how old were you? The distinction between motor and sensory types and other psychological questions were discussed in connection with the answers, and the method of securing mental statistics by asking questions was criticized. In conclusion, an instrument was shown for presenting objects alternately to each eye, and charts and photographs illustrating illusions of size, Listing's Law and the Horopter. These studies will be published in the forthcoming number of the *American Journal of Psychology*.

Professor Ormond, Professor Baldwin and others took part in the discussion that followed the reading of the paper. The discussion of the different papers was of nearly as great interest as the papers themselves, but to report it would carry us too far into details.

The second paper was on *The Psychic Development of Young Animals and its Physical Correlation*, by Professor T. Wesley Mills, of McGill University. The speaker emphasized the importance of comparative and genetic psychology — that is the study of the mental life of the lower animals and of children. He had observed the dog, cat, rabbit, guinea-pig and birds. They were watched from their birth, and notes were made several times during the day. The method was emphasized rather than the results, which will be published later.

Following Professor Mills' paper was one *On the Distribution of Exceptional Ability*, by Professor Cattell. The speaker explained how he had selected the 1,000 most eminent men by an objective method, and how this enabled him to measure and express numerically their mental traits. Curves were shown giving the time and racial distribution of great men. These demonstrate the rise and fall of leading tendencies in the past, and enable us, to a certain extent, to predict the course of civilization in the future.

Dr. A. Macdonald, of the Bureau of Education, presented a report on *Sensitiveness to Pain*. He exhibited the instrument used and described his method for measuring sensitiveness to pain. Women are more sensitive than men in the ratio of 7:5. Men taken from the street are not half so sensitive to pain as professional men. Americans are more sensitive than Englishmen or Germans. The right-hand side of the body is less sensitive than the left-hand side. Some instruments for anthropometric tests were also exhibited and described.

At the close of the morning session Brother Chrysostom, of Manhattan College, read a paper on *Freedom of the Will*. This time-honored problem was discussed from the point of view of St. Thomas Aquinas, with due recognition of recent writers. The Catholic Church certainly deserves honor for finding or putting modern science in the works of the great Schoolman.

The afternoon session was opened by the longest and most carefully prepared paper of the meeting, *Consciousness of Identity and So-Called Double Consciousness*, by Professor Ladd, of Yale University. Professor Ladd began by defining identity in material things and in minds. Changes heighten rather than diminish the consciousness of identity. A metaphysical ego is not needed —minds vary in their unity and reality. Double consciousness and hypnotic states should be treated in their relations to normal mental life, as it is not likely that the principle of continuity is violated in this case. Psychical automatism should be carefully studied—a man is not only that of which he is conscious. We can consider our automaton as well as our ego; one or the other may be predominant; they may be in conflict or act in coöperation. The automaton is evident in our daily life—in games, in dreams, in dramatic composition and acting, in prophecy. Ethically considered, a man is usually two or three, rather than one—hence the categorical imperative of Kant. The sanest minds are at times divided into two or more selves, as much as are the most extreme cases of hypnotic or pathological double-consciousness. Prof. Ladd's paper is included in his forthcoming work on Psychology, in the press of Charles Scribner's Sons. It excited much discussion and some criticism.

The remainder of the session was taken up by a paper on *A Preliminary Report and Observations on a Research into the Psychology of Imitation* by Professor Royce, of Harvard University. He began by noting the difficulty of defining imitation from other mental functions. He then described experiments now in progress in the psychological laboratory of Harvard University. An observer listens to a rhythmic series of taps which are later repeated or imitated by movements. The record was taken on a kymograph, and the impressions of the observers were noted and studied. The objective records have not been collated, but Professor Royce reported the subjective state as described by the observer, and its variations with different rhythms. In further discussion of the subject Professor Royce considered different kinds of imitation, and their relation to the rest of mental life and to the physical organism. The subject of imitation has recently become prominent and is evidently of the utmost importance in social psychology—not only the development of the child but also the thoughts, feelings and actions of men depend largely, if not chiefly, on imitation, and our theoretical knowledge has important practical applications.

The address of the President, Professor James, of Harvard University, occupied the evening session. The subject, *The Unity of Consciousness*, was treated with the speaker's unvarying clearness and literary skill. Professor James once said that metaphysics in a natural science 'spoils two good things,' but no natural science, be it physics or psychology, can draw a sharp line between its facts and its philosophy. It is also worth noting that what the physicist considers part of his science may be regarded as metaphysics by the psychologist, and conversely. The question of the unity of consciousness is, perhaps, as much a part of scientific psychology as the doctrine of the conservation of enery is a part of the science of physics. Professor James' address was largely made up of a review of the various theories proposed to account for the principle of union in the mind when many objects, susceptible upon occasion of being known separately, are brought together in the mind and known all at once. The Associationists say that the 'ideas' of several objects 'combine.' The Anti-Associationists say that such a process of self-compounding of ideas is incomprehensible, and that they must be combined by a higher synthetic principle, the Soul, the Ego, or what not. The speaker expressed dissatisfaction with the both these views. He said that his own aversion to the doctrine of the 'Soul' rested on an ancient prejudice, of which he could give no fully satisfactory account to himself, and he complimented Professor Ladd, of Yale, for his continued loyalty to this unpopular principle. Even Professor Ladd in his book prefers to speak of 'Soul' by some paraphrase such as 'real spiritual being.' Within the bounds of the psychological professor the 'Soul' is not popular to-day. Professor James conceived his problem as that of how we can 'know things together,' and in the first half of his address he incidentally said a good deal about knowledge. To the popular mind all knowledge involves a sort of mutual presence or absence as regards the object and the mind, which is treated as very mysterious. Professor James expelled this mystery from most cases of knowledge. He found the mystery of presence or absence, however, to abide in one little fact, from which it cannot be driven, and that is the very smallest pulse of consciousness, which always is consciousness of change. The present moment is no fact of experience; it is only a mathematical postulate, and the minimum real experience gives us a passing moment, in which a going and a coming fact meet on equal terms, and what was is known in one indivisible act with what does not quite yet exist. This is the original type both of our knowing at all and of knowing of things together, according to the speaker. He said there was no use trying to explain it, for it was the fundamental element of all experience. But we might seek to determine the exact conditions that decide what particular objects should be known together, and to this inquiry the end of the address was devoted. Various physiological, psychological and purely spiritual theories of the conditions were reviewed, without the speaker saying which one he favored. He hoped, however, that his remarks might stimulate inquiry which should bear fruit at the meeting next year. He closed with a modification of one of the most important doctrines of his own book on psychology, which in that state of mind, subjectively considered, ought not to be called complex at all. He admitted them to be complex, but is as far as ever from allowing the complexity to be described in the usually accepted way of the Associational school. The address will be printed in full in the March number of *The Psychological Review*.

The morning session of the second day was taken up by five papers on pleasure, pain and the emotions, and in the afternoon when the papers of the program had been read, the discussion returned to this subject and was carried on with much eagerness to the moment of adjournment. The papers were *The Classification of Pleasure and Pain*, By Prof. Charles A. Strong, of the University of Chicago; *A Theory of Emotions from the Physiological Standpoint*, by Prof. G. H. Mead, of the University of Chicago; *Desire*, by Dr. D. S. Miller, of Bryn Mawr College; *Pleasure and Pain Defined*, by Prof. S. E. Mezes, of the University of Texas; *Pleasure-Pain versus Emotion*, by Mr. H. R. Marshall.

It would not be easy to give an abstract of these papers that would be intelligible to men of science working in other departments—indeed, the mose careful attention was demanded of the audience. The kind of psychology presented is a development of descriptive psychology which may be called analytic psychology—a subject best represented in English by Dr. Ward's able but difficult article on *Psychology*, in the Encyclopædia Britannica. The question of the emotions and their expression has recently become prominent in psychological discussion—witness the articles on the subject by Professors James, Baldwin and Dewey in the last three numbers of the *Psychological Review*. Professor James' original theory that the mental state is rather the result of the 'expression' than that the expression is caused by the mental state is pretty well made out. The theory, to put the matter most bluntly, says that, "we feel sorry because we cry, angry because we strike, afraid because we tremble, and not that we cry, strike or tremble, because we are sorry, angry or fearful, as the case may be." Darwin's work, for example, should not be called *The Expression of the Emotions*. The movements are not caused by the emotions, but are aroused reflexly by the object, and are or have been useful. Thus the animal in the presence of its enemy may feign death or run away as will best contribute to its chances of escape, and a man may be 'paralyzed' by fear or flee according to circumstances. A man sneers because his ancestors were preparing to bite. The mental emotion results from movements and other changes in the body, being largely due to altered blood supply and the like.

Professor Strong's paper treated especially the classification of pains, reviewing the evidence in favor of special nerves for pain and the distinction between pain and distress (the German *Schmertz* and *Unlust*). Mr. Mead emphasized the importance of vaso-motor changes for pleasure and pain, attributing pleasure to increased blood supply and assimilation. Dr. Miller argued that desire is the essence of pleasure, and Mr. Marshall discussed the relations of pain, pleasure and emotion. It is interesting to note how even descriptive and analytic psychology is influenced by a psycho-physical point of view. Professor James aptly concluded the discussion by saying that such papers make us feel that we are in 'the place where psychology is being made.'

At the opening of the fifth and concluding session Professor Newbold read a paper entitled *Notes on the Experimental Production of Illusions and Hallucinations.* He reported that in twenty-two cases out of eighty-six tried, he had produced illusions by causing the patient to gaze into a transparent or reflecting medium, such as water, objects of glass and mirrors. The phantasm usually appeared within five minutes, was preceded by cloudiness, colors or illumination of the medium, and varied from a dim outline to a brilliantly colored picture. These were often drawn from the patient's recent visual experience, but were often unrecognized and sometimes fantastic. Successive images were usually related, if at all, by similarity, but often no relation was discoverable. The image was often destroyed by movements of the medium and by distracting sensory impressions and motor effort. The speaker was not inclined to regard the phantasms of the glass as demonstrating the existence of subconscious visual automatisms, but rather as illusions of the recognized types. But he was not prepared to deny that visual automatism might in some cases exist and be traced in such phantasms.

Mr. Griffing, of Columbia College, described *Experiments on Dermal Pain.* The pressure just causing pain (in kg) was for boys 4.8, for college students 5.1, for law students 7.8, for women 3.6. Experiments were also described giving the relations of area and duration and of velocity and mass for the pain threshold. These latter experiments are of special interest as determining the correlation of quantities followed by a given mental result.

The third paper of the session and last of the meeting was on *Recent Advances in the Chemistry and Physiology of the Retina*, by Mrs. Franklin, of Baltimore, who gave an account of the recent experiments by Professor König on the absorption spectrum of the visual purple of the retina, and of her own experiments which demonstrated that the fovea is color-blind for blue. The recent experiments on vision, largely carried out in the laboratories of Berlin, are of great importance, and make all the older theories of color-vision inadequate. The theory proposed by Mrs. Franklin is undoubtedly more satisfactory than any other, but even her theory meets difficulties in these new facts.

At the business meeting of the Association Professor Cattell (Columbia) was elected President, and Professor Sanford (Clark), Secretary. Several new members were elected and a new constitution was adopted. Under this constitution a council of six members is prescribed, and Professors Ladd (Yale), Cattell (Columbia), James (Harvard), Baldwin (Princeton), Dewey (Chicago), and Fullerton (Pennsylvania) were elected. Probably the most important business before the meeting was the invitation of the American Society of Naturalists offering affiliation. It was de-

decided to meet next year, if possible, at the same time and place as the Naturalists, and the Council was given power to decide the question of a closer affiliation.

J. MCKEEN CATTELL,
Secretary for 1894.

COLUMBIA COLLEGE.

CURRENT NOTES ON ANTHROPOLOGY, NEW SERIES—I.

THE 'MISSING LINK' FOUND AT LAST.

No publication of late date is likely to excite more interest than a quarto of forty pages which has just been issued from the local press of Batavia, with the title, '*Pithecanthropus Erectus. Eine Menschenänliche Uebergangsform aus Java.* Von Eug. Dubois, Militärarzt der Niederland. Armee.'

This noteworthy essay contains the detailed description of three fragments of three skeletons which have been found in the early pleistocene strata of Java, and which introduce to us a new species, which is also a new genus and a new family, of the order of primates, placed between the *Simiidæ* and *Hominidæ*,—in other words, apparently supplying the 'missing link' between man and the higher apes which has so long and so anxiously been awaited.

The material is sufficient for a close osteological comparison. The cubical capacity of the skull is about two-thirds that of the human average. It is distinctly dolichocepalic, about 70°—and its *norma verticalis* astonishingly like that of the famous Neanderthal skull. The dental apparatus is still of the simian type, but less markedly so than in other apes. The femora are singularly human. They prove beyond doubt that this creature walked constantly on two legs, and when erect was quite equal in height to the average human male. Of the various differences which separate it from the highest apes and the lowest men, it may be said that they bring it closer to the latter than to the former.

One of the bearings of this discovery is upon the original birth-place of the human race. The author believes that the steps in the immediate genealogy of our species were these: *Prothylobates: Anthropopithecus Sivalensis: Pithecanthropus erectus:* and *Homo sapiens.* This series takes us to the Indian faunal province and to the southern aspects of the great Himalayan chain, as the region somewhere in which our specific division of the great organic chain first came into being.

THE ANALOGIES OF RELIGIOUS SYMBOLISM.

A LEARNED Hungarian lady, Madame Sofie von Torma, has lately published an interesting little work, a prologue to a large one, in which she points out a number of close analogies or even identities between the symbols and myths of primitive peoples. Its title '*Ethnographische Analogieen; ein Beitrag zur Gestaltungs und Entwicklungsgeschichte der Religionen*' (Jena, 1894).

Beginning with the study of local archæology, she soon found that the analysis of her home relics took her back to ancient Arcadian and Egyptian prototypes, and the question arose, In what way were they related? To this it is her intention to devote an extended research; and in the volume before us, she states with force and brevity the many remarkable similarities she has noted, and presents the inquiries to which they give rise. The text is accompanied with 127 illustrations.

ETHNIC AFFILIATIONS OF THE JAPANESE.

AFTER a great deal of rambling discussion as to the ethnic relationship of the Japanese, it is gratifying to find a writer who has touched bottom at last, and brings a satisfactory theory with plenty of good evidence to support it. The writer is Dr. Heinrich Winkler, who, in his little pamphlet, *Japaner und Altaier* (Berlin, 1894), offers a solution of the problem which is certainly bound to stand.

He has studied the Japanese both from the anthropometric and the linguistic side. He points out that they present many and positive physical differences from the Chinese type, and can not be classed as a Sinitic people. On the other hand, the measurements bring them into close parallellism with the northern Ural-Altaic peoples, to that group which includes the Samoyeds, the Finns, the Magyars and, in a less degree, the Tungoose. This affiliation is strikingly supported by a careful comparison of languages. There is not a marked morphological trait of the Japanese tongue which is not also found in this Sibiric group. Dr. Winkler rehearses them with brevity and force. What is more, in the opinion of some, the material portion of the language, its vocabulary and radicals, present so many identities with this Ural-Altaic group that their primitive oneness must be conceded.

This, however, is not to be understood as if the Japanese was the Altaic *Ursprache;* but only as one of the children of a common mother, each of which has pursued independent lines of development, though always retaining the family characteristics.

D. G. Brinton.

University of Pennsylvania

HYGIENE.

THE NEW SERUM TREATMENT FOR DIPHTHERIA.

By cultivating the specific bacillus of diphtheria in broth, there is developed in the liquid a peculiar product, which is known as the toxine of this bacillus. When an extensive growth of the bacillus has occurred, so that a considerable quantity of this toxine is developed, the fluid is filtered through a porcelain filter, which permits the soluble toxine to pass through, but retains the bacilli.

If this filtered fluid is sufficiently strong, $\frac{1}{10}$ of a cubic centimeter of it will kill a guinea pig weighing 500 grammes, in from 48 to 60 hours. The effect produced is in proportion to the quantity injected, just as for any chemical poison, differing in this respect from the action of a fluid containing the bacilli themselves, which might multiply in the body. The bacilli in the fluid might be killed by heating, but this would also decompose the toxine; hence the separation is effected by simple filtration, or by the addition of some substance like tricresol which will kill the bacilli without affecting the toxine.

If small quantities of this toxine be injected under the skin of an animal, commencing with a dose which is not fatal and gradually increasing it, the animal gradually becomes immune to the effects of the poison and after several successive injections can receive a very strong dose without injury. The blood serum of an animal thus rendered immune against diphtheria has the power to confer a similar immunity on other animals if given in sufficient quantity in one dose, thus doing away with the need for the repeated and carefully graduated injections required to produce immunity in the first animal.

To obtain such an anti-diphtheritic serum to be used on man, a horse is injected with the solution of toxine, commencing with from 2 to 5 cubic centimeters and increasing the dose at intervals until within three months as much as 250 cubic centimeters may be injected without producing any serious effect. The horse is more resistant than many other animals to the action of the diphtheritic poison, being naturally somewhat immune. The blood serum of the horse produces no harmful effects on man, if injected in small doses, and it can readily be obtained in considerable quantities without killing the animal.

This serum, taken from a horse which has thus been rendered immune, will not only produce a temporary immunity in man against the diphtheritic poison, but will antagonize the effects of the diphtheritic poison after this has been already introduced into the system, in other words, it may be employed as a curative agent in cases of diphtheria. The immunity which it produces is a temporary one only, lasting from ten days to three weeks. Its curative effect in cases of the disease depends, to a considerable extent, upon its use in the early stages before the system has been saturated with the poison.

We have not yet sufficient data to speak positively of the value of this anti-diphtheritic serum as a means of treatment of the disease as compared with certain other methods of treatment, especially in the early stages, but the evidence thus far collected seems to indicate that such serum obtained in the proper manner, and used with proper precautions in the hands of experts, is a valuable addition to our means of combatting this terrible malady. The serum can only be properly prepared and tested by a skilled bacteriologist. It must be sufficiently strong in its immunizing power, and at the same time must contain no living pathogenetic germs of any kind. It must also have been comparatively recently obtained from the living animal, for it gradually loses its specific anti-diphtheritic powers. Special antiseptic precautions are also necessary in injecting the serum under the skin in the human subject to prevent the entrance of noxious germs.

One of the most useful points in applying the anti-diphtheritic serum to practical use is to have the cases diagnosed at the earliest possible date, and this can only be done by a skilled bacteriologist. In New York, Boston, and some other cities, means are now provided by which practicing physicians can have such diagnoses promptly made; and if the case of diphtheria can be seen by a physician in its earlier stages, it is possible to treat it with great hope of success by means of local applications to the throat of certain substances which will quickly destroy the bacillus, and prevent the further production of its peculiar toxine; for example, a solution of tri-cresol of the strength of one per cent. will usually effect this without producing undue irritation or causing any injury to the patient. Those who advocate the use of the immunizing serum say little about the local treatment, but this last is if anything the more important of the two, for the serum does not kill the bacilli which are on the surface of the mucous membrane of the throat, and therefore does not prevent a person rendered immune by it from being the means of spreading contagion.

OYSTERS AS A MEANS OF TRANSMITTING TYPHOID FEVER.

THE *Medical Record* of December 15, 1894, contains a paper by Professor H. W. Conn upon an outbreak of typhoid at Wesleyan University in October and November last, which included about twenty-six cases. When the serious character of the outbreak was recognized, an investigation as to causes was begun. The water supply was tested, and the house plumbing was examined without result. It was found that the disease was almost entirely limited to the members of three fraternities. The period of incubation of typhoid—that is, the time which elapses between the taking of typhoid bacillus into the body and the definite manifestation of the disease—is usually from ten to fourteen days, but may range from seven to twenty-eight days. The first cases of the fever among the students appeared October 20th, and suspicion soon fell upon the fraternity suppers of October 12th. Careful examination of the food supplied at these suppers showed that raw oysters, obtained by each of the three fraternities from the same oyster dealer, were the only things which were peculiar to their suppers, and inquiry was at once directed to these oysters. It was found that they had been obtained from the deep water of Long Island Sound and had been deposited in the mouth of a fresh water creek to freshen, or to 'fatten,' as it is termed, since under such circumstances the oyster absorbs the fresh water by osmosis and therefore swells and becomes plump. Further inquiry showed that, within about three hundred feet of the place where the oysters had been deposited, was the outlet of a private sewer coming from a house in which were two cases of typhoid fever at the time when the oysters were taken up and sent to the University.

The typhoid bacillus will live for a time in salt or brackish water, and it was proved by trial that if such bacilli are forced in between the two valves of the shell they remained alive long enough to enable the oysters to be carried and used at the fraternity suppers. Whether the bacillus will grow and multiply in living or dead oysters has not yet been determined, but experiments on this point are in progress.

It will be seen that the evidence that the outbreak of typhoid was produced by these oysters is purely circumstantial, but the links in the chain are well connected and strong.

It is by no means certain that there were any typhoid germs within the oysters or the oyster shells when they were sent to Middletown. If the shells were smeared on the outside with typhoid excreta some particles of this might easily have gotten among the oysters during the process of opening them. But it is evident that oysters grown or fattened in positions where sewage may come in contact with them are dangerous if eaten raw.

THE EVOLUTION OF INVENTION.

In a recent study that I have made on the evolution of invention I have divided the changings which underlie all examples of the process into those—

1. Of the thing or process, commonly called inventions.
2. Of the apparatus and methods used.
3. Of the rewards to the inventor.
4. Of the intellectual activities involved.
5. Of society.

Each one of these has undergone an evolution or elaboration, from monorganism to polyorganism, from simplicity to complexity, from individualism to coöperation, from use to comfort, and so on. This statement needs no extended proof; the roller mill is the descendant of the metals, machinery springs from tools, the device beneficial only to its originator becomes the world-embracing and world-blessing invention; the happy thought of one person at last comes to be the beneficent result of an endowed and perennial coöperation, a perpetual repository of invention renewed constantly by the removal of the senescent and the introduction of new and trained minds as in a university.

Now it requires great patience to get together the material evidence of this unfolding or evolution. The mental processes are no longer in sight. The nearest approach to them are the makeshifts of savages, and their minds are almost a sealed book. It has therefore occurred to the writer that among the questions proposed to those who are collating information relating to the psychic growth of children there should be a short series respecting the unfolding of the inventive faculty or process, the finding out originally how to overcome new difficulties or surmounting old ones in new ways.

O. T. Mason.

SCIENTIFIC LITERATURE.

Popular Lectures and Addresses.—Vol. II., Geology and General Physics.—Lord Kelvin.—Macmillan & Co., New York and London. Pp. 599. Price $2.00.

It is characteristic of the work of a really great genius, either in Science, Literature or Art, that it is not displaced and cannot be displaced by that which may come after it.

A bit of scientific work may later be found to be erroneous as to data, and, therefore, in the wrong as to conclusions, but if it be the work of an aggressive, original thinker, it will always have great value. In the brilliant galaxy of physicists, or, as he would himself call them, natural philosophers, which the present century has produced, it is moderation to say that none outshines Lord Kelvin, and it will not be denied that none has equalled him in aggressiveness and originality. The range of subjects upon which he has touched during his long and active life is so extensive as to certainly justify the use of the term Natural Philosopher in its broader sense (and capitalized at that), for he has never touched a department of human knowledge without leaving it richer and more extensive for his contact with it. That he has not been invariably infallible is recognized by no one more fully than by himself, and the new editions of his earlier papers which have been issuing from the press at intervals during the past few years, bear most interesting evidence of his readiness to change his attitude on great questions whenever the verdict of later investigations is against him. It is delightful to note the occasional parenthetical '*not*' put to-day into a sentence which twenty years ago declared very positively that '*there is*' so and so, or, '*we can*,' etc., completely reversing the meaning of statements which were once made with a good degree of confidence. Whatever else may be said, it cannot be asserted that Lord Kelvin has ever lacked the courage to express his own views in most forcible and unmistakable language. Indeed, in this respect, especially, he has set a splendid standard of unswerving scientific honesty for the innumerable workers who have been, and will be, more or less influenced by his methods and their tremendous productiveness.

His views as to the proper attitude of the philosopher in his relations to unexplored regions of human experience are concisely expressed in this noble sentence from his Presidential Address before the British Association for the Advancement of Science, in 1871: "Science is bound by the everlasting law of honor to face fearlessly every problem which can fairly be presented to it." When he comes, however, to touch upon some problems which have long been of great interest to the human race, but which have been assumed, usually, to lie outside the domain of experimental or exact science (and he touches upon them not infrequently in the volume under consideration), it is not difficult to see a very decided bias towards certain views, and a promptness to accept propositions not always well supported by evidence, very greatly in contrast with what is found in more vigorously scientific discussion.

This series of popular lectures and addresses is published in three volumes, the first and third having already appeared. The second (issued later than the third), to which attention is now invited, contains the important addresses on geological physics which have attracted so much attention during the past quarter of a century, together with a number of lectures and short papers on subjects related to general physics and extracts from addresses as president of the Royal Society since 1890. The geological papers are of great interest and have had much to do with the moulding of the views of geologists as to Dynamical Geology. The series begins with a short note covering but a single octavo page, entitled, '*The Doctrine of Uniformity in Geology Briefly Refuted*,' read at Edinburgh in 1865. It fairly 'opens the ball,' and may be regarded as the key note to the more elaborate disquisitions which followed at intervals up to recent dates. These papers are so well known, or ought to be so well known, to all geologists as to make it only necessary to say here that they will be found collected in this volume in convenient form and with a few notes and occasional comments by the distinguished author, made while the collection was being prepared for the press. The most important of the earlier papers are the address '*On Geological Time*,' given in Glasgow, early in 1868, and that on '*Geological Dynamics*' at the same place about a year later. In the first of these will be found the somewhat severe strictures upon '*British Popular Geology*' which brought forth the interesting and pointed criticisms of Huxley in his address to the Geological Society of London, and in the second the replies to Huxley's criticisms and futher remarks upon the subject. Nearly ten years later came a '*Review of the Evidence Regarding the Physical Condition of the Earth*,' read at the British Association meeting at Glasgow; two papers read before the Geological Society of Glasgow, on '*Geological Climate*,' and on the '*Internal Condition of the Earth;*' and after the lapse of another ten years a paper before the same society on '*Polar Ice Caps and their Influence in Changing Sea Levels*.' In these much of the ground of the earlier addresses is again gone over, in the light of later discovery in geology, physics and astronomy.

Indeed these same topics recur again and again, sometimes incidentally in other addresses in the volume, and Lord Kelvin makes it entirely clear that in thus taking up the discussion of geological problems and applying to them the methods and data of physics and astronomy, he does not wish to be considered an interloper. In his reply to Huxley, who had rather pointedly intimated that view of the situation, he good-naturedly remarks: "For myself I am anxious to be regarded by geologists, not as a mere passer-by, but as one constantly interested in their grand subject, and anxious in any way, however slight, to assist them in their search for truth."

It seems difficult to over-estimate the importance of these geological addresses, not only to the geologist, but to the physicist as well. They not only have a general interest to both, but are of special importance to each. To the one they open new possibilities of a somewhat exact and satisfactory treatment of a most important but hitherto rather unmanageable department of his subject; and to the other they offer a most instructive illustration of the power and scope of the methods of exact science, when applied by one who may justly be called not a master, but *the* master.

Of the other addresses, none, of course, is more important or interesting than the British Association Presidential Address of 1871, so well known to all. One of the earliest, on '*The Rate of a Clock or Chronometer as Influenced by the Mode of Suspension*,' is most entertaining and suggestive as an example of the many 'side-lights' of a remarkable intellectual activity. Of great historical value is the Royal Institution lecture of 1856 on the '*Origin and Transformation of Motive Power*'—already republished in Volume II. of the '*Mathematical and Physical Papers;*' and one of the most interesting is that of late date (1892) on the '*Dissipation of Energy*.' In this much attention is given to the principle of Carnot, and here also occurs a remarkable statement which the author himself has thought worth while to print in italics;—it is:—"*The fortuitous concourse of atoms is the sole foundation in Philosophy on which can be founded the doctrine that it is impossible to derive mechanical effect from heat otherwise than by taking heat from a body at a higher temperature, converting at most a definite proportion of it into mechanical effect, and giving out the whole residue to matter at a lower temperature.*"

The address on the opening of the Bangor Laboratories will be of interest to all who have to do with their like; that on the occasion of the unveiling of Joule's statue will interest everybody who cares for or who knows of the greatest generalization of modern science. In short, every page of this volume is deserving of the careful perusal of all who are devoted to Natural Philosophy in its most comprehensive sense, and who wish to know something of the spirit of one whose splendid contributions to physical science are, as a whole, greater than those of any other philosopher of the present time.

The mechanical execution of the book does not seem to be quite in keeping with the classical character of its contents, and its pages are occasionally marred by negligent proof reading. T. C. MENDENHALL.

WORCESTER POLYTECHNIC INSTITUTE.

Laws of Temperature Control of the Geographic Distribution of Life.

In the December issue of the *National Geographic Magazine*, Dr. C. Hart Merriam announces the discovery of the laws of temperature control of the geographic distribution of terrestrial animals and plants. Dr. Merriam has been engaged on this problem for sixteen years and believes he has at last obtained a formula which fulfills the requirements. He states that in the Northern Hemisphere animals and plants are distributed in circumpolar belts, the boundaries of which follow lines of equal temperature rather than parallels of latitude. Between the pole and the equator there are three primary belts or regions—Boreal, Austral and Tropical. In the United States the Boreal and Austral have each been split into three secondary transcontinental zones, of which the Boreal are known as the Arctic, Hudsonian and Canadian; and the Austral as the Transition, Upper Austral and Lower Austral.

The temperature data computed and plotted on maps as isotherms are not available in locating the boundaries of the zones, because they show the temperature of arbitrary periods—periods that have reference to a particular time of year rather than a particular degree or quantity of heat.

It is assumed that the distribution of animals and plants is governed by the temperature of the season of growth and reproductive activity—not by that of the entire year. The difficulty is to measure the temperature concerned.

Physiological botanists have long maintained that "the various events in the life of plants, as leafing, flowering and maturing of fruit, take place when the plant has been exposed to a definite quantity of heat, which quantity is the sum total of the daily temperatures above a minimum assumed to be necessary for functional activity." The minimum used by early botanists was the freezing point (0° C or 32° F), but recent writers believe that 6° C or 42.8° F more correctly expresses the temperature of the awakening of plant life in spring. "The substance of the theory is that *the same stage of vegetation is attained in any year when the sum of the mean daily temperatures reaches the same value*, which value or total is essentially the same for the same plant in all localities. This implies that the period necessary for the accomplishment of a definite physiological act, blossoming, for instance, may be short or long, according to local climatic peculiarities, but the total quantity of heat must be the same. The total amount of heat necessary to advance a plant to a given stage came to be known as the *physiological constant* of that stage." But students of geographic distribution are not concerned with the physiological constant of any stage or period in the life of an organism, but with *the physiological constant of the species itself*—if such a term may be used. "If it is true that the same stage of vegetation is attained in different years when the sum of the mean daily temperatures reaches the same value, it is obvious that the physiological constant of a species must be *the total quantity of heat or sum of positive temperatures required by that species to complete its cycle of development and reproduction.*" Now, "if the computation can be transferred from the species to the zone it inhabits—if a *zone constant* can be substituted for a *species constant*—the problem will be well nigh solved." This Dr. Merriam has attempted to do. "In conformity with the usage of botanists, a minimum temperature of 6°C (43°F) has been assumed as marking the inception of the period of physiological activity in plants and of reproductive activity in animals. The effective temperatures or degrees of normal mean daily heat in excess of this minimum have been added together for each station, beginning when the normal mean daily temperature rises higher than 6°C in spring and continuing until it falls to the same point at the end of the season." The sums thus obtained were plotted on a large scale map of the United States, and isotherms were run which were found to conform to the *northern* boundaries of the several zones. This is shown by colored maps. The data seem to justify the statement that "*animals and plants are restricted in northward distribution by the total quantity of heat during the season of growth and reproductive activity.*"

In the case of the *southern* boundaries of the zones, it was assumed that animals and plants in ranging southward would encounter, sooner or later, a degree of mean summer heat they are unable to endure. "The difficulty is in ascertaining the length of the period whose mean temperature acts as a barrier. It must be short enough to be included within the hottest part of the summer in high northern latitudes, and would naturally increase in length from the north southward. For experimental purposes, and without attempting unnecessary refinement, the mean normal temperature of the six hottest consecutive weeks of summer was arbitrarily chosen and plotted on a large contour map of the United States, as in the case of the total quantity of heat."

On comparing this map with the zone map, the isotherms of 18°, 22° and 26°C were found to conform respectively to the southern boundaries of the Boreal, Transition and Upper Austral zones, leading to the belief that "*animals and plants are restricted in southward distribution by the mean temperature of a brief period covering the hottest part of the year.*"

Except in a few localities the northern boundary of Austral species coincides with the southern boundary of Boreal species, but for a distance of more than a thousand miles along the Pacific coast a curious overlapping and intermingling of northern and southern types occurs. On looking at the temperature maps this is at once explained, for the mean temperature of the six hottest consecutive weeks from about lat. 35° northward to Puget Sound is truly Boreal, being as low as the mean of the corresponding period in northern Maine and other points well within the Boreal zone. On the other hand, the total quantity of heat is found to be the same as that required by Austral species. "It is evident, therefore, that the principal climatic factors that permit Boreal and Austral types to live together along the Pacific coast are a low summer tempera ture combined with a high sum total of heat."

A table is given showing the actual governing temperatures, so far as known, of the northern and southern boundaries of the several zones.

In conclusion, Dr. Merriam calls attention to the subordinate value of humidity as compared with temperature. "Humidity and other secondary causes determine the presence or absence of particular species in particular localities within their appropriate zones, but temperature predetermines the possibilities of distribution; it fixes the limits beyond which species cannot pass; it defines broad transcontinental belts within which certain forms may thrive if other conditions permit, but outside of which they cannot exist, be the other conditions never so favorable."

Grasses of Tennessee—Part II.—F. LAMSON-SCRIBNER.—University of Tennessee, Agric. Exper. Sta. Bull., VII. 1–141, 187 figures. 1894.

The first part of this important work treating of the structure of grasses in general, issued two years ago, is now supplemented by the part here noticed, containing descriptions and figures of all species known by the author to inhabit Tennessee. Carefully prepared keys to the genera and species are a feature of the book. The cuts are good, although printed on paper hardly firm enough to bring them out to the best advantage. The descriptions are diagnostic and couched in strictly technical language; on this point it is remarked: "Attempts to avoid technical or 'hard' words often result in obscuring the meaning of the author, and an undue simplicity of expression is often apt to be offensive by implying a lack of intelligence on the part of the reader." As the book is intended primarily for the farmers of the State, this may be considered by some as a position of doubtful value.

It is to be regretted that the rules of nomenclature adopted by the botanists of the American Association for the Advancement of Science, which are practically those approved by the zoölogists, have not been strictly followed. This will seriously hamper the usefulness of the book, for some of the names used by Prof. Scribner have become obsolete.

N. L. B.

NOTES.

PHYSICS.

THE newly discovered gas is to be the subject of a discussion at a meeting of the Royal Society on January 31st, when Lord Rayleigh and Prof. Ramsay will present their paper. This will be the first meeting under a resolution of the Council of the Society passed last session, whereby certain meetings, not more than four in number, are to be devoted every year each to the hearing and consideration of some one important communication, or to the discussion of some important topic.—*Nature.*

PERSONAL.

THE University of Berlin is seriously crippled by the deaths of Helmholtz and Kundt. Their places cannot be filled, but Prof. Kohlrausch will probably be called to one of the vacant chairs.

THE *Physical Review* has published excellent portraits of Helmholtz, Kundt and Hertz, with biographical sketches by the editor-in-chief, Professor Nichols. Probably the best account so far published in English of the work of Helmholtz is that contributed to the *Psychological Review* for January by Professor Stumpf, of the University of Berlin.

MR. F. Y. Powell, of Christ's College, succeeds Froude in the Regius Professorship of Modern History at Oxford.

ZOÖLOGY.

A PICTURE-PUZZLE of a remarkable kind appears in the *Zoölogist* for December. It is a reproduction of two photographs of a Little Bittern, showing the strange attitude assumed by the bird to favor its concealment. One of the figures shows the the bird standing in a reed-bed, erect, with neck stretched out and beak pointing upwards; and in this position it is difficult to distinguish the bird at all from the reeds. The eye is deceived in a similar manner when the bird is crouching against a tree-stump at the river side. Mr. J. E. Harting thinks that the curious attitudes adopted by the bird, on finding itself observed, are assumed in the exercise of the instinct of self-preservation. He mentions a similar habit, observed and described by Mr. W. H. Hudson, in the case of South American Little Heron, which frequents the borders of the La Plata, and is occasionally found in the reed-beds scattered over the pampas. Without the aid of dogs it was found impossible to secure any specimens of this bird, even after making the spot where one had alighted.—*Nature.*

NEW PUBLICATIONS.

Astronomy and Astro-Physics will hereafter be called the *Astrophysical Journal* and will be published from the University of Chicago, under the editorship of Profs. Payne and Keeler and a board of the leading men of science in this department.

A monthly *Magazine of Travel*, somewhat practical and popular in character, will hereafter be published from 10 Astor Place, New York.

The Aeronautical Annual for 1895, soon to be published by W. B. Clarke & Co., Boston, will contain reprints of some early treatises on aeronautics, among them da Vinci's *Treatise on the Flight of Birds*, Sir George Gayley's *Aerial Navigation* (1809), *A Treatise upon the Art of Flying*, by Thomas Walker (1810), and Franklin's aeronautical correspondence.—*Critic.*

P. Blakiston, Son & Co. announce *The Dynamics of Life*, by William R. Gowers, M. D., of London.

SOCIETIES AND ACADEMIES.

THE TEXAS ACADEMY OF SCIENCE.

DECEMBER 31, 1894.

Dr. Halsted, President, in the chair.

James E. Thompson; *Address.*

David Cerna; *The phonetic arithmetic of the ancient Mexicans.*

William Keiller; *Descriptive anatomy of the heart.*

Thomas Flavin; *Developmental anatomy and pathology of the kidneys.*

Thomas U. Taylor; *Present need of engineering education in the South.*

Robert A. Thompson; *The storm-water storage system of irrigation.*

T. H. Bryant, *Acting Secretary.*

NEW BOOKS.

Progress in Flying Machines. O. Chanute. New York, The American Engineer and Railroad Journal. 1894. Pp. iv+308.

Lectures on the Darwinian Theory. A. M. Marshall. Edited by C. F. Marshall. London, D. Nutt; New York, Macmillan & Co. 1894. Pp. xx+236. $2.25.

Sea and Land. Features of Coasts and Oceans with Special Reference to the Life of Man. N. S. Shaler. New York, Charles Scribner's Sons. 1894. $2.50.

Text-book of Invertebrate Morphology J. F. McMurrich. New York, Henry Holt & Co. 1894. Pp. 294. $4.00.

The Planet Earth. An Astronomical Introduction to Geography. Richard A. Gregory. London and New York, Macmillan & Co. 1894. Pp. viii+105. 60c.

Physiology for Beginners. M. Foster and Lewis E. Shore. New York and London, Macmillan & Co. 1894. Pp. ix+241. 75c.

The Rise and Development of Organic Chemistry. Carl Schorlemmer. Revised edition, edited by Arthur Smithells. London and New York, Macmillan & Co. 1894. Pp. ix+280.

Woman's Share in Primitive Culture O. T. Mason. New York, D. Appleton & Co. 1894. Pp. xiii+295.

SCIENCE.

NEW SERIES.
VOL. I. NO. 3.

FRIDAY, JANUARY 18, 1895.

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SCIENCE.

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FRIDAY, JANUARY 18, 1895.

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison on Hudson, N. Y.
Subscriptions (five dollars annually) and advertisements should be sent to the Publisher of SCIENCE, 41 East 49th St., New York.

THE BALTIMORE MEETING OF THE GEOLOGICAL SOCIETY OF AMERICA.

THE seventh annual meeting was held in Baltimore, December 27, 28 and 29, in the geological rooms of Johns Hopkins University.

The first session took place at 10 A. M., December 27, and was presided over by President Chamberlin. The Society was welcomed by President Gilman, of the University, who made a graceful and cordial address, that was warmly received. President Chamberlin in reply expressed the feelings of the members in a few felicitous words. A printed report of the Council was distributed, reviewing the events of the year. B. K. Emerson and J. S. Diller were elected an auditing committee. The results of the ballot for officers were as follows:

President, N. S. SHALER.
1st Vice President, JOSEPH LE CONTE.
2d Vice President, C. H. HITCHCOCK.
Secretary, H. L. FAIRCHILD.
Treasurer, I. C. WHITE.
Councillors, R. W. ELLS, C. R. VAN HISE.

Messrs. Clements, Cobb, Hopkins, Hubbard and Spurr were elected fellows.

The constitution was so amended that the qualifications for fellows shall hereafter be as follows, geographical location in North America being no longer a requisite, "Fellows shall be workers or teachers in geology." An amendment allowing the Treasurer to be elected without limit was also passed. After some announcements by the local committee the Society listened to a memorial of the late Professor George H. Williams, of Johns Hopkins University, and Second Vice President of the Society, by Professor William B. Clark. It was on Dr. Williams' invitation that the Society met in Baltimore and the great loss to the science by his death was the thought uppermost in the minds of all present. Dr. Clark's graceful and touching memorial to his late colleague was appreciated by all present. Brief additional tributes were also paid by Professor B. K. Emerson, of Amherst, Dr. Williams' first geological teacher and life-long friend; by J. F. Kemp, an old college-mate; by W. S. Bayley, his first student in petrography, and by his friends and colleagues, J. P. Iddings, I. C. White, C. D. Walcott and N. S. Shaler.

A memorial of Amos Bowman, of the Canadian Survey, was then presented by H. M. Ami, after which the Society listened to the reading of papers, as follows:

1. *On Certain Peculiar Features in the Jointing and Veining of the Lower Silurian Limestones near Cumberland Gap, Tenn.* N. S. Shaler, Cambridge, Mass.

The paper described peculiar forms of dolomitic limestone near Smiles, Tenn., in practically undisturbed strata which are ribbed and seamed by minute veins of calcite, in the form of small gash veins. They were regarded as due to some powerful, though local strains in the rock, but the subject was frankly admitted to be an obscure one.

2. *The Appalachian Type of Folding in the White Mountain Range, of Inyo Co., Cal.* Chas. D. Walcott, Washington, D. C.

The White Mountain range, which lies east of the Sierra Nevada, was shown to consist of conformable quartzite and cambrian shales and limestone. The series had been thrown into synclinal folds with intervening eroded anticlines and with a structure which, on the whole, closely reproduces the Appalachian sections of the East.

The paper was discussed by Messrs. Becker, Ami, Willis and Russell, after which recess was taken until the afternoon session.

3. *New Structural Features in the Appalachians.* Arthur Keith.

The paper reviewed the old generalizations of Appalachian structure, analyzed the recently published knowledge, described new structures, such as fan structure, cross folds, cross zones of shear, a secondary system of folding, the distribution of metamorphism, and advanced a theory to account for their production. According to the theory, the compressive strain which deformed the strata began in the crystalline gneisses and granites, thrust the crystallines against the sediments and by the differential motion along the shear zones produced buttresses around which the chief changes of structure were grouped.

In the discussion which followed, Mr. C. Willard Hayes considered two of the shear zones with the conclusion that the changes in structure were due to differences of rigidity in the sediments when they were thrust against the crystallines.

Mr. Keith replied that the changes of structure extended through the crystallines as well as the sediments, a fact incompatible with a merely passive resistance on the part of the crystallines.

Mr. Bailey Willis argued that the chief structural changes were due to original differences in sediment and in bases of sedimentation. His conclusion was that the sediments moved against a rigid crystalline mass, being actuated by a force acting from the westward, which was due to the isostatic flow of material from beneath the load of sediment.

4. *The Faults of Chazy Township, Clinton County, N. Y.* H. P. Cushing, Cleveland, O.

That the Lake Champlain region is, structurally, one of faulting without folding, is well known. The structure is well exhibited in Chazy township, which has not heretofore been mapped in detail, except for a small area around Chazy village. Its consideration is of importance, because of its bearing on the structure of the Adirondack region, in which, on account of the lithological similarity of the rocks, the determination of the precise structural relations is a matter of great difficulty, if not impossibility. The great number of the faults, and the consequent small size of the various faulted blocks, are striking facts.

In discussion C. D. Walcott showed how these faults had led Professor J. Marcou to believe that he had discovered colonies of Trenton fossils in rocks of the Potsdam.

5. *The Formation of Lake-basins by Wind.* G. K. GILBERT, Washington, D. C.

The paper described the formation of basins in the arid regions of the West, by the erosive action of wind-blown sand upon a shale devoid of vegetation. In time they became filled with water and formed small lakes.

6. *The Tepee Buttes.* G. K. GILBERT and F. P. GULLIVER.

The paper was read by Mr. Gulliver and described a series of conical buttes west of Pueblo, Col. They consist of Pierre shales, surrounding cores of limestone formed of shells of *Lucina*. It is supposed that as the shales were deposited, a colony of lucinas established themselves and grew upward pari passu, forming a conical or columnar deposit of limestone, whose greater resistance to erosion has left the buttes in relief.

7. *Remarks on the Geology of Arizona and Sonora.* W J MCGEE, of Washington.

The arid region was described as consisting of north and south mountain ranges with wide valleys between. In Arizona the surface is largely of volcanic rock, in Sonora of Mesozoic limestone. The rivers have definite courses and water in the mountains, but in the valleys they are lost by evaporation and absorption before the ocean is reached. Their valleys were transverse to the mountains and larger valleys because of the general southwesterly dip of the rocks. Buttes near the Gulf of California show slight talus, which fact gives good ground for thinking that the gulf has stood at an altitude, as regards the land, several hundred feet above its present level in recent geological time, or, in other words, that the land has been depressed by that amount.

8. *Geology of the Highwood Mountains, Montana.* WALTER H. WEED, Washington, D. C., and LOUIS V. PIRSSON, New Haven, Conn.

On account of the illness of Mr. Weed this paper was not read.

9. *Genesis and Structure of the Ozark Uplift.* CHARLES R. KEYES, Des Moines, Iowa.

On account of the author's absence the paper was not read.

10. *The Geographical Evolution of Cuba.* J. W. SPENCER, Washington, D. C.

The description of the physical geography of Cuba and of the adjacent submerged banks was given. Exclusive of a few areas locally older, the apparent basement is composed of volcanic rocks of Cretaceous or slightly earlier date. These are succeeded by fossiliferous Cretaceous sands, etc., and limestone greatly disturbed. The Eocene and Miocene deposits form a physical unit, and are composed mostly of limestone having a thickness of from 1,900 to 2,100 feet. The Pliocene period was mostly one of high elevation, accompanied by a very great erosion. At the close of the Pliocene period the Matanzas subsidence depressed the island so as to leave only a few small islets, and permit of the accumulation of about 150 feet of limestones. Then followed the great Pleistocene elevation with the excavation of great valleys, the lower portions of which are now fjords reaching in one case at least to 7,000 feet in depth before joining the sea beyond. The elevation was followed by the Zapata subsidence, reducing the island to smaller proportions than to-day, and permitting the accumulation of the loams and gravels like the Columbia of the continent. The subsequent minor undulations are also noted, as shown in terraces and recent small cañons now submerged. Also the modern coralline formations and harbors are notable.

On the completion of the paper the Society adjourned its business session until the following morning.

In the evening many members attended Professor Wm. Libbey's lecture on Greenland, and afterwards the reception which was hospitably tendered the visiting societies by the Johns Hopkins University in McCoy Hall. On reassembling Friday morning the council presented some minor points of business, and Mr. J. S. Diller, the chairman of the committee on photographs, read his annual report. It showed that some 1,200–1,500 photographs of geological phenomena and scenery had been presented to the Society, the same being on exhibition in the hall. The negatives of the U. S. Geol. Survey in many instances and also those of not a few geologists have been made accessible to the fellows for prints at cost. Mr. Diller finally tendered his resignation, which was accepted with regret. Mr. G. P. Merrill, of the U. S. National Museum, was appointed to the vacancy. The committee now consists of G. P. Merrill, W. M. Davis and J. F. Kemp.

The first paper on the programme was—

11. *Observations on the Glacial Phenomena of Newfoundland, Labrador and Southern Greenland.* G. FREDERICK WRIGHT. Oberlin, Ohio.

Note was made of the direction of the glacial scratches in Newfoundland and of the evidences of a preglacial elevation of the island; also of the contrast between the flowing outlines of the coast range of mountains in Labrador and the jagged character of the coast range of Southern Greenland. A description was also given of the projection of the inland ice which comes down to the coast near Sukkertoppen, in Lat. 65° 50′, and of the phenomena which indicate the former extension of the Greenland ice far beyond its present boundaries. Still, the bordering mountains were never covered with ice.

12. *Highland Level Gravels in Northern New England.* C. H. HITCHCOCK, Hanover, N. H.

Recent observations prove the existence of a glacial lake in the basin of Lake Memphremagog, whose beaches exceed a thousand feet above sea level, and others 1,500 feet above sea level in northern New Hampshire. The author wished to present a preliminary notice of what may prove to be of great service in a more exact definition of glacial work in New England and Canada.

The paper was discussed by Professor J. W. Spencer, who spoke of his own studies in the same region.

During the reading of the following six papers the petrographers and mineralogists adjourned to the room above and listened to the reading of papers of a petrographic character, as subsequently outlined. The principal session then listened to the following:

13. *Variations of Glaciers.* HARRY FIELDING REID.

The paper called attention to the desirability of keeping accurate records of the movements of glacial ice wherever possible. A committee was appointed to further this movement at the Geological Congress in Zurich last summer, and the writer urged the importance of the work, especially as regards our western glaciers.

14. *Discrimination of Glacial Accumulation and Invasion.* WARREN UPHAM, Somerville, Mass.

The accumulation of ice-sheets by snowfall on their entire area was discriminated from an advance or invasion by the front of the ice, extending thus over new territory. The former condition is shown to have been generally prevalent, on the glaciated portions of both North America and Europe, by the occurrence of comparatively small areas of ice accumulation beyond the extreme boundaries of the principal ice-sheets. The latter condition, or ice invasion, is indicated on the outer part of the drift-bearing area eastward from Salamanca, N. Y., through Staten and Long Islands, Martha's Vineyard and Nantucket, where the soft strata beneath the ice were dislocated and folded.

15. *Climatic Conditions Shown by North American Interglacial Deposits.* WARREN UPHAM, Somerville, Mass.

During the times both of general accumulation and growth of the ice-sheets and of their final recession, fluctuations of their borders were recorded in various districts by forest trees, peat, and molluscan shells, enclosed in beds underlain and overlain by till. Such fluctuations, while the ice accumulation was in progress, enclosed chiefly arctic or boreal species; but when the ice was being melted away, in the Champlain epoch, the remains of the flora and fauna thus occurring in interglacial beds, as at Toronto and Scarboro', Ont., may belong wholly to temperate species, such as now exist in the same district. The cold climate of the Ice age appears thus to have been followed by a temperate Champlain climate close upon the waning ice-border.

16. *Glacial Lakes in Western New York and Lake Newberry, the Successor of Lake Warren.* By H. L. FAIRCHILD, Rochester, N. Y.

The paper presented evidence that the finger lakes of central New York were all pre-glacial in character and that during the presence of the ice-sheet at their outlets they were backed up and discharged southward, as is abundantly shown by deltas at various heights on both sides of the present divide. Professor Fairchild cited eighteen glacial lakes from Attica on the west to the Onondaga river valley on the east. These he has named from important towns now on the sites, as Lake Ithaca for the glacial form of Cayuga lake, which was 35 miles long, 5–10 miles broad and 1100 feet deep. It has been long known that when the ice covered western New York the great lakes discharged at Chicago to the Mississippi and the great lake formed by them is called Lake Warren, and has left a good beach. At a much later stage, when the Mohawk was uncovered, the waters ran to the Hudson, and the great lake on the site of Ontario has been called Lake Iroquois. The intermediate stage between these two, when the discharge of the water covering western New York was through the low pass at the south end of Seneca lake through Horseheads near Elmira, Professor Fairchild has called Lake Newberry. The elevations of this and the Chicago pass are such that when allowance is made for the depressed condition of the area at that time, the existence of the lake can be demonstrated.

The paper was discussed by Messrs. McGee and Gilbert, who commended the choice of the new name as felicitous and timely. J. W. Spenser also spoke, but differed with the author in some points.

Meantime, in the upper laboratory (the Williams room), the petrographic section, under the chairmanship of Professor B. K. Emerson listened to

18. *The Relation of Grain to Distance from Margin in Certain Rocks.* ALFRED C. LANE, Houghton, Michigan.

A description of the variation in texture and grain of some quartz diabase dikes of Upper Michigan was given, and the same compared with effusive flows of similar mineral composition. These descriptions were based on series of thin sections of known distance from the margin. Interstitial micropegmatite is primary or pneumatolytic, and the feldspar crystallization begins before that of the augite, continuing until later. The distinction between the intrusive or dike type and the effusive type was pointed out. The main object of presenting the paper at this time is to elicit the best methods of measuring the coarseness of grain of a rock, the object being to express by some arithmetical or mathematical formula based on statistics, or in some other definite way, the relation of texture to walls and thickness in a dike. The paper elicited considerable discussion by Messrs. Hovey, Kemp, Iddings, Cross, and G. P. Merrill, in which the following points were made; the large size of the phenocrysts in some very narrow dikes; the importance of not measuring minerals of the intratelluric stage; the great variability of circumstances under which dikes cooled, as heated or cold walls, pressure, mineralizers, etc., and the difficulties of getting reliable data of the kind required by Dr. Lane.

19. *Crystallized Slags from Coppersmelting.* Alfred C. Lane, Houghton, Michigan.

This paper described (with exhibition of specimens) slags from the cupola furnaces used in coppersmelting, which contained large melilite crystals, between one and two centimeters square, interesting optically and in mode of occurrence. Crystallized hematite was also noted.

The specimens elicited great interest on account of the size and perfection of the crystals.

20. *On the Nomenclature of the fine-grained Siliceous Rocks.* L. S. Griswold, Cambridge, Mass.

The writer described the difficulties met first, in his study of novaculite, and later, in connection with other siliceous rocks, such as cherts, jaspers, etc., in applying definite names. The troublesome characters of opaline, chalcedonic and quartzose silica, as regards the origin of each, presented obstacles both for mineralogic and genetic classification.

This paper elicited an interesting discussion which threatened at times to take up the whole subject of the classification of rocks. The general feeling seemed to be that rocks could best be named primarily on a mineralogic and textural basis, and that these principles furnished the best solution of the difficulties presented by the paper. The speakers were Messrs. Wolff, Emerson and Lane.

21. On *Some Dykes containing 'Huronite.'* By Alfred E. Barlow, Ottawa. (Read by F. D. Adams.)

This paper contained a brief petrographical notice of certain dykes of diabase containing 'Huronite,' as the mineral was originally named by Dr. Thomson, of Glasgow, in his Mineralogy of 1836. Dr. B. J. Harrington's re-examination of this mineral in 1886 showed some very grave errors in Thomson's work and the 'huronite' must simply be regarded as an impure or altered form of anorthite, which has undergone either partial or complete 'saussuritization,' owing to metamorphic action. Certain localities were mentioned north and northeast of Lake Huron, where these dykes have been noted cutting the Huronian as well as the granitoid gneisses usually classed as Laurentian. Mr. A. P. Low, of the Canadian Geological Survey, noticed dykes containing this mineral cutting the Laurentian and Cambrian in the Labrador Peninsula.

22. *The Granites of Pike's Peak, Colorado.* Edward B. Mathews, Baltimore, Maryland. (Introduced by W. B. Clark.)

This paper gave an areal and petrographical description of the granites composing the southern end of the Rampart or Colorado range and showed that great macroscopic variation may result, while the microscopic characters remain monotonously uniform. Four types in all were distinguished, based on the size of phenocrysts and coarseness of grain. The paper was discussed by Whitman Cross and J. P. Iddings, after which the section adjourned to meet again at 4:30 p. m.

About the same time the main section also adjourned for lunch, which was most hospitably served to the visiting societies in the Johns Hopkins gymnasium. High praise is due the local committee for the excellent arrangements. After lunch the society reconvened and the first paper was:

23. *Notes on the Glaciation of Newfoundland.* By T. C. CHAMBERLIN.

The paper brought out the very interesting facts that the glaciation of Newfoundland is local and that the moraines and striæ show that it proceeded from the center of the island to the coast. The drift is all peripheral and can be easily traced to its sources.

24. *The Pre-Cambrian Floor of the Northwestern States.* By C. W. Hall. (Read in the absence of the author by WARREN UPHAM.)

The paper pointed out the distribution of the Pre-Cambrian areas in the territory under investigation so far as it is known at the present time. It then showed by means of records of deep and artesian well borings, within reasonable limits of probability, the depth of the Pre-Cambrian rocks over a considerable area beyond the surface area outlined.

Maps and a series of profiles accompanied the paper.

The paper was discussed by G. K. Gilbert, who called attention to the importance of the results.

25. *A Further Contribution to Our Knowl- of the Laurentian.* FRANK D. ADAMS, Montreal, Canada.

After referring briefly to the author's previous work on the anorthosite intrusions of the Laurentian, the paper gave a condensed account of the results of a study of the stratigraphical relations and petrographical character of the gneisses and associated rocks of the Grenville series in that portion of the protaxis which lies to the north of the Island of Montreal. By means of lantern slides Dr. Adams gave a very graphic account of the region in question. Some thin sections of rocks as large as an ordinary lantern slide were used to illustrate the passage of a massive rock into a crushed and sheared or gneissoid form. The paper formed not only an important contribution to the geology of the region, but to our knowledge of dynamic metamorphism as well. Discussion was reserved until after the reading of the next two.

26. *The Crystalline Limestones, Ophiolites, and Associated Schists of the Eastern Adirondacks.* J. F. KEMP, New York.

After a brief introduction and sketch of what others had done on the subject in hand, the areas of these rocks, especially in Essex county, were outlined and described with geological sections. It was shown that they are generally small, usually less than a square mile; that they consist of (*a*) white graphitic crystalline limestone, with great numbers of inclusions of silicates, (*b*) of ophiolites, (*c*) of black garnetiferous hornblende schists, (*d*) of lighter quartz schists, and (*e*) in one area, of closely involved granulite very like the Saxon granulite. The evidence of the plasticity of limestone under pressure was graphically shown by lantern slides. The trap dikes that often cut the limestones were referred to, and the relations with the intrusive gabbros were set forth, and the argument made that the limestones are older than the gabbros and anorthosites of the Norian series, and that they are the remnants of an extended formation which was cut up by these intrusions, metamorphosed largely by them and afterward eroded. A comparison was drawn with those on the western side of the mountains.

27. *The Relations of the Crystalline Limestones, Gneisses and Anorthosites in St. Lawrence and Jefferson Counties, N. Y.* C. H. SMYTH, JR., Clinton, N. Y.

The paper dealt especially with areas in the towns of Diana, Pitcairn and Wilna, but was really a review of the relations of these rocks in a wider region and was based on extended field experience. Petrographic details were presented of the several kinds of rocks, and especially of the varieties of the anorthosites, which were shown to shade into angite-syenites, and apparently into red gneiss. Many irruptive contacts of anorthosites and limestone were cited and the location of the classic mineral localities of this region was shown to be along these contacts. The same important thesis was worked out as in the preceding two papers, that the great intrusions of the Norian series were later than the gneisses and limestones.

The papers were discussed by Whitman Cross, who called attention to the close parallelism of the geology in the Pike's Peak district of Colorado; and by C. D. Walcott who referred to his own studies in the Adirondacks and similar conclusions to those advanced.

28. *Lower Cambrian Rocks in Eastern California.* Charles D. Walcott, Washington, D. C.

An account of the discovery of the Lower Cambrian rocks and fauna in the White Mountain range of Inyo County, Cal. See also No. 2 above. This important discovery affords a means of correllating the early Cambrian life in the remote West with those already known in the East.

29. *Devonian Fossils in carboniferous strata.* H. S. Williams, New Haven, Conn.

The paper described the fauna of the Spring Creek limestone of Arkansas, which lies between the Keokuk-Burlington strata below and the Batesville sandstone above, and is at about the horizon of the Warsaw and Chester of the Lower Carboniferous in the Mississippi Valley. The fossils are closely related to the carboniferous fauna described by Walcott from Eureka, Nev., and by J. P. Smith from Shasta County, Cal. But certain Devonian forms as *Leiorhyncus quadricostatum* and *Productus lachrymosus* of the New York Devonian are found with them, which are lacking in the Mississippi Valley, but are found in the Devonian of the West. The interpretation was then made, that the Arkansas fossils indicated a Devonian incursion from the westward.

During the reading of this and the succeeding titles the petrographers reconvened in the upper laboratory, as later recorded.

30. *The Pottsville series along the New River, West Va.* David White, Washington, D. C.

This paper was a careful description of the stratigraphy of the series, the determinations being based on the fossils, which evidence was presented in full.

31. *The Cretaceous Deposits of the Northern Half of the Atlantic Coast Plain.* Wm. B. Clark, Baltimore, Md.

The several formations established as a result of a detailed study of the Cretaceous strata of Monmouth county, New Jersey, were shown to have a wide geographical range towards the south. They have been traced throughout the southern portion of that State, while all except the highest members of the series are found crossing Delaware and the eastern shore of Maryland. Several representatives of these formations appear on the western shore, reaching to the banks of the Potomac.

32. *Stratigraphic Measurements of Cretaceous Time.* G. K. Gilbert, Washington, D. C.

The writer described a great series of Cretaceous rocks, 3500–4000 ft. thick, lying in the Arkansas River Valley, west of Pueblo, Colo. They consist of layers of limestone 1 ft. to 1 ft. 6 in. thick, separated by 1 in. of shale—this alternation being uniformly repeated through the whole thickness. The writer argued that frequent continental oscillation from deep to shallow water deposits was unlikely as having caused the beds, and hence appealed to climatic cycles.

The cycles of a year's changing seasons is too short to account for the limestone; the next longer cycle, the lunar, involves no changes of climate; hence the cycle of the precession of the equinoxes, 21,000 years long, was selected, and allowing four feet of deposit for each cycle, this portion of Cretaceous time was estimated at 21,000,000 years.

There was no discussion, but a very evident feeling of solemnity at the announcement.

33. *Notes on the Cretaceous of Western Texas and Coahuila, Mexico.* E. T. DUMBLE, Austin, Texas.

The author being absent the paper was only read by title.

The main section then adjourned until the presidental address at 7:30 the same evening. Meantime the petrographers listened to

34. *Spherulitic Volcanics at North Haven, Maine.* W. S. BAYLEY, Waterville, Me.

In the Journal of Geology a few months ago the late Dr. George H. Williams referred to the existence of old rhyolites on the coast of Maine. The author described very briefly the occurrence of these rocks, and exhibited specimens of them. The specimens showed very perfect spherulites, lithophysæ and all the common features of glassy volcanics. They brought out an interesting discussion regarding the abundance of these rocks along the Altantic sea-board. J. E. Wolff spoke of their great extent near Boston, and especially at Blue Hill, where the relations with the Quincy granite are a hard problem. A. C. Lane mentioned their frequency in central Maine, as shown by the collections of L. L. Hubbard. T. G. White referred to those near Mt. Desert. J. F. Kemp spoke of recent field and petrographic work in progress on the great areas near St. John, N. B. W. S. Yeates brought up the curious phosphatic spherulites lately found in Georgia, which closely simulate lithophysæ, and remarks were made on them by W. Cross and J. P. Iddings.

35. *The Peripheral Phases of the Great Gabbro Mass of Northeastern Minnesota.* W. S. BAYLEY, Waterville, Me.

On the northern border of the great gabbro mass in northeastern Minnesota are basic and granulitic rocks whose composition indicates their relationships with the gabbros with which they are associated. The basic rocks are aggregates of the basic constituents of the gabbro. They are characterized especially by the abundance of titanic iron. The granulitic rocks differ from the central gabbro mainly in structure. They consist of aggregates of rounded diallage, hypersthene and plagioclase, all of which minerals are present also in the normal rocks. The basic rocks are probably differentiated phases of the gabbro, of earlier age than the great mass of the normal rock. The granulitic phases are simply peripheral phases. Closely parallel cases were brought out in the discussion as existing in the Adirondacks (by C. H. Smyth, Jr., and J. F. Kemp), and in Quebec (F. D. Adams), where they have been been called granulites, augite-syenites and augite gneisses. H. D. Campbell mentioned the same phenomena in similar rocks in Rockbridge county, Virginia, and all the speakers commented on the peculiar development of orthoclase feldspar in the border facies of a gabbro mass.

36. *The Contact Phenomena at Pigeon Point, Minn.* W. S. BAYLEY, Waterville, Me.

The speaker distributed copies of his recent Bulletin U. S. Geol. Survey, No. 109, and exhibited a series of specimens which illustrate the peculiar contacts and transition rocks at Pigeon Point. Discussion followed by by J. P. Iddings and others.

37. *A New Discovery of Peridotite at Dewitt, 3 miles east of Syracuse, N. Y.* N. H. DARTON. *Petrography of same*, J. F. KEMP.

Mr. Darton described the opening up of this new boss of peridotite in the building of a reservoir. The wall rock is Salina shales, and the geological section of that part of the state was outlined in explanation. J. F. Kemp described the rock as a very fresh peridotite as these rocks go, with perfectly unaltered olivines and a ground mass of small augite crystals, with what was probably originally glass. Gabbroitic segregations were also mentioned containing feldspar. The interest of the rock lies in the fact that it gives much fresher material than that described by Dr. G. H. Williams from Syracuse, in which the larger original minerals were represented only by alteration products. No perofskite or melilite could be found in the Dewitt material.

Professor B. K. Emerson exhibited remarkable pseudomorphs of olivine from a rediscovered though long lost mineral locality in Massachusetts, and corundum with interesting enclosures.

The section then adjourned with the intention of having an exhibition of rock sections the following morning in the same place.

A goodly audience greeted President Chamberlin at 7:30 in the evening for the annual presidential address, the subject being *Recent Glacial Studies in Greenland.* The speaker brought out the distribution of the ice sheet over Greenland, described his observations at Disko Bay and elsewhere and his final location at Lieut. Peary's station, Inglefield Gulf. Many peculiar features of Greenland glaciers were brought out, such as their rampart-like terminal cliffs, their general foliation or banding and enclosed debris, their causeways of morainic material, etc. The glaciation is thought to be now near its maximum extent because just beyond the ice are unglaciated areas and jagged islands that have never been covered. A large series of lantern views followed and brought out still more forcibly the points of the address. President Chamberlin was listened to with close attention during the two hours occupied, and all thoroughly enjoyed the lecture, but it is nevertheless true that an hour and a quarter, or at most an hour and a half, is about as long as a speaker can wisely keep a general audience.

The Society reassembled in the geological laboratory about ten o'clock for the annual supper. After an excellent ménu had been cared for, Professor B. K. Emerson was chosen toastmaster, and by his characteristic sallies, in which he was ably aided by several speakers, resolved his hearers into intermittently active spiracles of mirth upon the lava stream of his wit.

When the Society reassembled on Saturday morning the first paper read was

38. *The Marginal Development of the Miocene in Eastern New Jersey.* Wm. B. Clark, Baltimore, Md.

The deposits which characterize the marginal phase of New Jersey Miocene in Monmouth and Ocean counties were especially discussed. The gravels, sands and clays were considered and their relations shown, together with the occurrence of glauconite in certain areas. The connection of the strata in the northern counties with the highly fossiliferous beds in South Jersey was explained. The paper was discussed by N. H. Darton bringing out some slight divergence of views on the classification of the deposits, in that the discovery of fossils by W. B. Clark had somewhat revised the earlier stratigraphic work.

39. *Sedimentary Geology of the Baltimore Region.* N. H. Darton, Washington, D. C.

An account of the local geology of Mesozoic and Cenozoic formations and some statements regarding certain unsolved problems in coastal plain geology, illustrated by maps and sections. The sections which passed through the crystallines of the Piedmont plateau and the city of Baltimore brought out admirably the relations of the later sediments to the older protaxis.

40. *The Surface Formations of Southern New Jersey.* ROLLIN D. SALISBURY, Chicago, Ill.

The surface formations of southern New Jersey, which have often been grouped together under the names, 'Yellow Gravel' and 'Columbia,' are believed to be divisible into five formations, the oldest of which greatly antedates the glacial period. The several formations are unconformable on each other and are believed to have been widely separated in time of origin. These formations were called the (1) Beacon Hill, (2) Canasaucon (the spelling may be wrong), (3) Jamesburg, (4) Trenton and the (5) Keyport. It is impossible as yet to say which are Columbia and which not, but (2) is probably Pleistocene, and formed during ice action on the north. Nothing later than (3) is Columbia. The paper was discussed by Warren Upham.

41. *New Forms of Marine Algæ from the Trenton Limestone, with Observations on Buthograptus laxus, Hall.* R. P. WHITFIELD, New York. (The paper was read by E. O. HOVEY.)

Certain fossils from Platteville, Wis., referred years ago by Hall with doubt to the graptotiles, were shown to be really articulated, marine algæ, and referable to several species. True corallines from the same horizon at Middleville were also described which are much older than any hitherto mentioned members of this group of plants.

42. *On the Honeycombed Limestones in the Bottom of Lake Huron.* ROBERT BELL, Ottawa, Canada. (Read by H. M. AMI.)

The Limestones over a certain region in the bottom of Lake Huron are extensively eroded in a peculiar manner which the writer calls honeycombing and pitting. He described this condition, the area within which it is found, the depth of the water and other conditions most favorable to its production and then attempted to account for its origin, enumerating various possible causes which might suggest themselves, and giving the most probable one, namely, a differential solubility of the rock in the presence of slightly acidulated water. Reasons in support of this view were stated. The geological ages and the lithological characters of the various limestones attacked were mentioned in trying to arrive at the conditions which produce the phenomena described. The localization of this form of erosion may be attributed to a slight acidity of the water in that part of Lake Huron, and reasons are given for believing that an acid condition actually exists. In addition to the considerations due to the structure and composition of the rock lying at the bottom of such water, certain external conditions were mentioned as favoring the honeycombing process, which appears to be still in active progress. Examples were given of somewhat similar erosion elsewhere, but the typical honeycombing here described appears to be confined to Lake Huron. The paper was illustrated by specimens and photographs.

43. *On the Quartz-keratophyre and its Associated Rocks of the Baraboo Bluffs, Wisconsin.* SAMUEL WEIDMAN. (Read by J. P. IDDINGS.)

In the vicinity of Baraboo, Wisconsin, occur acid porphyritic rocks which correspond chemically with quartz-keratophyres. They exhibit under the microscope fluxion, spherulitic, poicilitic, and other structures of volcanic rocks, and are associated with volcanic breccias which show them to have their origin in a surface flow. They are of Pre-Cambrian age, since they rest upon the upper Huronian quartzite and are overlaid by the Potsdam sandstone and conglomerate. In some portions of the area they have been completely changed to finely foliated sericite schists through the orographic movement which elevated the quartzites to form the Bluffs.

44. *The Characteristic Features of the California Gold Quartz Veins.* WALDEMAR LINDGREN, Washington, D. C.

The writer described the extent and associations of the veins, bringing out the fact that they are in all manner of wall rocks, although especially in the auriferous slates. They were shown to be true fissure veins that cut the walls at all angles, although mostly along the strike. Direct issue was taken with the view that they are replacements of limestone or related rock, for it was shown that while the veins are siliceous and filled with quartz, the wall rocks have very generally suffered carbonatization. Finally the source of the gold was placed in deep seated regions, whence it had been brought by uprising solutions.

On the conclusion of the paper, the customary votes of thanks were passed to the local committee, to the Johns Hopkins University and to others whose efforts had made the session a success. The next place of meeting, a year hence, has not been settled. On the whole, the meeting was the best attended and most interesting and successful yet held. J. F. KEMP.

COLUMBIA COLLEGE.

THE BALTIMORE MEETING OF THE AMERICAN MORPHOLOGICAL SOCIETY.

THE Society met on Thursday morning in the lecture room of the Chemical Building and again upon Friday afternoon, adjourning for the intermediate sessions of the Society of Naturalists. In the absence of Professor C. O. Whitman, President of the Society, Professor W. B. Scott, of Princeton, Vice-President, took the chair. Among those present at these sessions besides those who presented papers were Alpheus Hyatt, Edward S. Morse, Edward D. Cope, Samuel F. Clarke, C. F. Herrick, Henry F. Osborn, E. A. Andrews, W. H. Dall.

The officers elected for the year 1895 were:

President—Professor Edmund B. Wilson, Columbia College.

Vice-President—Professor W. B. Scott, Princeton College.

Secretary and Treasurer, Dr. G. H. Parker, of Harvard University.

The following are abstracts of the papers presented:—

Dr. C. W. Stiles, of the U. S. Agricultural Bureau, presented the first paper upon *Larval Stages of an Anoplocephaline Cestode* and exhibited specimens of Distoma (Polyorchis) molle (Leidy, '56), S. & H., '94; of Dioctophyme gigas, Rud., and of Distoma tricolor, S & H. Five hundred of the last named species are ready for distribution as exchanges to college zoölogists.

Professor William A. Locy, of Lake Forest University, presented the first paper on *Primitive Metamerism in Selachians, Amphibia and Birds.* It has been generally assumed that the metameric divisions of the Vertebrates depend primarily on the middle germ-layer, and that whenever they appear in the ectoderm they are secondarily moulded over the mesodermic segments. This proposition is not supported by these observations. We find in very young embryos of amphibians and birds, primitive metameric divisions which effect the entire epiblastic folds and in Selachians extend also out into the germ-ring. They are present before any protovertebræ are formed and are most clearly marked in the border regions. These segments become later coincident with the so-called neuromeres, but it is to be noted that they are by no means confined to the neural tube. The time-honored designation 'metamerism of the head' should be interpreted as meaning regional metamerism not as a different form of segmentation from that which affects the trunk region. This paper was discussed and the accuracy of the author's observations was questioned because of the conspicuous character which he assigned to certain surface markings never observed by others. The opportunity given for examining the specimens, however, proved that the markings could be faintly seen as described by the author.

Dr. Locy's second paper was a *Note on the Homologies of the Pineal Sense-Organ.* The basis for determining homologies of the two epiphysial outgrowths of Petromyzon, Teleosts and Lacertilia has been furnished by recent publications by Studnicka, Hill and Klinckowström. Basing a comparison upon innervation and also upon the history of the vesicles, we may regard the upper epiphysial vesicle in Petromyzon as corresponding to the epiphysis of Teleosts and Lacertilia, and the lower epiphysial vesicle as equivalent to the anterior vesicle of Hill (which early absorbs) in the teleosts, and to the pineal eye in the Lacertilia.

Under the title: '*The Quadrille of the Centrosomes' in the Echinoderm egg; a second contribution to biological mythology,* Professor E. B. Wilson, of Columbia, presented the somewhat surprising results of his renewed investigation of the phenomena of fertilization in the eggs of the sea-urchin. Rabl had predicted in 1889 that the union of the germ-cells would be found to involve a conjugation of centrosomes or archoplasmic elements in addition to the well-known conjugation of nuclear elements. Fol's celebrated paper on the *Quadrille of the Centrosomes* in 1891 was apparently a triumphant fulfillment of the prediction, and, having been immediately and universally accepted, exercised an important influence on the current theories of inheritance. A prolonged research upon the eggs of *Toxopneustes variegatus* shows, with a high degree of certainty, that Fol's results were based on material prepared by defective methods; that his account of the origin of the archoplasm is fundamentally erroneous; that no 'Quadrille' occurs in the American species at least, and that his account of it is largely mythical. Results essentially similar and fully corroborating the above have been reached in the Columbia Laboratory by Mr. A. P. Mathews in the eggs of *Arbacia* and *Asterias.* In all these cases the egg-centrosome and archoplasm degenerate and completely disappear after formation of the second polar body, and, therefore, do not play any part in the fertilization. The sperm-archoplasm is derived not from the tip of sperm but from the middle-piece (as in the earth-worm and in the axolotl) and by division gives rise directly to the amphiaster of the first cleavage without any participation of an egg-centre or egg-archoplasm. All the stages in the fertilization process of *Toxopneustes* were exhibited by the author in photographs taken with an enlargement of one thousand diameters with the coöperation of Dr. Edward Leaming, of the College of Physicians and Surgeons, New York. These photographs illustrated furthermore the effect upon the egg of various reagents, a considerable number of which have been carefully tested. Fol's picro-osmic mixture was shown to be very defective, causing more or less marked disorganization of the archoplasmic structures and producing various artefacts. The 'centers' (centrosomes) of Fol were unquestionably such artefacts, produced by the shrinking and clotting together of the archoplasmic recticulum. In properly preserved material (sublimate-acetic, Flemming's fluid, etc.,) the archoplasm-masses ('astrospheres') consist of a uniform reticulum and contain no centrosomes.

In a second paper on the '*Polarity of the Egg in Toxopneustes*' Professor Wilson described the results of his observations on the paths of the pronuclei in the transparent living egg. The very unexpected result was reached that in this case the ultimate vertical axis of the egg ('egg-axis' proper) does not necessarily coincide with the polar axis but may form any angle with it; but the plane of first cleavage is nevertheless always nearly through the entrance-point of the sperm. Regarding the former point there is a possible source of error in that the excentric egg-nucleus may wander from its original position (near the polar bodies), so that the diameter passing through it no longer represents the egg-axis. (This cannot be determined from the polar bodies, since they quickly become detached from the egg). Many facts indicate, however, that such wandering does not occur. If it does not, then the polarity of the egg is not primordial but induced, and one of the most fundamental characteristics of the egg is thus brought into the category of epigenetic phenomena.

Professor Charles S. Minot, of the Harvard Medical School, presented a paper upon *The Olfactory Lobe.* He showed that of eleven layers of cells in the olfactory lobe only the inner two layers belong to the cerebral cortex proper, proving that the olfactory lobe is a ganglion structure belonging to the sensory ganglion series with certain great secondary modifications. This is further supported by the fact that the lobe primarily connects with the brain at a point topographically similar with a point midway between the 'dorsal zone' and the 'ventral zone' of His. In a second paper Professor Minot pointed out as a *Fundamental Difference Between Animals and Plants*, of value principally in teaching, that while animals feed typically upon solids, plants always procure their food in a gaseous or liquid form. This paper was discussed by Dr. Locy, Dr. Humphries and several other botanists and zoölogists present, the point being raised that plants manufacture their own food and that when plant assimilation really begins it is practically analgous to that of animals, as it consists in the taking up of solid particles.

Dr. Arnold Graf, of Columbia, presented the next paper upon *The Origin of the Pigment and the Causes of the Presence of Patterns in Leeches.* The pigment originates in the excretophores. These are wandering cells which pick up excretory substances from the walls of the capillaries; one part of the cells wanders to the funnels of the nephridium and thus delivers their contents into the nephridium, while another part of the excretophores wanders under the skin emerging along the lines of least resistance, which lie between the muscle bundles. The color patterns of the leeches vary, therefore, according to the arrangement of the musculature. In *Nephelis* the longitudinal musculature is developed most strongly and consequently the pattern consists in longitudinal stripes. *Clepsine* has as a consequence of its parasitical mode of life a strongly developed dorso-ventral musculature and therefore the pattern consists in spots, the longitudinal stripes having been interrupted and broken up by the transverse and oblique muscle bundles. The bearing of these facts is very important. The color pattern of the leeches is not in itself adaptive; it is entirely incidental and secondary to the musculature which is essentially adaptive. A change in the musculature would result in a change in the superficial color pattern. This shows how a very striking superficial character may originate without any adaptive significance and as a secondary inheritance.

The following paper by Professor H. T. Fernald, of Central College of Pennsylvania, was entitled *Homoplasy as a Factor in Morphology.* A review of zoölogical literature in the past ten years shows that in every group of animals beginning with the sponges and extending up to the highest vertebrates the phenomenon of parallel or homoplastic development is becoming increasingly apparent. Numbers of cases were cited from all classes of animals showing that identical structures, produced independently in different phyla, are extremely numerous.. The paper was discussed by Professors Hyatt, Cope and Scott, who pointed out that while the term 'homoplasy' was proposed by Lankaster the phenomenon itself was early pointed out by Darwin and has been fully elucidated by palaeontologists.

Mr. Seitaró Gató, of the Johns Hopkins University, gave a demonstration of some parts of the Ectoparasitic Trematodes including a number of features from his full memoir upon this subject recently published in Japan.

Mr. A. P. Matthews, of Columbia, followed with a paper on the *Morphological Changes in the Pancreatic Cell, corresponding with Functional Activity.* The cells of *Necturus* are exceptionally large and favorable for observation of the changes which occur before and after feeding. The striated appearance of the outer zone of the pancreatic cell is due to coarse cytoplasmic filaments or threads which end in the centre of masses of chromatin within the nuclear membrane. In fact, these threads are directly continuous with the cytoplasmic reticulum in the inner zone; these threads are often coiled and in such cases explain the structures known as *Nebenkerne.* When the gland is secreting the zymogen granules and reticulum are washed out of the cell by lymph currents and new thread substance is manufactured by the chromatin. During the so-called 'rest' of the cell the thread substance degenerates into zymogen granules and the cytoplasmic reticulum of the inner zone. The zymogen granules grow by accretion. The thread substance grows by accretion at the chromatin end. The nucleus undergoes no appreciable changes. There are indications that the chromatin is a ferment, and that it is the essential formative element of the cell; probably this is true of all the cells and all chromatin; if so, the character of cytoplasm and new chromatin formed will depend on the character of the nutrition. It is possible that the chromatin of embryonic cells differentiates as a result of differentiations dependent upon the location in the segmenting cell mass of the chromatin of the original blastomeres. If this is true it is unnecessary to assume that characteristics are represented definitely in a so-called 'stirp' located in the chromatin.

Professor J. S. Kingsley, of Tufts College, next presented a paper upon the *Anatomy and Relationships of Pauropida*, on behalf of Mr. F. C. Kenyon.

Professor Alpheus Hyatt, of the Museum of Comparative Zoölogy, Cambridge, presented a paper summing up his researches upon the *Parallelisms between the Ontogeny and Phylogeny of Pecten.*

Professor Andrews submitted for Professor T. H. Morgan, of Bryn Mawr, some of his observations recently made in Naples at the American table supported by the Smithsonian Institution. It is found that the unsegmented eggs of a sea-urchin may be broken into minute fragments which develop into perfect larvæ. One such fragment may be one-fiftieth of the volume of the egg and yet develop into a gastrula if it contain a male and a female pronucleus. The gastrula thus produced is so exceedingly small that three in a row are no longer than an infusorian, such as *Paramoecium.* The volume of such a gastrula is one-sixty-fourth part of that of a normal gastrula. While the number of cells in a normal blastula on the point of invaginating is five to seven hundred, the number in one of the minute blastulas at the same stage may be as small as sixty. With such facts we explain the known difficulty in rearing larvæ from isolated cells of late cleavage stages, as due to a limit in the number of cleavages possible before gastrulation. That is, gastrulation comes after a definite number of cleavages and a cell has its possible cleavages reduced in a certain ratio by the number of preceding cleavages.

The paper of Professor F. H. Herrick, of

Adelbert College, upon the *Biology of the Lobster* will be printed in full in a later number of Science.

CURRENT NOTES ON ANTHROPOLOGY (II.).

NATIVE ASTRONOMY IN MEXICO AND CENTRAL AMERICA.

At the International Congress of Americanists, which met in Stockholm last August, two papers were presented which ought to give pause to those would-be critics who of late years have been seeking to belittle the acquirements of the semi-civilized tribes of Mexico and Central America. Both are studies of the positive astronomic knowledge which had been gained by the observers among those tribes. One is by Mrs. Zelia Nuttall, and bears the title, *Notes of the Ancient Mexican Calendar System.* It is intended merely as a preliminary publication to a thorough analysis of this system as it was carried out in Mexico, and contains only the outlines of her discoveries. These are, however, sufficient to support her thesis, that the astronomer-priests possessed a surprisingly accurate knowledge of the exact length of the solar year, of the revolution of the moon, and of the synodical revolution of the planet Venus.

The second paper is by Dr. Förstemann, who is the foremost student in Germany of the contents of the books written in the hieroglyphic script of the ancient Mayas. He takes up page 24 of the Dresden Codex, and explains its meaning. This page has been long recognized as a sort of abstract or table of contents of those which follow it in the Codex, but its exact bearing has not previously been interpreted. Dr. Förstemann shows by ingenious and accurate reasoning that it relates chiefly to the synodical revolution of the planet Venus and its relation to the courses of the sun and moon.

RECENT AMERICAN LINGUISTIC STUDIES.

It is gratifying to note that the immense field of native American languages is finding cultivators in many countries.

Even in England, where so little has been done in this direction, a special fund has been raised called the 'vocabulary publication fund,' which prints and issues (through Kegan Paul, Trench, Trübner & Co.) short grammars and vocabularies of languages from MSS. in the possession of learned societies and individuals. The first printed is a grammar and vocabulary of the Ipurina language, by the Rev. J. E. R. Polak. This is one of the Amazonian dialects, and though we were not without some material in it before, this addition to our knowledge is very welcome.

From the same teeming storehouse of Brazil, Dr. Paul Ehrenreich has lately published in the Berlin *Zeitschrift für Ethnologie*, his excellent studies in the language of the the Carayas and Cayapos. They are practically new in matter and form. The Puquinas are a rude tribe who live about Lake Titicaca. M. Raoul de La Grasserie has lately issued (through Koehler, Leipzig) a number of old texts in their language; and Dr. Max. Uhle has collected considerable material in it as spoken to-day. Dr. A. F. Chamberlain, in the American Anthropologist for April last, analyzes a number of neologisms in the Kootenay language; while our knowledge of the remote and confusing dialects of the Gran Chaco has lately been notably increased by the activity of the Argentine scholars, Macedo and Lafone-Quevedo, in editing from rare or manuscript works the notes collected by the early missionaries.

AMERICAN ONOMATOLOGY.

The study of the meaning and origin of geographical names has a higher purpose than to satisfy a passing curiosity. They are often the only surviving evidences of migrations and occupancy; they preserve extinct tongues or obsolete forms; and they indicate the stage of culture of the people who bestowed them. Especially useful in these directions are the aboriginal names on the American continent; for the shifting of the native population was so rapid, and the dialects disappeared so quickly, that the place-names are sometimes the only hints left us of the presence of tribes in given localities.

A model study in this field is that of Dr. Karl Sapper in *Globus*, Bd. LXVI., No. 6, on 'The Native Place-names of Northern Central America.' It embraces Guatemala, Chiapas, Tabasco, and portions of Yucatan, Honduras and San Salvador. The aim of the writer is to define the limits of the Mayan dialects, and to explain the presence of Nahuatl influence. He accomplishes his purpose in a thorough manner. Mr. De Peralta, in his *Etnologia Centro-Americana* (Madrid, 1893), did much the same for Costa Rica; and in the Algonkian regions of the Eastern United States, Mr. William Wallace Tooker (in the *American Anthropologist* and other periodicals) has supplied unquestionably correct analyses of the complicated and often corrupt forms derived from that stock.

SOME RECENT EUROPEAN ARTICLES ON AMERICAN ARCHÆOLOGY.

ALTHOUGH some lofty archæologists in the United States display an inability to perceive the value of the antiquities of this continent, it is gratifying to note that this purblindness does not prevail in Europe.

What native American skill could accomplish in the line of true art is well shown by the reproduction on the design on a beautifully colored and decorated vase from Chama, Guatemala, figured by Herr Dieselforff in the *Zeitschrift für Ethnologie*, 1894, Heft V. It will creditably bear comparison with the higher periods of Etruscan technique.

In a publication which has been lately started by the Museum of Ethnography of Berlin, called *Ethnologisches Notizblatt*, Dr. E. Seler, well known for his profound researches into Mexican antiquity, has a copiously illustrated article on the great stone sculptures of the National Museum of Mexico. He identifies several of the figures about which doubt has been entertained.

The Count de Charencey, also an author who has written abundantly on American subjects, has an article in the *Revue des Religions* for June last, on *Les Déformations Craniennes.* Unfortunately, he has not outgrown the theories of Angrand and other obsolete writers, who saw 'Toltecs' and 'Asiatic influence' and the 'Ten Lost Tribes' wherever they turned their gaze in the New World. It is a pity that his real learning should be thus misdirected.

The Report, the ninth, of the British Association on the *Northwestern Tribes of Canada*, contains this year but 11 pages, written by Dr. Boas. At the next meeting it will conclude its labors.

SOME OF ADOLPH BASTIAN'S LATER WRITINGS.

THE untiring activity of Professor Adolph Bastian, who for more than a quarter of a century has occupied the position of Director of the Royal Museum of Ethnography at Berlin, is something amazing.

He but recently returned from a long journey in the Orient, one of the products of which was a remarkable book with a not less remarkable title, *Ideal Worlds according to Uranographic Provinces*, in which he discusses at length the cosmogonies and theogonies of the philosophers of India. This indicates the special direction of his studies of late years. They have turned toward the elementary conceptions of primitive and early peoples concerning the universe, cosmogony and theogony, the nature and destiny of the soul, the life and supposed worlds hereafter, the processes of thought, the notions of social relation, traced as far into their abstract forms as it was possible for the human mind in that stage of development to conceive and express them.

This tendency is illustrated by the titles of some of his latest issues; as, *Vorgeschichtliche Schöpfungslieder in ihren Ethnischen Elementargedanken; Zur Mythologie und Psychologie der Nigritier in Guinea mit Bezugnahme auf Socialistische Elementargedanken; Wie das Volk Denkt; ein Beitrag zur Beantwortung sozialer Fragen auf Grundlage Ethnischer Elementargedanken*, etc.

These writings are all crammed with wide erudition and mature reflection; but, unfortunately, the author persists in following a literary style of expression which is certainly the worst of any living writer, intricate, obscure, sometimes unintelligible to a born German, as one of his own pupils has assured me. This greatly limits the usefulness of his productions.

D. G. Brinton.

University of Pennsylvania.

THE CONNECTICUT SANDSTONE GROUP.

The attempt to revive the abandoned name of Newark for the older designation of Connecticut, in its application to the Triassic terranes in the Atlantic geographic area, is supported by G. K. Gilbert and opposed by B. S. Lyman, in a joint discussion, in the *Journal of Geology*, Vol. II., No. 1. One would think that the considerations presented by me in the *American Geologist*, Vol. V., page 201, would have been sufficient to satisfy any one looking at the subject judicially and impartially, of the inadequacy of the name Newark to special recognition. In seeking a name for a terrane we should naturally inquire, *first*, where is the area which exhibits best the typical features? In answer to this we have the fact that in the Connecticut area the early exploration was the most thorough, the very unique occurrence of fossil footmarks was first recognized, and is the only one in which they have been thoroughly studied. At first these were thought to have been made by birds; but the later suggestion of deinosaurs has been verified by the masterly restorations of Anchisaurus by Prof. O. C. Marsh, obtained in the same Connecticut valley. Reptilian bones were known also from Pennsylvania, but no one has ever connected them with the tracks. Thus the feature which characterizes the American Trias is found in its perfection in the Connecticut and not in the Newark area. The fish are also more abundant in the first named area. The other features of importance are the coal and fossil plants, and these are best developed in a Virginia area.

Second. It is essential for the suitability of a geographical term, that the locality be one where the terrane should be exhibited in its entirety or maximum. The Connecticut valley has the whole series. The city of Newark 'does not contain one-fourth part of the thickness of this sandstone, and that which is visible is only a fraction of this fourth.' This early statement of mine is confirmed by Mr. B. S. Lyman, who says the exposures at Newark amount to 'one-tenth or one-twentieth of the beds to be included in the name.' Mr. Lyman has still later called attention to the probability that the Newark beds belong to the Permian instead of the Triassic.

Third. The name of Connecticut or Connecticut river sandstone has precedence over Newark. It was both in actual use before the suggestion of Newark, and was again proposed and used after 1856 and before 1892, because no one except Mr. Redfield employed the term Newark. The proposal was never accepted by the geological public.

In the early days of geology the use of local names was confined to the groups like

Silurian and Devonian. It was not until geologists found it necessary to specify the smaller divisions that it was discovered how convenient they were. The first users of names like Potsdam and Trenton did not make formal announcements that hereafter a particular name would be applied to a definite set of beds with special paleontological characteristics. It was the 'sandstone of Potsdam,' the 'limestone of Trenton Falls,' enunciated almost apologetically. We would not to-day question the validity of these early names because their authors did not set them forth in their perfection, like Minerva springing forth from the brain of Jupiter. I find the suggestion of Connecticut to have been made by E. Hitchcock in his report upon the Geology of Massachusetts in 1833, page 209. He says, 'the group which I denominate *new red sandstone in the Connecticut valley*' (the italics are mine). This was repeated in the Final Report, p. 441. Like his contemporaries he preferred the use of the European term of Trias, New Red or sometimes Liassic to the geographical one. We note that the expression of new red sandstone in the Connecticut valley is fully as definite as the later one of sandstone of Potsdam. This usage of Connecticut appears in all of E. Hitchcock's papers, and he distinctly included the terranes of New Jersey, Virginia and North Carolina. I quote later samples of its use. In the Ichnology of New England, 1858, page 20, may be found the following heading descriptive of an extended discussion; '5. Conclusions as to the Age and Equivalency of the Connecticut River Sandstone.' In 1859 he published in the Report of the Secretary of the Massachusetts Board of Agriculture a catalogue of the State Collection. The following is the headingused descriptive of the specimens from this terrane: "CONNECTICUT RIVER SANDSTONE. (*Liassic and perhaps Triassic and Permian sandstones and limestones.*)"

In 1860 Messrs. H. and C. T. Smith, 356 Pearl street, New York, published a wall map of Hampshire county, Massachusetts, based upon the surveys of Henry F. Walling. Hundreds, perhaps thousands, of these maps adorned the walls of houses belonging to citizens of that county. Upon it was placed a geological map of the county by Edward Hitchcock, and in explanation of the colors we have 'Connecticut River Sandstone, Lower and Upper,' and the words New Red or Trias do not appear at all. Thus the usage of the name Connecticut in the writings of this author has been constant and has passed from the employment of both the European and local terms conjointly to the use of the latter one exclusively.

Other earlier authors employed the geographical name in a geological sense. Thus Lyell in his Travels, 1845, page 100, Vol. 2, says 'the Connecticut deposits.' Dr. James Deane constantly speaks of the Connecticut river sandstone; and in his final work upon the footmarks, a quarto with 61 pages and 46 plates, published by Little, Brown & Co., Boston, in 1861, his title is 'A Memoir upon the Fossil Footprints and other Impressions of the Connecticut River Sandstone, by James Deane, M. D.'

Roderick Impey Murchison, in his anniversary address before the Geological Society of London, 1843, page 107, etc., speaks of the 'deposit in Connecticut' and the 'ornithichnite and Paleoniscus beds of Connecticut.'

Dr. John C. Warren, President of the Boston Society of Natural History, is reported as having given 'an historical account of the science of Ichnology, particularly as illustrated by the fossil footprints in the Connecticut River Sandstone;' Nov. 2, 1853, *Proc. B. S. N. H.*, Vol. IV., p. 376. Various remarks of his on these subjects were printed in 1854 in a book entitled '*Remarks on Some Fossil Impressions in the*

Sandstone Rocks of Connecticut River,' by John C. Warren, M. D., President of the Boston Society of Natural History.

Prof. W. B. Rogers, at a meeting of the Boston Society of Natural History, June 20, 1855, spoke of the discovery of the fern Clathropteris in the 'Connecticut River Sandstone.'

The use of the name Connecticut River Sandstone as applied to the rocks in question seems to have been universal among the members of the Boston Society of Natural History in the fifties, and it is applied as a matter of course in the index in Vols. V., VI., VII., etc. Mr. T. T. Bouvè also uses the expression prior to 1855.

A sufficient number of citations have now been made to prove the frequent application of the term Connecticut River Sandstone to the Triassic terranes before the proposal of W. C. Redfield in 1856 to apply the designation of Newark to the same. Others could be added. But I will in the next place call attention to the fact that no one had followed Redfield's suggestion till 1889, a period of a *third of a century*, until Mr. I. C. Russell proposed to revive the name of Newark. Every American geologist by his silence indicated his disapproval of the suggestion. Furthermore, the use of the expression Connecticut had become pronounced. In fact, its use, coupled with the rejection of Newark, is sufficient to establish the usage of the former without any regard to the usage previous to 1856. I will cite a few instances of its use. The catalogue of the Massachusetts State Cabinet in 1859, the Ichnology in 1858, the map of Hampshire county, 1860, and the title of Dr. Deane's book in 1861, belong to this category. H. D. Rogers, in the Geology of Pennsylvania, 1858, prefers the term 'older Mesozoic,' but certainly rejects the use of Newark, as he makes no reference to it, and uses the following expressions : 'The vegetable fossils in the Connecticut sandstone ;' 'the organic remains in the Connecticut red sandstone.' A title, 'Red Sandstones of the Connecticut Valley.' Roswell Field 'made a verbal communication on the footmarks of the Connecticut river sandstones' before the Boston Society of Natural History, June 6, 1860. In 1859, at the Springfield meeting of the A. A. A. S., he discusses the ornithichnites of the 'sandstone of the Connecticut valley.' This paper was reprinted the following year in the *American Journal of Science.*

Prof. O. C. Marsh presents in a section illustrating the occurrence of vertebrate life in America the name of *Connecticut river beds* which includes all the Atlantic areas. This has been printed with his 1877 address before the A. A. A. S., the third edition of Dana's Manual of Geology, 1880, the monograph on the Dinocerata, 1885, etc.

Prof. Joseph Le Conte in his Elements of Geology, 1878, and later editions describes the eastern Jura-Trias under the head of *Connecticut river valley sandstone.*

Prof. J. P. Lesley in C 4 of Second Pennsylvania Survey, p. 179, 1883, says, "American geologists now write habitually of the *Triassic red sandstone* of the Connecticut valley and of North Carolina." Although the Newark area was through Pennsylvania he prefers to select the locality name from either of the other principal areas. There are two references to the want of acceptance of the term Newark. I had the pleasure of attending Prof. J. D. Dana's course of lectures on Geology at Yale College in 1856. I noted that he then mentioned the fact that Mr. Redfield had proposed the name of of Newark for the American Trias. But he has never used the name in any publication, evidently for good reasons. In a sketch of the Geology of Massachusetts with map in Walling's Official Atlas, 1871, the following is printed, written by myself: "W. C. Redfield proposed the name of Newark sandstones for the group; but besides being inappropriate, it was of later date than the appellation of Connecticut."

This review of the usages of names for the trias shows that the name of Connecticut was distinctly proposed by E. Hitchcock in 1833, and was constantly used by the geologists specially interested in those works before 1856: W. C. Redfield proposed the name of Newark for the terranes in 1856: that instead of accepting the name geologists universally employed the name of Connecticut when using a local designation up to 1889: that in this period there were several unmistakable formal proposals of the use of Connecticut: and that there were in this period allusions to the fact that the name of Newark was not accepted. Even Mr. Russell, in his learned paper of 1878, used the name of Triassic in preference to Newark.

Mr. Gilbert mentions three 'qualifications of a geographic name for employment in stratigraphy, (1) definite association of the geographic feature with the terrane, (2) freedom of the term from pre-occupation in stratigraphy, (3) priority.' These are acceptable with the addition of a fourth, appropriateness of application. All of these qualifications are possessed by the term Connecticut , while the term Newark cannot satisfy a single one of them.

C. H. HITCHCOCK.

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LENGTH OF VESSELS IN PLANTS.

THE diameter of pitted and other vessels is easily measured upon the cross-section of any stem, but their length is less readily determined. Probably, if the question were put, a majority of botanists would say that they rarely exceed a few inches in length, especially if they still believe with Sachs that the water ascends through the walls of the vessels. As a matter of fact, the spiral and pitted vessels of plants often form open passageways of great length. Some experiments made upon woody stems by Strassburger (*Ueber den Bau u. die Verrichtungen der Leitungsbahnen in den Pflanzen*) seem to place this beyond dispute. His method of procedure was to fasten a glass tube to the upper end of a cut stem by a rubber band, insert a funnel into the upper end of the tube, and subject the cut surface to the pressure of a column of mercury kept at a uniform height of twenty centimeters, successively shortening the stem until mercury appeared at the lower end. Using this method, he obtained the following results:

(1.) In a branch of *Quercus rubra*, 1.5 meters long and about three centimeters thick, mercury ran out of thirty vessels on the lower cut surface almost as soon as it was poured into the funnel. When the branch was shortened to one meter fifty-four to fifty-six vessels were permeable. In a slender branch of *Quercus pedunculata*, one meter long, thirty-five vessels dropped mercury, and when this was shortened to one-half meter mercury came out of more than 100 vessels. Another branch five centimeters thick at the base and 3.6 meters long was tried, and drops of mercury fell in quick succession from eight vessels. In *Quercus Cerris* mercury came through seven vessels of a branch four meters long and six centimeters thick at the base. Shortened to 3.5 meters nine vessels dropped mercury; at three meters, twelve vessels; at 2.5 meters, numerous vessels. *Conclusion:* Vessels two meters long are quite common in the oaks, and it is probable that single vessels may be as long as the stem itself.

(2.) In *Robinia Pseudacacia*, a branch two meters long and three centimeters thick was impermeable and first let through mercury when shortened to 1.18 meters. Then it dropped from four vessels. Successively shortened mercury dropped from an increasing number of vessels as follows: One meter, nine vessels; fifty centimeters, thirty-eight vessels; twenty-five centimeters, fifty-seven vessels.

(3.) A stem of *Wistaria* 1.75 meters long and having seven internodes dropped mercury from seven vessels. Another stem three meters long and containing forty-seven internodes was first killed by heating for an hour in water at 90°, and then dried. This did not let mercury through until it had been shortened to 2.5 meters. Then it dropped pretty fast from four vessels. Reduced to two meters, nine vessels dropped mercury, and out of some it ran rapidly. Another shoot gave nearly the same results. A fresh and very long stem had to be shortened to three meters before mercury came through. Then it dropped from three vessels. Successively shortened, the number of permeable vessels was as follows: 2.5 meters, eleven vessels; two meters, eighteen vessels; 1.5 meter, twenty-seven to twenty-nine vessels. These stems were one to two centimeters thick. *Conclusion:* Some of the vessels in *Wistaria* are quite long, though scarcely more than three meters. Most of the wide vessels are about one meter long.

(4.) A cane of *Vitis Labrusca* 1.2 centimeter thick, which was previously killed by heating for an hour in water at 90° C. and then air-dried, first let mercury through (3 vessels) when shortened to 2.2 meters.

(5.) A shoot of *Aristolochia Sipho* 1.5 centimeters thick, 2.5 meters long, and having fifteen internodes was killed in the same way. This let mercury through fourteen vessels. Another shoot 2.1 meters long let the mercury through many vessels. A fresh stem five meters long, the longest he could get, dropped mercury from five vessels. When successively shortened, more and more vessels dropped mercury. At 3.5 meters twenty-five vessels let it through, and when the stem was cut down to three meters the number of vessels dropping mercury could not be determined. *Conclusion:* In this plant numerous vessels are three meters long, some are five meters long, and a few are probably longer.

In *Aristolochia* the vessels of different annual rings were equally permeable, but in the wistaria, the locust and the oaks the permeable vessels were mostly on the periphery. The records were made in from ten to thirty minutes from the beginning of the pressure, the time depending on the length of the stem. In general the mercury was passed through the stem in the same direction as the ascending water current, but a change of direction did not give contradictory results. These experiments were repeated, using a pressure of forty centimeters, but even this did not rupture any cross-walls. This increased pressure overcame the capillary resistance and forced the mercury through many smaller vessels, but otherwise the results were much the same.

ERWIN F. SMITH.

WASHINGTON.

SCIENTIFIC LITERATURE.

Introduction to Elementary Practical Biology.—By Charles Wright Dodge, M. S.—Harper Bros., New York. 1894.

This book is a laboratory guide for high school and college students. The teacher of biology who is endeavoring to train his students in the best manner is in modern times, amid the abundance of laboratory guides, in very much of a quandary as to the best of two opposite methods. If, on the one hand, he puts a laboratory guide into the hands of the student, the result is apt to be that the student soon learns simply to verify the facts mentioned in the book, and thus loses all stimulus for original observation, which should be the foremost result of practical work in biological science. On the other hand, if the teacher gives to an elementary student a specimen to study without laboratory directions, he is at such complete loss to know how to proceed, what to do, and particularly what points to notice, that a large proportion of his time is wasted through sheer lack of the proper knowledge of methods. To force a student to invent methods does stimulate indeed observation, but it is a very great waste of time on the part of most students. Between this loss of stimulus to original observation and the loss of time, the instructor is very puzzled how to proceed.

Prof. Dodge of Rochester University in the guide just published has attempted to solve the problem by a new method of direction. The laboratory guide here noticed gives the student some few directions as to methods of dissection and methods of procedure, but beyond this gives him practically no information in regard to his specimens. By a series of skilfully arranged questions it forces the student to make his own observations and to make them in the right direction. Instead of directing the student to observe a certain fact a question is asked which leads him to hunt for a solution, and the result is independent observation. This method of study renders the text book of no value unless the student has the specimen directly in front of him, for there is no possibility of answering these questions in any other way than from the specimen.

The method of teaching here planned is certainly an ideal one and has been quite successfully carried out by Prof. Dodge. It is true that the questions given are sometimes entirely beyond the possibility of the student's solution, and it must also be recognized that this method is one designed to occupy a very great amount of time. Some of the problems which are set before the student will require days for solution, and others have not yet been settled by the observation of scientific investigators. It will therefore take a great amount of time to complete the outline given, for the book is a comprehensive study of biology, including the study of the animal and vegetable cell, on the side of animals, the study of the sponge, hydra, campanularian hydroid, star fish, earthworm, the lobster, locust, clam, and the frog; and on the side of the vegetable kingdom, green felt, stone work, rock weed, mould, mushrooms, liverworts, ferns and flowering plants. Whether the student in the time allotted to the study of general biology even in our best colleges will be able to complete the list by the method outlined in the guide is doubtful, but there can be little doubt that the method of teaching adopted by Prof. Dodge in this book is an ideal one, and for stimulating observation and at the same time enabling the student to do the most work in the smallest amount of time, there is perhaps no laboratory guide in biology yet published which succeeds as well as the one here noticed.

H. W. CONN.

WESLEYAN UNIVERSITY.

Le Grisou [Fire Damp], par H. LE CHATELIER, Ingénieur en Chef des Mines.—Professeur à l'École nationale des Mines.—Paris, Gauthier Villars et Fils, 1894. Pp. 187. Broché 2 fr 50, Cartonné 3 fr.

The rapid extension of technical scientific knowledge, and the increasing call for specialists in every department, is best shown in the literature of the past few years. The discussion of general topics within the limits of a single volume is now possible only in the most elementary works designed for beginners and for the lower classes of our colleges. We have in place of the general text book a rapidly increasing library devoted to special subjects, each presented by specialists in their own field and each treating of some small part of the great sciences formerly considered as a unit. The present volume is of this nature, and, coming from the hand of an engineer of wide reputation, will be of great service to all advanced students of mining whether still within the college confine or employed in the active practice of their profession. 'Fiery' mines are common in our coal fields, and many mines long worked without suspicion of danger, or with carelessness engendered by delayed casualty, suddenly become the scenes of disaster and great loss of life. M. Le Chatelier has brought together a great mass of facts from many sources and has so presented them as to place them conveniently within reach of all workers in the field. Part I. treats of the nature and production of fire damp, its composition, manner of explosion, its limit of inflammability, and other properties, physical and chemical. Part II. is highly practical and is devoted to the consideration of the immediate cause of accidents, with precautions against the same, the use of safety lamps and of safety explosives, etc. To those desiring a more extended treatment of any of these subjects, or those wishing to consult original papers, the very complete Bibliography which is given at the end of the work will be of great service, particularly as a guide to continental publications.

CHARLES PLATT.

PHILADELPHIA.

At the North of Bearcamp Water.—Chronicles of a Stroller in New England from July to December.—By FRANK BOLLES.—Houghton, Mifflin & Co., 16 mo. pp. 297.

Any one who will go afield in the rain for the purpose of seeing how the wet birch trees look, or who will stay through a stormy night on a mountain top for the sake of the scenery, has certainly a lively interest in nature. The late Frank Bolles had all of this interest and in addition a kindly sympathy with every wandering creature. In his last book, *At the North of Bearcamp Water*, one does not find as many paragraphs suitable for quotations on a daily calendar as would occur in a volume of Thoreau, but his description of a July afternoon when "The air was full of quivering heat and hazy midsummer softness," has all the strength of beauty and truth.

The book particularly describes nature in the vicinity of Chocorua mountain, but there are also chapters on Old Shag, Bear and other White Mountain peaks. In these accounts of scenery of deer, foxes, birds and trees there is an evident truthfulness, as real as the objects themselves. The mass of detail brought into some of these chapters is surprising, and a frog did not jump across the path without being made to play his part in the account of the day's ramble.

Among the most interesting pages are those devoted to 'A Lonely Link,' and to 'A Night Alone on Chocorua.' Mr. Bolles had his red roofed cottage by the lake and describes the squirrels, muskrats, porcupines, and many birds that were his neighbors. The narrative is peaceful in tone, as restful as a quiet ramble in the woods, and those who wish to be transported in spirit to pleasing natural scenes will do well to accept Mr. Bolles as guide.

W. T. DAVIS.

NOTES.

THE BOTANICAL SOCIETY OF AMERICA.

The Botanical Society of America was organized during the meeting of the American Association for the Advancement of Science at Brooklyn, N. Y., in August, 1894. The following extracts from the Constitution adopted are of general interest.

"There may be two classes of members—active and honorary. Only American botanists engaged in research, who have published work of recognized merit, shall be eligible to active membership. Before the 1st of January following his election, each active member shall pay into the treasury of the Society a fee of twenty-five dollars ($25), and thereafter annual dues to the amount of ten dollars ($10), payable before the 1st of January."

"Candidates for active membership shall be recommended by three active members of the Society not members of the Council, who shall certify that the candidate is eligible under the provisions of the Constitution. These nominations shall be placed in the hands of the Secretary at least three months before the meeting of the Society which is to act on them. Two months before said meeting, the Secretary shall cause to be prepared and sent to each active member of the Society a list of the nominees, indicating the residence, occupation and qualifications of each and the names of those recommending him."

"The officers of this Society shall be a President, Vice-President, Secretary and Treasurer. Their duties shall be those usually performed by such officers in other bodies, and such additional duties as may be prescribed by the Constitution of this Society. They shall hold office through the annual meeting following the year of election, and until their successors have been elected and qualified. An address shall be delivered by the President at the annual meeting two years after his election."

"The officers, together with the last Past-President and two members elected by the Society at its annual meeting, shall constitute a Council, which shall be charged with such duties as are prescribed by the Society, and shall represent the Society in the interval between meetings of the latter, reporting any *ad interim* action at the next general meeting of the Society; but acts of the Council not specified in the Constitution, or for which special power has not been conferred by the Society, shall be binding on the latter only after they have been reported and approved at such general meeting. The Council shall constitute a Publication Committee, charged with editing, publishing and distributing such publications as may be authorized by the Society, and they shall have the power to select from their own number or the membership of the Society an editor to whom they may delegate the immediate duty of editing such publications. They shall all constitute a Board of Curators for the property of the Society, subject to such rules as are provided in the Constitution or otherwise prescribed by the Society."

"The Society shall hold an annual meeting at such time and place as the Council each year may select; and special meetings for the presentation of papers or the transaction of business, at such other times and places as the Society or Council may from time to time deem necessary."

The officers for the present year are: Prof. Wm. Trelease, Missouri Botanical Garden, President; Prof. N. L. Britton, Columbia College, New York City, Vice-President; Prof. C. R. Barnes, University of Wisconsin, Madison, Wis., Secretary.

PSYCHOLOGY.

The department of Philosophy and Psychology at Chicago has been made this year one of the strongest in America. Professor Dewey, formerly of the University of Michigan, has accepted a call to the Head Professorship of Philosophy; Mr. G. H. Mead, also of the University of Michigan, has been made assistant Professor of Philosophy; Mr. J. R. Angell, formerly of the University of Minnesota, has been made assistant Professor of Psychology, and Mr. S. F. McLennan has been made assistant in Psychology.

ARTICLES ON SCIENCE.

Among the articles of scientific interest in the popular magazines are the following:

A New Flying Machine, Abram S. Maxim (*Jan. Century*); Want of Economy in the Lecture System, John Trowbridge; The Genius of France, Havelock Ellis; Gallia Rediviva, Adolphe Cohn (*Jan. Atlantic Monthly*); The World's Debt to Astronomy, Simon Newcomb (*Dec. Chautauquan*); The World's Debt to Chemistry, H. B. Cornwall (*Jan. Chautauquan*); Mental Characterists of the Japanese, George Trumbull Ladd (*Jan. Scribner's*); Heredity, Part III., St. George Mivart (*Jan. Humanita-*

rian); Recent Science, Prince Krapotkin (*Dec. Nineteenth Century*).

Nature has reprinted (Dec. 13 and 20) in full the interesting address on *Endowment for Scientific Research and Publication* given by Mr. Addison Brown before the Scientific Alliance of New York, and published in the Report of the Smithsonian Institution for 1892.

Mr. Kumagusu Minakata has written, in view of the claims of priority recently made by two English writers, a letter to *Nature* (December 27), calling attention to the use of 'finger-prints' as a means of signing documents and identification in the laws and usage of China and Japan as early as 650 A. D.

The *Naturwissenschaftliche Rundschau* is publishing in its current numbers an account of the sixty-sixth *Versammlung der Gesellschaft deutscher Naturforscher und Aerzte*, held last year in Vienna.

FORTHCOMING PUBLICATIONS.

Following the publication of H. M. Ward's translation of Hartig's *Text-book of the Diseases of Trees*, the same publishers (Messrs. Macmillan & Co.) announce as nearly ready three other important translations: Rätzel's Völkerkunde, translated by A. J. Butler; the article *Construction* from Viollet le Duc's *Dictionnaire raisonné de l' architecture française*, translated by G. M. Duss, and Paulsen's *Universities of Germany*, translated by E. D. Perry, of Columbia College.

There will be issued this month as a supplement to *The Psychological Review* a *Bibliography of Psychological Literature for 1894*, compiled by Dr. Livingston Farrand, of Columbia College, and Mr. Howard C. Warren, of Princeton College. The bibliography will include so far as possible all books, monographs and articles in Psychology, and those publications in philosophy, biology, anthropology, neurology etc., which are important for psychology.

An *Année Psychologique*, edited by Professor Alfred Binet, will be issued in March.

Messrs. Macmillan & Co., announce for early publication *A Rural Science Series*, edited by Professor L. H. Bailey, of Cornell University.

SCIENTIFIC JOURNALS.

THE BOTANICAL GAZETTE, DEC.

Contribution to the comparative histology of pulvini and the resulting photeolic movements. (*With plate XXXIV.*) F. D. Heald.

Two new ferns from New England: George E. Davenport.

Some notes on the Leguminosæ of Siam: Glenn Culbertson.

Briefer Articles; Editorial; Current Literature; Notes and News; General Index.

THE PSYCHOLOGICAL REVIEW, JAN.

Hermann von Helmholtz and the New Psychology: C. Stumpf.

The Theory of Emotion (II.); The Significance of Emotions: John Dewey.

The Muscular Sense and its Localization in the Brain Cortex: M. Allen Starr.

A Location Reaction Apparatus: G. W. Fitz.

Discussion:—Paul Shorey; H. M. Stanley; H. R. Marshall; E. B. Titchener.

Psychological Literature; Notes.

THE ENGINEERING MAGAZINE, JAN.

Silver Coinage Historically Considered: H. D. McLeod.

Modern Theories as to Electricity: Henry A. Rowland.

The Drainage System of the Valley of Mexico: Hon. M. Romero.

Practical Hints for City Officials: E. C. Gardner, Lewis M. Haupt.

Selecting Motive Power for a New Plant: Charles E. Emery.

Plumbing Trade Schools and Their Influence: E. N. G. LeBois.

Laboratory Training for Mining Engineers: R. H. RICHARDS.

Operating Machine Tools by Electricity: GEORGE RICHMOND.

First Principles in Architecture: WM. HENRY GOODYEAR.

SOCIETIES AND ACADEMIES.

THE LINNÆAN SOCIETY.

THE Linnæan Society of New York City, in coöperation with the American Museum of Natural History, has arranged for a series of illustrated lectures to be given in the large lecture hall of the museum, on Tuesdays at 8 P. M. The lectures are:—

FRANK M. CHAPMAN, assistant Curator in the American Museum of Natural History. *A Trip through the Lesser Antilles.* Physical and Natural History of the Islands, their Products and Inhabitants. January 8.

HENRY FAIRFIELD OSBORN, Sc. D., Da Costa Professor of Biology, Columbia College. *The Great West, a Half Million Years Ago.* An account of our Continent when it was separated from South America and joined to Asia, and the Climate and Vegetation were Sub-tropical. February 5.

WILLIAM LIBBEY, JR., Sc. D., Professor of Physical Geography and Director of the E. M. Museum of Geology and Archæology, Princeton College, New Jersey. *Hawaii, the Paradise of the Pacific.* March 12.

FREDERICK W. PUTNAM, Professor of American Archæology and Ethnology in Harvard University, and Curator of Anthropology in the American Museum of Natural History. *Ancient Earthworks in the Ohio Valley.* April 2.

UNIVERSITY ARCHÆOLOGICAL ASSOCIATION.

The University Archæological Association of Philadelphia offers a course of lectures to be given at 4 p. m., in the Library building of the University of Pennsylvania, as follows:—

January 9.—MR. TALCOTT WILLIAMS, *Some Morroccan Relations.*

January 16.—DR. DANIEL G. BRINTON, *The Beginnings of the Fine Arts.*

January 23.—MR. HENRY G. BRYANT, *Notes on the Most Northern Eskimos.*

January 30.—DR. HARRISON ALLEN, *The Human Skull; what is its Place in a Museum of Archæology?*

February 6.—CAPTAIN RICHARD S. COLLUM, U. S. M. C., *The Evolution of Small Arms.*

February 13.—DR. DANIEL G. BRINTON, *Love Charms and Tokens.*

Eebruary 20.—MR. STEWART CULIN, *The Wand of the Conjuror.*

STEWART CULIN, *Secretary.*

THE ROCHESTER ADADEMY OF SCIENCE.

Program of Meetings, 1895.

January 14.—Annual Meeting; Election of Officers; Illustrated Paper by the President, PROF. H. L. FAIRCHILD, *The Geology of the Pinnacle Hills.*

January 21.—EMIL KUICHLING, *The New Conduit of the Rochester Water Works.*

January 28.—Popular Lecture, J. D. MALLONEE, *The Structure of Rocks as Shown by Polarized Light.*

February 11.—J. STANLEY-BROWN, *The Pribilof Islands and the Seal Industry.*

February 25.—J. EUGENE WHITNEY, *The Depotism of the Plurality.*

March 11.—CHARLES H. WARD, *The Teeth of Man.*

March 25.—PROF. W. W. ROWLEE, *The Evolution of Seeds.*

April 8.—CHARLES WRIGHT DODGE, *Diphtheria and Anti-toxine.*

April 22.—ADELBERT CRONISE, *The Panama Canal.*

May 13.—RICHARD M. MOORE, *The Coleopterous Fauna of Rochester and Vicinity.*

May 27.—H. L. FAIRCHILD, *Glacial Lakes of Western New York.*

June 10.—H. L. FAIRCHILD, *The Geology of Irondequoit Bay.*

AMERICAN SOCIETY OF CIVIL ENGINEERS.

December 19.

Mansfield Merriman, *The Strength and Weathering Qualities of Roofing Slates.*

This paper, which will be published in the transactions of the Society, about February 1st, gave an account of original physical and chemical tests of the properties of different slates.

GEOLOGICAL SOCIETY OF WASHINGTON.

January 9.

Mr. J. S. Diller, *Artificial wire silver, prepared by* F. C. Phillips.

Mr. G. P. Merrill, *On the disintegration of the granitic rocks of the District of Columbia.*

Mr. W. Lindgren, *Characteristic features of the gold quartz veins of California, with specimens.*

Whitman Cross, *Secretary.*

THE BIOLOGICAL SOCIETY OF WASHINGTON.

January 12.

L. H. Bailey, *The Plant Individual in the Light of Evolution.*

Frederic A. Lucas, *Secretary.*

BOSTON SOCIETY OF NATURAL HISTORY.

January 14.

J. Walter Fewkes, *The new fire ceremony at Walpi.* Samuel Henshaw, *Secretary.*

THE NEW YORK ACADEMY OF SCIENCES.

SECTION OF BIOLOGY.

Exhibition of microscopical and lantern slides with notes on technique.

R. H. Cunningham, *On the Sources of Illumination for Photo-micrography.*

C. F. Cox, *The Lantern Slides of* Mr. E. F. Smith, *F. R. M. S., of London, illustrating the latest Theories of Diatom Structure.*

O. S. Strong, *Notes of new histological Nerve Methods.*

Edward Leaming, *Exhibition of photomicrographic slides, bacteriological, neurological, biological.*

Bashford Dean, *Secretary.*

THE NEW YORK ENTOMOLOGICAL SOCIETY.

January 15.

Meeting at American Museum of Natural History.

R. L. Ditmars, *Notes on a collecting trip through Connecticut.*

Lewis H. Joutel, *Secretary.*

NEW BOOKS.

Radiant Suns. Agnes Giberne. New York, Macmillan & Co. 1894. Pp. vii+328.

Race and Language. André Lefèvre. New York, D. Appleton & Co. 1894. Pp. vi+424.

Die Samoanische Schöpfungs-Sage und Anschliessendes aus der Sudsee. Adolf Bastian. Berlin, Emil Feller. 1894. Pp. 50.

Die Gross-Schmetterlinge Europas. Prof. Ernst Hofmann. 2d Ed. C. Hoffmann. 1894. Pp. xl+24. M. 28.

Model Engine Composition with Practical Instructions to Artificers and Amateurs. J. Alexander. London, Whittaker & Co.; New York, Macmillan & Co. 1894. Pp. viii+324. $3.00.

Ein geologische Querschnitt durch die Ost-Alpen. A. Rothpletz. Stuttgart, E. Schweizerbart. 1894. Pp. iv+268. M. 10.

Geotektonische Probleme. A. Rothpletz, Stuttgart, E. Schweizerbart. 1894. Pp. 175. M. 8.

Biological Lectures Delivered at the Marine Biological Laboratory of Wood's Hall, Boston. Ginn & Co. 1894. Pp. 242.

Introduction to Chemical Analysis for Beginners. Fr. Rudorff. Translated from the Sixth Edition by Charles B. Gibson and F. Menzel. Chicago, The W. T. Keener Co. 1894. $1.00.

The Etiology of Osseous Deformities of the Head, Face, Jaws and Teeth. Eugene S. Talbot, 3d Ed. Chicago. The W. T. Keener Co. Pp. xvi+487, $4.

SCIENCE.

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Nature.

A Weekly Illustrated Journal of Science. Yearly Subscription, $6.00. Single numbers, 15 cents. Sent for two months, as a trial subscription, on receipt of $1.00.

*** "Nature" has just completed its first quarter-century. The number for November 1st begins Volume 51, and contains an Introductory Article by the Rt. Hon. T. H. HUXLEY, entitled "PAST AND PRESENT."

The Psychological Review.

Edited by J. MARK BALDWIN and J. MCKEEN CATTELL, with the co-operation of ALFRED BINET, JOHN DEWEY, H. H. DONALDSON, G. S. FULLERTON, WILLIAM JAMES, JOSEPH JASTROW, G. T. LADD, HUGO MÜNSTERBERG, M. ALLEN STARR, CARL STUMPF and JAMES SULLY. Published bi-monthly. Single numbers, 75 cents; annual subscription, $4.00.

The Physical Review.

A Journal of Experimental and Theoretical Physics. Conducted by EDWARD L. NICHOLS, and ERNEST MERRITT. Issued bi-monthly. Vol. I., No. 1, July—August, 1893. Price, 50 cents; annual subscription, $3.00.

Contributions to THE PHYSICAL REVIEW should be addressed to the Editors, Ithaca, N. Y.; subscriptions to the Publishers, 66 Fifth Avenue, New York.

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SCIENCE.

FRIDAY, JANUARY 25, 1895.

CONTENTS:

MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison on Hudson, N. Y.
Subscriptions (five dollars annually) and advertisements should be sent to the Publisher of SCIENCE, 41 East 49th St., New York.

*THE PAST AND FUTURE OF THE AMERICAN MATHEMATICAL SOCIETY.**

HAVING been requested by the Council to address the Society on retiring from the presidency, it has appeared to me that I must choose between the discussion of the position and prospects of some branch of mathematics with which I may be familiar and a more general and discursive review of the present position and future prospects of our Society. I have, after some hesitation, chosen the latter subject. It seems desirable, on the whole, that there should be made at this time some permanent record, however slight, of the steps by which so large and flourishing a society has come together, and of the views concerning its present scope and the hopes concerning its future possibilities which are entertained by those who have hitherto been most immediately concerned in the conduct of its affairs.

The New York Mathematical Society, originating in 1888, was at first not much more than a small mathematical club meeting periodically at Columbia College. The first meeting was called by a circular signed by three young men. The number of those who could be expected to attend these meetings was not great, but all who were able and who were sufficiently interested to do so were invited to join the Society. It was fortunate in securing for its first president Professor Van Amringe, distinguished alike by scientific attainments, official eminence, and administrative ability. The professor of astronomy at Columbia was also active in it from the first. The meetings of the young Society were, as I am informed (for at that time I did not reside in New York),

*Address delivered by the retiring President, before the American Mathematical Society at the annual meeting held December 28, 1894.

attended with more than interest, I might say with zeal. The three who called the Society into being may, without invidiousness, be mentioned as having aided materially in the prosecution of its work. One of these of course need not be named to you. He has served from the beginning as Secretary, and again as the leading member of the publication committee. It is no flattery to him to say that the growing energies of the Society must at various stages have become chilled or misdirected, except for his comprehensive intelligence and untiring industry. Another was our former Treasurer, now absent from the country. Still another has been elected by you to-day to the office of Librarian. The meetings were, as the ordinary meetings still are, held at Columbia College, and at that time the majority of the members of the Society were connected with that institution. The President, after two years' service, fearing that the continuance of a representative of that College as presiding officer would tend to hamper its usefulness, proposed the election of a new President not connected with any college. It was in this way and for this reason that you honored me with the post from which I retire to-day. It is not improper for me to add that I am myself an outspoken believer in the doctrine of rotation in office, and that I was only prevented from retiring at an earlier period by urgent representations concerning the presumed welfare of the Society, at a time when all were not yet fully agreed upon the expediency of changes which have since taken place.

The Society was therefore distinguished from all American mathematical clubs or associations by two circumstances: it was formed in and took the name of the largest city of America, and it was distinctly understood to be unconnected with any institution of learning. Suggestions came to be made that its usefulness would be decidedly increased by the publication of a periodical journal. Consideration of these suggestions by the Council led to the establishment of the Society's *Bulletin*, with the nature and scope of which you are all familiar. It was decided to be inexpedient to publish original investigations, that field being already occupied by successful American periodicals. To meet the expense of the publication, the fees of the members were somewhat increased, and for the same reason, as well as to extend the usefulness of the Society, well-known mathematicians in all parts of the country were invited to become members. That this movement towards enlargement was judicious and timely was proved at once by the rapid growth of the membership, which since the middle of 1891 has included a large proportion of the prominent mathematicians of the United States and Canada. As the Society thus became in reality an association of American mathematicians as a body, the change of name effected this year was only a natural sequence. Finally, the result of the change of name has been that a number of persons, including several of the highest repute, who had not previously joined the New York Mathematical Society, regarding it as a local organization, have connected themselves with the American Mathematical Society; and I need hardly say that, if any one of prominence still holds aloof, from inattention or otherwise, his entrance at any time as a member will be greeted with a hearty welcome.

It is said that when the London Mathematical Society was organized there had been no previous example of a similar organization, and that fears were felt and expressed that its management might naturally drift into the hands of a few having time and energy to give to its affairs, and that there might thus be serious danger of its falling into the control of a clique. The lapse of time has developed the fact that the leading members of that society have been men of broad views, unusually free from personal prejudice and quick to recognize talent wherever displayed. We may almost conclude from the history of that society that proficiency in the science of mathematics is distinct evidence of a well-balanced mind. It may be doubted whether an equally numerous body of poets or musicians could have held so successfully on its course during half a century. It is of course impossible to predict that the management of our own Society will be equally prudent, energetic and successful during a half century to come. All that can be said at present is that not a trace of personal self-seeking on the one hand or of personal prejudice on the other hand has as yet become visible in our counsels. One single motive has thus far been conspicuous among all who have interested themselves in the Society: a strong belief in its prospective usefulness combined with an earnest desire to further its success.

Thus far I have spoken only of the progress of the organization as such. The organization, however, is merely the framework. It has certain living objects, and even during its period of formation and growth it has been distinctly successful in promoting those objects. I have spoken of objects; the Constitution, however, reminds me that there is but one object: to encourage and maintain an active interest in mathematical science. It is, however, possible to subdivide this very general statement of the aims of the Society. In order to encourage and maintain an interest in mathematical science, we may say, then, (1) that mathematicians must be brought to know more about each other and concerning each other's work; (2) that the number must be increased by the encouragement of the study of the higher mathematics among the young; (3) that information should be disseminated fully and speedily concerning mathematical publications abroad as well as at home; (4) that, as regards the more important of such publications, competent critics should be induced to write and publish papers descriptive of their contents and indicating their merits or defects; (5) and that every member of the Society should be stimulated to the most successful effort possible in his own line of mathematical labor, whatever it may be. This subdivision is not presented as scientific and exhaustive. Others would doubtless make variations of their own; and it is certain that the separate points I have indicated are not mutually exclusive. I mention these several objects merely as they occur to me for the purposes of this occasion.

That by entering the Society and receiving its monthly *Bulletin* the mathematicians of the United States and Canada have been and are brought to know far more about each other and concerning each other's work than they ever knew before or could possibly have known otherwise is obvious to all. The mere list of members, which conveys to each of us the names, addresses and occupations of all the rest, would alone justify this statement. The *Bulletin*, with its lists and reviews of new books, together with many notes concerning the higher mathematical work of different institutions, has afforded much additional information, and it may be expected that further experience will enable its conductors from time to time to add to its usefulness in this direction.

While the Society is not directly concerned in encouraging the study of the higher mathematics among the young, its indirect influence in that direction has undoubtedly been felt, and must be felt increasingly as time goes on. Years ago, when the present century was much younger, the course of study in our colleges was so arranged as to give a large proportion of the time of the undergraduates to the study of mathematics. Subsequently the tendency in colleges having uniform courses of study was to cut down the number of hours given to this science, as well as to the classics, and to parcel out the time among the modern languages and various sciences. It is believed that even already the organization, the meetings, and the publications of the Society have, by the effect of numbers in association, perceptibly strengthened the tone of the mathematical departments of many institutions of learning and assisted in enabling them, more or less successfully, to stem the hostile tide of sentiment to which I have just referred. I say 'assisted,' for other agencies, especially the journals, have done great good. That the dissemination of knowledge concerning the gigantic strides lately made and still making in mathematical science must in the future have the same favorable effect to an even greater extent is not to be doubted.

As to the next point in my list of objects, I need hardly mention to you that the Society has succeeded and is succeeding in disseminating information fully and speedily concerning mathematical publications abroad as well as at home. In addition to this general statement, for the proof of which we need only refer to the monthly numbers of the *Bulletin*, I may recall to you that the Society is at this moment engaged in publishing, at its own expense, supplemented by personal subscriptions, one of the largest and most important volumes ever published containing nothing but original investigations; namely, the extensive and very valuable collection of papers—mostly by European authors—prepared for and presented to the Mathematical Congress in 1893, held in connection with the World's Fair at Chicago.

Again, considerable success has been attained in inducing competent critics to write and publish papers descriptive of the contents and indicating the merits or defects of the more important current mathematical publications in all countries. In this respect it is hoped that the usefulness of the *Bulletin*, already recognized, will be largely augmented as time goes on. You have to-day strengthened the Publication Committee by the addition of a third member of tried capacity. The Committee depends for its critical papers upon the coöperation of other members of the Society, and it is especially pleased to receive voluntary offers of such papers from members who have not not yet contributed to the *Bulletin*. It is to this resource as much as to any other that we must look for the enlargement and improvement of the *Bulletin*. The Committee must be aided freely by the presentation of an increasing amount of material from which to choose; and I use this occasion to urge upon each member that he take every opportunity consistent with other engagements to impart to his fellows the historical and critical results of his own reading in any special branch, and particularly in connection with any new and important work recently published in that branch. The well-known saying of Bacon cannot be too constantly before our minds: "I hold every man a debtor to his profession, from the which as men of course do seek to receive countenance and profit, so ought they of duty to endeavor themselves by way of amends to be a help and ornament thereunto."

Finally, and I might say above all, it is the object of the Society that every member should be stimulated to the most successful effort possible in his own branch of mathematical labor, whatever it may be; whether it be in teaching, or writing, or original investigation, or in any combination of these lines of activity. The investigator must also be a writer; the writer may present his own investigations, or comment upon or summarize or write the history of those of others, or elaborate a treatise or text-book upon some special subject; but whoever may investigate, and whoever may write, it is the lot of almost all of us in one way or another to teach. For this reason it is plain that this Society is, and must always remain, a society of teachers. Any tendency to restrict its usefulness solely to the paths of investigation and publication should, for every reason of prudence and wisdom, be resisted. The management of any organization which does not commend itself to the great majority of those interested must not indeed necessarily end in failure, but must certainly fail of producing the most appropriate, the most useful, and therefore the best results. While, however, expressing this general opinion, I would by no means be understood to disparage the work of the writers and investigators. Not every teacher, however successful, feels impelled to write for publication, and not every writer has time and facilities for original investigation; yet we all of us take pride in such work when done by others, and we all of us, as members of the Society, feel that it would fail of its highest objects if it did not encourage in every way the production of good papers and books and, above all, the prosecution of original discovery.

In encouraging the writing of books, as distinguished from the prosecution of original research, the Society can do little except indirectly, by increasing the possible demand for such works. The need of what we may call advanced text-books giving, as far as possible, summaries of existing knowledge in the several higher branches, has long been felt, and of late years has to some extent been supplied. Some of these fields, however, are still open; and as time goes on, fresh books, to take the place of those now fresh, will still be wanted; for our science is in all points, even those sometimes regarded as most stationary, in a condition of advancing evolution. It is, if you please, the same old oak, but what formerly were twigs are now sturdy limbs, and what now are tiny stems may soon be recognized as vitally important branches. As to the making of thorough and systematic books on mathematical subjects, it has before now been remarked that the task is really more difficult, for some at least, than that of working up original papers. Some of the reasons for this were clearly stated by Mr. Glaisher in his presidential address in 1886 before the London Mathematical Society. I recall the case of a friend who at one time began the preparation of a summary of knowledge in a special field; but he had not gone far before he found such temptations in the way of unifying theories or bridging over gaps that the result was the production of two or three contributions to the journals and the abandonment of the book. We must, I think, accord unusual honor to those who apply themselves successfully to the task, more arduous every year as the mass of original work rolls up, of summarizing and condensing into clear bodies of doctrine all existing important discoveries in special fields of mathematical labor, certainly without hope of pecuniary reward and usually without prospect of any wide circle of readers.

As yet the Society has done little towards the encouragement of writing and investigation. The existing well-known and successful journals maintained, whether by a great university, by scientific societies of a general character, or by the generous efforts of individuals, have afforded opportunities for the publication of extensive papers with which the Society's *Bulletin* is not intended to compete. For much the same reason those of its members who have been personally solicited to give their aid have been appealed to for contributions to the *Bulletin* rather than for original papers to be read and discussed at the meetings. It is to be hoped that, as time goes on, the members of the Society will become more and more accustomed to present their original papers for reading at the meeting, before publication. It may perhaps also be expected that a closer connection may be developed between the reading and the publication of such papers, whereby, on the one hand, perhaps, editors of journals may, as members of the Council or otherwise, have some preliminary oversight of the acceptance of papers for reading, and whereby, on the other hand, the acceptance of a paper for reading shall insure its speedy publication.

While the Society can thus do little directly to encourage the writing of important treatises, it can and should, without doubt, do much to stimulate original research. Original discovery has always been recognized as the quickest and surest road to distinction. A permanently valuable paper read and discussed at a meeting of the Society becomes an immediate object of interest to those who attend; the subsequent record of the reading in the *Bulletin*, supplemented, perhaps, by a brief abstract, excites a still wider interest among the membership at large; and in this way many of the readers are prepared to welcome the publication of the paper when it appears. There are—apart from the institution of medals or prizes, which would be within the Society's province—many other ways in which, directly or indirectly, the influence of the Society may be felt in turning the attention of individuals to the importance of original work. And as some slight contribution towards this desirable end, I shall close this address with a few remarks and suggestions intended more particularly to reach those members of the Society whose attention is turning in this direction, but who have not as yet produced original papers. If in doing so I happen to give good advice, particularly as regards style, of which I have not always succeeded in following myself, I trust I may be favored with the same kindly personal consideration as is customarily accorded to an emaciated physician or to a stammering professor of rhetoric. Yet as to the style in which a mathematical paper should be written, as distinguished from good English style in general, there is not really very much to be said. Such papers should contain good English, and enough of it. Obscurity, above all things, should be avoided. The printer should not be annoyed unnecessarily by complicated fractions and other things difficult to print. Phrases and symbols familiar to the writer, but not necessarily familiar to his readers, should not be introduced without explanation. Such phrases and symbols can always be explained by taking the time and trouble; and though the paper be made somewhat longer, it becomes far more satisfactory. It is of course possible, especially if one has not much to say, to err in the opposite direction by diffuseness and verbosity. The golden mean lies in the distinct explaining of every symbol, of every phrase not universally understood, and of every step in the discussion in language otherwise extremely concise.

It would doubtless excite a smile were it known that any young man was for the first time saying to himself: "Go to! let me make a discovery." Yet that is what each one implicitly does say to himself who makes any discovery. It is hard to imagine how any new point could occur fortuitously to an investigator not engaged in investigating. No one can tell until he tries whether or not he is fitted for that sort of work. No one can be sure, even though failure come to him after failure, that he shall not later meet with success. One sort of failure, indeed, should convey the most flattering encouragement. It is when a supposed discovery is made, which proves on further inquiry to have been made long before by some one else. The immediate effect is disheartening; and yet the occurrence has established the existence of the power of discovery. When once anything, no matter what and no matter how old, is discovered afresh and originally, the beginner has only himself to blame for any subsequent want of success. It may, in fact, be doubted whether every earnest mathematician who takes pleasure in his work has not in him, to some extent at least, the capacity for discovery. Indeed, any fresh solution of an interesting problem, any new proof of an old proposition, is in itself a piece of original work. Undoubtedly some are born with greater capacity than others; yet no one can tell, without trying, the limits of his own capacity in this direction; and it is probably true in this, as in other lines of effort, that genius consists in an infinite capacity for taking pains.

He who for the first time makes an attempt towards original mathematical research must do so either in pure or in applied mathematics. By far the greater number of papers relate to the former class of investigations; and yet it would seem that greater opportunities for attaining important results lie in the latter direction. We all of us know, in a general way, that many important improvements in pure mathematics are the direct result of efforts connected with practical applications. Our knowledge of the laws of physics is constantly undergoing development. Just now, perhaps, the most important improvements are those connected with the laws of electricity, in which some of the members of this Society have taken a prominent part. Mr. Walker, in his presidential address of 1890 before the London Society, brought forward numerous instances illustrating the enormous influence of applied mathematics upon the progress of the pure science. The numerous illustrations which he adduced should be consulted by every one interested in the applications, and should encourage him to active effort in extending the domain of applied mathematics, and thereby almost necessarily adding to existing knowledge in the region of pure mathematics.

For some reason which no one has undertaken to explain, but probably connected with the much wider dissemination of elementary instruction in pure mathematics as compared with applied, by far the greater number of investigations thus far have related to pure mathematics; and it may be presumed that for some time to come this disproportion will continue. In other words, our young mathematician who says to himself that he will make a discovery is most likely to confine his efforts to that in which he has been most thoroughly instructed and with which he is therefore the most familiar—the pure science. How, then, is he to set about it? One way, and a most satisfactory one, would be to take part in some such *seminar* as that at Göttingen, described some time since in our *Bulletin*. Another quite similar method is to begin by assisting some active investigator and carrying out his suggestions faithfully. The impulse given to a number of our best men in this way by Professor Sylvester when he was in this country is well-known to us all. On the other hand, any attempt at collaboration between two equals would seem almost certainly predestined to failure. Though exceptions are well known, it is really rare to find any fresh and important development in a paper worked up by two friends of equal skill, and still rarer to find a succession of papers by the same pair of authors. Good practice, however, can be had in correspondence between two friends on some fresh subject, each sharpening the mind of the other, provided the correspondence be carried on as a matter of growing interest between the two, rather than for the purpose of producing a joint publication. As a rule, however, the young discoverer works alone, and he will most likely find, before he gets to the publishing stage, that his first discoveries have been made earlier by others. He must choose his subject according to his own taste. Usually he will be led most easily to some fresh result, if he reads and digests with keen interest the latest publications of others upon some growing subject. He may, perhaps, perceive that one of these papers has not exhausted all the possibilities; or he may, by an alteration in the point of view, find himself enabled to obtain the same result by a much shorter and more satisfactory process. He must not fear that he is giving his mind to a subject too trivial. No matter how slight the addition which he makes to the sum of knowledge, it is yet an addition, and unless it is superseded by the doing of the same thing by some one else in a better manner, it is a permanent contribution to science. Some are helped greatly, at times, by working first on some numerical illustration of the problem in hand; others, again, by a preliminary geometrical representation; and the first path to any discovery is not usually the best. It is sometimes supposed that the mass of original work done in so many countries and published in so many languages makes it likely that any ordinary piece of work will be overlooked in the great mass. Nevertheless, *litera scripta manet;* and what may now seem an unimportant addition to an unimportant branch may probably one day, when that branch is no longer unimportant, and when its special history comes to be itself a topic of discussion, receive its due recognition. Meantime, every little helps. The most trifling addition to the actual sum of knowledge will be at least useful as a step to aid the next investigator; but whether important or unimportant, whether appreciative recognition comes or not, whether others are helped or no one takes notice, there is a degree of personal pleasure in the mere fact of origination which is the just and certain reward of every piece of successful investigation.

EMORY McCLINTOCK.

NEW YORK.

THE ORIGIN OF OUR VERNAL FLORA.

THOSE who have collected flowering plants for many years, without a doubt have been impressed with the wonderful regularity and precision displayed in the successive flowering of different species, even genera of plants. The character of the vernal flora in the northern United States* depends on the seasonal development of plants belonging to different natural orders. Each plant, even orders of plants, have definite times of appearance, when their flowers open, fertilization takes place, and seeds are distributed. At times, a lull or break in the continuity of this floral procession takes place just before the true summer plants appear. Such a break seems to occur in the neighborhood of Philadelphia between the twenty-fifth day of May and the tenth or fifteenth day of June, when the first true summer plants appear. Curiously enough, this period corresponds with the time of the ice saints in the United States, when there is a possibility of frost over a large portion of our con-

* The advent of spring may properly be considered as taking place at the approach of an isotherm one degree higher than 42.8° F., the general limit of protoplasmic activity. There is no temperature in the extreme South, in the vicinity of the Gulf, below 43.8° on the average, and there is therefore no advent of spring; no real beginning of vegetation and reclothing of trees with leaves. On February 1st, the isotherm in question is found crossing the United States from the vicinity of Cape Hatteras on the east to the north of El Paso, then northward to the Pacific near San Francisco Bay. The phenomena of winter are to be found north of that line. See *Harper's Monthly Magazine*, May, 1894, page 874, article by Mark W. Harrington.

tinental area.* A floral calendar might be constructed with the dates of germination, seed discharge and death of annual plants, and it would be found that a plant year after year departs very little in the time of its appearance from the dates put down in this vegetal almanac. Take a common agricultural plant by way of illustration. The planting season for Indian corn is from the 1st to the 10th of May in favorable weather. One hundred and ten days from date of sprouting to date of ripening or security from frost is about the average season. In many cases in the corn belt, Nebraska for instance, the farmers are quite sure of at least one hundred and twenty days. All this goes to prove that each plant has a peculiarity of its own with regard to temperature and environment; that the sum of the mean daily temperatures from the time of sprouting until the time of seed discharge is pretty nearly a constant one, and that if a plant be watched for years in succession it will be found that this thermometric sum oscillates little either way from the plant's normal. It is desirable that our native plants should be investigated as to temperature conditions, for some rule must determine the appearance of plants, the time of flowering and the time of suspended growth. It is no haphazard process, but depends on fixed laws of growth and development. The daily appearance of new plants depends considerably more on the habits of their ancestors than on the controlling influence of present meteorological conditions.

Our forest trees show some very interesting peculiarities in their early spring development, which is apparently caused by their past conditions of growth and development. Heredity seems to play a very important role in their vegetative habits. The facts condensed in the accompanying table will help to elucidate this statement:

*See *Harper's Magazine*, May, 1894.

Plants wind-fertilized, flowering from March to June.	Cretaceous or Chalk Period.	Quercus (oaks), Fagus (beeches), Salix (willows), Platanus (plane-trees), Sassafras, Laurus, *Magnolia*, *Liriodendron*, (tulip-trees), Myrica (wax myrtles), Betula (birches), Liquidambar (gum-trees), Juglans (walnuts), Acer (maples).
	Post-Cretaceous.	Cornus (dog-wood), Nyssa (sour-gums), Fraxinus (ashes).
	Eocene.	Ulmus (elms), planera, Celtis, Carya (hickory), *Vaccinium* (blue-berries).
	Miocene.	Alnus (alders), Carpinus (horn-beam), Negundo.

Italicized genera insect-fertilized.

It will be seen from this table that the more important genera of trees flower in the early spring. The cause for this is to be found in the past history of the plants, for if we arrange them, as in the table, as to their appearance in geological time, we discover that nearly all of them appeared before or during the Miocene (middle Tertiary or Mammalian Age) Epoch, when the northern hemisphere was many degrees warmer than at present, and when a mild climate extended far into the arctic regions. It is impossible to ignore the force of the testimony as to the continuous warm climate of the north temperate and polar zones throughout Tertiary (Mammalian) Times. We have in the lower Cretaceous (Chalk) Period an almost tropical climate down to the upper Eocene (Lower Tertiary), when it remains warm temperate, for instance, in central Europe and cold temperate within the polar area. It then gradually cools down and merges through the Pliocene (Upper Mammalian) into the Glacial Epoch. That being the case, it is highly probable that the season of growth of our forest trees during the Miocene Period was uninterrupted, and that flowers followed rapid vegetation, as night follows day. The Glacial Period succeeded with its cold acting as a disturbing influence, cutting off the growth of the trees sharply just before the flowers opened. The stately monarchs of the Pliocene forests began to change their habit and adapt themselves to the new meteorological condition, the ever increasing cold. The unopened flowers were enveloped by the yet undeveloped leaves, which became harder and firmer, forming membranaceous and resinous covered bud scales, as a protection against the ice and cold. Flowers thus protected remained dormant during the long glacial winter, and on the return of the next growing season opened their flowers for wind fertilization. This habit of early flowering became impressed so strongly on the plants that it became hereditarily fixed. Trees of abnormal habit frequently show atavism, flowering in the late autumn, if exceptionally warm. This apparently indicates that the cold cut into two periods the normal process of plant growth. The division, thus, of the period of growth into two unequal halves by the glacial cold explains why our forest trees have varied little during the process of time from a wind pollinated (anemophilous) state, because their floral organs are developed in the spring before the appearance of the most highly specialized flower visiting insects. Two causes have operated to keep our trees permanently in an anemophilous condition, first, the separation of the vegetative and reproductive stages by the cold of the Glacial Epoch, and their early spring flowering; and secondly, the association of trees together into forests, flower visiting insects loving essentially open glades, or areas devoid of timber.

More difficulty is experienced in explaining the appearance of the herbaceous vernal flora. In order to arrive at a clear understanding of the problem, a few statistics are necessary.

The following table compiled from a variety of sources arranged for convenience of presentation according to the system of A. L. Jussieu (now little used) will be of use as approximately showing the statistical systematic distribution of our spring plants.

			Darrach.[1]	Darlington.[2]	Gray.[3]	Rothrock.[4]	Burk.[5]
Dicotyledones	Polypetalæ	Stamina epigyna	6		—		3
		" hypogyna	91	61	86	75	22
		" perigyna	6	18	17	30	16
	Monopetalæ	Corolla hypogyna	13	12	9	11	17
		" perigyna	—	—	—	—	—
		" epigyna	8	5	9	13	9
	Apetalæ.		6	15	13	—	6
	Monocotyledones.		26	15	12	—	21

A predominant number of the plants, tabulated in the foregoing table, fall into eight natural orders: Ranunculaceæ (buttercup family), Cruciferæ (cress family) Violaceæ (violet family), Caryophyllaceæ (pink family), Rosaceæ (rose family), Saxifragaceæ, Ericaceæ (heath family), Compositæ (sunflower family). The plants belonging to these eight natural orders form the major and characteristic part of our spring flora, and with the exception of the Ericaceæ and Compositæ (few in number) are all polypetalous (many petals, distinct), and monocotyledons hypogynous (stamens and parts below the ovary) in its make-up. The more complex and irregular flowered families appear later in the year. Now this order of flowering corresponds curiously with the order of evolution of the flowering plants, which was suppositiously as follows:

A. Monocotyledons. Wind Fertilized. Grasses, Sedges.

B. Dicotyledons.

1. Wind Fertilized. Trees.
2. True Insect Fertilized.
   (a) Polypetalæ. (Petals distinct, 4 or 5.)
   (b) Gamopetalæ. (Petals united.)

This comparison leads us to infer the ab-

[1] Darrach, Proc. Acad. Nat. Sci., Phila., 1860, 145; [2] Darlington, Flora Cestrica; [3] Gray Manual; [5] Rothrock, Flora of Alaska; [4] Burk, Flora of Greenland, Proc. Acad. Nat. Sci., Phila., 1894.

sence of true flowers until late geologic times, for it is only by the visits of insects and their irritating action on vegetal protoplasm that the most irregular flowers have been slowly evolved, for there is a broad parallelism between the more differentiated types of the vegetal kingdom and the appearance of the various orders of insects, which was:

GEOLOGICAL SUCCESSION OF INSECTS.

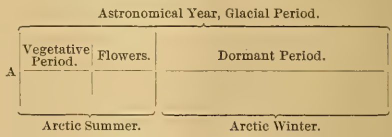
Devonian, Orthoptera (ear-wigs, grasshoppers), Neuroptera (ant-lions).
Carboniferous, Coleoptera (beetles).
Cretaceous Olite, Hymenoptera (bees), Hemiptera (lice), Diptera (flies).
Tertiary, Lepidoptera (butterflies).

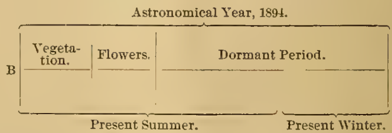
We know from the close association of insects and flowers that the insects were modified by their visits to flowers, and conversely that flowers have been changed to suit the visits of insects, and it is therefore not improbable that our most highly specialized flowers, and most irregular ones, appeared and were modified by the Lepidoptera in the late Tertiary time; for moths and butterflies are most highly specialized to insure cross fertilization, or allogamy. This variation in flowering plants must have been most strong at the close of the Miocene period, and after the retreat of the glaciers still more rapid than before, for it is probable that the intense struggle which took place by the migration and intermixture of forms of different kinds, occasioned by the change of environmental conditions, was a powerful factor in causing the striking variety of flowers and insects. The 'responsive power' of the protoplasm of the plants, acting in concert with the external impulses received from the environment, must have been strong after the disappearance of the glaciers, on account of the re-occupation of a barren glacial country by northward moving plants, whose protoplasm had become responsively mobile during the long continued struggle in the south.

It is not at all improbable that the polypetalous groups of plants were northern ones during the Miocene period, and that their flowering period depends on this past geographical position. Those plants which lived far north during late Miocene and Pliocene times were least modified, for it is likely that moths and butterflies were then few in number, and the time was not sufficiently long for change to take place before the glacial ice sheet moved southward, mixing the northern and southern types, and introducing a struggle which was to last until the ice disappeared by the temperate heat. Many tropical plants remained associated with the northern forms crowded southward by the glaciers, notwithstanding that a great number perished under the more rigorous conditions of a colder climate. When the glaciers retreated, the predominant polypetalæ adapted to a cold climate did one of three things: 1. They retreated northward. 2. They retreated up the high mountains. 3. They took almost exclusive possession in their growth of the spring months, for the temperature conditions are such as to suit well their hereditarily impressed preference for the cold.

These plants flower and mature their seeds quickly before the summer is well advanced, which mark them as physiologically adapted to the influences of the short glacial summer, alternating with the long glacial winter. This rapid growth production of flowers and seed in a short space of time is possible from the quantity of nutritive material stored up in the plant. The beet, turnip, parsnip and carrot are familiar examples of biennials with the reserve substance packed in the roots; the houseleek, lily and onion with the bases of the leaves enlarged and thickened to contain the stores of starch, sugar and proteids. Even under these favorable conditions, when the plant would be in a condition to grow most vigorously, every externally perceptible vital motion nevertheless ceases, and it is only after a dormant period of some months that growth commences anew, and this frequently under circumstances which appear far less favorable—especially at a conspicuously lower temperature. "This periodic alternation of vegetative activity and rest is in general so regulated that, for a given species of plant, both occur at definite times of the year, leading to the inference that the periodicity only depends upon the alternation of the seasons, and therefore chiefly upon that of temperature and moisture." A few well-known examples are selected for illustration. "The leaf-shoot and flowers contained in the bulb of the Crown Imperial commence to grow vigorously in the spring-time with us, even at the beginning or middle of March, when the soil in which the bulb has passed the winter possesses a temperature of 6–10° C.; the leaf-shoots protrude forcibly from the cold earth to grow vigorously in the but slightly warmer air. There would be but little to surprise us in this, if we did not at the same time notice the fact that a new leaf-shoot is already formed in embryo in the subterranean bulb in April and May; this shoot, however, does not grow to any extent in the warm soil during the summer and autumn. On the contrary, this favorable period of vegetation passes by, until at the end of the winter an inconsiderable rise of temperature above the freezing point suffices to induce vigorous growth; and as is well known, the same is the case with most bulbous and tuberous plants, as the meadow saffron, potato and kitchen onion."

"I have many times attempted to induce the tubers and bulbs ripened in autumn to put forth their germinal shoots during November, December and January, by laying them in moist, warm loose soil; but as in the case of the potato, as well as in that of the kitchen onion, no trace of germination appeared. If, on the other hand, the attempt is repeated in February, or still better in March, the germinal buds begin to grow vigorously even in a few days. It is evident that some internal change must have taken place in the tubers and bulbs during the winter months, when it is impossible to bring them into activity from their state of rest." Our spring plants in this agree physiologically with their arctic congeners. The period of rest described above in such early spring plants, as the winter aconite, crocus, Erythronium, etc., has in my opinion been due to the influence of the glacial cold hereditarily impressed on these plants in connection with the chemical changes which go on. The following diagrams will illustrate my meaning. Diagram B shows that the period of vegetative activity of our spring plants corresponds with an arctic or a glacial summer, while the dormant period corresponds with an arctic winter, although our present summer has encroached on the former glacial winter.





It was necessary for this rapid growth that the food material should be prepared beforehand, because the arctic or glacial summer is an exceedingly short one. Mr. Henry Seebohm,* in his presidential address before the Geographical Section of the British Association, gave a graphic description of the succession of the seasons in high arctic latitudes. A few sentences are worth quoting in this connection. He said that the stealthy approach of winter on the confines of the polar basin is in strong contrast to the

*See *Popular Science Monthly*, XLV., 138, May, 1894.

catastrophe which accompanies the sudden onrush of summer. "One by one the flowers fade and go to seed, if they have been fortunate enough to attract a bee or other suitable pollen-bearing visitor. The arrival of summer happens so late that the inexperienced traveler may be excused for sometimes doubting whether it really is coming at all. When continuous night has become continuous day without any perceptible approach to spring, an Alpine traveler naturally asks whether he has not reached the limit of perpetual snow. During May there were a few signs of the possibility of some mitigation of the rigors of winter, but these were followed by frost. At last, when the final victory of summer looked hopeless, a change took place; the wind turned to the south, the sun retired behind the clouds, mists obscured the landscape, and the snow melted 'like butter upon hot toast,' and we were in the midst of a blazing hot summer picking flowers of a hundred different kinds and feasting upon wild ducks' eggs of various species."

The polypetalous families which blossom early in the season, although old geologically speaking, have not been greatly modified since Pliocene times, because their flowers open in the spring before the Lepidoptera hatch out from their cocoons. It is obvious that every species of flower can only be visited and fertilized by those insects which occur at the time when the plant is in flower and in stations where it grows. The insect visitors of a plant are therefore limited by the season and by the time of day when it flowers, by its geographical distribution and by the nature of its habitat. The high northern polypetalæ have remained therefore regular while those plants growing in the southland have become highly irregular by the visits of numerous highly organized insects in great number near the equatorial zone. We must be cautious, however, in generalizing too broadly, for we can only call those parts perfect which fulfill their purpose in the life of the plant essentially well; that is to say, which under existing conditions insure the sexual reproduction of the species with particular success.

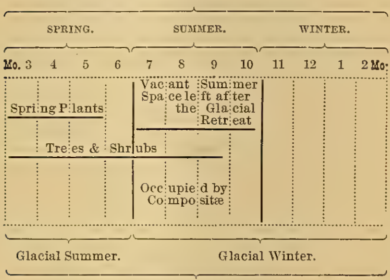
The Compositæ (sun-flower family), the highest expression of evolution amongst Dicotyledons, appeared latest in geological succession, for no undoubted form of them (Synantheræ) has been found farther back than the middle Miocene. Müller says:* "The numerical preponderance which this family has attained in species and genera (1000), and the extreme abundance of many of the species, are due to the concurrence of several characters, most of which singly, or in some degree combined, we have become acquainted with in other families, but never in such happy combination as in the Compositæ. The following points deserve special mention: (1) the close association of many flowers; (2) the accessibility of the honey as well as the plentiful secretion and security from rain; (3) the possession of a pollen mechanism, which renders cross fertilization certain in the event of insect visitors." It is a masterful order of plants most commonly met with in the late summer and autumn, flowering profusely until the heavy frosts of early winter, when they cast their seeds abundantly. An enumeration of the Compositæ growing in the vicinage of Philadelphia shows that the plants are essentially late summer growers.

Flowering to Fruit Ripening.	Number of Plants.
April–May,	2
May–June,	4
June–July,	6
June–August,	4
July–August,	8
July–September,	15
August–September,	32
August–October,	35
September–October,	15
	121

* Müller, *The Fertilization of Flowers.*

These are the latest group of plants to appear geologically, they grow and flower in the warm season added to the short arctic summer by the retreat of the glacial winter. The following diagram will indicate more clearly what is meant, and will show why it is that the Compositæ of the north temperate zone are the characteristic herbaceous vegetation of the late summer and autumn months.

Present Astronomical Year.



Miocene Season of Growth.

The land area left bare by the retreat of the glaciers was one of low tension, although by the increase in the length of the summer (some three months) it had a climate in every way suited for the growth of plants. The country to the south was one of very high pressure tension, which must be relieved. The great strain was removed partially by the movement of plants to the northward. "Of all the plants which went south before the first invasion of the glacial ice sheet, none showed greater capacity for variation and improvement than the ancestral forms of the modern dominant family of Compositæ." Such plants in having seeds adapted to fly before the prevalent north winds had reached a low latitude, where great change of form took place owing to the intense struggle for existence. The composite plants were assisted northward by the same structural means as carried them south. Modified considerably into new forms by their migrations and life in the south, they retained their fondness for a warm climate. By the extension of the arctic summer, some three months, they had an opportunity for extensive migration over the country formerly ice bound.

It is thus from the high and low pressures, caused alternately by the glacial epoch, that the distribution of our flora in time has been accomplished.

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ON CERTAIN HABITS AND INSTINCTS OF SOCIAL INSECTS.

If the mere inductive evidence for the Lamarckian theory of the hereditary transmission of acquired characters be strong anywhere, it is assuredly in the region of nervous and mental phenomena. Romanes, whose reserve on the inheritance of acquired characters of a physical nature is everywhere manifest, admits that many instincts are due to the 'lapsing of intelligence.'* "Just as in the lifetime of the individual, adjustive actions which were originally intelligent may by frequent repetitions become automatic, so in the lifetime of the species actions originally intelligent may, by frequent repetitions and heredity, so write their effects on the nervous system that the latter is prepared even before individual experience to perform adjustive actions mechanically which in previous generations were performed intelligently."

Even Weismann, with all his wealth of imagination and capacity for elaboration of details, has nowhere attempted to trace out the mechanism for the evolution of instinct on the line of his 'germ plasm theory,' nor applied to it the manifold combinations of 'biophors' and 'determinants,' 'ids' and 'idants' which he assumes as the machinery of inheritance. So far the only

*Mental Evolution in Animals, p. 178.

key to many instincts is found in the conception that they are inherited habits, themselves the originally conscious reactions of the individual to its surroundings; and this conception has never been seriously attacked from the front in open field. Yet Darwin and all his followers have regarded the habits and instincts of social insects as mainly if not wholly evolved by casual variations and natural selection. For the origin of the instincts and habits of these creatures cannot obviously be explained on Lamarck's principle, since they are for the most part evinced by the workers and soldiers, who are neuters; and such, of course, cannot transmit their instincts by blood to their followers, who are only collaterals and outside the direct line. Here and there, indeed, these neuters may lay eggs, unfertilized but not infertile, since in the bees they produce drones and in some ants also males; but we have no evidence that this occurrence is frequent or regular enough really to influence the race. However, there are two matters, the so-called instincts of neuters generally, and those of slave-makers in particular, that may be dealt with from a point of view which will show that an explanation is available that makes no excessive demand on Lamarckians.

It is a truism to say that one of the most potent factors in education is the imitation of one's peers. As a teacher of experience, I know well how the presence of a few bright and handy students eases my annual task of breaking in a class of book-taught lads to a study requiring handiwork and observation. The nearer akin the model, the more powerful is his example. Thus, the trained elephant is an almost necessary aid to the tamer of wild elephants; no bird-organ can do as well as a good songster; and if we wish to train a daw, magpie or starling to speak, its best teacher is a loquacious parrot.

Animals may readily thus acquire *habits* which, if we did not know their origin, we might well mistake for *instincts*. Thus a dog reared by a she-cat has acquired the habit of sitting up on his tail, licking his paws and washing his face—watching a mouse-hole for hours together; 'and had in short all the ways and manners and disposition of his wet nurse.'* So that in considering the behavior of any species we have to be cautious and bear ever in mind that manifestations which at first sight seem unequivocal instinct may be really habit, and habit only.

Now every neuter insect is born from the pupa (as it was born from the egg) into a community of busy workers of its own kind, practising the art that she† will have to practise in turn. If then her mental powers and emotional development are up to the average of the race there can be no difficulty in her qualifying for the place she will take in the nest. Again we must remember that this neuter insect hatches from the egg into a helpless larva, to be fed and tended with most devoted care by the adult sister workers until it passes into the chrysalis or pupa stage, where it sleeps out the transformations that make it an adult. We know well that neuter insects show every sign of varied emotion; everyone can tell the difference of demeanor between the busy bee and the angry one; and observers have shown us ample evidence of many other emotions. If then memory of the earlier larval state survives the pupa trance‡ our

*See Romanes, op. cit., p. 226.

† The so-called neuter is always an imperfect female.

‡ Lubbock has shown that ants will tend any young whatever of their own species even if born in other nests; but none the less they do reject them as strangers after they have passed through pupadom into the adult state, while they welcome back the offspring of their own nest that have been fostered by strangers. The converse experiments have not been tried, to ascertain whether the new-born adults that have been nursed outside their own nest show any memory of or preference for their own folk or their fosterers respectively. (See Lubbock, 'Ants, Bees and Wasps.')

newly emerged neuter should revive with the liveliest gratitude and almost filial affection for its mates, who have tended it as devotedly as elder sisters in charge of a family do even among ourselves.

The only possible objections to this view are, first, that the insects have not intelligence enough for imitation, and secondly, that teaching presupposes communication between the teacher and the taught, which we have no right to assume. But these objections fall as baseless when we observe for ourselves, or trace with a Huber, a Forel, a Lubbock, or a Bate the unmistakable intelligence and the unequivocal signs of communication to be found among these animals.

We may still assign to natural selection a certain part, much more limited than has hitherto been supposed. It conserves the general intelligence of the race at a high pitch, by constantly weeding families prolific of foolish virgins; and it checks all excessive development of individuality by destroying families with an undue proportion of those geniuses who aim at striking out new paths for themselves instead of devotedly working at their settled coöperative tasks. But the singular mixture of ability and routine displayed by ants and bees is just what we should expect if their arts were largely attained by the influence of strong tradition. Our lawyers till quite recently showed the severe limitations imposed by tradition on intelligence. And this is my case for regarding the ways of neuter insects as habits and practices, not instincts. Many ants make slaves; they raid the nest of other species, killing the adults and bringing home the helpless young. These are nursed by workers of the same slave race that were once themselves brought in the immature state to the nest. Some of these slave makers can neither clean themselves nor feed themselves; everything has to be done for them by their slaves, save the work of war and capture.

Lubbock writes: "They have lost the greater part of their instincts; their art, that is, the power of building; their domestic habits, for they show no care for their own young, all this being done by the slaves; their industry—they take no part in providing the daily supplies if the colony changes the situation of its nest, the masters are all carried by the slaves on their backs to the new one; nay, they have even lost the habit of feeding . . . However small the prison, however large the quantity of food, these stupid creatures will starve in the midst of plenty rather than feed themselves."

The origin of this character is not far to seek; the fertile insects, *i. e.*, the males and perfect females of social insects, contribute little or nothing to the work of their nest save their offspring*; hence in the parents of each generation there is a constant fostering of selfishness and dependence to be transmitted to their offspring.

The female or queen termite (or White ant), indeed, is guarded from all exertion and tended in a way to satisfy the indolence of the most languid creole fine lady; the only drawbacks of her position being lack of amusements and of lovers on the one hand and an excessive fertility on the other. Where all or many of the neuters are workers, indolence and selfishness are checked and natural selection constantly eliminates those families whose altruism is insufficient for a social life. But if once circumstances arrive in which slaves are present to do the duties, it is easy to see how all the traditions of work or self help—save in war—can die out and be utterly lost, bravery, pugnacity and honorable coöperation being the sole virtues to survive. It seems at first sight strange

*The amount done is perhaps greatest among Wasps and Humble Bees, least among Termites.

that the slave holders have lost the power of feeding themselves; but this is not unexampled in human affairs. Surely many a fine lady might starve outright in a place with no provender but live fowls and unthreshed wheat and water, no utensils but dry sticks and a few stones. Yet we know that savages of far lower wit could kill and pluck the fowls and get fire, spit and roast them, crush the wheat between the stones and make a damper cook it in the embers. This is a case of the loss of the power of self help by peculiar education, and if we admit this explanation for the fine lady we have no right to reject it for the slave holding ant.

I am aware that I have not dealt exhaustively with the whole question of social insects. There are lots of cruxes in their manners and customs, and especially in the manifold forms that occur in one and the same species. Why, for instance, worse food and a narrower cell should make a fertilized bee's egg become a sterile worker instead of a queen, no one knows; and the problems presented among ants are far more difficult and complicated. But it is as well to take stock frequently of our speculations, and to place our certain realized assets to the credit side, even though we have to keep most of our accounts open indefinitely.

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THE PROPER SCIENTIFIC NAME FOR BREWER'S MOLE.

THERE are three species of moles in the Eastern States, the Star-nosed mole, *Condylura cristata*, the common or Shrew mole, *Scalops aquaticus*, and a third less familiar species known as Brewer's mole, or the Hairy-tailed mole. It is to this last species that my remarks relate. It was described by Bachman in 1842 in the *Boston Journal of Natural History* (vol. 4, page 32) under the name of *Scalops breweri*, and was cited under that designation until 1879, when Dr. Coues proposed to change the specific name to *americanus*. This proposition was based on the fact that in Harlan's *Fauna Americana*, published in 1825, the name '*Talpa americana*, black mole, Bartram's manuscript notes,' occurs in synonymy at the head of a description which Dr. Coues thought might be in part, at least, applicable to the species under consideration.

I find, however, that this is a literal translation of Desmarest's description of the European mole, *Talpa europæa*, with no additions whatever, and no other alteration than the omission of a word or sentence here and there. It is evident, therefore, that Harlan included nothing from Bartram's manuscript, whatever it may have contained, and that the name *Talpa americana* has no validity.

It will be necessary to return to the specific name *breweri*. I recently separated Brewer's mole as the representative of a distinct genus, which I called *Parascalops*. If this distinction be accepted, the proper name of the species will be *Parascalops breweri* (Bachman).

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THE AMERICAN FOLK-LORE SOCIETY.

THE annual meeting of the Society was held at the Columbian University, Washington, December 27th and 28th. Owing to a death in his family, the President, Dr. Alcee Fortier, of Louisiana, was prevented from attending.

The Secretary, Mr. W. W. Newell, submitted a report in which he detailed the publications of the Society for the year. These included two volumes of 'Folk Tales of Angola,' prepared by Heli Chatelain, late United States commercial agent at Loanda, West Africa, and papers by various well-known authors as follows: 'Notes on the folk-lore of the mountain whites of the Alleghanies,' J. Hampton Porter; 'Three epitaphs of the seventeenth century,' Sarah A. P. Andrews; 'Popular medicine, customs and superstitions of the Rio Grande,' Capt. John G. Bourke; 'Plantation courtship,' Frank D. Banks; 'Retrospect of the folk-lore of the Columbian Exposition,' Stewart Culin; 'Eskimo tales and songs,' Franz Boaz; 'Popular American Plant Names,' Fannie D. Bergen.

A large number of papers were read before the Society and discussed by the members present. The first was by Dr. Washington Matthews, entitled 'A Navaho Myth,' which related in detail one of the sacred legends of the tribe.

Capt. R. R. Moten then read a paper on 'Negro folk-songs,' in which he spoke of natural musical tendencies of the colored race and reviewed a number of the old songs of the South before the war. Negro music, he said, might be divided into three kinds, that rendered while working, a different kind for idle hours, and a third and more dignified sort used for worship. Capt. Moten said the general public had but little idea of the old negro music, and that many of the so-called negro songs rendered by white men in minstrel performances were abortions. There were some old familiar melodies, however, which were true to nature, and full of inspiration.

A quartet of colored men was present, and sang a number of negro songs illustrating the points brought out by Capt. Moten.

Several speakers dwelt upon the important question of the diffusion of folk-tales and the explanation of striking similarities found in localities widely apart. Mr. W. W. Newell was inclined to explain such by theories of transmission; while Major J. W. Powell and Dr. D. G. Brinton, both of whom had papers on closely related topics, leaned toward the 'anthropologic' explanation, which regards those similarities as the outgrowth of the unity of human psychological nature and methods.

Dr. J. W. Fewkes gave a detailed description of the figures in the ancient Maya manuscript known as the 'Cortesian Codex.' Other papers presented were: 'Kwapa folk-lore,' Dr. J. Owen Dorsey; 'Korean Children's games,' Stewart Culin; 'Burial and holiday customs and beliefs of the Irish peasantry,' Mrs. Fanny D. Bergen; 'Bibliography of the folk-lore of Peru,' Dr. Geo. A. Dorsey; 'Mental development as illustrated by folk-lore,' Mrs. Helen Douglass; The game of goose with examples from England, Holland, Germany and Italy,' Dr. H. Carrington Bolton; 'The Swastika,' Dr. Thomas Wilson; 'Folk-food of New Mexico,' Capt. John G. Bourke, U. S. A.; 'Opportunities of ethnological investigation on the eastern coast of Yucatan,' Marshall H. Saville; 'Two Ojibway tales,' Homer H. Kidder.

The officers elected for the ensuing year were: President, Dr. Washington Matthews; Vice Presidents, Rev. J. Owen Dorsey, Captain John G. Bourke, U. S. A.; Permanent Secretary, William Wells Newell, Cambridge, Mass.; Corresponding Secretary, J. Walter Fewkes, Boston, Mass.; Treasurer, John H. Hinton, New York, N. Y.; Curator, Stewart Culin, Philadelphia, Pa.

D. G. Brinton.

University of Pennsylvania.

SCIENTIFIC LITERATURE.

Les oscillations électriques.—H. Poincaré, Membre de l'Institut. Paris, George Carré, 1894.

This work contains, briefly stated, a clear mathematical discussion of the general features of the Faraday-Maxwell electromagnetic theory in Hertzian form, and of those special problems bearing upon this theory which are of particular interest to the experimentalist. The mathematical solution of these problems is compared carefully with the results obtained, principally by the experiments of Hertz and of other investigators who have extended the field of the Hertzian method of investigation. But it should be observed that the experiments of the pre-Hertzian epoch receive their full share of attention, as, for instance, the experiments of Rowland, Röntgen, and others.

The work will undoubtedly exert a very strong influence upon the future developments of the electromagnetic theory, and deserves, therefore, more than ordinary attention. This circumstance should, in the opinion of the reviewer, excuse the length of this review.

General Theory.—Poincaré's discussion divides itself naturally into two parts. In the first part an electromagnetic field with conductors at rest is considered. In the second part the discussion extends to electromagnetic fields with conductors in motion.

The Hertzian method of presentation is adopted in preference to the Maxwellian. Two distinct differences between these two methods should now be pointed out. The first difference is essential, and may be stated briefly as follows :—

Hertz described Maxwell's electromagnetic theory as the theory which is contained in Maxwell's fundamental equations ; he stated, however, very clearly that the suppression of all direct actions at a distance is a characteristic feature of this theory. But if it is not a sufficient hypothesis, and if no other hypotheses are clearly stated by Maxwell, then his deduction of the fundamental equations which form the heart and soul of his theory must necessarily lack in clearness and completeness. This is the difficulty which Hertz discovered in Maxwell's systematic development of his own electromagnetic theory, and Hertz obviates this difficulty by starting from the equations themselves as given, proving their correctness by showing that they are in accordance with all our experience.

The second difference is formal only. It may be stated briefly as follows: Maxwell considered the electrotonic state, discovered by Faraday, as of fundamental importance. The mathematical expression of this state, the vector potential, was considered by him as the fundamental function in his mathematical presentation of Faraday's view of electromagnetic phenomena. Hertz, just as Heaviside did some time before him, considered the vector potential as a rudimentary concept which should be carefully removed from the completed theory just as the scaffolding is removed from a finished building. In place of the vector potential Hertz substituted the electric and the magnetic force as the fundamental quantities. This enabled him to state the fundamental equations of Maxwell in a more symmetrical form than Maxwell did.

It seems that it is principally this second, the formal, difference which decides Poincaré in favor of the Hertzian method. But there is still considerable difference between the presentation of the electromagnetic theory given by Hertz and that which Poincaré gives in this book. For whereas Hertz proceeded from the symmetrical form of Maxwell's fundamental equations as given and by deducing from them and from several clearly defined assumptions the general experimentally established laws of electrical phenomena proved the correctness of these equations, Poincaré deduces them from the following experimentally established facts :

1. The energy of the electromagnetic field consists of two parts, one due to the action of the electric and the other to that of the magnetic forces. They are each homogeneous quadratic functions of the two fundamental quantities, that is of the electric and of the magnetic forces respectively. This experimental relation defines the units of the electric and of the magnetic force and also the physical constants of the medium, that is the specific inductive capacity and the magnetic permeability.

2. Having defined the meaning of mag-

netic and of electric induction and of their fluxes in terms of the corresponding forces, Poincaré states then the fundamental law of electromagnetic induction in a closed conducting circuit as an experimental fact and deduces immediately the first group of the Maxwellian equations. This group is nothing more nor less than a symbolical statement that the law of electromagnetic induction is true for every circuit whether it be conducting or not.

3. Joule's law is stated as an experimental fact. In a homogeneous conductor the heat generated per unit volume and unit time at any point of the conductor is proportional to the square of the electric force at that point; the factor of proportionality is electrical conductivity by definition. Another quantity is then introduced which is defined as the product of the electrical force into the conductivity and the name of conduction current is given to it.

By means of these definitions, the principle of conservation of energy, and the first group of Maxwellian equations, the second group, in the form given by Hertz, is then deduced. This completes the Maxwellian electromagnetic theory for a homogeneous isotropic field in which both the medium and the conductors are at rest.

Poincaré loses no time in commenting upon the physical meaning of these equations, but proceeds rapidly to Poynting's theorem, which introduces one of the most important quantities in the wave-propagation of electromagnetic energy. It is the radiation vector, as Poincaré calls it. A brief remark, however, prepares the reader for the good things that are to come. A comparison of Maxwell's fundamental equations with those of Ampère shows them to be identical except for rapid electric oscillations, when the displacement currents (Poincaré does not mention this name, but only refers to a mathematical symbol) in the dielectric cease to be negligibly small. For these no provision was made in Ampère's or any other of the older theories. Here then is the starting point of the radical departure of the Faraday-Maxwell view from that of the older theories. Hence the study of Hertzian oscillations takes us into a new region of electrical phenomena, a region entirely unexplored by the older theories, and first brought before our view by the discoveries and surmises of Faraday, by Maxwell's mathematical interpretation of these discoveries and surmises, and by Hertz's confirmation of Faraday and Maxwell.

Hertzian Oscillations.—It is the study of these rapid oscillations which forms the subject of the rest of Poincaré's work under consideration.

Sir William Thomson's theory of the discharge of a Leyden jar forms a fitting introduction to this study. It states clearly the essential elements which should be considered in the study of electric oscillations. They are the period and the decrement. The relation of these to the self-induction, the electrostatic capacity, and the resistance of the circuit are given by this theory and it was verified by many experiments, especially those of Feddersen, who measured the period of these oscillations and also their decrement by a photographic method. But inasmuch as these oscillations were of a comparatively long period, 10^4 per second, they were not apt to furnish a test of the Faraday-Maxwell theory. The waves of the oscillations studied by Feddersen would have been 30 kilometers long and would, therefore, have escaped experimental detection.

Hertz was the first to produce very rapid oscillations, 10^8 per second; but since their period was too short to be measured directly, another method of testing the agreement between theory and experiment had to be devised. This was done by Hertz, who measured the wave length (about 3 metres in the earliest experiments) of the waves produced by these rapid oscillations by means of the intensity of the spark in the spark-gap of a secondary circuit, the so-called resonator. The period was calculated by the Thomson formula and dividing the wave-length by the period gave the velocity of propagation, which, according to the Faraday-Maxwell theory, should be equal to that of light, and that, too, both in the immediate vicinity of the conductors and in the dielectric. A mere sketch of these experiments is given for the purpose of outlining the plan of the discussion to be carried out in the succeeding chapters of the book. Hertz's method of calculating the period of his oscillators is reproduced more or less faithfully and the various objections against it discussed.

Theory of Hertzian Oscillations.—This discussion paves the way gradually for the general theory of the Hertzian oscillator to be taken up in the next chapter. This theory can be described as the mathematical discussion of the following problem: Given a homogeneous dielectric extending indefinitely. This dielectric is acted upon by a steady electrical force applied at a conducductor, the oscillator. It is therefore electrically strained. Describe the process by means of which the dielectric returns to its neutral state when the initial electrical strain is suddenly released.

The discussion must necessarily start from Maxwell's fundamental equations. They are in the form given by Hertz, partial differential equations connecting the components of the electric and of the magnetic forces at any point in the dielectric. Hence, using the language of the mathematician, the solution of the above problem will consist in the integration of Maxwell's differential equations, which, translated into the language of the experimental physicist, means that the solution will consist in finding the resulting electrical wave, that is, its period, its decrement due to radiation and dissipation, and its direction and velocity of propagation. It is evident, therefore, both to the mathematician and to the physicist that the conditions at the boundary surfaces separating the dielectric from the conductor must first be settled. To these Poincaré devotes careful attention. A lucid demonstration is given of the theorems that in the case of rapid oscillations there will be: a. Very slight penetration of the current into the conductor; b. A vanishing of the electric and the magnetic force in the interior of the conductor. c. Electric force normal and magnetic force tangential to the surface of the conductor, etc.

Then follows a beautiful mathematical solution of the general problem mentioned above. It is this: The law of distribution of the conduction current on the oscillator being given the electric and magnetic force, and therefore the state of the wave, at any point in the dielectric and at any moment can be calculated by a simple differentiation of a quantity called the vector potential. This quantity is determined from the current distribution in a manner which is the same as that employed in the calculation of the electrostatic potential from the distribution of the electrical charge, but on the supposition that the force between the various points of the dielectric and the surface of the oscillator is propagated with the velocity of light. The value of this solution rests on the fact that the law of distribution of the conduction current can be closely estimated in some oscillators, as, for instance, in the case of Blondlot's oscillator consisting of a wire bent so as to form a rectangle in one of whose sides a small plate condenser is interposed. A special form of this vector potential applicable to oscillators whose surface is that of revolution is deduced and applied to Lodge's spherical oscillator, whose oscillations are due to a sudden release of a uniform electrostatic field. The solution of this case is complete. The actual values of both the period and the decrement are expressed in terms of the radius of the sphere. The smallness of the period and the exceedingly rapid rate of decay of the wave are striking.

This theory throws much light upon Hertz's method of calculating the period of an oscillator. Poincaré applies it also to the explanation of the Hertzian method of calculating the decrement due to electrical radiation and the force of Poynting's theorem is exhibited in a masterly manner, although, of course, the calculation for more general cases is not as complete as that for Lodge's oscillator. More experimental guidance is necessary and will not be sought in vain in subsequent chapters.

Phenomena of Electrical Resonance.—Wave Propagation along a Wire.—Having described Hertz's method of calculating the period and the decrement, Poincarè discusses next some of the more important experimental researches dealing with these two principal characteristics of an oscillating system. The earliest method employed in researches of this class is that devised by Hertz. A secondary circuit, the resonator, consisting of a turn of wire with an adjustable spark gap is brought into the inductive action of the oscillator. The length and intensity of the induced spark measures the inductive effect between the two. When the periods of the two are equal the effect is a maximum; they are then in resonance. But experiment reveals the fact that the resonance effect is not as pronounced as in the case of acoustical resonance. Sarasin and de la Rive (Arch. des sciences phys. 23, p. 113; 23, p. 557, Génève, 1890) inferred from this that the oscillator sends forth a complex wave which, if analyzed in the manner of a ray of sunlight, would give a continuous spectrum. Poincarè, guided by a carefully worked general theory of resonance, ascribes the absence of a strong resonance effect to the large decrement of the oscillator. An appeal is then made to experiments bearing on this point and the subject of stationary waves in long wires is taken up. Such waves are produced in the same way as in the case of sound waves. When a train of electrical waves travels along a wire and the leading wave reaches the end of the wire it is reflected there and by the interference between the direct and the reflected waves stationary waves are formed. Hertz's theory of propagation of these waves is given, showing that their velocity is the same all along the wire and equal to that of light for all wave lengths. If the view of Sarasin and de la Rive be correct then stationary electrical waves should have no pronounced nodes and ventral segments and, therefore, a resonator which, unlike the oscillator, gives a simple wave of definite periodicity will pick out of the stationary waves that component only which is in resonance with it. In other words, every resonator, within large limits, will respond to stationary waves and if moved along a wire which is the seat of such waves its spark will rise and fall in intensity every time the resonator passes by a node or a ventral segment of that component contained in the complex stationary wave with which it is in resonance. It measures, therefore, the wave length corresponding to its own period and not that corresponding to the period of the oscillator. This wave length divided by the calculated period of the vibrator will give, therefore, a wrong velocity of propagation. A mistake of this kind was suspected in Hertz's earliest experiments by which he obtained a different velocity of propagation along a wire from that in the dielectric. Sarazin and de la Rive called this phenomenon, first observed by them, the phenomenon of multiple resonance. It is undoubtedly one of the most important discoveries in the region of Hertzian oscillations. It was probably [1] Poincaré (his modesty prevents him from mentioning this fact) who first recognized its full value and detected its true meaning. He devotes a large part of the present work to the discussion of this phenomenon and every serious student will appreciate heartily this very interesting feature of the noble work before us. Briefly stated Poincarè's explanation of multiple resonance is this. Ordinarily the oscillator has a large decrement; that of the resonator is very small, according to the results of Bjerkness' experiments. The train of waves excited in a long wire by the inductive action of an oscillator after each disruptive discharge consists of a big wave followed by a small number of waves of very rapidly decreasing amplitude. Such a train of waves is evidently not capable of forming interference waves after reflection. Their effect upon the resonator is practically the same as that of a single wave, giving the resonator an impulse when passing it on its way toward the end of the long wire and another impulse when it returns after reflection. Hence, if the time interval between these two impulses is a multiple of the period of the resonator the resulting oscillation in the resonator will be stronger than otherwise. If, therefore, the resonator be moved along the long wire its oscillations will vary, passing through a maximum at regular intervals; the distance between these intervals being equal to a wave length corresponding to the period of the resonator. But, obviously, the maxima will be most clearly pronounced when the resonator is in resonance with the oscillator. This is especially true in the case of oscillators possessing a less strongly developed decrement, as for instance, Blondlot's oscillator. This explanation is illustrated by a mathematical discussion of rare elegance and simplicity. Blondlot's experiments (Jour. de Phys. 2 serie t. X., p. 549) are then carefully described and the close agreement between them, especially as regards the velocity of propagation along conducting wires, and the above theory pointed out.

[1] It is no more than just that a strong emphasis should be put upon the fact that Bjerkness independently (Wied. Ann. 44 p. 74 and p. 92, July, 1891) worked out the same theory and proved it by experiment at about the same time that Poincaré first published his theory (Arch. des sciences phys. 25 p. 608, Génève 15 Juin, 1891).

Attenuation of Waves.—An important feature connected with wave propagation of Hertzian oscillations along wires was strongly emphasized by these experiments, namely, the diminution of the wave amplitude with the distance passed over. This has long since given Mr. Oliver Heaviside many an anxious thought. Poincaré is evidently not aware of that and he attacks the problem with just as much of his well-known mathematical vigour as if its solution had not been given long ago by Mr. Heaviside. (Electr. Papers, Vol. II., p. 39, etc.) A few bold strokes of Poincaré's unerring pen disclose the interesting fact that the attenuation is due, principally, to distributed capacity of the wire, since the decrement, calculated by Poynting's theorem, is shown to be inversely proportional to the diameter of the wire. Experimental evidence bearing upon this point is then reviewed. In these experiments the employment of the resonator had to be discarded and the intensity of the wave at various points of the wire measured directly. Various methods were employed in these experiments. The most important among them are the following:—

a. Hertz's method (Wied. Ann. 42, p. 407, 1891) of measuring the intensity of the wave at any point of a long wire by the mechanical force exerted upon another small conductor suspended in the vicinity of the wire. This method permits a study of the distribution of the magnetic and the electric force along the wire separately.

b. The method of Bjerkness (Wied. Ann. 44, p. 74) in which two symmetrically situated points of a long loop are connected to the quadrants of a small electrometer and the difference of potential measured.

c. The thermoelectric method [first suggested by Klemencic (Wied. Ann. 42, p. 416)] employed by D. E. Jones (Rep. Brit. Assoc., 1891, p. 561–562). The intensity of the wave at any point of the wire is measured by the thermoelectric effect produced in a thermopile placed in the immediate vicinity of that point.

d. The bolometric method first employed by Rubens and Ritter (Wied. Ann. 40, p. 55, 1890).

e. Perot's micrometric spark gap method (C. R. t. CXIV., p. 165) by which the intensity of the wave at any point is measured by the maximum length of the spark gap when attached to the wire at that point.

The theory of each method is discussed briefly but quite completely, and it is shown very clearly that the results of the experimental investigations cited above are in good agreement with the theory and that they all lead to the conclusion that the oscillations of the oscillator produce simple waves, possessing a rapid rate of decay. This is in accordance with Poincaré's view of multiple resonance.

Bjerkness' experimental method (Wied. Ann. 40, p. 94, 1891) of determining the decrement of a resonator and Poincaré's theory of it are then given and it is shown that this decrement is a hundred times smaller than that of the oscillator.

A brief theoretical discussion of the curves plotted by Perot from the experiments cited above closes this exceedingly interesting and instructive part of the book.

It is pointed out now that the experiments so far discussed do not decide the superiority of the Maxwellian theory over the older theories because it can be and has been predicted by older theories (Kirchhoff, Abhandl. p. 146) that the velocity of propagation of electromagnetic disturbances along a long straight wire suspended in air is the same as the velocity of light. A review of some of the older experiments in this direction is then given.

Direct Determination of the Velocity of Propagation along Conducting Wires.—The earliest experiments carried out according to methods against which no serious objections could be raised were those of Fizeau and Gounelle (1850) over telegraph lines between Paris and Amiens, a distance of 314 kilometers. The method was similar to that employed by Fizeau in the determination of the velocity of light. The mean velocity was found to be 10^5 kilometers per second for iron wire and 18×10^4 kilometers per second for copper wire. They employed signals of, comparatively speaking, long duration, and Poincaré shows by a reference to well known theoretical relations that in this case there is a strong distortion of the signals, so that a disturbance starting in form of a short wave returns, after passing over the whole line, in form of a more or less steep wave front followed by a long tail. This made the measurements very uncertain and the velocity of propagation necessarily much smaller than it ought to have been. The experiments of Siemens in 1875 avoided this objection, in a measure, by employing the disruptive discharge of a Leyden jar for the purpose of starting an electrical disturbance on lines of varying length, between about 7 and 25 kilometers. The velocity found was in several cases nearly 250,000 kilometers for iron wire. Here again the velocity came out smaller than that of light and for obvious reasons.

The last and in all respects most successful direct determination of the velocity of propagation was that recently carried out by

Blondlot (C. R., 117, p. 543; 1893). The signals were sent over a wire of about one kilometer in length and another of about 1.8 kilometers. In the first case the mean velocity was found equal to 293,000 and in the second to 298,000 kilometers per second which is very close to the velocity of light. Poincaré proceeds now to the discussion of the most severe test of the Maxwellian theory, that is the propagation of electromagnetic waves through dielectrics.

M. I. PUPIN.

COLUMBIA COLLEGE.

(*To be Concluded.*)

Model Engine Construction.—J. ALEXANDER. —New York and London, Whitaker & Co. 1894. Illustrated by 21 sheets of drawings and 59 engravings in the text. 12mo, pp. viii + 324. Price, $3.00.

This little book is an excellent treatise on the construction of models of stationary locomotive and marine engines, and contains also instructions for building one form of hot-air engine. It is written by an author evidently familiar with his subject, and the text and illustrations are such as will serve the purpose of both artificer and amateur, desiring to produce model representations of real working engines of standard forms. Bright young mechanics will find here business-like statements of details of drawing, pattern-making, and finishing such models; and, if heedfully complied with, these instructions will result in the production of steam-engines which will actually 'steam,' and which will delight the heart of the mechanician. The drawings are all representative of British practice, and, in some respects, therefore, quite different from familiar practice in the United States; but British practice is 'not so bad,' after all, and many old mechanics, and probably every amateur, will be able to profit greatly by the careful study of this little work.

R. H. T.

NOTES.

PERSONAL.

KARL HANSHOFER, Professor in the University of Munich, and well known through his researches in crystallography and other branches of mineralogy, has died at Munich at the age of fifty-four.

PROF. G. LEWITZKY has been appointed Director of the Observatory in Dorpat, and Dr. L. Sturve succeeds Professor Lewitzky at Charkow.

PROF. F. KOHLRAUSCH, of Strassburg, was proposed as the successor of Hertz at Berlin, but the death of Helmholtz intervening he will now succeed the latter in the Directorship of the Physico-Technical Institute.

GENERAL.

THE discontinuation of the *Index Medicus* is threatened unless sufficient subscriptions are secured before February 1 to defray the costs of publication.

ACCORDING to the *Publishers' Circular* there were 5,300 new books and 1,185 new editions published in Great Britain during 1895, 203 more than during 1894. Of these, 98 new books and 30 new editions are placed under the heading 'Arts, Sciences and Illustrated Works.'

MR. GEORGE F. KUNZ, Special Agent, Division of Mining Statistics and Technology, U. S. Geological Survey, has sent letters asking for information concerning the fresh-water pearl fisheries, and concerning precious and ornamental stones of the United States.

PROF. S. P. LANGLEY, Secretary of the Smithsonian Institution, has addressed a letter to the competitors for the Hodgkins Fund Prizes of $10,000, of $2,000, and of $1,000, stating that in view of the very large number of competitors, of the delay which will be necessarily caused by the intended careful examination, and of the futher time which may be required to consult a European Advisory Committee, if one be appointed, it is announced that authors are now at liberty to publish these treatises or essays without prejudice to their interest as competitors.

CONGRESSES.

The sixth International Geographical Congress will be held at London, on July 26, 1895, and continue until August 3. There will be an extensive exhibition in connection with the congress.

NEW AND FORTHCOMING PUBLICATIONS.

W. B. Saunders, Philadelphia, has in preparation *An American Text-book of Physiology*, by Henry P. Bowditch, M. D., John G. Curtis, M. D., Henry H. Donaldson, Ph. D., William H. Howell, M. D., Frederic S. Lee, Ph. D., Warren P. Lombard, M. D., Graham Lusk, Ph. D., Edward T. Reichert, M. D., and Joseph W. Warren, M. D., with William H. Howell, Ph. D., M. D,. as Editor.

The idea of holding International Mathematical Congresses is crystallizing into shape. Prof. Vassilief, of Kazan, has suggested an assembly of mathematicians in 1896, in order to definitely decide the organization of such congresses. The matter was pushed a little further at the Vienna meeting of the Deutsche Mathematiker Vereinigung, in September last, when it was unanimously resolved that the Committee of the Mathematical Union should take part in framing the necessary arrangements; and the Mathematical Section of the French Association for the Advancement of Science have also expressed their support of the scheme. A circular now informs us that the Editors of the *Intermédiare* will be glad to receive the names of mathematicians who are in favor of international meetings of the kind suggested. M. C. A. Laisant's address is 162 Avenue Victor-Hugo, Paris; and that of M. E. Lemoine, 5 rue Littrè.—*Nature.*

Félix Alcan has just issued the first part (extending as far as *Aliment* only) of an elaborate *Dictionnaire de Physiologie*, edited by M. Charles Richet with the coöperation of the leading French physiologists. The work is expected to contain about 5,000 pages, and to be completed in fifteen parts or five volumes.

Ginn & Co. announce for publication in February *Molecules and the Molecular Theory of Matter*, by A. D. Risteen.

Appleton & Co. announce *The Dawn of Civilization*, by Prof. Maspero, and *The Pygmies*, translated from the French of A. de Quatrefages, by Prof. Frederick Starr.

Whittaker & Co. are publishing this year a weekly journal of science combining *The Technical World* and *Science and Art.*

W. Engelmann has begun the publication of an *Archiv für Entwickelungsmechanik der Organismen*, edited by Dr. W. Roux.

The Rose Polytechnic Institute of Terre Haute, Ind., has begun the publication of a series of bulletins of which the first number is *Physical Units*, by Prof. Thomas Gray.

SOCIETIES AND ACADEMIES.

THE ANNUAL MEETING OF THE AMERICAN MATHEMATICAL SOCIETY.

The annual meeting of the American Mathematical Society was held Friday afternoon, December 28th, at Columbia College, New York. In the absence of the president, Dr. Emory McClintock, and of the vice president, Dr. G. W. Hill, Professor R. S. Woodward, of Columbia College, presided. Among those present were Professor Simon Newcomb, Professor J. M. Van Vleck, Professor Henry Taber, Professor Mansfield Merriman, Professor H. D. Thompson, Professor Mary W. Whitney, Dr. E. L. Stabler, Mr. P. A. Lambert, Mr. R. A. Roberts, Dr. Charlton T. Lewis, Mr. Gustave Legras, Professor J. H. Van Amringe, Professor Thomas S. Fiske, Dr. E. M.

Blake and Mr. G. H. Ling. In the secretary's report, it was stated that the total membership of the Society was 251. The council and officers elected for 1895 were as follows: President, Dr. George W. Hill; Vice President, Professor Hubert A. Newton; Secretary, Professor Thomas S. Fiske; Treasurer, Professor R. S. Woodward; Librarian, Dr. E. L. Stabler; Committee of Publication, Professor Thomas S. Fiske, Professor Alexander Ziwet, Professor Frank Morley; Other Members of the Council, Professor Thomas Craig, Dr. Emory McClintock, Professor Mansfield Merriman, Professor Henry B. Fine, Professor E. Hastings Moore, Professor Ormond Stone, Professor Simon Newcomb, Professor Charlotte Angas Scott, Professor Henry S. White.

The address of the retiring president, Dr. McClintock, was read to the Society by Dr. Charlton T. Lewis. It was entitled *The Past and Future of the Society.* The following papers were also read: *On a Certain Class of Canonical Forms*, by Mr. Ralph A. Roberts; *A New Definition of the Hyperbolic Functions*, by Professor Mellen W. Haskell.

Thomas S. Fiske, *Secretary.*

Columbia College.

IOWA ACADEMY OF SCIENCES.

Ninth annual session, Des Moines, Iowa, December 27 and 28, 1894.

Thursday Morning, December 27.

1. *Inter-Lœsial Till near Sioux City:* J. E. Todd and H. Foster Bain.

2. *Pre-Glacial Elevation of Iowa.* 3. *The Central Iowa Section of the Mississippian Series:* H. Foster Bain.

4. *Secular Decay of Granitic Rocks.* 5. *Structure of Paleozoic Echinoids.* 6. *Opinions Concerning the Age of the Sioux Quartzite.* 7. *Illustrations of Glacial Planing in Iowa:* Charles R. Keyes.

8. *Record of the Grinnell Deep Boring.* 9. *The Topaz Crystals of Thomas Mountain, Utah:* Arthur J. Jones.

10. *The Lansing Lead Mines:* A. G. Leonard.

11. *How Old is the Mississippi?* 12. *On the Formation of the Flint Beds of the Burlington Limestones.* 13. *Coincidence of Present and Pre-Glacial Drainage Systems in Extreme Southeastern Iowa.* 14. *Extension of the Illinois Lobe of the Great Ice Sheet into Iowa.* 15. *Glacial Markings in Southeastern Iowa:* F. M. Fultz.

16. *The Maquoketa Shales in Delaware County, Iowa.* 17. *On Some Supposed Devonian Outliers in Delaware County, Iowa:* S. Calvin.

18. *On the Occurrence of Megalomus Canadense in the LeClaire Beds at Port Byron, Ill.*

19. *Geological Section of Y. M. C. A. Artesian Well at Cedar Rapids, Iowa:* William H. Norton.

Thursday Afternoon.

20. *President's Address: Some Recent Work on the Theory of Solutions:* L. W. Andrews.

21. *Report of Committee on State Fauna:* C. C. Nutting.

22. *A New Method of Studying the Magnetic Properties of Iron.* 23. *On the Design of Transformers and Alternating Current Motors.* 24. *Note on a Phenomenon of Diffraction in Sound:* W. S. Franklin.

25. *A Kymograph and its Use:* W. S. Windle.

26. *The Volatility of Mercuric Chloride:* A. C. Page.

27. *Notes on Applying Pollen in the Crossbreeding of Plants:* N. E. Hansen.

Friday Morning, December 28.

28. *Changes that Occur in the Ripening of Indian Corn:* C. F. Curtiss.

29. *Methods of Soil Analysis:* G. E. Patrick.

30. *The Coal Supplies of Polk County, Iowa:* Floyd Davis.

31. *A Study of the Nitrogen Compounds of the Soil:* D. B. Bisbee.

32. *A Chemical Study of Honey:* W. H. Heileman.

33. *Notes from the Chemical Laboratory, Iowa Agricultural College, 1894:* A. A. Bennett.

Friday Afternoon.

34. *Effects of Heat on the Germination of Corn and Corn Smut:* F. C. Stewart.

35. *A General Discussion of the Family Psyllidæ, with Descriptions of New Species found at Ames, Iowa:* C. W. Mally.

36. *New Species of Thripidæ:* Alice M. Beach.

37. *Studies of Migration of Certain Aphidideæ:* Herbert Osborn and F. Atwood Sirrine.

38. *Description of a Species of Aphid Occurring on Carex:* F. Atwood Sirrine.

39. *The Pollination of Cucurbits*—by title: L. H. Pammel and Alice M. Beach.

40. *Notes on the Pollination of Some Flowers:* Alice M. Beach.

41. *On the Migration of Some Weeds.* 42. *Notes on Fungus Diseases of Plants at Ames, Iowa, 1894*—by title. 43. *Notes on the Flora of Western Iowa*—by title: L. H. Pammel.

44. *The Action of Antiseptics and Disinfectants on Some Micro-organisms:* L. H. Pammel and O. H. Pagelsen.

45. *Notes on a Micrococcus which Colors Milk Blue:* L. H. Pammel and Robert Combs.

46. *On the Structure of the Testa of Polygonaceæ:* Emma Sirrine.

47. *A Study of the Glands in Hoptree (Ptelea Trifoliata.)*: Cassie M. Bigelow.

48. *Graphic Representation of the Properties of the Elements.* 49. *Strata Passed in Sinking a Well at Sidney:* T. Proctor Hall.

50. *Notes on the Minerals of Webster County:* Arthur C. Spencer.

51. *Some Notes on the Reptiles of Southeastern Iowa.* 52. *Bones Found in a Cave in Louisa County.* 53. *Mastodon and Mammoth Remains in Southeastern Iowa:* A. H. Conrad.

54. *Cement Clays in Iowa.* 55. *Conclusions as to the thickness of the Upper Carboniferous in Southwestern Iowa:* E. H. Lonsdale.

56. *A Geographical and Synonymic Catalogue of the Unionidæ of the Mississippi Valley:* by title, R. Ellsworth Call.

Officers for 1895 were elected as follows:

President, H. W. Norris.

1st Vice President, C. R. Keyes.

2d Vice President, T. P. Hall.

Secretary-Treasurer, Herbert Osborn.

Librarian, H. Foster Bain.

Executive Committee, Elective Members: W. H. Norton, N. E. Hansen and T. H. McBride.

SCIENTIFIC JOURNALS.

AMERICAN JOURNAL OF SCIENCE, JAN.

Late Glacial or Champlain Subsidence and Re-elevation of the St. Lawrence River Basin: By W. Upham.

Automatic Mercury Vacuum Pump: By M. I. Pupin.

Graphical Thermodynamics: By R. de Saussure.

Application of the Schroeder-Le Chatelier Law of Solubility to Solutions of Salts in Organic Liquids: By C. E. Linebarger.

Preliminary Notice of the Plymouth Meteorite: By H. A. Ward.

Scientific Intelligence.

NEW BOOKS.

Elementary Lessons in Electricity and Magnetism. Sylvanus P. Thompson. New York and London, Macmillan & Co. 1895. Pp. xv + 628. $1.40.

Popular Scientific Lectures. Ernst Mach. Translated by J. McCormack. Chicago, The Open Court Publishing Co. 1895. Pp. 313. $1.00.

Laboratory Exercises in Botany. Edson S. Bastin. Philadelphia, W. B. Saunders. 1885. Pp. 540. $2.50.

The Aeronautical Annual. Edited by James Means. Boston, W. B. Clarke & Co. 1895. Pp. 172.

Outlines of Dairy Bacteriology. H. L. Russell. Madison, Wis., Published by the Author. 1894. Pp. vi + 186.

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PROCEEDINGS OF THE AMERICAN PHYSIOLOGICAL SOCIETY.

The American Physiological Society held its Seventh Annual Meeting in Baltimore, Md., December 27th and 28th, 1894. The mornings were devoted to the reading of papers, and the afternoons to demonstrations and to visiting the laboratories of Johns Hopkins University. The success of the meeting was largely due to the hospitality of Johns Hopkins University, the University Club and friends of the Society residing in Baltimore.

ELECTION OF NEW MEMBERS.

DR. A. C. ABBOT, First Assistant at the Laboratory of Hygiene, University of Pennsylvania.

DR. G. CARL HUBER, Assistant Professor of Histology and Embryology at the University of Michigan.

DR. P. A. LEVENE, of New York City.

DR. FRANZ PFAFF, of Boston.

ELECTION OF THE COUNCIL FOR 1894-95.

H. P. BOWDITCH, *President.*

R. H. CHITTENDEN.

W. H. HOWELL.

F. S. LEE, *Secretary and Treasurer.*

W. P. LOMBARD.

Reading of Papers and Demonstrations by Invited Guests and Members of the Society.

On the Occurrence of Diethyl Sulphide in the Urine of the Dog, with a Demonstration of Reaction for the Detection of Alkylsulphides of the Series $(C_nH_{2n+1})_2S$. J. J. ABEL.

Dr. Abel demonstrated in a series of reactions, many of them new, that the volatile, odoriferous compound that is liberated when dog's urine is treated with alkalies is ethyl sulphide $(C_2H_5)_2S$, and also that the organic sulphides of the series $(C_nH_{2n+1})_2S$ may readily be detected, wherever found, with the help of his reactions.

On the Use of the Trichloride of Acetonic Acid as Anæsthetic for the Laboratory, with Some

Account of its Fate. J. J. ABEL and T. B. ALDRICH.

Drs. Abel and Aldrich gave an experimental demonstration of the use of the solid trichloride of acetonic acid of Willgerodt, the so-called acetone chloroform, as an anæsthetic for the laboratory, with an account of its physiological action and of its fate, from a chemical point of view, in the economy.

Demonstration of Instances of Experimental Cachexia Tyreopriva in Dogs. J. J. ABEL and A. C. CRAWFORD.

Drs. Abel and Crawford showed a number of dogs whose thyroid glands had been removed. They also gave an account of their results in treating the diseased conditions thus induced, and outlined the methods and aims of a research on the functions of the thyroid gland.

Equilibrium in the Crustacea. G. P. CLARK. (Introduced by F. S. LEE.)

Dr. Clark stated that he had studied two kinds of crabs, the 'Fiddler,' *Gelasimus pugilator* (Latr.), and the 'Lady,' *Platyonichus ocellatus* (Latr.). The former is an active runner, the latter an active swimmer. The movable eyestalks show marked compensating movements when the body is inclined. The compensating positions are maintained without reaction so long as the inclination of the body continues. No compensating movements accompany turning around the vertical axis. The otocysts contain no otoliths. Removal of both antennules, inclusive of the otocysts, caused no abnormal position of the body and no forced movements, but was followed by a tendency of the 'Fiddler' crab when attempting to run, and of the 'Lady' crab when attempting to swim, to roll over on to the back. A similar tendency has been observed by others in the crayfish and dogfish after removal of the otoliths. Removal of both antennules was followed by no abnormal position of eyestalks, but by marked diminution of their compensating movements. Removal of otoliths from both ears of a dogfish is reported to be without effect on position of eyeballs, but to cause a loss of the *maintenance* of compensation which is observed in those rotations which involve inclination of the body. Compensating eye movements in the crab occur only in those planes in which in the dogfish the compensation is maintained, and loss of corresponding structures in these animals tends to destroy compensation in the one and the maintenance of compensation in the other. In many cases it was found that a small amount of compensation remained after the 'Fiddler' crab had lost both antennules; if eyes were then covered with a thick black mixture it was completely stopped.

Galen's Technical Treatise upon Practical Anatomy and Experimental Physiology. J. G. CURTIS.

Dr. Curtis spoke upon Galen's technical treatise on practical anatomy and experimental physiology, usually cited as '*De anatomicis administrationibus.*'

This was written between A. D. 150 and 200, and is the earliest existing technical treatise upon these subjects.

The Greek text of Books I. to VIII., and of part of Book IX., is extant in print, and also Latin translations of the same.

The rest of the work, viz., the latter part of Book IX., and Books X. to XV., is inedited, and is contained only in two MSS. of an Arabic version of the 9th century, attributed to HONAIN IBN ISHAK or to his nephew HOBAICH.

One of these two MSS. is at the Bodleian Library at Oxford. By the kindness of the authorities, Books IX. to XV. of this MS. have been photographed for Dr. Curtis, who is also, through the good offices of the late DR. GREENHILL, of Hastings, England, in possession of an inedited MS. sketch of a translation of these books into French, by the late M. GUSTAVE DUGAT.

Dr. Curtis proposes to edit, and to have published, a translation into English of the entire treatise, the Greek portion to be translated by himself, and the inedited Arabic portion by a collaborator not yet named.

This English translation will be the first complete edition of the 'epoch-making' Galenic work in question published in any language since the invention of printing.

The Normal Defect of Vision in the Fovea. MRS. C. L. FRANKLIN. (Introduced by H. P. BOWDITCH.)

König's announcement, in May, 1894, that the relative absorption by the visual purple of the different portions of the spectrum is in very close coincidence with the relative brightness of the different portions of the spectrum, (1) for the totally color-blind, and (2) for the normal eye for faint light after adaptation (with the obvious inference therefrom that the vision of the totally color-blind and that of the normal eye in a faint light was conditioned upon the presence of the visual purple in the retina), made necessary some assumption to take account of the fact that in the fovea, which is the portion of the retina where vision is most acute, no visual purple has hitherto been found. Two assumptions were possible, either that the cones (and hence the fovea) do contain visual purple, but that it is here of such an extremely decomposable character that it can never, no matter what precautions are used, be detected objectively; or, that vision does actually not take place in the fovea under the above circumstances (that is, for the totally color-blind and for the normal eye at such intensities as are visible only after adaptation). As I had already made the prediction that total color-blindness consists in a non-development of the *cones* of the retina (*Ztsch. f. Psych. u. Phys. der Sinnesorgane*, Bd. IV.) and also that the adaptation which renders vision possible after twenty minutes in a faint light is conditioned by the growth of the visual purple (*Mind*, N. S., III., p. 103), both predictions being naturally suggested by my theory of light-sensation, I was most anxious to put the latter assumption to the test. I therefore undertook to determine, in the dark rooms of Prof. König's laboratory, the threshold for light-sensation for different parts of the retina and for different kinds of monochromatic light.

The blindness of the fovea for faint light did not at once reveal itself; the act of fixation means holding the eye so that an image falls on the part of the retina best adapted for seeing it, and hence it would involve keeping the image *out of* the fovea in a faint light, if the fovea were really blind in a faint light. But after the total disappearance of the small bright object looked at had several times occurred by accident, it became possible to execute the motion of the eye necessary to secure it at pleasure. It was then found that the simple devices of presenting a group of small bright objects to the eye of the observer was sufficient to demonstrate the 'normal night-blindness of the fovea' (as it may best be called) without any difficulty; one or the other of them is sure to fall into the dark hole of the fovea by accident. It was only by means of this arrangement of a number of small bright spots that the total blindness of the totally color-blind boy in the fovea could be detected; he had, of course, learned *not* to use his fovea in fixation. Professor König then proceeded to demonstrate the total blindness in the fovea of the normal eye to blue of about 470.*

[These experiments upon the normal eye were exhibited.]—It was shown that König's proof that the pigment-epithelium

*Professor v. Kries is said by Professor Gad to have shown that the experiments in question do not establish the *blue*-blindness of the fovea (*Berichte der Naturforschenden Gesellschaft zu Freiburg*, IX., 2. S. 61). I have not yet had access to this criticism.

is the only layer of the retina which is affected by red, yellow and green light is not wholly conclusive. The interpretation of the new facts and their bearing upon several theories of light sensation were discussed.

[This paper will appear in full in *The Psychological Review* for March, 1895.]

The Influence of low Percentages of Alcohol upon the Growth of Yeast. C. F. HODGE.

The influence of decomposition products upon cellular metabolism is a question of wide physiological interest and has increased in significance since the advancement of recent theories regarding autointoxication. Do the decomposition substances of initial activity stimulate the cells to more active metabolism? Aside from the general question of the physiological effect of alcohol upon cellular processes, the influence of alcohol upon the cell which produces it would seem to be one of the best instances upon which to test the theory of autointoxication. Yeast can grow in a saccharine solution until by the decomposition of sugar it has brought the alcohol content of the liquid up to 14%. With a greater amount of alcohol no growth is possible. Flügge also states that at 12% growth is hindered. Experiments were made with exceedingly attenuated pure cultures in large amounts of nutrient solution, containing from .01%, .1% up to 14 %. Counts were made as often as possible during the first three days. The general result up to the present is that yeast grows nearly twice as fast in pure solution as in 1% alcohol. An average of nine experiments thus far give the following figures representing proportional growth in the various cultures.

Growth in:	0%,	1%,	2%,	3%,	4%,	5%, alcohol.
	77,	45,	16,	1.5,	0.3,	0.11.

Beyond 5% no growth appreciable by the method employed occurred within the three days. In cultures containing 0.1% and 0.01% growth was considerably less than in the normal solution; but it is desirable to experiment further before giving the figures. As yet no evidence in favor of autointoxication theories has been obtained.

A Means of Recording Daily Activity of Animals and the Influence upon it of Food and Alcohol. C. C. STEWART. (Introduced by C. F. HODGE.)

Thus far the animals experimented on have been rats, mice and squirrels. They are kept in circular, easily rotated cages, so arranged that any motion of the animal rotates the cage, and by means of a tambour or levers this motion of the cage is recorded upon kymograph paper kept moving night and day. An electromagnetic circuit with a clock marks hours and minutes. We thus have the manner in which an animal divides his time between rest and activity recorded by himself. Rats and mice divide their days into about 12 hours rest and 12 hours intermittent work during the night. During the work period, short intervals of activity, rarely exceeding an hour, are interrupted by almost equal periods of rest. The squirrel, in winter, works almost continuously for from twenty minutes to two hours early in the morning, with sometimes a short interval of activity late in the evening, and rests nearly 22 hours in the day.

Food has a most marked influence upon diurnal activity. In general the richer the diet in proteid, the greater the activity. Fat has the opposite effect, reducing the activity of mice from 6 to 8 hours' actual work to a few minutes a day. To test the influence of alcohol on spontaneous activity, rats kept on dry corn were given instead of water alcohol of from 5% to 60%. During 50 days of his treatment, no uniform effect of the alcohol could be demonstrated. All normal animals experimented on tended to work more minutes per day, when barometric pressure was high, and this must be taken into careful account in estimating the effect of any condition upon daily activity.

A Study of the Operative Treatment for Loss of Nerve Substance in Peripheral Nerves. G. CARL HUBER. (Introduced by W. P. LOMBARD.)

The report covered the results obtained in 50 experiments on dogs, in which the various methods that might be employed in the surgical treatment of divided peripheral nerves, where there is loss of nerve substance to the extent that an ordinary suture cannot be made, were tried. Segments varying in length from 5–8 cm were removed from the ulnar and sciatic nerves of the dogs. In 26 experiments a portion of another nerve (usually the sciatic of a cat) was implanted between the resected ends of the nerve operated upon, and retained in place by means of sutures; in 8 experiments the resected ends were united by means of decalcified bone tubes; in 7 they were united with a number of catgut threads; a flap from the peripheral end of the central stump was made in 7 experiments; and grafting the central end of the peripheral portion of a resected nerve to an accompanying nerve trunk was tried twice. After carefully closing the wounds, the animals were allowed to live for periods varying from 2 to 182 days; before killing the animals the nerves operated upon were tested as to their conductivity; they were then removed and prepared for histological examination.

1. In all experiments the peripheral portion of the divided nerve degenerated, as also ½ cm. of the distal end of the central stump.

2. Regeneration was obtained after implantation of a nerve segment, tubular suture and suture *à distance* with catgut threads.

3. Regeneration was from the central end, buds given off from the central axis cylinders growing toward the periphery.

4. The implanted substance serves only as a guide to the down growing axis.

5. Regeneration takes place most rapidly (120 to 130 days in dogs) after implantation of a nerve segment.

Demonstration of a New Gas Pump for the Extraction of Blood-Gases. G. T. KEMP.

Dr. Kemp exhibited and explained the action of a new form of gas-pump. This pump is, except for slight modifications, a combination of the Sprengel pump with the Neeson and Bessel-Hagen additions to the Toepler pump. The large bulb is used in accordance with a suggestion of Pflüger and is about the size of those in the large pumps used in the laboratory at Bonn. The pump is made in two halves for ease of transportation. The vacuum space on each side of the bulb prevents the mercury from spitting back into the bulb, during the first few lowerings of the reservoir, as occurs in the Neeson-Bessel-Hagen-Toepler pump. The advantage of this form of pump over all patterns which have a 3-way stopcock at the top of the bulb, is that there is no danger of smashing the stopcock from the impact of the mercury, and the pump can be worked very much faster. No precaution has to be taken against raising the reservoir bulb too rapidly.

The Sprengel attachment can be made to work either separately or together with the other part of the pump.

There is no stopcock which is not completely under mercury seal, so that leakage is out of the question.

The essential requisite of such a pump is to extract all the oxygen as soon as possible, certainly before the blood clots, and to keep the tension in the blood bulb from rising above 20 mm. of mercury, as this prevents the complete disassociation of the oxygen from the oxyhaemoglobin. When blood is drawn into the vacuum the oxygen is given off very rapidly, in a 'puff,' so to speak, and the carbon dioxide is given off more slowly and regularly. By having a large Hg bulb which can be filled and emptied rapidly, the exhaustion can easily be maintained so as to keep the tension below 20 mm. of mercury, and after the oxygen is set free the Sprengel part is left working alone, and that carries off the CO_2, as it is slowly evolved, without necessitating close attention of the operator or the fatigue of raising and lowering the reservoir bulb of mercury.

Further Experiments Upon Equilibrium in Fishes. F. S. Lee.

Previous work of Dr. Lee has shown that the organs of the sense of equilibrium lie in the ear, the semicircular canals mediating sensations of movements in curves, the otolithic parts sensations of the resting body. Recent experiments prove that the otolithic parts are, moreover, sensory organs for progressive movements, *i. e.*, movements in a straight line. Hence the ear deals with all three groups of equilibrium sensations of which the living body is capable.

Stimulation of the central end of the lateral nerve causes coördinated movements of the fins, analogous to those resulting from stimulation of the acoustic. This indicates that the organs of the lateral line are organs of equilibrium.

All experiments to prove that fishes possess a sense of hearing have so far given only negative results.

Equilibrium in the Ctenophora F. S. Lee.

Dr. Lee reported the results of experiments made under his direction by Mr. J. C. Thompson on the equilibrium phenomena of the Ctenophora. The normal animal exhibits definite positions of rest and definite coördinated movements. After removal of the otolith the resting positions are no longer maintained, and incoördination in movement appears. Forced movements do not result. If the body be cutinto two parts, one with and one without the otolithic organ, the former maintains its equilibrium, he latter doest not. All attempts to demonstrate a sense of hearing failed.

The two following papers, because of the lack of time, were read by title:

On changes of Structure in the Pancreatic Cell corresponding with Functional Change. A. P. Mathews. (Introduced by F. S. Lee.)

On the Existence of Secretory Nerves. A. P. Mathews. (Introduced by F. S. Lee.)

On Cardio-oesophagogeal Movements. S. J. Meltzer.

Dr. Meltzer has shown in a former paper that the outflow of arterial blood from, and the inflow of venous blood to, the thorax produce the cardiac movements which are obtainable from the pleuritic cavity as well from the trachea and the nose. In this paper he described the cardio-oesophageal movements arising from the same cause. He exhibited tracings which he obtained fourteen years ago from his own oesophagus, while studying the mechanism of deglutition. His recent studies were made on curarized dogs. By means of vagus inhibition the beginning and the end of each cardiac cycle were made recognizable. Nearly all the curves have the character of a 'negative pulse' and have no similarity either to a sphygmo- or cardiogram. The constant characteristic undulationse seen at the beginning of each cardiac cycle are due to the movements of the auricle, which are more marked in the posterior mediastinum.

Cortex of the Brain: (a) Localization; (b) Development of. T. W. Mills.

Dr. Mills undertook this research in connection with a study of the psychic development of young animals. It became necessary, however, as a precaution and guide in studying the functional development of cortical centres to make experiments on mature animals. While, during these experiments, most of the commonly accepted localization as set forth by Ferrier was verified in a general way, the results did not all harmonize with those of this investigator. Attention was called to details in the cortical motor localization of the rabbit and pigeon more especially, which were at variance both positively and negatively with those announced by Ferrier.

There had been found a great difference in the degree of cortical development of mammals not born blind as compared with those born with the eyes unopened; but as the work was not complete the author preferred not to make many very definite statements at the present time. Cortical development and psychic development took place *pari passu*.

The Active Principle of Rhus Toxicodendron and Rhus Venenata. FRANZ PFAFF. (Introduced by H. P. BOWDITCH.)

Dr. Pfaff stated that his experiments had been made with the assistance of S. Sanford Orr. He said that it is the general opinion that *Rh. tox.* and *Rh. ven.* contain a volatile proximate principle, which causes the well-known dermatitis venenata. Maisch's toxicodendric acid has been generally accepted as the active poison. P. and O. could not believe that a very volatile substance is the cause of the trouble, as this would be contrary to the pharmacology of vegetable skin irritants. They isolated Maisch's toxicodendric acid in the form of the barium salt, and found it nontoxic. The same is true of a solution of the free acid in water. As the real active principle they found a non-volatile oil. This oil, when applied to the skin, causes the well-known eruption. Photographs demonstrating the effect of the oil upon the human skin were shown. As preventive treatment P. and O. proposed a thorough washing with water, soap and brush, or, still better, a repeated thorough washing with an alcoholic solution of lead acetate. The oil being soluble in alcohol, and forming a nearly insoluble lead compound in alcohol, is thus best removed from the superficial skin. Further investigations will be undertaken, and an attempt made to classify Maisch's toxicodendric acid and the new poisonous oil, which seems to be of the kind called cardol, obtained from *Anacardium occidentale.* These two oils are, however, not identical.

Inhibition Hypothesis in the Physiology of Respiration. W. T. PORTER.

Dr. Porter said that it is known that transverse division of the spinal cord between the bulb and the phrenic nuclei causes fatal arrest of the respiratory movements of the trunk. If death be prevented for a time by artificial respiration, the reflex powers of the cord gradually increase, and in the course of a few hours they may become so great that pinching the paws, blowing on the skin, suspending the artificial respiration, etc., may cause extended muscular contractions, including contractions of the respiratory muscles.

It is claimed that these contractions of the respiratory muscles after the separation of the cord from the bulb are proof that the respiratory impulse for muscles of the trunk is not derived from respiratory cells in the bulb but originates in the spinal cord. Against this hypothesis of spinal respiration is urged the fatal arrest of the respiration of the trunk caused by separating the bulb from the cord. It is replied that section of the cord stimulates inhibitory fibres in the cord and thus suspends the action of the spinal respiratory cells. This inhibition, it is assumed, usually lasts throughout the period of observation; in some animals, however, after long artificial respiration, it is partially overcome, permitting the respiratory contractions mentioned above.

The doctrine of prolonged inhibition of spinal respiration is easily overthrown by the following experiment. Hemisection of the cord usually arrests the contractions of the diaphragm on the side of the hemisection. (Exceptions are explained by 'crossed respiration.') This arrest is not an inhibition, for the diaphragm on the side of the hemisection begins at once to contract when the opposite phrenic nerve is cut. Hence, hemisection of the cord between the bulb and the phrenic nuclei does not inhibit the the phrenic cells on the side of the section.

It follows that two hemisections, completely separating the cord from the bulb, do not inhibit the diaphragmatic respiration on their respective sides. The phrenic cells often send out no respiratory impulses after such a section because they receive none from the bulb. The phrenic cells cannot themselves originate respiratory impulses. Hence, the respiratory impulse does not arise in the spinal cord.

Demonstration—Hemisections of the Spinal Cord above the Phrenic Nuclei do not inhibit Thoracic Respiration. W. T. Porter.

Acuteness of Vision in St. Louis Public School Children. W. T. Porter.

The Weight of Dark-haired and Fair-haired Girls. W. T. Porter.

Exhibition of Some New Forms of Galvanometers Suitable for Physiological Use, With Remarks Upon the Same. Prof. H. A. Rowland, at the Physical Laboratory of Johns Hopkins University.

Professor Rowland exhibited two new forms of high resistance galvanometers. One was a modification of the Thompson galvanometer, but less expensive in construction, and possessed a greater delicacy; the other was a modification of the Darsonval galvanometer, and was arranged with the observing telescope on a convenient wall support. It was shown that they were well adapted for laboratory use in Physiological work.

Demonstration of an Apparatus for the Plethysmographic Study of Odors, with Report of Results. T. E. Shields. (Introduced by W. H. Howell.)

Mr. Shields exhibited his apparatus, and gave the following account of its use:

1. It consists of a device for holding the arm firmly in place in the Plethysmograph. Two hard rubber clasps, one fitting the wrist and the other the arm above the elbow, are rigidly connected by two metal rods. The latter of the clasps fits against the Plethysmograph under the rubber membrane, where it is held in place by two other rigidly connected clasps, one against it outside the rubber membrane, and the other against the flange of the Plethysmograph.

2. A device for separating the pulse and vaso-motor curves. A short *wide* tube leads from the Plethysmograph to a vertical glass cylinder in which the water level can be made to register the pressure on the arm. Over the water is an air cushion connected with the tambour by a small tube through a piston movable in the cylinder. The motion of the piston controls the size and pressure of the air cushion. The lever of the tambour is made to move the point of an independently supported pen. A long *narrow* tube leading from the Plethysmograph dips into a test-tube of water swung from a delicate spiral spring. (Method described by Professor H. P. Bowditch.) A vertical thread from the bottom of the test-tube passes under a pulley, thence horizontally over a second pulley, and is held taut by a small weight. On its horizontal part is fastened a thin aluminum plate capable of holding a glass pen at right angle to the thread. The bulb of the pen is independently suspended by a vertical thread. The pendular motion due to the latter in the direction of the horizontal thread is so adjusted as to neutralize the curvilinear motion of the pen arising from the sag in the horizontal thread. The point of the pen may thus be made to describe a straight horizontal line. The resistance to the motion of the water in the *narrow* tube is sufficient to destroy all but vaso-motor effects; pulse effects are, in consequence, only felt through the *wide* tube.

The odors are contained in a series of bottles. The turning of a stopcock, which sends the constant current of air through any particular odor-bottle, at the same time, by an electrical arrangement, marks the instant, and opens the terminal end of the corresponding tube near the subject's nose.

A pneumograph records the respiration. The pulse, vaso-motor and respiratory curves, the signal and time records (in seconds) are all traced in ink on a horizontal kymograph.

Explanation of Natural Immunity. George M. Sternberg.

Dr. Sternberg, after a review of the experimental evidence relating to the cause of the natural immunity which exists among animals against parasitic invasion by various pathogenic bacteria and by putrefactive microörganisms, said that the experimental evidence submitted, considered in connection with the extensive literature relating to 'phagocytosis,' leads us to the conclusion that natural immunity is due to a germicidal substance present in the blood serum, which has its origin (chiefly at least) in the leucocytes, and is soluble only in an alkaline medium. And that local infection is usually resisted by an afflux of leucocytes to the point of invasion, but that phagocytosis is a factor of secondary importance in resisting parasitic invasion.

Warren P. Lombard,
University of Michigan. *Secretary for 1894.*

*AN INHERENT ERROR IN THE VIEWS OF GALTON AND WEISMANN ON VARIATION.**

Weismann's name has become so intimately associated with the doctrine of germinal continuity that he is often regarded as its first advocate, although it is an old conception which has found expression in many writings.

Among others I myself stated it in the following words in a book printed in 1883, before the publication of Weismann's first essay on inheritance.

"The ovum, like other cells, is able to reproduce its like, and it not only gives rise, during its development, to the divergent cells of the organism, but also to other cells like itself. The ovarian ova of the offspring are these latter cells or their direct unmodified descendants."

After the appearance of Weismann's essays, and the revival of discussion on the views of Lamarck, I was much surprised to find my book referred to as a Lamarckian treatise, and my reason for quoting this passage now is not to claim priority, but to show that, in 1883, I, like Weismann, attributed inheritance to germinal continuity.

I may take this occasion to say that I still regard inheritance as a corollary or outward expression of the continuity of living matter, although I am less confident than I was in 1883 of the importance of the distinction between somatic and germinal cells. So much for the doctrine of germinal continuity.

Passing now to another topic, we find that the two most prominent writers on inheritance, Wiesmann and Galton, base their views of variation on the assumption that, at each remote generation, the ancestors of a modern organism were innumerable, although a little reflection will show that this assumption is untenable.

Weismann, at least in his earlier and simpler writings, finds the cause of variation in the recombination, by sexual reproduction, of the effects of the diversified influences which acted upon the innumerable protozoic ancestors of each modern metazoon.

If it can be proved that these protozoic ancestors were not innumerable, but very, very few, and that these few were the common ancestors of all the modern metazoa, his position is clearly untenable.

Galton's view of the cause of individual diversity is very similar to Weismann's. He says: "It is not possible that more than one-half of the *varieties* and number of the parental elements, latent or personal, can on the average subsist in the offspring.

* A paper read, by invitation, at the meeting of the Society of Naturalists, in Baltimore, Dec. 27, 1894.

For if every variety contributed its representatives each child would on the average contain, actually or potentially, twice the variety and twice the number of the elements, whatever they may be, that were possessed at the same stage of its life by either of its parents, four times that of any of its grandparents, 1024 times as many as any of its ancestors in the tenth degree and so on."

As he holds that each offspring must therefore get rid, in some way, of one-half the variety transmitted from its ancestors, he finds an explanation of the diversity between individuals in the diversity of the retained halves of their variety.

Each person has two parents and four grandparents; but even in a country like ours, which draws its people from all quarters of the earth, each of the eight grandparents is not always a distinct person; for when the parents are cousins, this number is six, or five, or even four, instead of eight.

Among more primitive people who stay at home generation after generation, and marry within the narrow circle of their neighbors, a person whose ancestors have transgressed none of our social laws may have a minimum ancestry of only four in each generation.

The maximum ancestry and the minimum fixed by our customs are given for ten generations in the two lines below.

2–4–8–16–32–64–128–256–512–1024=2046.

2–4–4–4–4–4–4–4–4–4=38.

Few persons who can trace their ancestry back for ten generations are descended from 1024 distinct persons in that generation, and in all old stable communities of simple folks the number is very much smaller. In the long run the number of ancestors in each generation is determined by the average sexual environment, and it is a small and pretty constant number.

All genealogy bears indirect evidence of this familiar fact which has not been adequately recognized by students of inheritance.

I have made a computation from the history of the people of a small island on our Atlantic coast. They lead a simple life, or have done so in the past, but most of the men have been sailors, and have ranged much farther in search of mates than agricultural people. I have selected three persons whose ancestry is recorded in detail for some seven or eight generations. These three persons have no parents or grandparents of the same name, and they would not be popularly regarded as near relations, although two of their twelve grandparents were cousins. The generations are not quite parallel, and the period covered by eight in one line is covered in the two others by about seven, and it may be put at about $7\frac{1}{2}$ for the three. In $7\frac{1}{2}$ generations the maximum ancestry for one person is 382 or 1146 for three persons.

The names of 452 of them, or nearly half, are recorded, and these 452 named ancestors are not 452 distinct persons, but only 149; many of these in the remoter generations being common ancestors of all three persons in many lines. If the unrecorded ancestors were interrelated in the same way as they would surely be in an old community, the total ancestry of the three persons for $7\frac{1}{2}$ generations would be 378 persons instead of 1146.

Few persons know even the names of all the living descendants of each of their sixty-four ancestors of the sixth generation, and marriage with one of them is a pure chance, depending on the size of the circle of acquaintance and the distance to which ancestors wandered.

If a city like Baltimore, where the strangers to each one of us outnumber our acquaintances a thousand fold, could be quarantined against people from outside for a thousand years, each generation would be much like the present one so far as known relations are concerned, although at the end of the period the inhabitants would certainly not be descended from the Baltimorians of our day, but from only a very few of them. Most of our lines would be extinct, and the few which survived would include most of the Baltimorians of the year 2900 among their descendants, who, while unconscious of their common origin, would be allied with each other by common descent from their virile and prolific ancestors of the year 1894.

This is proved indirectly but conclusively by genealogical statistics, and while a thousand years are but as yesterday in the history of species, zoölogical considerations furnish evidence that allied animals at two successive geological periods must be related like these successive generations of Baltimorians. Of all the individuals of a species which lived at a given period, very few would have descendants at a later period, and these few would be the common ancestors of all the individuals which represent the stock at the later period.

The extinction of species is a familiar conception. The extinction of the lines of descent from individuals is no less real, and infinitely more significant in the study of inheritance.

As we trace back the ancestral tree it divides into two branches for the parents, and again into four and eight for the grandparents and great-grandparents, and so on for a few generations, but a change soon takes place. The student of family records may be permitted to picture genealogy as a tree whose branches become more and more numerous as we get farther and farther from the starting point; but this cannot be permitted to the zoölogist.

On the contrary, we must admit that, on the average, the number of ancestors in each generation can never be greater than the number of individuals in the average sexual environment. It may be very much less, however, since most of the individuals in each generation must fail to perpetuate their lines to remote descendants.

Now no animal in a state of nature ranges so far as man in search of a mate, and the sexual environment of many plants and animals, such as the fishes in a brook or a pond, or the parasites in the intestine of a mammal, is very narrow. While new blood, no doubt, finds its way in from time to time, this is more than balanced by the extinction of genetic lines. The series of ancestors of each modern organism is long beyond measure, but the number of ancestors in each remote generation can never be very great, though it may be extremely small.

The data of systematic zoölogy also force us to believe that the ancestry of all the individuals of a species has been identical, except for the slight divergence in the most recent part of their history.

The zoölogist must picture the genealogy of a species not as a tree, but as a slender thread, of very few strands, a little frayed out at the near end, but of immeasurable length and so fine that the thickness is as nothing in comparison. The number of strands is fixed by, but is much smaller than, the average sexual environment. If we choose we may picture a fringe of loose ends all along the thread to represent the ancient animals which, having no descendants, are to us as if they had never been. Each of the strands at the near end is important, as a possible line of union between the thread of the past and that of the distant future.

The gist of the whole matter is this, that we must picture this slender thread as common to all the individuals of the species, whose divergence from each other is infinitesimal compared with the ancestry they share in common.

The branches of a human genealogical tree diverge for a few generations by geometrical progression, but we soon find traces of a change, and if the record were long enough to have any evolutionary significance we should surely find all the members of a species descended from a few remote ancestors, and these few the common ancestors of all. If one metazoon is descended from pre-Cambrian unicellular ancestors, the same unicellular individuals were the common ancestors of all the metazoa, and we may be confident that there were not very many of them in each generation. It is quite possible that they were even so few as a single pair or even one.

There is nothing very novel in all this. Galton has himself devoted an appendix to the mathematical study of the extinction of family names, although he and other writers on inheritance seem to forget it when they assume that the remote ancestors of two persons, A and B, were, like the parents, distinct individuals, and that the offspring must have twice as much ancestry as either parent, and, therefore, twice as much variety, unless there is some way to cancel out half of it at each step.

I called attention to the bearing of this convergence of ancestry on the problem of inheritance in 1883, in words which still seem to be a clear statement, although the views on variation of both Galton and Weismann are based on the unfounded assumption that each sexual act brings together two totally dissimilar sets of factors, instead of factors which are identical in innumerable features for each one in which they differ.

My statement is as follows: "In order to breed together, animals must be closely related; they must belong to the same species or to two closely allied species. Since the individuals which belong to two closely related species are the descendents of a common and not very remote ancestral species, it is clear that almost the whole course of their evolution has been shared by them in common; all their generic characters being inherited from this ancestor. Only the slight differences in minor points which distinguish one species of a genus from another have been acquired since the two diverged, and not even all of these slight differences. * * We know that the duration of even the most persistent species is only an infinitesimal part of the whole history of their evolution, and it is clear that the common characteristics of two allied species must outnumber, thousands of times, the differences between them. It follows that the parents of any possible hybrid must be alike in thousands of features for one in which they differ. * * Crossing simply results in the formation of a germ by the union of a male and a female element derived from two essentially similar parents, with at most only a few secondary and comparatively slight differences, all of which have been recently acquired."

I trust that you will not think me unwarranted in the assertion that due consideration of the substance of this extract might have saved us much unprofitable discussion of the causes of variation, for I hope I have made it clear that these must be sought in the modern world and not in the remote past; that, as I expressed it in 1883, "the *occurrence* of a variation is due to the direct action of external conditions, but its precise character is not."

I sought by these words to express the familiar fact that the stimulus under which a vital action takes place is one thing, while the character of the action itself is quite another thing.

This fact seems, from its very simplicity, to slip out of the minds of naturalists, and I should like to improve this opportunity to approach it from another standpoint.

We have been familiar for many years with two views of the nature of the process of development from the egg.

One school of embryologists holds that the organism arises from the egg by virtue of its inherent potency; that the constitution which the germinal matter has inherited is in some way an embodiment of all that is to be unfolded out of it; while the other school finds, in the stimulus which one part of the segmenting egg or of the growing organism exerts on other parts, the explanation of each successive step in the process of development.

Advocates of these two views generally regard themselves as opponents, but is there any real antagonism?

We now have positive evidence enough for each view to convince me that both are true; that every change which takes place in the organism from the beginning of segmentation to the end of life is called forth by some external stimulus either within the body or without; and yet that the outcome of the whole process of development is what it is because it was all potential in the germ.

The gun does not go off until the cap explodes; but it hits the mark because it is aimed.

While the distinction between the stimulus to a vital change and the nature of the change itself is obvious enough in simple cases, we may easily become confused and lose sight of it in handling complicated problems.

A hen's egg does not develop without the stimulus of heat, but the view that heat causes the chick is too grotesque for a sane mind.

What interests us is not that it becomes a chick while a duck's egg in the same nest becomes a duckling, but that the one grows into exquisite adjustment to the life of fowls, while the other becomes as admirably adapted for the life of ducks.

Here the stimulus comes from the external world, but the case is just the same when it is internal.

The well-known results of castration prove that the normal development of male animals is dependent on some stimulus which comes to the parts of the growing body from the reproductive organs, but who can believe that this is an adequate explanation of the short, sharp horns, the thick neck and the ferocity of the bull, or the bright colors and high courage of the cock?

The only explanation of the origin of these useful structures worth considering is that which attributes them to the retention by the germ of the effects of past ages of selection.

We have no reason to take a different view when the result varies with the stimulus. Under one internal stimulus a bud becomes a jelly-fish, while under others it may become a hydranth, or a machopolyp or a blastostyle, but the problem we have to solve in this case as in others is the origin of a beautifully coördinated organism, with the distinctive characters of its species, and with exquisite fitness for a life like that of its ancestors.

I showed some years ago that a small crustacean, Alpheus heterochelis, develops from the egg according to one plan at Beaufort in North Carolina, according to a second at Key West in Florida, while it has still a third life history at Nassau in the Bahama Islands, but no one can believe that the influences which cause this diversity have anything to do with the final outcome of the process.

The case is exactly the same when a cell which normally gives rise to a half or a quarter of the body produces the whole under a different stimulus.

All the machinery in a great industrial exposition may be started by a single electric contact, but however much the discovery of the button may interest us, it helps us little to understand the result.

So it is with living organism. External conditions press the button, but it takes all the inherited potency of living matter to do the rest.

It is an error to believe that great knowledge is needful for a clear grasp of first principles. Too often a great store of information is like riches, "it cannot be spared nor left behind, but it hindereth the march;

yea, and the care of it sometimes loseth or disturbeth the victory."

Students who are drifting on the sea of facts with which the modern laboratory has flooded us declare that the doctrine of adaptation is antiquated and unscientific and pernicious.

They tell us organisms have many properties which are not adaptive, and that in many other cases we cannot tell whether a property is adaptive or not. Of course this is true. No one supposes that susceptibility to poisons, for example, is adaptive, and our knowledge of nature is incomplete beyond measure.

They tell us, too, that many attempts to explain the uses of parts are fanciful and worthless. Unfortunately, this is true also, but the logic which makes it a basis for denying the reality of adaptation is enough to call Paley from his grave.

While protoplasm is the physical basis of life, the intellectual basis of biology is adjustment.

I should like to see hung on the walls of every laboratory Herbert Spencer's definition to the effect that life is not protoplasm but adjustment, or the older teaching of the Father of Zoölogy that the essence of a living thing is not what it is made of nor what it does, but why it does it.

Spencer has given us diagrams to prove that the vertebral column has become segmented by the strain of flexion, but Aristotle tells us that Empedocles *and the ancients* are in error in their attempts to account for the jointing of the backbone by the strain of flexion, for the thing to explain, he says, is not how it becomes jointed, but how the jointed backbone has become so beautifully adjusted to the conditions of life.

"Is there anything of which it may be said: See, this is new. It hath been already in the old times which were before us."

W. K. Brooks.

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CURRENT NOTES ON ANTHROPOLOGY (III.).

THE EARLIEST ENGLISHMEN.

Some interesting studies as to the earliest signs of human industry in England deserve a notice.

The description by Professor Prestwich of some flint implements found by Mr. Harrison in pre-glacial strata on the chalk plateau of Kent seems to have added an impetus to such researches. Mr. O. A. Shrubsole describes a series of those relics from pre-glacial hill gravels in Berkshire, in the Journal of the Anthropological Institute for August, 1894; and in the May number of the same journal, Mr. A. M. Bell replies with considerable force to the objections which had been urged againstProfessor Prestwich's reasonings; vindicating for the Kent implements an antiquity beyond that of the formation of the present river valleys.

A pleasantly written volume on the subject is one by Mr. Worthington G. Smith entitled, *Man the Primeval Savage.* He discovered a true palaeolithic workshop, or rather several of them, in undisturbed relations, near Dunstable, about thirty miles north of London. The heaps of chips and broken flints lay just as the primeval artist had left them, covered to many feet in depth by the washings from the boulder clay. Mr Smith was able to collect the chips in a number of instances, and by fitting them together, reconstruct the original flint block from which the instrument had been formed; and then to make a cast of the size and shape of the tool represented by the cavity. This beautiful demonstration leaves nothing to be desired. He does not believe, however, that either his finds or those of the others mentioned are pre-glacial. His book is agreeably written and well illustrated. (Published by E. Stanford, London.)

THE TRIBES OF THE 'GRAN CHACO.'

The 'Gran Chaco,' or 'Great Hunting-ground,' merits its name, for it extends 850 miles in length by 350 in breadth, one vast forest and marsh, in the northern portion of the Argentine Republic. Much of it is unexplored and almost inaccessible. Its sparse human inhabitants are savage and wandering tribes, still in the stone age, shy and treacherous. Their linguistic classification presents extraordinary difficulties. Explorers have extended the same name to different stocks; and applied diverse names to the same stock.

An excellent monograph published in the Atti Della Societá Romana di Antropologia by Guido Boggiani is helpful as far as it goes. It is entitled '*I Ciamacoco.*' This is another form of *Zamuco*, the name of a tribe converted in the last century by the missionaries. But the modern is not a descendant of the ancient clan, scarcely any linguistic relative. The author presents an accurate vocabulary of about 250 words, and gives a full description of the primitive arts of the tribe, with 62 beautifully prepared illustrations. They still use the stone axe, the bow and arrow, feather and shell decorations, and other appurtenances of the pristine condition of culture.

Another band, the Chunupies, of the southern Chaco, is the subject of an article by J. B. Ambrosetti, in the Anales de la Sociedad Scientifica Argentina for 1894. He gives a short vocabulary and an ethnographic description.

Such work cannot be accomplished too soon, as these Chaco tribes are dying out with fearful rapidity, and probably half a century more will complete their extermination.

ARCHÆOLOGY AS A DEDUCTIVE SCIENCE.

WITHIN the last two years an interesting issue has arisen between two schools of archæologists, the one which *knows* just what man's early activities yielded, the other which prefers to learn about them by studying what relics can be found, and confining conclusions to their obvious teachings.

In America the former school is ably represented by Mr. W. H. Holmes and Mr. J. D. McGuire, of Washington. Mr. Holmes' lines of thought are fully set forth in the Proceedings of the Chicago Congress of Anthropology, in an article entitled *Natural History of Flaked Stone Implements.* He maintains that an implement is to be studied 'as the biologist studies the living creature;' and he therefore classifies such remains into 'species' and 'genera,' speaks of their 'lines of evolution,' and even of their 'ancestral forms,' and adds diagrams showing their genealogies.

Mr. McGuire, who has published several interesting articles on the methods of chipping and rubbing stone, in the *American Anthropologist*, has become so thoroughly master of the situation in that connection that he more than intimates that European archæologists have blundered in drawing a distinction between the 'rough stone age' and the 'polished stone age;' a position with which Mr. Holmes seems to sympathize. That neither of these learned writers has ever examined a European site, seems to them of light weight, as the 'natural history method' is sufficient.

Those of a different way of thinking have not been silent. In this country such students as Prof. Henry W. Haynes, of Boston, Mr. H. C. Mercer, of Philadelphia, and Mr. Thomas Wilson, of Washington, all of whom are personally familiar with the oldest 'stations' on both continents, have condemned as narrow and inapplicable the views of Messrs. Holmes and McGuire; and in the *American Naturalist*, for December, Mr. Charles S. Read, of the British Museum, in an exhaustive article, sets forth the uncertainties which must attend conclusions based on studies limited to one field of research. In the same tone are several articles in recent issues of *L' An-*

thropologie. Mr. McGuire returns to the charge in the January number of the *Naturalist*, but hardly strengthens his position.

The discussion is not yet terminated. 'Replies' are announced; but at present, it must be said that the deductive and inferential method in archæology appears to be a dubious mode of procedure.

THE VANNIC LANGUAGE.

Most readers need not be told that the Vannic language means that which was once spoken in the region around Lake Van, in modern Armenia, by the people who called themselves Kaldi.

They came into contact with the Assyrians about 885 B. C., and adopted from them the cuneiform writing, by means of which they preserved their records in their own tongue. These have been zealously studied and collected of recent years, but without positive results. Professor Sayce maintains that the Vannic was a Georgian dialect, and has published from it various translations. Last summer, before the French Academy, M. Oppert pronounced all these translations illusory, denied that we know a single word of the tongue, and laughed at the names of the kings so seriously put forth by Sayce. The latter, however, in the *Journal of the Royal Asiatic Society* for October last, prints a bilingual inscription in good Assyrian and Vannic, where the texts correspond almost line for line, and claims in a number of examples to have proved by this confrontation the correctness of his earlier translations. He acknowledges that our defective acquaintance with the Assyrian is a difficult obstacle to a complete rendering.

The evidence that the Vannic was akin to the Georgian is, however, not increased by this bilingual text. It still remains more probable that it was either ancient Armenian, or some other long extinct Aryan dialect; possibly near to the Thracian, for which there is a little evidence in the similarity of proper names. The point is one of considerable ethnographic importance.

RECENT PUBLICATIONS ON CRANIOLOGY.

Two important contributions on the Craniology of the South American Indians have recently appeared.

The first is by Dr. Ten Kate on the skulls of the Araucanians of the Argentine Republic. His material was 119 crania in the Museum of La Plata (where his paper was published). He confirms the statement quoted in my *American Race*, p. 324, that these Indians are markedly brachycephalic, 96 out of the 119 having a cephalic index above 80. The proportion of artificially deformed specimens is large, numbering about 82 per cent. They present quite diverse varieties of deformation.

Two series from Southern Argentina, in the valley of the Rio Negro, are described with his customary minuteness by Dr. R. Virchow in the Proceedings of the Berlin Anthropological Society for 1894, pp. 386–408. One series was from the base of the Cordillera, and evidently was of Araucanian origin; the other, from near the Atlantic coast, presented marked dolichocephaly and probably came from Tzoneca burials. In this article Dr. Virchow incorporates some instructive observations on artificial cranial deformities in America generally, making a useful appendix to his remarks on that subject in his *Crania Ethnica Americana*.

The Smithsonian Miscellaneous Collections, No. 969, just issued, is a translation of *The Varieties of the Human Species* by Giuseppe Sergi, Professor of Anthropology in the University of Rome. His method of classification is based upon the theories of craniology of which he himself is the author. Instead of multiplying, *ad infinitum*, the measurements of the skull as so many craniologists affect, he classifies according to broad outlines of cranial shape, believing that such are far more permanent and therefore more racial than the minor variations which have engaged the attention of others. His arguments are drawn from a conscientious study of ample series from various quarters of the globe, and though some of his refinements may not be sufficiently established, the general principles he advocates merit the careful consideration of cranial specialists, as containing some new and certainly correct observations. A short prefatory note by myself introduces the author to the American public.

THE ARYAN CRADLE-LAND.

If anybody thinks that the question whether the primitive Aryan horde lived in Europe or Asia has been settled, he is mistaken. Two publications of late date show that the defenders of the old theory of their central Asian origin are nowise lacking in vigorous argument.

Prof. August Boltz, of Darmstadt, in a pamplet *Das Vedavolk in seinen Gesamtverhältnissen*, has worked out the problem of the origin and earliest migrations of the Aryans quite to his own satisfaction. He adds two maps, on which the reader can trace very clearly how they began in the great Tarim basin and about Lob Nor, and journeyed westward across the Pamir plateau, on the western slope of which they diverged, the Celtic stem wandering northwest into Europe north of the Black Sea; the Greek, Latin, Etruscan and Slavic branches by way of the Hellespont and the islands; the Iranian group remaining in Persia, while the Veda-folk or Indo-Aryans, ascended the mighty passes of the Hindu Kusch and Karakorum ranges to reach the fertile valleys to the south. These are pretty plans, but we look in vain for a substantial support to them.

Turning to Europe, M. De Nadaillac's admirable summary of the results of the investigations in the lake-dwelling of that continent (in a contribution to the *Revue des Questions Scientifiques* for October last, entitled *Les Populations Lacustres de l'Europe*) lifts the veil as far as at present possible on European culture in neolithic times—those times when the Aryan stock began its wide wanderings. The writer inclines to their Asian origin; but with his customary frankness he acknowledges that nowhere in the debris of these ancient dwellings has there a single positive sign of Asiatic art been discovered, nor any relic such as we might suppose even a savage tribe would carry from its pristine home. Until down to a late period of prehistoric time, European culture seems to have been indigenous. For a clear and accurate summary of what it was among the lake-dwellers, the student would do well to peruse the article referred to.

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*TCHÉBYCHEV.**

Of Russian mathematicians, second only to Lobachévsky should be ranked Pafnutij Lvovitsch Tchébychev.

Born in Russia in 1821 and formerly professor at the University at St. Petersburg, he reached deservedly the very highest scientific honors, being privy councillor, the representative of applied mathematics in the Imperial Academy of St. Petersburg, in 1860 made member of the famous Section I.–Géométrie, of the French Académie des Sciences, and afterward *Associé étranger*, the highest honor attainable by a foreigner.

His best known work is the justly celebrated *Mémoire sur les nombres premiers*, Académie Impériale de Saint Pétersbourg, (1850), where he established the existence of limits within which the sum of the logarithms of the primes inferior to a given number must be comprised. This memoir is given in *Liouville's Journal*, 1852, pp. 366–390.

* Deceased December 8, 1894.

Sylvester afterward contracted Tchébychev's limits; but the original paper remains highly remarkable, especially as it depends on very elementary considerations.

In this respect it is in striking contrast to the equally marvelous paper of the lamented Riemann, *Ueber die Anzahl der Primzahlen unter einer Gegegebenen Grösse* presented to the Berlin *Academia* in 1859. Tchébychev had in 1848 presented a paper with this very title to the St. Petersburg Academie; *Sur la totalité des nombres premiers inferieurs á une limite doninée.* (Given in Liouville's Journal, 1852, pp. 341–365.)

Riemann speaks of the interest long bestowed on this subject by Gauss and Dirichlet, but makes no mention of Tchébychev. However, Sylvester speaks of 'his usual success in overcoming difficulties insuperable to the rest of the world.'

But though best known for his work in the most abstract part of mathematics, in reality Tchébychev was of an eminently practical turn of mind.

Thus it was his work, *Theorie des mechanismes connus sous le nom de parallélogrammes* (Mémoirs des savants étrangers, Tom. VII.), which led him to the elaborate dissertation *Sur les questions de minima qui se rattachent a la représentation approximate des fonctions*, 91 quarto pages in Mémoirs de l' Académie Impériale des Sciences de Saint Pétersbourg, 1858. While the variable x remains in the vicinity of one same value we can represent with the greatest possible approximation any function $f(x)$, of given form, by the principles of the differential calculus. But this is not the case if the variable x is only required to remain within limits more or less extended. The essentially different methods demanded by this case, which is just the one met in practice, are developed in this memoir.

The same line of thought led to his connection with a subject which has since found a place even in elementary text-books, namely rectilineal motion by linkage.

He invented a three-bar linkage, which is called Tchébychev's parallel motion, and gives an extraordinarily close approximation to exact rectilineal motion; so much so that in a piece of apparatus exhibited by him in the London Loan Collection of Scientific Apparatus, a plane supported on a combination of two of his parallel motion linkages seemed to have a strictly horizontal movement, though its variation was double that of the tracer in the simple parallel motion.

Tchébychev long occupied himself with attempting to solve the problem of producing exact rectilineal motion by linkage, until he became convinced that it was impossible and even strove long to find a proof of that impossibility. What must have been his astonishment then, when a freshman student of his own class, named Lipkin, showed him the long sought conversion of circular into straight motion. Tchébychev brought Lipkin's name before the Russian government, and secured for him a substantial reward for his supposed original discovery.

And perhaps it was independent, but it had been found several years previously by a French lieutenant of engineers, Peaucellier, and first published by him in the form of a question in the *Annales de Mathematique* in 1864. When Tchébychev was on a visit to London, Sylvester inquired after the progress of his proof of the impossibility of exact parallel motion, when the Russian announced its double discovery and made a drawing of the cell and mounting. This Sylvester happened to show to Manuel Garcia, inventor of the laryngoscope, and the next day received from him a model constructed of pieces of wood fastened with nails as pivots, which, rough as it was, worked perfectly. Sylvester exhibited this to the Philosophical Club of the Royal Society and in the Athenæum Club, where it delighted Sir Wm. Thomson, now Lord Kelvin, and led to the extraordinary lecture *On Recent Discoveries in Mechanical Conversion of Motion*, delivered by Sylvester before the Royal Institution on January 23, 1874. This in turn led to Kempe's remarkable development of the subject, and to Hart's discovery of a five-bar linkage which does the same work as Peaucellier's of seven.

Henceforth Peaucellier's Cell and Hart's Contraparallelogram will take their place in our text-books of geometry, and straight lines can be drawn without begging the question by assuming first a straight edge or ruler as does Euclid.

Thus Kempe's charming book, '*How to Draw a Straight Line*,' is a direct outcome of Tchébychev's sketch for Sylvester. As might perhaps have been expected, the immortal Lobachévsky found in his compatriot a devoted admirer. Not only was Tchébychev an active member of the committee of the Lobachévsky fund, but he took the deepest interest in all connected with the spread of the profound ideas typified in the non-Euclidean geometry. Knowing this, Vasiliev in his last letter asked that a copy of my translation of his address on Lobachévsky be forwarded to the great man. His active participation in scientific assemblies is also worthy of note; for example, at the 'Congrès de l' association française pour l'avancement des sciences, à Lyon,' he read two interesting papers, *Sur les valeurs limites des intégrales*, and *Sur les quadratures*, afterwards published in *Liouville's Journal*.

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SCIENTIFIC LITERATURE.

Les Oscillations Électriques. H. POINCARÉ. (CONCLUDED.)

Propagation of Electrical Oscillations Through Air.—The velocity of propagation of electromagnetic induction through dielectrics offered the first experimental test of superiority of the Faraday-Maxwell theory over the older theories. According to these that velocity should be infinite; according to the Faraday-Maxwell view of electromagnetic phenomena it should be the same as that of light. Poincaré reviews carefully all the experimental evidences bearing upon this point. Hertz's experiments in Carlsruhe are first discussed and his early failures in arriving at a satisfactory result are pointed out. Two methods employed in these measurements by Hertz at Carlsruhe and at Bonn are described briefly. One of these consisted in measuring by means of a resonator the difference of phase between two waves sent forth by the same oscillator, one wave along a conducting wire and the other through the dielectric in the vicinity of the wire. The other method consisted in measuring what Hertz considered the wave length of stationary electric waves in air formed by the interference between the direct waves sent forth by an oscillator and the waves reflected by a large flat mirror consisting of a metal sheet 2 meters wide and 4 meters high. In all these experiments the velocity of propagation along the wire seemed to come out considerably different from and generally less than that in air. But the methods were open to several criticisms. In the first place, the hall in which these experiments were carried out was too small for the wave lengths employed; secondly, the influence of the waves reflected from the walls was entirely neglected; thirdly, the dimensions of the reflecting mirror were not large enough in comparison to the wave length to prevent errors of observation due to the misleading influence of diffraction phenomena. All these objections were in a measure overcome in the earliest experiments of Sarasin and de la Rive (C. R. t. CX. p. 72). In these experiments the methods of Hertz were employed, but they were performed in a large hall, with a large mirror and with smaller resonators. The results improved with the increase of the dimensions of the mirror and the diminution of the size of the exploring resonators. In a subsequent series of experiments (C. R. CXX., p. 688) carried out in a very large hall with a mirror 8 meters high and 16 meters wide and employing circular resonators of 50 and 75 centimeters in diameter these investigators obtained completely satisfactory results, proving beyond all reasonable doubt that the velocity of propagation of electromagnetic waves through dielectrics is the same as along conducting wires and equal to the velocity of light. The sources of error in Hertz's experiments were clearly demonstrated by these experiments, for no matter how large were the hall and the mirror a sufficient increase in the dimensions of the exploring resonators would always give misleading results, similar to those obtained by Hertz.

But among the many encouraging results obtained by Sarasin and de la Rive there is one result which causes much anxiety to the mathematical physicist. It is the serious disagreement between the theoretically calculated period of the resonator and that determined experimentally by the illustrious physicists of Geneva. In an exceedingly interesting mathematical discussion of the functions of the resonator Poincaré shows that the wave length of the fundamental vibration can differ but little from twice the circumference of the resonator, whereas Sarasin and de la Rive found it to be equal to eight times the diameter. The cause of this disagreement must be explained by the theory, but how? Poincaré gives no definite answer to this question. Many valuable suggestions are thrown out, however, and the subject is then dismissed after showing by a reference to Blondlot's and Bjerkness' experiments that the theory of the resonator just given is correct in its main features. No other theory of the resonator has been given since that given by Hertz, and Poincaré's discussion contains many valuable additions to the rough outline of the subject sketched out by Hertz. In this connection the reviewer ventures to refer to a paper by Professor P. Drude (*Zum Studium des Electrischen Resonators*, Wied. Ann. Nov. 1894).

Reflection and Absorption of Hertzian Waves.—Resonator and mirror form the essential instruments in every method of studying electrical waves in the dielectric. The phenomena of reflection and absorption of these waves deserve, therefore, careful analysis. To these Poincaré devotes his attention now. The case of orthogonal incidence upon a plane metal mirror is first discussed. It is shown that the penetration of the wave into the metal is inversely proportional to the square root of the product of conductivity and permeability of the metal and directly proportional to the square root of the wave length. For instance, a wave of a periodicity of 50 millions per second, which is the ordinary Hertzian frequency, will be reduced to nearly one-third of its initial intensity at a distance of $\frac{1}{50}$ mm. below the surface of a mirror of copper. The relation, however, which Poincaré obtains between the penetrability of the wave and the wave length, the conductivity, permeability, and specific inductive capacity of the metal does not hold good for frequencies as high as those of light, for on the one hand it gives by approximation a negative value for the specific inductive capacity of all metals, and on the other hand it gives a conductivity 300 to 400 times smaller than that obtained by ordinary resistance measurements. The same relations hold good for oblique reflection. It is interesting to note that if, as Cauchy believes, the fundamental equations of Fresnel (slightly modified) hold good for metallic reflection then a retardation in phase equal to half a period takes place at the reflecting surface when the electric force of the incident wave is normal to the plane of incidence; no retardation takes place if this electrical force is in the plane of incidence. The extinction of the wave in its passage through the metal develops heat and Poincaré calculates the rate at which the heat is developed by a given current, obtaining the interesting result that it is proportional to the square root of the product of frequency, specific resistance and permeability. The results of these considerations are now compared to experiment. The most important experiments bearing upon this part of the theory are those of Bjerkness (l. c.). A circular resonator having a small plate condenser interposed in place of the spark gap was employed. Between these plates a small aluminum sheet was suspended and measured by its deflection the mean square of the potential difference between the plates. The oscillator was gradually tuned and the resonance effect in the resonator measured by the deflection of the aluminum sheet. Six resonators of the same dimensions but of different material were investigated. The resonance curve of copper was highest, then followed brass, silver, platinum, nickel and iron, in the same order as required by theory. The resonator decrement of iron, for instance, was nine times and that of platinum twice as large as that of copper. To measure the depth of penetration these materials were deposited electrolytically, say iron on a copper resonator, or *vice versa*, and the resonator effect measured for the various thicknesses of the deposit. Results agreeing very fairly with the theory were obtained.

Propagation of Electrical Waves through Dielectrics other than Air.—Another crucial test of the correctness of the Faraday-Maxwell theory is furnished by the well known relation that the specific inductive capacity of a dielectric is equal to the square of its index of refraction. This relation is an immediate inference from the new electromagnetic theory. Since the index of refraction of a substance is equal to the ratio of the velocity of propagation in vacuum to that in the substance it follows that the velocity of propagation of a Hertzian wave in dielectrics having a specific inductive capacity larger than unity should be smaller than in air. This relation was tested by Blondlot in the experiments cited above by immersing both the conducting wire and the resonator in a liquid dielectric and measuring the wave length. Another method based upon the same principle was that employed by Rubens & Arons (Wied. Ann. 40 p. 585). The neutral point of a rectangular resonator was connected directly to one side of the spark-gap of the oscillator. No spark was then observed in the spark-gap of the resonator. If, however, the balance of the resonator was now disturbed by inserting on one side of it a certain length of wire immersed in a dielectric the spark appeared. The balance was again restored by inserting a sufficient length of wire in the other side of the resonator. The ratio of these two lengths of wire measured the ratio of the velocities of propagation in air and in the dielectric.

Another method, first employed by J. J. Thomson (Phil. Mag. 30, p. 129), was based on the relation which exists between the capacity of a plate condenser and the dielectric constant of the insulator separating its plates. The period of an oscillator or resonator will vary with the dielectric between the condenser plates. Thomson measured the period of an oscillator for various dielectrics placed between its condenser plates and calculated from it the specific inductive capacity. Several other electromagnetic methods are described briefly by Poincaré, and then the statical methods, belonging most of them to the pre-Hertzian epoch, are passed in quick review. Finally the experimental results are coördinated and briefly discussed. In a large number of cases Maxwell's relation is confirmed; but, again, the cases are numerous in which the agreement between theory and experiment is far from satisfactory; this is especially true of dielectrics showing traces of conductivity and large electric absorption, and even more true of electrolytes. This part of Poincaré's work is rather incomplete, probably because it offers fewer opportunities to a mathematical physicist than any other part of Maxwell's electromagnetic theory. The most serious criticism, perhaps, that may be brought against it is its omission of some of the most important investigations on dielectric constants, as, for instance, the investigations of Boltzmann. Again, not a single word is said concerning the influence which the study of the dielectric properties of substances had upon Faraday and Maxwell and how much it had contributed to the formation of their electromagnetic theory.

The reflection of electrical waves from the surface of a dielectric is taken up and it is shown by a reference to analogous phenomena in optics why reflection cannot occur when the thickness of a dielectric plate is small in comparison to the wave length of an electrical wave. Trouton's experiments (Nature, Vol. 39, p. 391) form the basis of this discussion.

The experimental evidence furnished by the study of the reflection of electrical waves is cited which supports the view that the plane of polarization as defined in optics is perpendicular to the direction of the electrical force in the wave-front.

A very interesting experimental investigation published by Klemencic (Wiener Sitzungsber, 19. Feb., 1891) is next described. It treats of wave reflection by dielectrics. The dielectric experimented with was a slab of sulphur 120 cm. long, 80 cm. wide and 7 cm. thick. The wave length employed was 60 cm. A rectilinear oscillator placed in the axis of a cylindrical parabolic mirror furnished the plane waves. The reflected and refracted waves were studied by means of thermoelectric couples attached to rectilinear oscillators placed in the axis of parabolic mirrors similar to the one used in connection with oscillator. There was a reflection at every angle of incidence when the direction of oscillation of the electrical force was perpendicular to the plane of incidence. But when it was parallel to it then there was an angle of incidence at which no reflection occurred. Fresnel's fundamental formulæ, however, were not quite satisfactorily verified. Poincaré ascribes it to the insufficient thickness of the slab. Klemencic found also that the energy of the incident wave was smaller than the sum of the energies of the reflected and refracted wave, a result which he believed to be due to the presence of diffraction.

Conductors in Motion in an Electromagnetic Field.—The last chapter gives the essential features of Hertz's essay: On the fundamental equations of the electromagnetic field for conductors in motion.

Poincaré considers first the *electromotive force* induced in a circuit which is moving through a variable electromagnetic field. He proceeds as follows: Consider a surface formed by the circuit under consideration. Let it move with the circuit. Consider two consecutive positions of this surface, the time of passage from the first to the second position being infinitely short, the velocity of motion being finite. Consider now the space bounded by the initial and the final position of the surface and by the ring-shaped surface whose boundary is the initial and the final position of the circuit. The total magnetic flux through this surface is according to well known relations proportional to the total amount of what Hertz and Poincaré call *true magnetism* included in the bounded surface. The total induced electromotive force being equal to the total rate of variation of the magnetic flux through the circuit the last relation leads to the following final result: The total electromotive force induced in an infinitely small circuit which moves through a variable electromagnetic field is composed of three parts. First, the electromotive force due to rate or variation of the magnetic flux through the circuit and produced by the time variation of the field itself. Second, the electromotive force due to the rate of variation of the magnetic flux through the circuit produced by the motion of the circuit. The third component of the induced electromotive force can be described as follows: Suppose that permanent magnetic charges are distributed in any way whatsoever throughout the field. There is then a transference of magnetic matter through the moving circuit. We may call it the magnetic convection current, following a suggestion of Hertz (Unters. ueb. d. Ausbr. der el. Kraft, p. 265). This magnetic convection current is equal to the quantity of magnetic matter contained in the volume traced out per unit of time by the moving circuit, and is proportional to the third component of the induced electromotive force. This component does not appear in Maxwell's theory, so that the Hertzian equations seem to be more complete than those of Maxwell.

Poincaré recognizes in this quite a difference between Maxwell's presentation of the electromagnetic theory and that of Hertz; but this difference will evidently exist only if it is proved that a distribution of permanent magnetism, whose induction flux over a closed surface is a constant, different from zero, can exist. The physical meaning of such a distribution is far from being clear, and Poincaré might have well devoted more attention to the elucidation of this perplexing feature of the Hertzian equations. On this point the student will do well to consult Boltzmann (Vorles. über Maxwell's Theorie d. Elec. & d. Lichtes, II. Theil, IX. Vorles.).

The second group of equations refers to the magnetomotive force induced in a circuit which is changing its position with respect to a field of given distribution of electrical force and it is shown that the total magnetomotive force induced in an infinitely small circuit in motion is composed of four components. The first component is proportional to the rate of change of the flux of electric induction which constitutes the conduction current. The second component is proportional to the rate of change of the flux of electric induction which constitutes the displacement current. The third component is proportional to the rate of change of the electric flux due to the motion of the circuit, and the fourth component is proportional to the convection current of permanent electrostatic charges, corresponding to what was called above the convection current of permanent magnetism. There is, however, no difficulty of conceiving a permanent electrification of the dielectric such that the total flux of its induction through a closed surface should be different from zero, and, therefore, the magnetomotive force induced by an electrical convection current is *a priori* evident as soon as the correctness of the fundamental assumptions in the Faraday-Maxwell theory is admitted. There is no difference between this second group of equations and those given by Maxwell.

It is pointed out that the existence of the third component was verified by Rowland's experiments (Pogg. Ann. 158, p. 487), and the existence of the fourth component by the experiments of Roentgen (Wied. Ann. 35, p. 264). The magnetomotive force due to displacement currents was, of course, first pointed out by the experiments of Hertz.

Next follows a beautiful mathematical discussion of the mechanical forces acting upon a body which is moving through an electromagnetic field. The following types of forces are passed in quick review: 1. An ordinary magnetic force due to the presence of permanent magnetism. 2. Ordinary electrostatic force due to the presence of electrostatic charges. 3. Electromagnetic force consisting of four distinct components. One component is the electromagnetic action of the field upon conduction currents. The second component is the electromagnetic action of the field upon the displacement currents. The third component corresponds to the electromagnetic action of the field upon the currents observed by Rowland and Roentgen. The fourth type of force is that between a variable current and the electrical reactions set up in the field by its variation. All these forces except the last have been observed experimentally. The last one is too feeble to be detected by any of the known experimental methods.

The work is, unfortunately, marred by quite a number of typographical errors. Some of them occur in the midst of important and rather difficult mathematical operations and will undoubtedly be a source of considerable perplexity to the younger students for whom, especially, this work is intended.

The reviewer is of the opinion that he will reëcho the sentiment of every lover of the Faraday-Maxwell electromagnetic theory when he states that this, the latest, contribution of the brilliant French mathematician will be a welcome guide to everyone who wishes to keep in close contact with the latest advances of the electromagnetic theory.

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Columbia College.

The Steam Engine and Other Heat Engines. By J. A. Ewing, Professor of Mechanism and Applied Mechanics in the University of Cambridge. Cambridge University Press; New York, Macmillan & Co. 1894. 8vo., pp. xiv + 400. Price, $3.75.

Professor Ewing, in his article on the steam engine in the *Encyclopædia Britannica*, gave good measure to his ability and knowledge of the subject by the production of a treatise in which, for the first time, a systematic and fairly complete discussion was attempted of the theory of the real steam engine, as distinguished from the purely Thermodynamic Theory of the Ideal Heat Engine, which only had previously been presented by writers on that wonderful machine. Clark and Hirn and Iserwood had cleverly shown the wide discrepancy between the ideal and the real engine, and Cotterill had discussed with elegance and clearness the extra thermodynamic losses of the machine; but Ewing brought together, for the first time, and in such form as to make his discussion useful, to theorist and 'practical man' and professional engineer alike, in the study of existing engines and in the attempt to improve upon them by scientifically accurate designing and construction. His article was a condensed, but complete, exposition to its date, of scientific and practical knowledge of the methods of economical production of heat in the boiler, and of the economical thermodynamic utilization of the energy thus made available at the engine, with exact accounts of the various methods of waste of thermal and of dynamic energy. Had its author written nothing else, this article would have sufficed to give him a full share of fame.

His new treatise on the steam engine, now issued in book form, is based upon his earlier discussion, but is entirely rewritten to give it a shape better adapted to its present purpose, and to permit the introduction of new matter. "The endeavor has been, throughout, to make evident the bearing of theory on practical issues." Some space is devoted to experimental work and the discussion of facts and data revealed by it. In so condensed a work it would have been impossible to introduce as complete a study of pure thermodynamics as may be found in

Wood or Peabody, as full treatment of the extra-thermodynamic wastes as in Cotterill, or of experimental methods as in Carpenter; but the book exhibits much of that rarest of talents, ability to condense, and, for an abridged work, maintains an extraordinarily high standard of scientific quality. The discussion of the 'entropy-temperature' diagram of Professor J. Willard Gibbs, which is only now, after many years, finding its place in the treatment of the heat motors, is the fullest and most satisfactory yet produced, not even excepting the work of its first trans-Atlantic advocate, Mr. J. Macfarlane Gray. This method of graphical treatment is gradually finding its place, and a very useful one, in the discussion of thermodynamic machines. Following Wood and Peabody, and later writers, this author has adopted, in all his own computations, the value, 778, for the thermodynamic equivalent obtained by Rowland. It may probably be safely asserted that this value is now universally accepted.

The unavoidable brevity with which all topics are treated in so small a space gives the reader occasion, frequently, to wish that the volume had been doubled in size, and fuller discussion and more of result thus secured; but the book takes its place, among the many other treatises on the steam engine, as meeting a need that is being continually felt more and more by engineers, and which is not as well supplied by any other of the existing abridged discussions of the theory of the machine. It is well up to date in its practical aspects, as well as in the van on its purely scientific side.

R. H. THURSTON.

CORNELL UNIVERSITY.

An Introduction to Chemical Analysis for Beginners.—From the Sixth German Edition of DR. FR. RUDORFF.—Translated by CHAS. B. GIBSON and F. MENZEL.—Chicago, The W. J. Keener Co. 8 vo., 96 pp. Price $1.00

This book is divided into two parts: Part I, Reactions; and Part II, Systematic Course of Qualitative Analysis. Metallic copper is the first substance examined, and then follow copper, zinc, zinc chloride, manganous sulphate, iron, lead, etc., in the order named. A careful examination of this part fails to detect any great novelty either of matter or arrangement. In Part II the metals are grouped under the familiar group reagents except that lead, mercury and silver are placed along with those precipitated by hydrogen sulfid and not, as is usual, separated under hydrochloric acid as group reagent. The scheme of analysis is well conceived, but offers little of novelty. The explanations and notes have been carefully adjusted to meet the needs of the student and are a valuable feature. The translation is, however, a very slovenly piece of work, and the nomenclature is especially bad. For example, on page 72, we find 'ammonic' sulfid written Am_2S, and lower down we have NH_4OH. Why the authors deny to bismuth cobalt and nickel the ic terminations which they give to nearly all the other metallic salts is not apparent. Several very awkward sentences occur. For example, in the introduction, "We have made a few additions calculated to assist the medical and dental student who suffers mainly the disadvantage of being unable to devote but a small part of his time to chemical studies."

The mechanical execution of the book is pretty good. There is no index.

EDWARD HART.

LAFAYETTE COLLEGE.

NOTES AND NEWS.

PALEOBOTANY.

A LARGE collection of fossil plants made by Professor W. P. Jenny in the Cretaceous rim of the Black Hills during the past field season has just been opened at the National Museum and proves to be of the highest interest to paleontology. It was made under unusual difficulties and in the pure love of science in connection with Professor Jenney's work as a mining expert in the Black Hills. All the material comes from the lower portion of what was regarded by Professor Newton as the Dakota group; most of it from nearly the same horizon as that from which the gigantic cycadean trunks now so well known and the small collection of plants made by Jenney and Ward in September, 1893, were obtained (see *Journal of Geology* for April–May, 1894, Vol. II., No. 3, pp. 250–266). The collection has not yet been systematically worked up, but a casual examination of it shows that the plants have no relation to the true Dakota group, but are certainly as old as Lower Cretaceous and are probably of Kootanie age. The genera *Gleichenia, Cladophlebis, Zamites, Athrotaxopsis,* and many others characteristic of the Kootanie, the Trinity and the Potomac formations are represented, while no dicotyledonous leaves occur. Upon the whole they may be considered as a complete confirmation of the conclusion previously reached that the Dakota group of Newton must be subdivided and that a large portion of it belongs to the Lower Cretaceous. Professor Jenney is able to separate it into five distinct horizons, only the uppermost of which belongs to the Dakota of Meek and Hayden, between which and the underlying beds he finds an unconformity.

Mr. Lester F. Ward delivered two lectures on Jan. 8 and 10 before the Peabody Institute of Baltimore, on the *Vegetation of the Ancient World,* illustrated by over fifty lantern views. These were arranged in such a manner as to pass in review in their ascending geological order all the fossil floras known from the Silurian to the Pleistocene. The greater part of the illustrations were drawn from American material, and all the great plant bearing horizons of North America were represented by groups of typical and characteristic forms. Special attention was given to the wonderful fossil forests of this country, and especially of the National Yellowstone Park. The fossil flora of the Potomac formation, and particularly that of the State of Maryland and the City of Baltimore, were duly emphasized. Interspersed with these more scientific illustrations there were thrown on the screen a number of the magnificient ideal landscapes conceived and executed by the great scientific artists, Unger, Heer, Saporta and Dawson. The lectures were well adapted to give to the general public a systematic and comprehensive view of the forms of plant life that have inhabited the earth and especially those that have flourished in America throughout the past ages of geological time.

A TOPOGRAPHICAL ATLAS.

The Director of the United States Geological Survey has recently submitted to the Secretary of the Interior an amendment to the 'Sundry Civil Bill,' now before Congress, authorizing the printing and distribution of an atlas of ten topographical map-sheets to the schools, academies and colleges of the country, the proposed atlas to contain illustrations of the various types of topographical form observed in the country, and to be accompanied by an explanatory bulletin which will serve as a primer of topography for school use.

If the amendment is carried, and the atlas meets the approval of teachers, it is proposed to distribute additional series in later years. Those who are interested in the advance of geography in the schools cannot do better than promptly to address their Congressman, asking for support of this excellent proposition. It is in effect an economical measure, for it will at a moderate cost give a wide and novel use to a large amount of material that has been gathered at great expense, and that is now stored in the office of the Geological Survey, awaiting a limited distribution some years hence.

BIBLIOGRAPHY OF AMERICAN BOTANY.

THE Bibliography Committee of American botanists has just completed its first year of organized work in the production of an author catalogue of papers relating to American Botany. This has been printed in the monthly issues of the *Bulletin of the Torrey Botanical Club* and then reprinted on library cards by the Cambridge Botanical Supply Co. The editors have endeavored to make the record as complete as possible and it includes 575 titles. The committee and the editors earnestly request that their attention be called to omissions and that all interested aid in insuring completeness.

Foreign botanists are particularly requested to call our attention to any of their writings which refer to American plants. Communications may be addressed to the Editor of the Torrey Botanical Club, Columbia College, New York City.

GENERAL.

ON January 10th, Dr. George M. Dawson, C. M. G., F. R. S., was appointed Director of the Geological Survey of Canada, succeeding Dr. Selwyn, retired.

THE next annual meeting of the British Association for the Advancement of Science will be held at Ipswich, commencing on Wednesday, September 11th. Sir Douglas Galton is President-elect.

ACCORDING to the daily papers a party composed of Prof. Charles E. Hite, Alfred C. Harrison, Jr., Henry C. Walsh and Dr. J. Donnell McDonald sailed on Wednesday to Central America with a view to obtaining natural history and archæological collections. The expedition is under the auspices of the biological department of the University of Pennsylvania.

SCIENTIFIC JOURNALS.

AMERICAN CHEMICAL JOURNAL, JAN.

Contributions from the Laboratory of General Chemistry, University of Michigan:—(1) *On the Action of Chlorcarbonic Ester on Sodium Acetone*: By PAUL C. FREER. (2) *The Action of Metals on Nitric Acid:* By GEORGE O. HIGLEY. (3) *An Introductory Study of the Influence of the Substitution of Halogens in Acids, upon the Rate and Limit of Esterification:* By D. M. LICHTY. (4) *On the Action of Sodium on the Esters of Aconitic and Citric Acids.* Preliminary Notice, by PAUL C. FREER.

The Combination of Sulphur with Iodine: By C. E. LINEBARGER.

Contributions from the Chemical Laboratories of the Massachusetts Institute of Technology:—*An Investigation of the Twitchell Method for the Determination of Rosin in Soap:* By THOMAS EVANS and I. E. BEACH.

A Laboratory Method for the Preparation of Potassium Fericyanide: By M. S. WALKER.

Reviews.

THE PHYSICAL REVIEWS, JAN.–FEB.

The Apparent Forces between Fine Solid Particles Totally Immersed in Liquids—I: W. J. A. BLISS.

The Distribution of Energy in the Spectrum of the Glow-lamp: EDWARD L. NICHOLS.

The Influence of Heat and the Electric Current upon Young's Modulus for a Piano Wire: MARY C. NOYES.

Minor Contributions: (1) *On Magnetic Potential:* FREDERICK BEDELL. (2) *A Method for the Study of Transmission Spectra in the Ultra-violet:* ERNEST NICHOLS. (3) *The Photography of Manometric Flames:* WILLIAM HALLOCK.

THE AMERICAN NATURALIST, JAN.

Birds of New Guinea: GEORGE S. MEAD.

Leuciscus Balteatus (Richardson), A Study in Variation: CARL H. EIGENMANN.

On the Evolution of the Art of Working in Stone: J. D. McGuire.

Recent Books and Pamphlets; Recent Literature. General Notes: Mineralogy. Petrography. Geography and Travels. Botany. Zoölogy. Embryology. Entomology. Psychology. Archæology and Ethnology. Microscopy.

Scientific News.

THE BOTANICAL GAZETTE, JAN.

Undescribed Plants from Guatemala and other Central American Republics, XIV. (With plates I–III.) John Donnell Smith.

Notes from my Herbarium: Walter Deane.

The crystallization of cellulose. Duncan S. Johnson.

Noteworthy anatomical and physiological researches.

Briefer Articles; Editorial; Current Literature; Open Letters; Notes and News.

THE AMERICAN ANTHROPOLOGIST, JAN.

Stone Art in America: By J. W. Powell.

The Huacos of Chira Valley, Peru: By Samuel Mathewson Scott.

Caste in India: By J. H. Porter.

Micmac Customs and Traditions: By Stansbury Hager.

The Writings of Padre Andres de Olmos in the Languages of Mexico: By James C. Pilling.

Chinese Origin of Playing Cards: By W. H. Wilkinson.

Col. Garrick Mallery, U. S. A.; an Obituary: By Robert Fletcher.

Book Notices; Notes and News; Bibliography of Anthropologic Literature.

NEW BOOKS.

A Text-book of Organic Chemistry. A. Bernthsen. Translated by George McGowan. London, Blackie & Sons; New York, D. Van Nostrand. 1894. Pp.x ix+596. $2.50.

A Text-book of Mechanics and Hydrostatics. Herbert Hancock. New York, D. Van Nostrand. 1894. Pp. v + 408. $1.75.

A Treatise of Industrial Photometry with Special Application to Electric Lighting. A. Palaz. Translated from the French by George W. Patterson, Jr., and Merib Rowley Patterson. New York, D. Van Nostrand; London, Sampson Low, Marston & Co. Limited. 1894. Pp. vii + 322. $4.00.

Proceedings of the International Electrical Congress held in the City of Chicago, August 21st to 25th, 1893. New York, American Institute of Electrical Engineers. 1894. Pp. xxiv + 487.

The Life and Writings of Rafinesque. Richard Ellsworth Call. Louisville, Ky., Filson Club Publications, X. Quarto, pp. xii + 227.

History of Higher Education in Rhode Island. William Stowe Tolman. Washington, Government Printing Office. 1894. Pp. 210.

The Birds of Eastern Pennsylvania and New Jersey. Witmer Stone. Philadelphia, Delaware Valley Ornithological Club. 1894. Pp. vi + 185.

An Illustrated Dictionary of Medicine, Biology and Allied Sciences. George M. Gould. Philadelphia, P. Blackiston & Sons. 1894. xv + 1633.

Municipal Government in Great Britain. Albert Shaw. New York, The Century Co. Pp. 385. $2.

Eine Discussion der Kräfte der Chemischen Dynamik. Ludwig Stettenheimer. Frankfort, H. Bechhold. 1895. Pp. 85. M. 6.

On the Origin of Language and The Logos Theory. Ludwig Noiré. Chicago, Open Court Publishing Co. 1895. Pp. 57. 15 cents.

Geological Survey of Alabama. Eugene Allen Smith. Montgomery, Alabama, The Brown Printing Co. 1894. xxiv + 759; also Geological Map of Alabama.

Freytag's Technique of the Drama. Translated by Elias J. MacEwan. Chicago, S. C. Griggs & Co. 1895. Pp. ix + 366.

Social Growth and Stability. D. Ostrander. Chicago, S. C. Griggs & Co. 1895. Pp. 191. $1.

SCIENCE.

NEW SERIES. VOL. I. NO. 6. FRIDAY, FEBRUARY 8, 1895. SINGLE COPIES, 15 CTS. ANNUAL SUBSCRIPTION, $5.00.

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison on Hudson, N. Y.
Subscriptions (five dollars annually) and advertisements should be sent to the Publisher of SCIENCE, 41 East 49th St., New York, or Lancaster, Pa.

AN HISTORICAL SURVEY OF THE SCIENCE OF MECHANICS.*

Our age is at once the age of excessive specialization and the age of excessive popularization of science. Every smallest field of scientific activity has its gleaners and classifiers and builders of technical terminology. The workers in each field proceed, as a rule, without much regard to the interests and objects of the workers in adjoining fields, and it may easily happen that the precise and lucid, if not romantic, literature current in one field will be well-nigh unintelligible in another. So far, indeed, has this specialization gone that the various classes of specialists have but little common ground on which to meet, and it is sometimes difficult, if not impossible, for them to dwell together in peace and harmony. In a general scientific assembly, for example, the naturalists feel great uneasiness in listening to a paper from a mathematician or physicist, while the latter are almost certain to seek relief in the open air from the depression induced in them by the wealth of terminology essential to the description of a new species. The general public, on the other hand, busy though it be with multifarious affairs, is quick to appreciate the results of science and eager to know how they have been attained. To meet this legitimate demand for information, scientific and pseudo-scientific men have given us a flood of popular literature explaining almost every discovery, principle, theory, and speculation known to scientific thought. Nay more, and worse, this popularization has gone so far that many have come to think that the royal road to learning has been found; that it is only necessary, in fact, to acquire a little of the technical terminology, to read a few books, and to witness a few pyrotechnic experiments to come into possession of

*Address delivered by Professor R. S. Woodward, at a meeting of the New York Academy of Sciences, November 26, 1894.

sound knowledge. Thus we hear of university courses in science carried on by correspondence and completed in a few weeks or a few months. The professional popularizer has been developed. He expounds science from the platform and through the the press; and there is no subject so abstruse as to deter him from producing a treatise on it in sixty days. Verily, it may be said, whosoever hungers for the bread of science may find an abundance ready made; but out of this abundance few are able to select the real staff of scientific life.

As a worker in one of the narrow fields of scientific thought, I find myself in difficulties to-night in seeking to say something which may be at the same time interesting and instructive concerning a science which is more than twenty centuries old, but which has rarely if ever attracted much popular attention. How to steer clear of the rocks of obtrusive technicality, on the one hand, and of the shoals of popularization on the other, is, you will no doubt agree with me, a rather appalling task. Moreover, there are special reasons why you might expect the science of mechanics to be the driest if not the dullest of subjects for a popular discourse. One reason lies in the fact that those who, from accident or force of circumstances, find themselves obliged to pursue the study of mechanics seriously for a few months in college, are wont to celebrate the completion of such study by making this science the subject of mock funeral rites or by relegating it to the bonfires of oblivion. Another reason finds expression in a very common notion, even among highly educated people, that the mathematico-physical sciences are like so many highly perfected mills whose remorseless and monotonous grinding soon converts their operators into mere automatons destitute of every human sentiment and deaf to every human song. In explanation of this notion, at a convention of professional educators held in this city about a year ago, a distinguished college president said with appropriate solemnity:—"The line AB cuts the line CD at right angles. Who ever shed tears over such a proposition as that?" he went on; and after the applause which followed had subsided he added, "and who ever laughed at such a proposition before?"

Notwithstanding these unfavorable auspices and the profound embarrassment they entail, I have ventured to invite your attention for the hour to some of the salient features of mechanical science, and to the element of human nature which is indissolubly connected with this as with every department of orderly knowledge; believing that neither the cold facts of the science nor the hard reasoning of its expounders can be devoid of interest when recounted in our vernacular.

In our search for the beginnings of a science we look always for the person who first formulated one or more of its principles in a way intelligible to his fellow men. The law of progress admonishes us that such a person is not necessarily or generally the sole discoverer, for ideas grow by slow accretions and become susceptible of clear statement only after being entertained in many minds. But of the many who think of the laws of nature few reach the high plane of generalization, and it thus happens that the duly accredited originators of any science are usually small in number and scattered through a long lapse of time. The name which deserves first mention in the history of mechanics is that of Archimedes. He was not only the founder of the science of mechanics, but he was also the first theoretical engineer. Indeed, he may be said to have laid the foundation for mechanics and engineering so securely with the cement of sound mathematics that its stability has sufficed for the weighty superstructure reared during the succeeding twenty centuries. He knew how to weigh and to measure and how to work out the numerical relations of things; and it is a singular fact, that in an age when fancy ran riot and when men were able to put together fine phrases without troubling themselves much with the ideas which ought to accompany their words, that Archimedes should have concentrated his attention on such unpoetic things as the principle of the handspike and the crowbar, and the laws of hydrostatics. His appreciation of the doctrine of the handspike and crowbar, or of the lever as it is technically called, was worthy of its far-reaching consequences; and the saying attributed to him—"Give me a fulcrum on which to rest and I will move the earth"—is a favorite though commonly ill-understood popular expression of his most important contribution to mechanical science.

For whatever purpose we read history, we are continually reminded that the absorbing occupation of humanity has been fighting one another. The thirst for blood and butchery has always been, and we fear still is, greater than the thirst for knowledge. Thus it was in the days of Archimedes; and although devoted to those abstract studies which engender no malice toward men, he served his king and country by building engines of destruction, and perished finally at the hands of a Roman soldier in the massacre which followed the fall of Syracuse.

The slowness of the growth of ideas and the blight upon scientific thought which followed the decay of the Grecian and Roman civilizations are forcibly brought to mind by the fact that scarcely an increment to mechanical science was attained during the eighteen hundred years which elapsed between the epoch of Archimedes and the epoch of Galileo. But, as if in compensation for this long period of darkness, the torch of science relighted by Galileo has burned on with increasing intensity until now its radiance illumes almost every thought and action of our daily life. The fame of Galileo in the popular mind rests chiefly on his invention of the telescope and on his battle with the Church in the field of astronomy. But he was able to see things at short as well as at long range; and his observations on the vibrating chandelier in the cathedral at Pisa and on the laws of falling bodies must be rated as of much higher value than his discovery of the satellites of Jupiter. The peculiar merit of those observations lay in the fact that they led him to correct notions of the properties of moving masses, and of the behavior of matter under the action of force. Archimedes had dealt with matter in a state of relative rest, or with statics only. Galileo rose to the higher concept of matter in motion, and founded that branch of mechanics now known as dynamics.

It seems strange at first thought when we look back through the light of modern analysis on these advances that they should have been so slowly achieved and still more slowly accepted and utilized. We must remember, however, that the elaboration of the principles which Galileo added to our science involved the removal of much scholastic rubbish. It was essential first of all to establish the validity of precise and correct observation. He had to recognize that in studying the laws of falling bodies the most important question was not *why* they fall but *how* they fall. In doing this he set an example which has ever since been followed with success in the investigation of the phenomena of nature. Considering the times in which he lived, the amount of work he accomplished is little short of prodigious. For besides his capital contributions to mechanics and astronomy, he was the founder of our modern engineering science of the strength and resistance of materials, a science which has recently grown into a great department of mechanics under the title of the mathematical theory of elasticity. Thus, like Archimedes, he added to the practical side of science; indeed, a rude woodcut in one of his discourses, showing a beam built into a stone wall and loaded with a weight at the free end, proves that he had no scorn for common things and gives the key to a long line of subsequent researches. He was also the inventor of the thermometer, the hydrostatic balance, and the proportional dividers, all of which instruments are still in use; and for the edification of those who think the pursuit of his favorite studies leaves no room for the play of the fancy, it should be mentioned that he found time to give popular lectures on the site and dimensions of Dante's *Inferno.*

Although it is an axiom of modern philosophy that coincidence of events is no adequate evidence of their connection, yet there seems to be an innate tendency of the mind to anticipate a relation between nearly simultaneous occurrences and to attach much importance to them when they are historically allied. It is one of the curious coincidences in the history of the founders of mechanics that the year of Galileo's death is also the year of Newton's birth. Thus it might seem that Nature took care that Galileo should have a fitting successor.

During the interval of nearly a hundred years which elapsed between the epoch of Galileo and the period of Newton's activity, not a few philosophers added to the growth of mechanical science. Most conspicuous among these was Huyghens, who distinguished himself as a mathematician, astronomer, mechanician, and physicist. Of his varied and valuable contributions to these departments of knowledge, what would strike the general reader as least worthy of attention was really of the highest importance. Nothing is commoner now than the pendulum clock. The town clock and Grandfather's clock are so proverbial that few would suppose that a grand treatise could ever have been written about such a commonplace mechanism. But true it is that Huyghens, taking up Galileo's discovery of the near isochronism of the swinging chandelier, not only produced a working pendulum clock, but also a great theory of it. The introduction of this instrument for the exact measurement of time made the subsequent progress of astronomy possible, while his theory of the oscillating pendulum has been justly called the true prelude to Newton's *Principia.* The laws of vibration indeed play a wonderfully important rôle in the science of mechanics, and it may be said that he who understands the doctrine of the pendulum in all its phases has in his possession the key to the secrets of nearly every mechanical system from the common clock to the steam engine, and from the steam engine to the solar system. Well may we retain the euphonious title of *Horologium Oscillatorium* for this important memoir of Huyghens.

Chaucer—

> "Dan Chaucer, the first warbler, whose sweet breath
> Preluded those melodious bursts that fill
> The spacious times of great Elizabeth
> With sounds that echo still,"

has been called the Father of English literature. In a broader sense, because not limited by language, we may regard Newton as the Father of Natural Philosophy.

It was the happy lot of Newton to attain these brilliant achievements. First and greatest of these was the well nigh perfect statement of the laws of dynamics; the second was the discovery of the law of gravitation; and the third was the invention of a calculus required to develop the consequences of the other two. As we have seen, however, the laws of matter and motion were not unknown to the predecessors and contemporaries of Newton. Galileo, in fact, discovered the first two, and the third in one form or another was known to Hooke, Huyghens and others; but it was the peculiar work of Newton to state these laws so clearly and fully that the lapse of two centuries has suggested little, if any, improvement.

What, then, are these laws, you may enquire? Let me turn them into the vernacular. The first two assert that matter never starts off on a journey without solicitation; having once started it never changes its speed or direction unless forced to do so; when put to this extremity, it shows perfect impartiality to every deflecting force; and finally, it never stops unless arrested. Add to these the obvious fact that action and reaction are equal and opposite, and we have a body of doctrine which, simple as it may seem, appears to be coextensive with the material universe. It must be admitted, of course, that a mere comprehension of these laws does not suffice to make a mechanician. Between these stepping stones and the table-land from which Newton looked out on the order of nature there is a long and steep ascent; but whoso would scale the heights must go by way of these stepping stones.

The law of gravitation, though commonly considered the greatest of Newton's achievements, is, in reality, far less worthy of distinction than his foundation for mechanics. Its chief merit lay in the clear perception of the application of the law to the smallest particles of matter, for the mere notion of gravitation between finite masses was familiar to his contemporaries; in fact, according to Newton's own statement, the law of inverse squares as applicable to such masses was within the reach of any mathematician some years before the publication of the *Principia*.

A matter of the greatest importance in the history of Newton's work relates not so much to the substance as to the form of it. It is now known that the grand results brought out in his *Principia* were reached chiefly by means of his calculus, or fluxions, as he called it, a contribution to science hardly less important than either of his others. But the fashion of his day did not favor reasoning by means of infinitesimals, those mysterious increments and decrements which the learned and eloquent Bishop Berkeley a half century later called 'the ghosts of departed quantities.' The fashion, or rather prejudice, of Newton's day was strongly in favor of geometrical reasoning; and it would seem that he felt constrained to translate the results to which his calculus led him into geometrical language. It was desirable, he thought, that the system of the heavens should be founded on good geometry. Subsequent history shows that this course was an ill-judged one. The geometrical method of the *Principia* renders it cumbersome, prolix, and on the whole rather repulsive to the modern reader; and the only justification which appears at all adequate for the exclusive adoption of this method, lies in the fact that his fellow countrymen would not have readily appreciated the more elegant and vastly more comprehensive analytical method. The result was very unfavorable to the growth of mechanical science in his own country. The seed he sowed took root on the continent and has ever since grown best in French and German soil. According to Prof. Glaisher, in an address delivered by him at the celebration of the 200th anniversary of the publication of Newton's great work, "the geometrical form of the *Principia* exercised a disastrous influence over mathematical studies at Cambridge University for nearly a century and a half, by giving rise to a mistaken idea of the relative power of analytical and geometrical processes."

Readers of English mathematical text books and treatises can hardly fail to notice that the bias they show for geometrical methods, and especially for the formal, Euclidean mode of presentation, in which the procession of ideas too frequently consists of formidable groups of painfully accurate and technical paragraphs labelled Proposition, Corollary and Scholium. This formalism leads to a strained and unattractive literary style, which frequently degenerates into intolerable complexity and obscurity. It is against this sort of 'logic-chopping' that most minds rebel, against this excessive attention to the husks rather than to the kernel of the subject. Another and equally serious result of the apotheosis of pure geometry is the tendency to magnify the importance of ideal problems and the work of problem solving. The exclusive pursuit of such aimless puzzles constitutes the platitude of mathematical research, though it often happens that the devotees to this species of work are mistaken for mathematicians and natural philosophers.

It is not specially difficult in our day to understand how a mind of Newton's capacity should achieve so many important results. The simple fact is that he possessed just such powers of observation and reflection as were needed to correlate the facts his predecessors and contemporaries had collected; and the most instructive lesson of his life to us is the success which attended the industrious application of those powers. But, on the other hand, it cannot be said that the circumstances of his life were very propitious for his work, or that he availed himself to the fullest extent of his opportunities. His favorite studies were, in fact, pursued somewhat fitfully, and not always with a just appreciation of their merits. Possessing to a painful degree that modesty which is born of a knowledge of things, he shrunk from the controversy into which his discoveries drew him; and it appears probable that his *Principia* would never have been written had not his friend Halley urged him on to the marvelous feat which brought out that masterpiece in less than two years' time. The demand for works on natural philosophy in his day and the appreciation of the public for natural philosophers may be inferred from the fact that neither Newton nor the Royal Society of London, to which his great work was dedicated, was able to furnish the funds essential to print an edition of 250 copies. The entire expense of this first edition was born by Halley, who may thus be justly called the discoverer of his more famous fellow-countryman. In such hard times and under such depressing circumstances, it is not strange that Newton should have sought and obtained a position in the public service; though it seems a pity that one of the greatest of philosophers, one who said his head never ached except when studying the mechanics of the motions of the moon, should have busied himself during his declining years with the dreary details of fiscal business as master of the mint.

The period of about a hundred years which followed the epoch of the culmination of Newton's activity is remarkable for the diversity of mechanical problems to which mathematicians devoted their attention. The discoveries of Newton comprised and superseded the discoveries of Copernicus and Kepler. The sun with his planets and the planets with their satellites became grand mechanical systems under the law of gravitation. But a crowd of additional consequences of this law demanded serious study and prolonged observation. Newton had seen that the gravitation and rotation of the earth ought to make it flattened at the poles. To test this question it was essential to devise ways and means for measuring the size and shape of the earth. Out of this necessity grew the science of geodesy. Maupertius and Clairaut had to be sent to Lapland, and Bouguer and La Condamine to Peru to measure arcs of meridian before definite ideas of the figure and dimensions of our planet were attained. The precession of the equinoxes had been discovered by Hipparchus. The law of gravitation supplied a reason for this phenomenon; but to understand it fully the properties of rotating bodies had to be elaborately studied by Euler and d'Alembert. Observational astronomy began far earlier than the era of Hipparchus; but precise observational astronomy was not possible before Huyghens' invention of the pendulum clock and before Newton's law led the way to separating the motions of the earth from those proper to the stars and to light.

The earlier part of the period in question was also characterized by the variety of special processes used in the applications of mechanics. This peculiarity is due partly to the fact that the great method of investigation now known as the differential and integral calculus was not duly understood and appreciated. Newton, as we have seen, devised and used this method under the name of fluxions, but dared not bring it into prominence in his *Principia*. Independently of, though a little later than Newton, Leibnitz discovered substantially the same method. Priority of publication of the method by Leibnitz led to one of the most remarkable and bitter controversies in the history of science; proving amongst other things that scientific men are no better than other folks, and giving color to Benjamin Franklin's allegation that mathematicians are prone to be conscientiously contentious. But this war of words, in which personal and national prejudice figured shamefully enough, did not long disturb the minds of continental mathematicians. The Leibnitzian form of the calculus, by reason of its intrinsic merits, came into general use. The Bernoullis, Euler, Clairaut, and d'Alembert, who were the leading mathematicians of the time, adopted the calculus as their instrument of research and paved the way to the age of extraordinary generalizations which began near the end of the eighteenth century.

The variety of problems considered and the diversity of methods employed during this period served to call attention to the need of more comprehensive mechanical principles. Before the publication of d'Alembert's treatise on dynamics in 1743, each problem had been considered by itself, and although many important results were attained, the principles employed did not appear to have any close connection with one another. There was thus an opportunity for rival schools of mechanicians, and they fell into the habit of challenging one another with what would now be called prize problems. The first step toward a unification of principles and processes was made by d'Alembert in the treatise just mentioned. This treatise announced and illustrated a principle, since known as d'Alembert's principle, which put an end to rivalry by showing how all problems in dynamics can be referred to the laws of statics. By the aid of this principle, d'Alembert showed how to solve mechanically not only the splendid problem of the precession of the equinoxes, but also that more recondite question of the nutation of the earth's axis. The fact of nutation had been discovered a year and a half earlier by the astronomer Bradley; but d'Alembert's explanation of this fact, according to Laplace, is not less remarkable in the history of mechanics than Bradley's discovery in the annals of astronomy.

The work of the devotees to mechanics in the times of which we speak is not generally fully appreciated. Their fame is, indeed, eclipsed by that of Newton and by that of their immediate successors. But their contributions were important and substantial. Clairaut gave us the first mathematical treatise on the figure of the earth; while his colleague, Maupertius, in the famous Lapland expedition, announced the the principle of 'least action' and the 'law of repose,' both of which have proved fruitful in later times. The Bernoullis, a most distinguished family of mathematicians, of whom John the first and his three sons were then active, worked in all fields of mathematical research, and rendered especially good service in extending the theory of elasticity founded by Galileo. The industrious Euler, a pupil of John Bernoulli, and a companion of his sons, enriched analysis in every direction, gave for the first time the correct theory of rotating bodies, and wrote on almost every question in the mathematics, physics, and astronomy of his day. It is estimated that his memoirs if fully printed would fill sixty to eighty quarto volumes. Not the least noteworthy of his works are his Letters to a German Princess, giving a popular account of the principles of mechanics, optics, acoustics, and astronomy.

Notwithstanding the broad foundation for mechanics laid by Newton in his *Principia*, and notwithstanding the indefatigable labors of Clairaut, d'Alembert, the Bernoullis, and Euler, there was near the end of the eighteenth century no comprehensive treatise on the science. Its leading principles and methods were fairly well known, but scattered through many works, and presented from divers points of view. It remained for Lagrange to unite them into one harmonious system. Mechanics had not yet freed itself from the restrictions of geometry, though progress since Newton's time had been constantly toward analytical as distinguished from geometrical methods. The emancipation came with Lagrange's *Mécanique Analytique*, published one hundred and one years after the *Principia*. How completely the geometrical method was supplanted by the analytical, at the hands of Lagrange, may be inferred from a paragraph in the advertisement to his *Mécanique Analytique*. "One will find" he says, "no diagrams in this work. The methods I expose require neither geometrical construction nor geometrical reasoning, but only algebraical operations subjected to a regular and uniform procedure."

From a philosophical and historical point of view this characteristic feature of the *Mécanique Analytique* is of the greatest importance. The mere statement of the fact, however, conveys no adequate idea of the immense value of Lagrange's treatise. The value of his work consists in the exposition of a general method by which every mechanical question may be stated in a single algebraic equation. The entire history of any mechanical system, as for example, the solar system, may thus be condensed into a single sentence; and its detailed interpretation becomes simply a question of algebra. No one who has not tried to cope with the difficulties presented by almost any mechanical problem can form a just appreciation of the great utility of such a labor-saving and thought-saving device. It has been well called 'a stupendous contribution to the economy of thought.' But Lagrange did more than this for the science of mechanics. He not only perfected a unique and comprehensive method, and showed how to apply it to many of the most important and recondite problems of his day, but he was the first to draw sharply the line of demarcation between physics and metaphysics. The mechanical ideas of Descartes, Leibnitz, Maupertius, and even of Euler, had proved to be more or less hazy and unfruitful from a failure to separate those two distinct regions of thought. Lagrange put an end to this confusion, for no serious attempt has since been made to derive the laws of mechanics from a metaphysical basis.

The age which witnessed the culmination of the splendid generalization of Lagrange in his *Mécanique Analytique* was also the age in which Newton's law of gravitation received its verification, and the age in which the foundations of the modern science of mathematical physics were laid. Lagrange himself is closely identified with these two important events in the history of mechanics; but the names which outshine all others are those of Laplace and Poisson.

It was the life-work of Laplace to deduce the consequences of the law of gravitation as applied to the solar system. No problem of equal magnitude has ever been attacked and treated single-handed with such consummate skill and success as shown by Laplace in his *Mécanique Céleste*. The five volumes of this work, together with the popular exposition contained in his *Système du Monde*, constitute, I think, the greatest systematic treatise ever written. Think, for a moment, of the mental equipment essential to begin such an investigation. Copernicus and Kepler had discovered by observation the salient features of the motions of the planets about the sun. Newton showed that these features were immediately and easily derived results of the law of gravitation. But these were the salient features only. Had our planet been the sole one of the system, had it been moonless and devoid of rotation, the task of Laplace would have been easy. But instead of a single planet, there is a crowd of them, each rotating on its axis while traveling about the sun, and most of them accompanied by lunar attendants. When this array of facts is considered, the simple law of gravitation leads to great complication. The motion of our planet at any time depends not only on its position relatively to the sun, but on its position relatively to the neighboring planets. Our moon also plays an important rôle in the motions of the earth. By reason of these interactions the earth's axis of rotation, which is the principle line of reference for astronomical observations, pursues a devious course in the heavens. Add to these difficulties those arising from the facts that our planet is surrounded by an atmosphere which prevents us from observing our true relative position, and that light travels with a finite though great speed, and the magnitude of the task Laplace set for himself is in some degree apparent. A complete mastery of every branch of the mathematics and physics of his day and a capacity to enlarge the boundaries of either were the indispensable prerequisites, which, supplemented by a boundless genius for industry, enabled him to make dynamical astronomy the most perfect of the applied sciences. His conception of the magnitude and importance of the work he undertook is clearly but modestly set forth in the preface to the *Mécanique Céleste*. "Astronomy," he says, "considered in the most general manner is a grand problem of mechanics, whose solution depends on the precision of observations and on the perfection of mathematical analysis. It is extremely desirable to avoid all empiricism in our treatment of this problem and to draw on observation for indispensable data only. The present work is destined to accomplish, as far as I am able, this interesting object. I trust that, in consideration of the difficulties of the subject, mathematicians and astronomers will receive the work with indulgence."

Not less important than the contributions of Lagrange and Laplace to pure mechanics and dynamical astronomy were the voluminous and luminous writings of Poisson during the same period. Equally at home with Lagrange and Laplace in their favorite researches, many of which he corrected and extended, he explored the additional fields of heat, light, elasticity, electricity, and magnetism. To his penetrating insight into these abstruse subjects and to the wealth of analytical resources he developed are due more than to any other single source the subsequent developments of mathematical physics, by which is meant the application of mechanics to physical questions. His discoveries and researches are scarcely less brilliant than those of his two eminent contemporaries, while he outstripped both of them in his range and grasp of mathematical and physical principles. Moreover, he was the prince of expositors of mathematical subjects. His memoirs (of which there are more than 150) must even now be classed amongst the best models of scientific exposition.

It is a striking series of facts that the three most eminent workers in our science during the period in question, a period extending, say, from 1775 to 1825, were all Frenchmen, that they were warm personal friends, and that they all resided, in their later years at least, at Paris. Still more striking is the fact that this period of extraordinary development in mechanical science was coincident with a period of most profound social agitation with Frenchmen in general and with Parisians in particular. How was it possible to pursue abstract theories of matter and motion, how was it possible to contemplate the grandeur of the celestial universe at a time when the heads of statesmen and philosophers were falling into the waste basket, not before the metaphorical axe of changing ministers, but before the whetted blade of the guillotine? Tocqueville, in his Democracy in America, has warned us against the depressing effect on abstract thought of the incessant attrition of American life. Why did not the stormy times of the French Revolution check the current of scientific progress? The answer to these questions is to be found, I think, in the fortunate circumstance that Frenchmen and the French government, whatever may have been their shortcomings in other respects, have developed a higher appreciation for science and scientific men than any other nationality. However they may have fallen out as a people on questions of religion and politics, they have maintained a high regard for scientific thought. It was his admirable devotion to celestial mechanics that saved Laplace from disgrace, or a worse fate, at the hands of his fellow-countrymen. Even the sorry figure he cut during his brief career as Minister of the Interior, into the business of which he introduced the 'spirit of the infinitesimals,' as the future emperor said, did not deprive him of favors due to a man of science.

The personal characteristics and the intimate friendship and association of Lagrange, Laplace, and Poisson are amongst the most attractive features of their lives, and worthy of a brief digression.

Lagrange was of French descent, though he was born at Turin and became famous before taking up a residence at the focus of French civilization. While yet a youth, the ample means of his family were lost in commercial speculation; and to this early lesson of adversity is due, probably, the determination of his career, for he was wont to say that had he been rich he might never have pursued mathematical studies. Like most mathematicians of distinction, he seems to have owed much less to scholastic instruction than to his own efforts and industry. At the age of eighteen he was appointed professor of mathematics at the royal school of artillery at Turin; and at nineteen he was in correspondence with Euler concerning isoperimetrical problems, which ultimately led to his perfection of that highest branch of pure mathematics, the caculus of variations. At twenty-two he was one of the founders of a society which afterwards became famous as the Turin Academy of Sciences. At the early age of thirty he was called to the post of director of the mathematical department of the Berlin Academy of Sciences as the successor of the distinguished Euler. Here he remained for twenty years' working with marvelous industry and success. About the time of the appearance of his great work on analytical mechanics in 1788, he removed to Paris at the instance of the French court, which made him a 'veteran pensioner' and received him with the most flattering honors. He lived through the stormy period of the Revolution, winning additional favors and distinctions from the French government, and closing his remarkable career at the ripe age of seventy-seven.

Little seems to be known of the ancestry and early life of Laplace. It appears, however, that he was the son of a farmer and that he had achieved some local distinction as a teacher of mathematics at the age of eighteen, when he went up to Paris with such letters of recommendation as he could get, and applied for a position in the government schools. He appealed to d'Alembert, who was then the leading mathematician at the French capital, but d'Alembert, it is said, gave no heed to either the application or the recommendations of the aspirant for office. Thereupon the unknown Laplace wrote the great geometer a letter on the principles of mechanics which brought an immediate reply. "You needed no introduction or recommendation," said d'Alembert, "you have recommended yourself; my support is your due." Through the influence of d'Alembert, Laplace was soon given a professorship of mathematics in the military school of Paris, and his scientific career was thus begun. He was not yet twenty-five years of age when he made one of the most important advances in the history of dynamical astronomy toward the solution of the grand problem of the stability of the solar system. By this step he became at once the peer of his older and eminent contemporaries, Euler, d'Alembert, and Lagrange. From this time on until his death in 1827, his indefatigable labors and penetrating insight brought to light a continuous series of brilliant discoveries. The history of dynamical astronomy, indeed, for the half century ending with 1825, is essentially the history of the work of Laplace as recorded in his *Mécanique Céleste*. A persistent and lofty enthusiasm for the system of the world is displayed in all his works; his latest writings even being no less inspiring than his earliest. His zeal recognized no bounds. "He would have completed the science of the skies," says Fourier, "had that science been capable of completion." He died at the age of seventy-eight, and his last words were worthy of the philosopher he was. "What we know is very little; what we are ignorant of is immense."

Poisson, the youngest of this famous trio, was forty-five years younger than Lagrange and thirty-two years younger than Laplace. He was born of humble parentage at Pithiviers, in 1781, his father at that time being a petty government official. While yet an infant, Poisson was confided to the care of a neighboring peasant-woman, at whose hands he received rather startling treatment for one who was destined to become famous in the annals of science. Poisson relates that his father came one day to see how his son was getting on, and was horrified to find that the peasant-nurse had gone to the fields, leaving the child suspended from the ceiling by a small cord at a height just sufficient to secure immunity from the teeth of the swine which, it seems, had free access to the house. In relating this novel incident in his early life, Poisson used to say that "a gymnastic effort carried me incessantly from one side of the vertical to the other; and it was thus, in my tenderest infancy, that I made my prelude to those studies of the pendulum that were to occupy me so much in my mature age."

As the youth grew up, receiving the bare elements of education from his father, the question was raised in his family as to what calling he should follow. It was suggested that he should become a notary, but the better judgment of the family councils decided that the business of a notary required too much intellectual capacity for the young man, and it was therefore determined to make a surgeon of him. He was apprenticed to an uncle who practiced the art of blood-letting and blistering of that day, and who set the beginner at work pricking cabbage leaves with a lancet. How he got on at surgery, Poisson himself relates best: "One day my uncle sent me," he says, "to put a blister on the arm of a sick child; the next day when I presented myself to remove the apparatus, I found the child dead. This event, very common, they say, made a profound impression upon me; and I declared at once that I would never become a physician."

He returned to his home, where, soon afterwards, an accidental circumstance revealed the true bent of his mind. His father, being still a government officer, received a copy of the *Journal de l'École Polytechnique.* The son read it, and was able, unaided, to understand some of its contents. He was encouraged to study and soon went to the school of Fontainebleau. Here he was fortunate in finding a good and sympathetic teacher in one M. Billy, who took a warm interest in, and formed a life-long attachment for, his pupil.

At the age of seventeen Poisson went to Paris to enter the *École Polytechnique.* His genius soon disclosed itself, and at the end of his first year he was excused from the requirements of the set curriculum and allowed freedom of choice in his studies. Before he had been at the school two years, or before he was twenty years of age, he published two memoirs which attracted the attention of mathematicians, and led to his speedy entrance into Parisian scientific society, whose leaders at that time were Lagrange and Laplace. They were quick to recognize and appreciate Poisson's ability, and it was doubtless through their good offices that Poisson was appointed to a professorship at the *École Polytechnique*, where he succeeded the distinguished Fourier in 1806. From this time to the end of his life in 1840, Poisson was connected with the educational system of France. As a scientific investigator his untiring patience, industry, and success have been equalled only by those of Euler, Lagrange, and Laplace. "Life," he was wont to say, "is good for two purposes only: to invent mathematics and to expound them."

One of the best estimates of the character and scope of Poisson's work may be inferred from the esteem in which he was held by Lagrange and Laplace. They treated him with the greatest consideration; and that Lagrange considered him a worthy successor in the footsteps of the most eminent of mechanicians is shown by the following incident related by Arago: "I am old," said Lagrange to Poisson one day; "during my long intervals of sleeplessness I divert myself by making numerical calculations. Keep this one; it may interest you. Huyghens was 13 years older than Newton; I am 13 years older than Laplace; d'Alembert was 32 years older than Laplace; Laplace is 32 years older than you." Arago remarks that no more delicate way could be conceived of intimating to Poisson his admission to the inner circle of the fraternity of mathematical genius.

The dazzling spendor of the achievements in dynamical astronomy during the epoch of Laplace not only diverted attention from other applications of mechanical science, but it would seem also to have led to an underestimate of the importance of such applications. Thus the work of Fourier and Poisson in the theory of heat, and that of Fresnel and Green in the theory of light, were not duly appreciated by contemporary philosophers. All eyes were turned towards the heavens. The permanence of the solar system and the dangers of encounters with comets were more important questions than those presented by phenomena close at hand. For nearly a quarter of a century after the epoch of Laplace, comparatively little progress was made in the fundamental ideas of our science, though its machinery received many important accessions, especially from Green and Gauss.

About 1850, however, the accumulating data of experimental philosophers and the reflections of a number of theorists led to the announcement of the principle of the conservation of energy, a doctine which is now held to be the highest generalization of mechanical science. This doctrine asserts that the total energy of any mechanical system is a quantity which can neither be increased nor dimished by any mutual action of the parts of the system, though it may be converted into any one of the forms of which energy is susceptible. Thus, the solar system, supposing it to be isolated from all other systems of the universe, contains a definite amount of energy, and whatever may have been or may be the vicissitudes of the sun and planets, that quantity of energy was and will be the same.

But what, in common parlance, some one may properly enquire, is energy in a mechanical sense? The answer to this question is not difficult. If we raise a weight, as, for example, an elevator car above the surface of the earth, work must be done. On the other hand, if it be elevated and its cable be cut, the car will fall back to the earth and do work of destruction in its fall. The work stored up in raising the car to a given height is called energy of position, or potential energy. The work the car can do by reason of its fall is called energy of motion, or kinetic energy. If a strict account of the expenditure is kept in this case, it is found that the sum of the energies of position and motion at any instant is constant. Similarly, it was found by Count Rumford and Joule that in boring cannon and in agitating liquids heat is produced, and that if in these cases accurate record is kept, the amount of heat developed bears a definite ratio to the amount of energy expended. Thus heat is brought into the category of energy, hot bodies being such, as we now think, by reason of the more or less furious agitation, or kinetic energy, of their ultimate particles.

The law of the conservation of energy, then, is a simple statement of Nature's balance-sheet with respect to material systems. The capital invested remains always the same, however diversified may be the investments. A part may be entered as potential energy; a part as kinetic energy; a part as heat; etc., but when properly added together, their sum is constant. Broadly speaking, it is believed that the various forms of energy may be comprised in two categories: the energy of position, or potential energy, and the energy of motion, or kinetic energy.

It is interesting to note in connection with the history of this doctrine that the ideas which led up to it go back certainly to the time of Newton and Leibnitz. The conservation of matter is, indeed, a fundamental concept of mechanics; but the earlier philosophers, from Newton and Leibnitz down, were acquainted with the conservation of momentum and energy in a variety of special cases. And it is probable that our modern science owes something to the metaphysical notions of Descartes, Maupertius and others, who held that Nature performs her operations in the most economical ways and is, on the whole, conservative.

It appears not a little remarkable that this important doctrine eluded the insight of Lagrange and Laplace. Lagrange, especially, was so near to it that he supplied nearly all the analytical machinery essential to put it into practical use. Indeed, that machinery meets a much higher demand. It not only enables us to express and interpret the properties of systems which are obviously mechanical, but it shows clearly what must be the characteristic features of a mechanical explanation of any phenomenon. Thus, in the direct application of the doctrine of energy to a mechanical system, we express the kinetic energy in terms of the masses involved, their coördinates of position, and the time from any assumed epoch; while the potential energy is expressed in terms of the masses and their relative positions, irrespectively of the time. From the expressions for these two parts of the energy, all of the properties of the system can be derived by means of the Lagrangian machinery. In the case of most phenomena it is impossible to observe more than a very limited number of the circumstances of motion; such as, for example, the coördinates of one or more of the masses at definite epochs, the rates of variation of those coördinates, etc.; but if we can express the two parts of the energy, and if the derived circumstances agree with the observed circumstances, the mechanical explanation is regarded as complete. On the other hand, a phenomenon may not be clearly or obviously mechanical, and it becomes important in many cases to learn whether it is susceptible of mechanical explanation. The criterion supplied by the Lagrangian machinery is this: If the phenomenon can be defined by two expressions or functions having the properties of kinetic and potential energy, a system of masses with appropriate positions may be found to satisfy those functions and hence explain the phenomenon mechanically.

The law of the conservation of energy, then, affords a very comprehensive view of mechanical phenomena; and when we add that this law is believed to be coextensive with the material universe, one can see why it should have played so important a rôle in the recent developments of mechanical science. Along with the growth and applition of this law has come a degree of perfection in the technical terminology of mechanics surpassing that of most other sciences. The terms mass, force, energy, power, etc., as now used in mechanics, possess a precision of meaning which, strange as it may seem, was largely wanting in them, thirty to fifty years ago. Nothing illustrates this fact more forcibly than titles to some of the important papers published during the past half century. Thus, the great memoir published in 1847 by Helmholtz on what we now call the conservation of energy was entitled 'The Conservation of Force.' In 1854 Prof. Thomson, now Lord Kelvin, published an interesting and important 'Note on the possible density of the luminiferous medium, and on the mechanical value of a cubic mile of sunlight.' We should now render the 'mechanical value of a cubic mile of sunlight' as meaning the energy of a cubic mile of the ether due to the action of the sun. About thirty years ago the late Professor Tyndall published his capital work on 'Heat Considered as a Mode of Motion.' We must now translate this into Heat a Mode of Energy. There was, thus, in the writings of experts of a half century or less ago, much obscure phraseology, while the literature of less careful authors was often provokingly ambiguous. The word force, for example, in a number of treatises published since 1850, has been used to denote the three radically different things we now call stress, impulse and energy.

To the development of the law of energy, and its applications in electricity and magnetism especially, are due also an important fixation of our ideas with respect to the units and the dimensions of units which enter into mechanical quantities. Less than a quarter of a century ago our science was in a certain sense restricted by its terrestrial moorings. So strong, indeed, had been the influence of our earthly abode that only experts like Lagrange, Laplace, and Poisson would have known how to formulate a treatise suitable for instruction in any other part of the universe. Thanks to the half forgotten labors of Fourier and Gauss, however, when it became essential to state the laws of mechanics in a way readily applicable to phenomena wherever the investigator may be, the restrictions of terrestrial attraction were easily removed. By the introduction of the so-called absolute systems of units, one form of which is known as the C. G. S. system, a great step in advance was made. It is no exaggeration, in fact, to assert that one properly educated in the mechanics of our day and planet would be as well fitted to investigate mechanical phenomena on the companion of Sirius, as on our diminutive member of the solar system.

The rigorous definiteness of terminology, and the application of the C. G. S. system of units in mechanics, are humorously set forth in a little poem published over the signature '*dp dt*' about twenty years ago in the journal *Nature*. It is now known to have been written by Clerk-Maxwell. This poem purports to give an account of certain lectures on the C. G. S. system delivered to women by one *Professor Dr. Chrschtschonovitsch*. The author figures as one of the auditors, and her lamentations and criticism run as follows:

Prim Doctor of Philosophy
  From academic Heidelberg!
Your sum of vital energy
  Is not the millionth of an erg.
Your liveliest motion might be reckoned
At one tenth-metre* in a second.

"The air," you said, in language fine
  Which scientific thought expresses—
"The air" (which with a megadyne
  On each square centimetre presses)—
The air, and I may add, the ocean,
Are naught but molecules in motion."

Atoms, you told me, were discrete,
  Than you they could not be discreter,
Who knows how many millions meet
  Within a cubic millimetre;
They clash together as they fly,
  But *you!* you dare not tell me why.

Then, when, in tuning my guitar,
  The intervals would not come right,

"This string," you said, "is strained too far,
  'Tis forty dynes, at least, too tight."
And then you told me, as I sang,
What over-tones were in my clang.

You gabbled on, but every phrase
  Was stiff with scientific shoddy;
The only song you deigned to praise
  Was "Gin a body meet a body;"
And even there, you said, collision
Was not described with due precision.

"In the invariable plane,"
  You told me, "lay the impulsive couple;"
You seized my hand, you gave me pain,
  By torsion of a wrist too supple.
You told me what that wrench would do;
"'Twould set me twisting round a screw."

Were every hair of every tress
  Which you, no doubt, imagine mine,
Drawn towards you with its breaking stress,
  A stress, say, of a megadyne,
That tension I would sooner suffer
Than meet again with such a duffer!

Our survey of the development of mechanical science is thus brought down to the present time. But no account of progress can be complete without some allusions to the grand problems which are now occupying the attention of mechanicians. It is hardly necessary to say that these are the problems presented by the phenomena of heat, light, electricity, and magnetism, or, in short, the phenomena of that unseen medium we call the ether. Just as the problems presented by the solar system were the absorbing questions in mechanics at the close of the 18th century, so are the problems presented by the ether the engrossing questions at the close of the 19th century.

In approaching this subject, whether for the present purpose of popular exposition, or for the higher purpose of investigation, one must confess to a difficulty, apparent at least, which might be raised by any hard headed reasoner. It might be asked, for example, by what right we speak of the ether as a medium, when nobody has ever seen any such thing? May we not be merely juggling with mathematical symbols

*One-tenth metre = 1 metre x 10^{-10}.

which stand for no reality? In answer to such questions we should have to admit that most of our evidence is what would be called indirect, or circumstantial. Nevertheless, we could maintain that the evidence of things unseen may be very strong, and that it is nowhere stronger than in the domain of the mechanics of the ether. It seems essential, therefore, to recall, briefly, the salient features of this evidence.

In the first place, it is known that light travels through the celestial regions with a definite speed of about 186,000 miles a second. Induction from a wide variety of observations leads also to the conclusion that heat travels with the same speed, and that it and light are, in fact, only different aspects of the same phenomenon. Year in and year out our astronomical tables proceed on the assumption that eight minutes and seventeen seconds after the sun has risen above the plane of our horizon, we may perceive his light and feel the glow of his heat. The earth is traveling in its orbit around the sun at the rate of about eighteen miles in a second, a fact which, taken in connection with the speed of propagation of light, makes the apparent position of a star a little different from its real position. This is the beatiful phenomenon of aberration discovered by the astronomer Bradley more than two generations ago. The impressive feature of the phenomenon lies in the fact that it is always the same, due allowance being made for the speed and direction of the earth's motion. Thus we are forced to the conclusion that the velocity of light in the stellar spaces is the same, regardless of the source and direction of a luminous ray. The step from this conclusion to the conception that light is propagated by means of some sort of an elastic medium is easy and natural, and experience with gross matter, like water and air, leads quickly to the suggestion that vibration of such a medium must be the mode of propagation. A crowd of readily observable facts of reflection, refraction, and diffraction confirms the suggestion and dignifies it with the title hypothesis, and finally we are led to accept the undulatory theory of light, and to speak as confidently of the luminiferous ether as of any visible matter. Indeed, Lord Kelvin asserted, a few years ago, that we know more of the ether than we do of shoemaker's wax. Certian it is that the labors of Fresnel, Green, Cauchy and their successors have given us a splendid development of this mechanical theory of light. But, alas! they do not enable us to express in common parlance a very definite idea of the medium. No one, it is safe to say, would undertake with any degree of confidence to predict how a portion of the ether, a cubic foot say, would look if isolated and rendered visible. It might appear like a very tenuous and tremulous jelly. Its weight would certainly escape detection, for a bulk equal in volume with the earth would weigh somewhat less than one ounce. Arguing from the phenomena of light alone, it would be found to possess a slight rigidity, but whether it would prove compressible or incompressible we cannot say.

But the strain on the imagination in trying to visualize the ether does not end here. Quite recently it has been rendered almost certain that new and still more complex properties must be attributed to this invisible but omnipresent medium. About thirty years ago, Maxwell, taking up the brilliant experimental researches of Faraday, sought to give mechanical expression to the phenomena of electricity and magnetism. The characteristic idea of Faraday and Maxwell concerning these phenomena was that their seat lies not so much in the electrified and magnetized bodies themselves as in some kind of medium surrounding and permeating them. The result of Maxwell's labors was the publication, in 1873, of a grand but enigmatic treatise—grand, because of its thought-provoking qualities; enigmatic, because no one has yet been able to say just what Maxwell's views were The pursuit of his treatise is like a journey through a dreamland, wherein the travelers seem never to reach their destinations. But the leading idea is plain. It is that the medium is the important factor, and on the medium the attention must be riveted if we would seek a satisfactory explanation of electricity and magnetism.

Faraday died twenty years before, and Maxwell nine years before, anything like crucial experiments decided in favor of their theory. The old theories of action at a distance, without the aid of an intervening medium, but with their fluids and positive and negative subtilties, died hard, if indeed they can be said to be quite dead yet. The recent investigations of Hertz and others, however, seem to render it practically certain that the Faraday-Maxwell conception is the correct one, and that the medium in question can be no other than the medium of light and heat.

Thus the multifarious phenomena of the four sciences of heat, light, electricity, and magnetism appear destined to become unified as the mechanical properties of a universal plenum. The present concentration of activity along this line of inquiry seems fraught with results of the greatest interest. We seem to be, in fact, on the eve of discoveries no less brilliant and important than those whose record has already adorned the history of mechanics. Nevertheless, it may not be our good fortune to witness such advances. The ether may prove intractable for a century or more. It is conceivable, at any rate, that the full comprehension of this medium lies beyond the present range even of that extra sense which the late Charles Darwin attributed to mathematicians. It may be essential, in fact, to first give attention to visible and tangible substances, like shoemaker's wax, before the mind will be prepared to visualize the hidden reality.

But however this may be, mechanical science will remain worthy of the arduous labors of its devotees. The phenomena of matter and motion, though subject to few and simple laws, are infinitely varied and infinitely instructive. The knowledge of those phenomena already acquired gives assurance, as Helmholtz said in these halls a year ago, that we possess the *right method* of investigation. We may therefore expect that a diligent application of this method will yield in the future a not less inspiring body of truth than that which has come down to us from Archimedes and his successors. R. S. WOODWARD.

COLUMBIA COLLEGE.

THE FIVE BOOKS OF HISTORY.

IN the study of the phenomena of history scientific men resort to five great classes of records. The science of geology seeks to discover the history of the earth—of the rocks of which it is composed and of the plants and animals which have lived from time to time. In this research the geologist discovers that nature's last chapter contains a story of mankind, for it is found that the bones of man and some of the works of his arts have been buried by natural agencies in the geologic formations. Sometimes these materials of history are buried in cave drift and in deposits derived from mineral waters which drop from the ceilings or ooze from the crevices of the caves. In flowing away and evaporating, such waters leave behind certain mineral constituents, especially carbonate of lime, which, consolidating and crystallizing, accumulate over the floors and walls of the caves and form pavements of calcite and aragonite. From the waters dropping down from the ceilings stalactites are formed above and stalagmites below, in marble columns of great natural beauty. Under and within such formations the bones of men and vestiges of their arts are sometimes discovered associated with the bones of animals, some of which are found to be of extinct species; but the relics of man are found in other formations. Altogether, the finds are not many. The geologic record of man we may call the Stone Book. It records but a meager tale; the rock-leaved bible of geology has but a postscript devoted to mankind, but in it are facts which prove to be of profound interest.

Man was scattered widely over all the habitable earth in the early period of his development. The 'Garden of Eden' was walled with ice, so that man was not dispersed to the poles, for the outer or polar lands were uninhabitable. Within these walls men were scattered far and wide, on the coasts of every sea, on the shores of every lake, and on the banks of every stream, for everywhere between the frigid zones the vestiges of primeval man are discovered. The ruins of his habitations are thus widely spread—in palefits erected over lakes, in habitations constructed in every valley, in villages where men gathered by tribes, and in cities where they were gathered by nations. The ruins of his ancient dwelling places and the vestiges of his arts scattered over the lands are now esteemed of priceless value by the scientific historian. The ruins furnish much more material than the rocks for the ancient history of mankind. Stone implements are found in great abundance over all the earth; implements of bone, horn, shell and wood are in like manner widely dispersed. In ruins of habitations and vestiges of arts a story is told of developing activities in all of the five great departments of art, for by them we learn much of the industries, pleasures, speech as recorded in glyphs, institutions as illustrated by the paraphernalia of social organizations, and even of opinions as they are expressed in picture writings and ideographs. Let us call this the Ruin Book. It is a strange book, studied by aid of the pickaxe and the shovel. Sometimes habitations are found in ruins piled one over another, giving evidence of the occupancy of sites for many centuries during successive culture periods extending from ruder to higher life.

In all ages birth and death have been abroad in the land. From the infant's wail at birth to the mourner's cry at death men are engaged in the five great activities. Primeval man learned to bury his dead, and as the swarming generations have come down from antiquity through fields of life whose sheaves were garnered by the sickles of death, the tombs have become the granaries of arts, to which the scientific historian resorts that he may discover the vestiges of the earlier humanities. Over all the earth these granaries are scattered in graves, mounds, catacombs, sepulchers and mausoleums, and the whole habitable earth is a necropolis. Sometimes more than bones are found in the ancient tombs, for often they contain works of art. Primeval men were organized into tribes by bonds of affinity and consanguinity. The ownership of property was mainly in the tribe and in the clans and gentes, which were organized tribal units; hence property was chiefly communal in the clan or gens and in the tribe. But some articles of property belonged to individuals, chiefly clothing and ornaments, though a few implements and utensils were owned by individual men and women. In order that controversy should not arise about the ownership of property of this character, it was a fundamental doctrine of this early life that personal property should be inherited by the grave. With the dead person, therefore, were buried the clothing, ornaments, instruments and utensils which he possessed at his death. Gradually this institution became a sacred rite, as about it were thrown the sanctions of religion; and in this more highly developed stage property belonging to the mourning friends was sometimes added to the sacrifice. This was especially the case when personages of great importance were buried. In connection with the rite a mythologic lore sprang up in many tribes by which special virtues were attributed to the sacrifices as necessary to the happiness and prosperity of the dead on their journey to the spirit abode and for their welfare on their arrival in the land of the ghosts.

In the burial of these works of art, records of the stage of culture to which they and their contemporaries had arrived were placed with the dead. It is thus that the tombs become priceless relics of antiquity. In later times, when tribes had been organized into nations and higher arts developed, catacombs, sepulchers and mausoleums were constructed, sometimes hewn in the rock. In the sarcophagi and in the chambers of death many vestiges of culture are found, and often inscriptions are discovered, all of which are now of priceless value. It is thus that the tombs of the ancients constitute a book of history. Let us call it the Book of the Tombs.

Tribes and nations are still scattered over the whole habitable earth, and the people who dwell on the continents and islands labor in many arts, sport in many pleasures, speak in many tongues, are governed by many institutions, and entertain many and widely divergent opinions. In all of these forms of culture some peoples have passed beyond others on the five highways of life, so we are able to study peoples in various stages of culture. No people have invented a culture at one great effort, but whatever arts they practice have been gradually acquired by effort extending from primeval to present time. The humanities discovered as existing in any tribe or nation constitute an epitome of the history of welfare, which has been developed by minute increments of progress through untold generations of effort. Their arts, then, have been inherited from generation to generation, while every generation has made its contribution to their development. The primeval arts of industry, therefore, have not been lost, but have grown to something higher.

In like manner, the pleasures in which a people primarily engaged far back in antiquity, when the habitable earth was first peopled by lowly tribes, still remain, transformed into a higher life of childish sports, athletic exercise, more beautiful decorations, more intellectual games, and more elaborate fine arts. There is thus an immortality of the arts of pleasure by inheritance from generation to generation.

Speech is produced by generations of peoples. Words are lost in the air, but the meanings of words and the knowledge of their formation remain and are taught from generation to generation, so that even evanescent oral language has perennial life.

Institutions, which are devised to regulate conduct, live on, and gradually develop as new conditions arise which demand new solutions. Old forms are inherited, but by minute increments they are transformed, as new concepts of justice are developed.

So opinions have a personal existence by inheritance and a constant change by development as knowledge increases.

I see the germ bursting from the acorn, with its stem and plumule of leaves; I see the plantlet bourgeoning from the earth; I see the scion stretching its green arms into the air; I see the old oak with its great branches in a benediction of shade. Discovering oaklets in acorns, and mighty oaks with dead branches and dying trunks and multitudes of intermediate forms in every oak grove, I learn the history of the growth of oaks without watching the germs until they become dead trees. In like manner, all of the humanities may be studied in various stages of growth by studying the forest of tribes and nations scattered over the face of the earth. A host of men are engaged in scientific research for the purpose of discovering the characteristics of the five great systems of humanities as they are represented in the daily life of peoples. This is found to be a book of many books, gathered into libraries of tribes and nations. Let us call this the Folk Book.

Gradually man has developed written speech. He has learned to write his thoughts in glyphs of meaning on rocks, on bark, on the skins of animals, on tablets of stone and clay and on parchments made of many fibers. It is thus that we have tomes in written language which are gathered in libraries scattered over all the world of enlightenment.

In these books the opinions of mankind are gradually collected, and the process has been going on since the dawn of civilization. The erroneous and the correct, the true and the false, have both been recorded, so that the books contain a strange mixture of truth and error. Yet when rightly read in the spirit of modern scientific criticism, they tell interesting stories and contain valuable instruction. Scientific men do not appeal to history for the truths of science about the objective world. From the beginning of culture to the present time man has interpreted the external world sometimes truthfully, sometimes erroneously. That which is true remains, that which is error dies. Yet ever in recording error something of value has been preserved, for these errors reveal the development of mind and exhibit the methods by which the facts of nature have been interpreted from time to time.

But more; that which the writers of the books of the ages sought to teach is one thing; that which they unconsciously taught is another. In the telling of an event of history something more becomes a matter of record, for a statement may contain many facts, though the author purposely or unconsciously sought to propagate a lie. If we read of an army sailing in a fleet of vessels to pursue a predatory war, the item of history may be true or false, but unconsciously the writer in making his statement records many facts of value about the time in which he writes. He may truthfully explain arts, habits, customs or institutions. In all of these ancient writings something of value is stored. Many of the earlier writings are in poetic form, and in these and others the ostensible subject-matter may be mythical. Everywhere we find exclamatory and emotional passages informed with the mysticism and ignorance of the age, but these myths and mystical hymns and devout prayers reveal to scientific criticism a world of meaning relating to the history of opinions. So the writings of antiquity are held to be of profound interest and importance when used in the proper manner. Science does not appeal to Aristotle as an authority on the constitution of the mind, for he supposed the brain to be a refrigerator for the blood, but it appeals to Aristotle's ideas of the constitution of the mind for the purpose of exhibiting the state of thought to which he had arrived and of illustrating the evolution of philosophy. Science does not appeal to Homer as authority on the nature of the gods and the constitution of the earth as ruled by these gods, for he thought that the winds were kept in caves and transported in sacks, but from Homer it learns how the powers of nature were personified and how these personages as gods were supposed to take part in the affairs of mankind at the time Homer wrote. Science does not appeal to the novels of Plato for the purpose of discovering the best forms of institutions, though he elaborated his opinions with literary charm in 'The Republic,' but it does appeal to Plato to discover how the best minds of his age theoretically solved the problems of government in his time. Science does not appeal to the writings of Confucius or the Buddhistic scriptures for the purpose of discovering the true religion, but for the purpose of discovering the history of religious opinions. If we use the writings of antiquity in this spirit the records of the past are of priceless value for the lessons of history which they teach. Let us call this the Scripture Book.

Modern history resorts to the Stone Book, the Ruin Book, the Tomb Book, the Folk Book and the Scripture Book for the materials to be used in discovering and formulating the development of the industries, pleasures, languages, institutions and opinions of mankind.

The present generation has inherited all the labors of the past. The culture of the day is but a slight modification of the culture of the last generation, and that was derived from the antecedent generation; so all the generations have wrought for us, and our culture is the product of their labor and invention. Every generation has added its minute increment, and hence there has been progress. We cannot dissever our life from that of the past. We inherit its arts and improve them a little; we inherit its pleasures and make but a slight change; we inherit its speech and improve our expression only to a slight degree; we inherit its institutions and modify the forms of justice only in small particulars, and we inherit its opinions and entertain new ideas only as we have discovered a few new facts. So we are indebted to the dead for that which we are, and are governed by the dead in all our activities. Yet the past is not a pall on the present, hiding the truth, but a searchlight that may be turned on the future. The past is not a tyranny on the present, but an informing energy which evolves through us that the future may be improved. Science endeavors to guide the way by a study of the past and to conserve and direct our energies in a legitimate course of development. The past is the chart of the future; if misread it is a false guide, if correctly read the way is clear. It is thus that the five volumes of the pilot book of life are of profound importance.

J. W. POWELL.

WASHINGTON, D. C.

UNITY OF NOMENCLATURE IN ZOÖLOGY AND BOTANY.

SYSTEMATIC biologists have reason to rejoice at the appearance of the completed list of ferns and flowering plants of northeastern North America,* on which a committee of leading botanists has been engaged for the past two or three years. Following the example set by American ornithologists in 1883, a number of prominent botanists determined to sink individual preferences for the sake of that much sought goal—uniformity and stability in the names of genera and species. In 1892, therefore, a committee was appointed by the Botanical Club of the American Association for the Advancement of Science, comprising N. L. Britton, J. M. Coulter, H. M. Rusby, W. A. Kellerman, F. V. Coville, Lucien M. Underwood and Lester F. Ward; and was afterward increased by the addition of Edward L. Greene and William Trelease.† Although the De Candolle or Paris Code of 1867 is the alleged basis of departure, it is evident at a glance that nearly every important rule is borrowed direct from the American Ornithologists' Union Code of Nomencla-

* List of Pteridophyta and Spermatophyta growing without cultivation in Northeastern North America. Prepared by a Committee of the Botanical Club, American Association for the Advancement of Science. (From Memoirs Torrey Botanical Club.) New York. 1893–1894. [Also issued in dated signatures, as published, during 1883 and 1884.]

† In addition to the members of the committee the following botanists have contributed special parts to the 'List': L. H. Bailey, T. H. Kearney, Jr., Thomas Morong, F. Lamson-Scribner, John K. Small, J. G. Smith and Wm. E. Wheelock.

ture, published in 1886. The latter code has been already adopted, not only by ornithologists, but also by leading mammalogists, paleontologists, herpetologists and ichthyologists, and its essential features have been accepted by many prominent entomologists and other writers on invertebrates. It is a matter for special congratulation, therefore, that the botanists have 'fallen into line' so that, for the first time, the naturalists of a great continent are in substantial accord on the main points involved in the nomenclature of genera and species. Better still, the agreement is by no means confined to America, for many of the more progressive naturalists of the Old World have already accepted the same guiding principles.

These principles, as applied in the work under consideration, may be briefly stated as follows: (1) Priority of publication the fundamental principle of nomenclature; (2) Botanical nomenclature to begin with 1753, the date of the first edition of Linnæus's *Species Plantarum;* (3) Original specific name to be retained without regard to generic name; (4) A name once a synonym always a synonym; (5) Original name retained 'whether published as species, subspecies or variety'; (6) Varieties [subspecies] written as trinomials; (7) Double citation of authorities.

The well printed volume is not wholly above criticism. One is surprised to find that the original spelling of generic names has been violated—as *Buettneria* for *Butneria* (p. 163), *Gleditschia* for *Gledetsia* (p. 192), and so on. The retention of capitals in certain specific names is also to be regretted. A word of explanation respecting the synonymy, and also a more explicit statement as to the exact scope of the 'List', would have been acceptable. But these matters are trivial compared with the obvious merits of the work.

C. Hart Merriam.

SCIENTIFIC LITERATURE.

CAN AN ORGANISM WITHOUT A MOTHER BE BORN FROM AN EGG?

1. *Ein geschlechtliche erzeugter Organismus ohne mütterliche Eigenschaften.*—Boveri.—Berichte d. Gesellsch. f. Morph. u. Phys. zu München, 1889.

2. *Giebt es geschlechtliche erzeugte Organismen ohne mütterliche Eigenschaften.*—Seeliger.—Arch. f. Entwickelungsmechanic, I., 2, 1894.

In 1889 Boveri gave an account of certain experiments which seemed to him to prove that a denucleated fragment of the egg of one species of sea-urchin may be fertilized by a spermetazoon from another species, and that it develops into a larva with none of the characteristics of the species which supplied the egg, but exactly like, though smaller than, the normal larvæ of the species which supplied the spermetazoon. He believes that his experiments demonstrate the law that the nucleus alone is the bearer of hereditary qualities; that with the removal of the maternal nucleus are removed at the same time the maternal hereditary tendencies of the egg, and that while the maternal protoplasm furnishes a large share of the material for the production of the new organism, it is without influence on the form of this organism.

This paper was welcomed with great enthusiasm as a contribution of the utmost value to the solution of the problem of inheritance, although careful study of it, or of the translation which was published in the *American Naturalist* for March, 1893, will show that Boveri's evidence for his belief is not direct but very circumstantial.

Seeliger has repeated Boveri's experiments with great care, and on a much more extensive scale, and he shows that the indirect evidence, upon which Boveri bases his belief that the larvæ in question were born from denucleated eggs or fragments of eggs, is fallacious. Seeliger also brings forward positive or direct evidence to show that Boveri's generalization is an error.

W. K. B.

The Rise and Development of Organic Chemistry, by CARL SCHORLEMMER, LL. D., F. R. S., revised and edited by ARTHUR SMITHELLS, B. Sc., Prof. Chemistry in Yorkshire College, Leeds, Victoria Univ. Macmillan & Co., New York. Pp. 280. Price $1.60.

The first edition of the late Professor Schorlemmer's history of organic chemistry made its appearance in 1879. Until the publication of the present volume no revision appeared, although a German edition, carefully edited, was printed in 1889. It was while Schorlemmer was engaged in the preparation of this second English edition that death overtook him, and his unfinished task fell into the hands of Professor Smithells, who has ably completed it.

A brief but exceedingly interesting biographical sketch of Schorlemmer precedes the real subject-matter of the book. From this we gather that the researches which made the author famous were first begun in 1861, as a result of the study of oils obtained from cannel coal. From them were isolated the aliphatic hydrocarbons. A large field was opened up in this study of the paraffins, and Schorlemmer's results were of great importance in the development of organic chemistry.

In the first chapter considerable space is devoted to the discussion of the origin of the word chemistry; attention is directed to the earliest attempts at classification; the labors of Lemery, Stahl, Scheele, Lavoisier, Berzelius and Gmelin are fully reviewed, while a concise account of the *aetherin* theory closes the chapter.

In the second chapter attention is given to Berzelius' attempt to emphasize the difference between organic and inorganic bodies as pointed out by Gmelin; the synthesis of urea by Wöhler, which created such a high degree of excitement in the chemical world; and the beginnings of the controversy which was waged between Dumas, Liebig and Berzelius. The presentation of the substitution theory and the attacks to which it in turn was subjected are fully and clearly narrated.

From time to time the story is interrupted. Thus, in the fifth chapter, the author brings together the various definitions of organic chemistry. The early definition of Liebig, viz.: that organic chemistry is the chemistry of the compound radicals, was shown to be inadequate through the efforts of Williamson and Odling, who demonstrated the existence of the same in inorganic compounds. As carbon was recognized as the element common to all organic bodies organic chemistry might, even in the early days, have been defined as the chemistry of the carbon compounds, or of radicals containing carbon, had it not been that compounds like carbon monoxide, phosgene, carbon disulphide and the carbon chlorides were not produced in the organism. In 1848 Gmelin, believing that he had found a boundary line, wrote, 'hence *organic compounds* are all *primary compounds* containing more than one atom of carbon.' This definition no longer sufficed after the chemical world accepted Gerhardt's atomic weights. In 1851 Kekulè, recognizing the difficulties in the way of a simple, satifactory definition, recorded himself in these words, "organic chemistry is the chemistry of the carbon compounds." He held it to be a special part of *pure* chemistry, but because of the great number and importance of the carbon compounds believed that it should be separately treated. Erlenmeyer wrote "their study requires in many respects peculiar methods of investigation, different from those employed in the study of the compounds of other elements, and thus the necessity for a division of labor has also made itself apparent in the interest of scientific research." Butlerow gave as his opinion that organic chemistry must be defined as the chemistry of the carbon compounds. After giving place to the definitions of the earlier writers Schorlemmer defines 'organic chemistry as the chemistry of the hydrocarbons and their derivatives.' He, however, recognized that it did not place a sharp boundary line between the inorganic and organic fields.

In the remaining chapters the further development of the organic field is traced with great care. The different views in regard to the constitution of benzene, the arrangement of atoms in space, geometrical isomerism, various striking syntheses in both the paraffin and aromatic series are clearly presented. In regard to the great revolution produced in calico-printing and in the manufacture of madder preparations by the synthesis of alizarin by Graebe and Liebermann, Schorlemmer writes "madder finds to-day only a very limited application in dyeing of wool. Twenty years ago the annual yield of madder was about 500,000 tons . . . . . when a friend of the author asked to see the madder plantations at Avignon he was told 'it is no longer grown, as it is now made by machinery.'"

The book closes with a chapter upon the unsolved problems. "If to-day we cannot make morphine, quinine, and similar bodies artificially, the time is near at hand . . . . If we cannot make quinine we have already found a partial substitute in antipyrine." Yes, in the language of Schorlemmer "organic chemistry advances with giants' steps. About fifty years ago only twelve hydrocarbons were known, and twelve years ago this number had increased to about 200. To-day we are acquainted with more than 400, and many of them, as well as their derivatives, have been carefully studied."

The little volume from which we have quoted is well constructed and replete with information for the student of chemistry. Its careful study will be well repaid. The editor and publishers deserve much credit for again presenting such a valuable work.

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NOTES AND NEWS.

MILK IN ITS RELATIONS TO DIPHTHERIA.

VLADIMIROW, in the Second Part, Vol. III., of the *Archives des Sciences Biologiques publiées par L'Institut Impérial de médecine Expérimentale*, St. Petersburg, page 84, gives the results of some researches made by him in Nencki's laboratory on the effects of the diphtheria bacillus upon cows, and especially as to the possibility of producing in the cow, by subcutaneous injections of this organism, a disease which would result in the infection of the milk by the same organism, so that such milk might become a carrier of the germs to those who used it.

Dr. Klein, of London, has reported, as the result of such hypodermic injections, the production of an eruption upon the udder of the cow, in which eruption the diphtheria bacillus was found to exist.

These experiments were repeated by Dr. Abbott, of Philadelphia; but while he found that the injection produces disease, and even death, in the cow, there was no eruption in the udders, and no diphtheria bacillus in the milk. Vladimirow confirms the results obtained by Dr. Abbott. He found that if the diphtheria bacillus was introduced into the milk ducts of the teats upon one side of the udder of the cow, an inflammation was produced upon that side of the udder, and general fever occurred, which, in one case, produced death. The milk secreted by the injected half of the gland acquired a greenish tint, coagulated, contained pus, had an alkaline reaction, and contained less sugar and more albuminoids than the milk coming from the sound side of the gland. The diminution in the quantity of sugar was due to the decomposition of this substance by the diphtheria bacillus, with the production of lactic acid. The diphtheria bacilli only remained alive in the udder for a short time—from four to five days—and their number steadily diminished. Subcutaneous injections of cultures of the diphtheria bacillus in the cow produced a serious fever, with loss of appetite, etc., but there was no irritation on the udder, the milk did not change in its appearance and contained neither diphtheria bacilli nor the toxins due to these.

CONSUMPTION OF WINE AND BEER IN DIFFERENT COUNTRIES.

DURING the years 1886–90 the mean annual consumption of wine, stated as number of litres per head of population, was, in Spain, 115; in Greece, 109.5; in Bulgaria, 104.2; in Portugal, 95.6; in Italy, 95.2; in France, 94.4; in Switzerland, 60.7; in Roumania, 51.6; in Servia, 35.0; in Germany, 5.7; in Belgium, 3.2; in Holland, 2.2; and in Great Britain and Ireland, 1.7.

In 1890 average consumption of beer, stated as number of litres per head of population, was, in Belgium, 177.5; in Great Britain and Ireland, 136.2; in Germany, 105.8; in Denmark, 102.9; in the United States, 58.0; in Switzerland, 40.0; in Norway, 37.5; in Holland, 34.6; in France, 22.5; and in Italy, 0.9. (*Bulletin de l' Inst. internat. de Statistique*. VII. 2.ᵉ Sive. 1894. p. 309.)

MAGNETIC WAVES.

AT a late meeting of the Mathematico-Physical Club in Cambridge, Mass., Professor Dolbear showed that magnetic waves produced by the vibrations of a magnet making two thousand vibrations per second could easily be heard by listening to a magnetic telephone held in the neighborhood without any employment of its coil. The inductive action of the waves upon the magnet of the telephone being direct instead of being first transformed into an electric current as in the common way of using it. Two sympathetic tuning forks may, if magnetized, react in the same way as they will from sound vibrations and one make the other vibrate through a thick wall, thus showing that such walls are transparent to magnetic waves. The reactions show that the periodic change of form due to vibration changes the strength of the magnetic field at the same rate. A few turns of wire about the bend of a U magnet may have the ends fastened to a telephone circuit, when, if the magnet be struck so as to produce a sound, it will give so loud a sound in the telephone as to probably surprise one who has not tried the experiment before.

ANATOMY.

THE *Bibliographie Anatomique* begins its third year with the announcement of increased success. It is to be enlarged to make room for a greater number of original articles, and at the same time the subscription is to be raised from seven and a-half to ten francs. This excellent publication gives a current classified list of all anatomical articles published in French, and differs from other similar journals in adding brief *resumés* of all the more important articles. In practice it covers quite thoroughly the field of vertebrate morphology, and it may therefore be recommended for the support of American investigators.

CARNIVOROUS PLANTS.

PROFESSOR THOMAS MEEHAN, in an article on *Darlingtonia Californica* in the January issue of *Meehan's Monthly*, notes that the so-called carnivorous plants are just as able to get their food from the earth as other plants do, and that the animal food which they undoubtedly consume through their foliage can only be looked upon as a gastronomic luxury in no way to be classed among the necessaries of life.

TOADS ON THE SEASHORE.

During a vacation recently spent at Cape May, New Jersey, I was much interested in observing the habits of the toads on the seashore. Between the 'board-walk' and high-water mark is a narrow belt of uneven sand, dotted with tufts of beach-grass and raised here and there into miniature 'dunes.' Here the toads congregate in considerable numbers, and as evening draws on they may be seen hopping about in quest of food. As they were not to be seen during the heat of the day, I became interested to know where they concealed themselves. A short search revealed their whereabouts. Like so many of the small animals of the contiguous waters, they bury themselves in the sand for concealment. Upon looking attentively over the surfaces of the little dunes, one saw here and there a pair of bright eyes, not unlike the sand in color and as fixed as gems in a rock. It was only necessary to touch the sand in the immediate vicinity of the eyes, when a toad would hop out and tumble clumsily over the hummocks in endeavors to escape.

Whether the toads captured any prey while concealed in the sand I was unable to discover, but I should think it improbable, as their mouths were usually beneath the surface and there would be little chance for them to shoot out their tongues.

Frederick W. True.

GENERAL.

Professor Arthur Cayley, the eminent mathematician, died at Cambridge, England, on January 26, at the age of seventy-four.

John S. Burdon-Sanderson, M. A., Fellow of Magdalen College, and Waynflete Professor of Physiology, has been appointed Regius Professor of Medicine, at Oxford, in place of Sir Henry W. Acland, Bart., Christ Church, resigned. Professor Burdon-Sanderson continues to direct the lectures and practical instructions in the Department of Physiology, with the assistance of Dr. Haldane and Mr. Pembrey.

Applications for the table at the Biological Laboratory of Cold Spring Harbor, maintained by the American Association should be sent to Professor W. H. Conn, Wesleyan University, Middletown, Conn., or to Professor F. W. Hooper, Brooklyn Institute of Arts and Sciences, Brooklyn, N. Y.

The Johns Hopkins University Circular for January consists of scientific notes on work done at the University. It includes a reprint from the *Journal of Geology* of Professor Brooks' paper, *On the Origin of the Oldest Fossils and the Discovery of the Bottom of the Ocean*, and a reprint from *Natural Science* of a review of Professor Brooks' monograph, *The Genus Salpa.* It also contains notes in chemistry, astronomy and botany.

The French Minister of Education, M. Leygues, has opened the new buildings for the scientific departments of the Sorbonne.

The list of books for sale issued by Bernard Quaritch in January includes many valuable works in natural history, especially in botany and ornithology.

SOCIETIES AND ACADEMIES.

NEW YORK ACADEMY OF SCIENCES.

Biological Section : January 14, 1895.

Notes on Neurological methods and exhibition of photo-micrographs.

A paper on *The Use of Formalin in Golgi's method* was read by Mr. O. S. Strong. The writer found that formalin (40% solution of formaldehyde) may be used (instead of osmic acid) mixed with potassium bichromate. Pieces of adult brain were placed in the following: Potassium bichromate ($3\frac{1}{2}\%$–5%) 100 volumes + formalin $2\frac{1}{2}$ to 5 vol. During several days or more the tissue is transferred to the silver nitrate solution (1%). Or the tissue after 1 to 2 days may be transferred from the above bichromate-formalin mixture to the following: Pot. bich. (5%) 2 vols. + formalin 1 vol. After 12 to 24 hours the tissue is put into silver solution. The advantages of this method are that it avoids the use of osmic acid and that the stage of hardening favorable for impregnation lasts longer than when the osmium-bichromate mixture is used and good results are consequently more certain. In other words, the formalin-bichromate does not overharden. In this respect it is also superior to the lithium bichromate method of the author (N. Y. Acad. of Sc. Pro. vol. XIII., 1894). For embryonic tissue the formalin method is probably not equal to the osmium-bichromate method, possibly because it does not harden sufficiently. For such tissue *lithium* bichromate (which hardens more rapidly than potassium bichromate) had better be mixed with the formalin instead of potassium bichromate. While good results are obtainable, especially with advanced embryonic tissue, with either of the above, yet the author believes that for such tissue the osmium-bichromate method is probably in certain respects somewhat superior.

A fuller account will be published later.

Dr. Ira Van Gieson reported some preliminary observations on the action of formalin as a fixative and preservative of the central nervous system for the ordinary histological staining methods; solutions of formalin, four, six and ten per cent. were used, followed by 95 per cent. alcohol and celloidin embedding. Sections of the human cord, cerebellum and cortex prepared in this way gave very thorough fixation of the ganglion cell, neuroglia cells, and fine nerve fibres.

Weigert's haematoxylin method can be applied to such sections, and gives very good results for the plexus of fine fibres in the cortical and spinal grey matter. The myelin of the fine fibres is well preserved and gives the characteristic bluish black reaction with the Weigert haematoxylin stain, as in chrome hardened preparations. The background of the grey matter is especially clear and the fibres sharply delineated. The formalin hardened sections should be soaked in the neutral copper acetate solution, diluted one-half with water, for 2 hours, then thoroughly washed in water and immersed in the Weigert lithium-carbonate haematoxylin solution two to twelve hours. Weigert's borax-prussiate of potassium solution is used for differentiation. The differentiation takes place rapidly and must be watched carefully.

The formalin sections of the central nervous system may also be used for Rehm's modification of Nissl's method; but the staining of the chromatin and minute structure of the nucleus and cytoplasm is not quite so sharply outlined as with absolute alcohol fixation.

The duration of the hardening in formalin solutions has a very important and varying influence on the nerve fibers and ganglion cells with reference to the application of such methods as the Weigert and Nissl groups of stains. A further study to define the more exact limitations of formalin as a new histological preservative for the nervous system will be published later and the more exact periods of time in the hardening necessary for different stains detailed.

Mr. R. H. Cunningham, *On the Sources of Illumination for Photo-Micrography*, noted a practical mode of employing the arc light with satisfactory results.

Mr. C. F. Cox illustrated the *Latest Theories of Diatom Structure*, exhibiting lantern slides of Mr. T. F. Smith, of London.

Dr. Edward Leaming projected a series of his micro-photographs of bacteria, fertilization processes of sea-urchin, *Toxopeneustes*, and Golgi preparations.

Bashford Dean, *Recording Secy.*

THE BIOLOGICAL SOCIETY OF WASHINGTON, JAN. 26.

Council meeting at 7:30 P. M.

A New Cotton Enemy, brought over from Mexico: MR. L. O. HOWARD.

Anatomy of a Leaf-gall of Pinus virginianus: MR. THEO. HOLM.

Abnormal Feet of Mammals: MR. F. A. LUCAS.

The Mesozoic Flora of Portugal compared with that of the United States: PROF. LESTER F. WARD.

FREDERIC A. LUCAS, *Secretary.*

SCIENTIFIC JOURNALS.

THE ASTROPHYSICAL JOURNAL, JAN.

On the Conditions which Affect the Spectro-Photography of the Sun: A. A. MICHELSON.

Photographs of the Milky-Way: E. E. BARNARD.

The Arc-Spectra of the Elements I. Boron and Berylium: H. A. ROWLAND and R. TATNALL.

On Some Attempts to Photograph the Solar Corona Without an Eclipse, made at the Mount Etna Observatory: A. RICCÒ.

Discovery of Variable Stars from their Photographic Spectra: E. C. PICKERING.

Preliminary Table of Solar Spectrum Wave-Lengths I.: H. A. ROWLAND.

Observations of Mars made in May and June, 1894, with the Melbourne Great Telescope: R. L. J. ELLERY.

Recent Changes in the Spectrum of Nova Aurigæ: W. W. CAMPBELL.

The Modern Spectroscope. X. General Considerations Respecting the Design of Astronomical Spectroscopes: F. L. O. WADSWORTH.

Minor Contributions and Notes.

Reviews.

Recent Publications.

AMERICAN JOURNAL OF MATHEMATICS, JAN.

Sur une transformation de mouvements: Par PAUL APPELL.

Extrait d'une lettre adressée à M. Craig: Par M. HERMITE.

On the First and Second Logarithmic Derivatives of Hyperelliptic Functions: By OSKAR BOLZA.

Sur la definition de la limite d'une fonction. Exercice de logique mathématique: Par G. PEANO.

Theorems in the Calculus of Enlargement: By EMORY MCCLINTOCK.

On Foucault's Pendulum: By A. S. CHESSIN.

BULLETIN OF THE TORREY BOTANICAL CLUB, JAN.

Family Nomenclature: JOHN HENDLEY BARNHART.

A Revision of the North American Species of the Genus Cracca: ANNA MURRAY VAIL.

A Revision of the Genus Scouleria *with Description of one new Species:* ELIZABETH G. BRITTON.

Studies in the Botany of the Southeastern United States—III.: JOHN K. SMALL.

New Plants from Idaho: LOUIS F. HENDERSON.

Buxbaumia Aphylla: GEO. G. KENNEDY.

Herbert A. Young: WM. P. RICH.

Proceedings of the Club.

Index to Recent Literatare Relating to American Botany.

NEW BOOKS.

The Factors in Organic Evolution: A Syllabus of a Course of Elementary Lectures. DAVID STARR JORDON. Pp. 149. Ginn & Co. $1.50.

The Geological and Natural History Survey of Minnesota. N. H. WINCHELL. Minneapolis, Harrison & Smith. 1894. Pp. 210.

Anatomy and Art. President's address before the Philosophical Society of Washington. ROBERT FLETCHER WASHINGTON. 1895. Pp. 24.

Annual Reports of the Bureau of Ethnology of the Smithsonian Institution, 1890–1891. J. W. POWELL. Washington, Government Printing Office. Pp. 742.

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FRIDAY, FEBRUARY 15, 1895.

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison on Hudson, N. Y.
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THE INFLUENCE OF CERTAIN AGENTS IN DESTROYING THE VITALITY OF THE TYPHOID AND OF THE COLON BACILLUS.

DURING the last year a series of researches upon the influence of light, of desiccation, and of the products of certain micro-organisms upon the vitality of some of the pathogenic bacteria has been carried on in the Laboratory of Hygiene of the University of Pennsylvania, by Dr. Adelaide W. Peckham, in accordance with a general scheme for such investigation prepared by Dr. Weir Mitchell and Dr. Billings, the Director of the Laboratory, and with the aid of a grant from the Bache fund. A portion of the results obtained in this research has been communicated to the National Academy of Sciences at its meetings in April and in October, 1894; but as the volume of the Transactions of the Academy which will contain these papers will not be issued before next year, it has been thought best to publish some account of these experiments without further delay.

That direct sunlight kills or stops the growth of certain bacteria has been known since 1877, when Downes and Blunt presented to the Royal Society a report on "Researches on the effects of light upon bacteria and other organisms."* Since that date a number of papers on this subject have been published, the most important one in relation to the typhoid bacillus being that of Janowski in 1890.† The first series of experiments by Dr. Peckham was made with the *staphylococcus pyogenes aureus*, the object being mainly to determine the best methods of investigation.

* Proc. Roy. Soc. 1877, vol. 26, p. 488.

† Zur Biologie der Typhus Bacillen, Centralbl. f Bakteriol, etc., VIII., 1890, pp. 167, 193, 230, 262.

Photobacteriographs were made by Buchner's method, namely, by placing a square of black paper, or of glass of different colors, upon the bottom of a plate containing inoculated agar-agar during insolation; but although the protected portion was visible after fifteen minutes' insolation and incubation for twenty-four hours, and sharply defined after two hours' insolation and incubation as before, no accurate estimate could be made of the difference in the destructive power of different periods of insolation. Successful photobacteriography requires inoculation of large quantities of bacteria, in order that the colonies may be set so closely together that a ground-glass appearance is produced; in which case counting of the colonies is practically impossible.

For this reason the following method was used for each of the three organisms, the *staphylococcus pyogenes aureus*, the *bacillus coli cammunis* and the *bacillus typhi abdominalis.*

To obtain an accurate measure of the effects produced by lights of different intensity or of different colors, it is necessary to ensure, as far as possible, that the bacteria to be experimented on shall be uniformly distributed in the culture media. Tubes containing each 10 cc. of bouillon were inoculated with one drop of a bouillon culture and then placed in an incubator for twenty-four hours. A small quantity of sterilized gravel was then added to the culture tube and it was thoroughly shaken, after which 10 cc. of a one-half per cent. salt solution was added and the culture drawn into a Nuttall's dropping apparatus. From this, one-twentieth of 1 cc. of the bouillon culture was dropped into a tube of melted agar-agar, which was slowly and thoroughly agitated, and the contents were then poured into a Petri dish, carefully levelled on a levelling tripod over ice water. In the first method used the Petri dishes were found to be so uneven on the bottom that the layer of medium under the protective square was often very thick or very thin as compared with that about the circumference of the plate, and, therefore, comparisons made between the centre and the circumference would be in almost every case unreliable. To overcome this difficulty, just one-half of the plate was shaded with black paper or colored glass.

The plates were then exposed to sunlight, bottom upwards, so as to allow the sun to shine as directly as possible on the inoculated agar-agar. At intervals of fifteen minutes a plate was removed and placed in the incubator. The temperature of the plates during insolation was always below 34° C. as shown by a thermometer with a blackened bulb which was placed in the sun and the temperature noted every fifteen minutes. Sunny, still days were utilized for insolation, beginning at 10 A. M. during the months of October, November and December. After insolation, the plates, and also a non-insolated control plate were incubated for twenty-four hours.

The colonies were counted in the following manner: A number 1 eye-piece was divided into fields (as done by Nuttall in counting tubercle bacilli), by introducing a disk of black cardboard which had a square opening divided into four parts by two hairs placed at right angles. This eye-piece and an objective of low power were used in counting. The percentage of germs destroyed by insolation was estimated from the mean of four counts taken on both the insolated and the protected halves of the plate. By this method an accurate statement can be made regarding the difference in protective power given by the different colors, not from simple observation, but by comparison of a definite number of colonies counted.

The following table shows the comparative effect of the blue rays and of complete shadows on the growth of the organisms experimented on :

		Percentages of organism destroyed in the insolated half of the plate as compared with the protected half.							
Number of minutes exposed		15	30	45	60	75	90	105	120
Typhoid Shaded with	black paper	17	28	33	34	65	63	90	98
	blue glass	7	14	30	32	24	38	35	52
Colon ... Shaded with	black paper	25	15	25	71	83	88	97	99
	blue glass	13	29	32	35	56	59	60	52
Aureus .. Shaded with	black paper	..	..	55	..	72	72	80	90
	blue glass	..	38	34	54	51	41	48	50

From this series of experiments the following results were obtained :

Insolation for fifteen minutes destroys to a slight extent each of the three organisms experimented upon. Two hours' insolation destroys 98% of the germs and from three to six hours kills all. The colon bacillus is more easily destroyed by insolation than is the typhoid bacillus. Exposure to diffuse daylight, to gas light, or to the incandescent electric light produces little effect. Red, orange, yellow, and green light produce little effect, during two hours' insolation ; while the blue and violet rays kill nearly as rapidly and as certainly as full sunlight. Insolation from six to eight hours lessens the number of colonies under the protective square to a slight extent, for the colors red, orange, yellow and green.

Plates were made in the same manner and exposed to diffused light for periods varying from fifteen minutes to two days. The exposure was made on clear sunny days in the light part of a room. In this experiment the result was negative, the number of colonies on the two sides of the plate being approximately the same.

An ordinary gas-burner and an incandescent light were each used as the source of illumination. The plates were placed bottom-upwards in a dark room near the light used. Illumination for sixteen hours with gas produced no effect on the growth of the organism as shown by counting of the colonies.

Illumination for four and one-half hours with an incandescent light also gave negative results.

A series of experiments was made with tubes of bouillon inoculated with the different organisms and then enclosed in larger tubes containing fluids of different colors—red, orange, yellow and blue, which were exposed to sunlight with control tubes, one placed in water, and the other in a similar tube covered with black paper. The materials used for making the colored solutions were corallin, chromate and bichromate of potassium, and methylene blue. From these tubes, plates were made, and the number of colonies counted.

It was found that an increase in the number of colonies continued to the eighteenth day, the number being greater in the colon and aureus cultures than in the typhoid. The colonies then began to decrease, and on the fifty-eighth day the plates contained but few colonies. In this experiment, as in the last, plates made from culture tubes placed in blue fluid showed fewer colonies.

Since the presentation of the above results, with details, charts and tables, to the National Academy, in April, 1894, Dr. Dieudonné has published in the *Arbeiten aus dem Kaiserlichen Gesundheitsamte*, a paper on the effects of sunlight on bacteria, in which he reports results substantially the same, and obtained by almost the same methods as those of Dr. Peckham.

Sunlight not only weakens or kills the typhoid and the colon bacillus, but it affects culture media so as to render them less capable of supporting the growth of these organisms. Dr. Peckham found that sterile bouillon insolated from one to ten days and then inoculated with the *bacillus typhi abdominalis* showed no diminution in the number of colonies as compared with a control plate made from a similar culture not so exposed. Twenty days insolation and then inoculation with the typhoid bacillus showed great decrease in the number of colonies on all the plates; some of them were sterile. Insolation of forty days, and inoculation in the same manner, gave very few colonies for each plate, probably the same as the number of germs introduced, *i. e.*, there had been no development. Bouillon insolated 50—60 days and inoculated gave sterile tubes. This insolated bouillon after inoculation and incubation remained perfectly clear, and plates made after a week of incubation gave no more colonies than those made at the end of twenty-four hours. Its reaction was alkaline, but not intensely so.

Insolated agar-agar—Of twenty-three tubes of agar-agar insolated twenty days, and then inoculated with the *bacillus typhi abdominalis*, all except one remained sterile, and neither the *bacillus typhi abdominalis* nor the *bacillus coli communis* grew when inoculated in stripes on these plates. Of seven tubes of agar-agar insolated forty days and then inoculated with the bacillus of typhoid, all remained sterile. On four of these plates mould appeared after some days. Of seven tubes of agar-agar insolated forty days and then inoculated and incubated as before, all remained sterile.

Insolated gelatine.—Of ten gelatine tubes insolated forty days and then inoculated with the *bacillus typhi abdominalis*, six remained sterile, two contained a few colonies of *bacillus typhi abdominalis*, and two were contaminated.

The insolated bouillon was then kept in diffuse daylight for forty days and again inoculated with the typhoid bacillus. Within twenty-four hours the tubes of bouillon became turbid and plates made from them showed innumerable colonies.

It is difficult to account for the effect of insolation on culture media. Roux in his experiments on anthrax found that insolation of bouillon for *two* or *three hours* rendered it unsuitable for germination of the spores, but if the bacilli were introduced they would thrive. He attributes this alteration to some chemical change which the culture media undergo during the insolation. He found also that if the insolated media were kept in the dark or in diffuse daylight for a time, the original nutritive qualities were restored and germination of spores would take place. Geisler and Janowski observed the bactericidal properties of insolated media, but the latter could find no chemical alteration in such media.

Percy Frankland in his chapter on action of light on micro-organisms* concludes from the results obtained by many investigators 'that the effect is due to a process of oxidation possibly brought about through the agency of ozone or peroxide of hydrogen, or both; that all apparently direct low temperature oxidations require the presence of water. And inasmuch as the bactericidal action of light is unquestionably a case of low temperature oxidation, there is the strongest presumptive evidence, as well as weighty experimental evidence, that moisture, which practically means the possibility of the presence of peroxide of hydrogen or of some similar material, is essential for its manifestation.'† Westbrook ('Some of the effects of sunlight on tetanus cultures, Jour. of Pathol. & Bacteriol. III., Nov. 1894, 71') found that old broth cultures of the tetanus bacillus in an atmosphere of hydrogen were not in the least affected by exposure to sunlight, either in regard to their virulence or their rapidity of growth on reinoculation. When the same culture was sealed up in the presence of air, the

*Micro-organisms in water, p. 390.

†Gelatine, to which were added different amounts of the peroxide of hydrogen, was inoculated with the *bacillus typhi abdominalis* and poured into plates. Those plates in which more than one part of the peroxide to 5000 of gelatine was used, were sterile.

micro-organisms, were not only killed, but the material was completely harmless when inoculated into white mice. It was, however, possible to obtain vigorous and virulent growths from cultures which had been made quite innocuous by the action of the sun. Oxygen was used up in the process. Under ordinary circumstances one might be tempted to explain the effect of sunlight in destroying bacteria by the drying of the organisms exposed to it, especially in the case of those bacteria which do not form spores, but our experiments show that desiccation for months has little effect on the vitality of the typhoid or of the colon bacillus. To determine the influence of desiccation upon these organisms, and also upon the *staphylococcus aureus*, the following experiments were made:

Bouillon cultures of the *bacillus typhi abdominalis*, the *bacillus coli communis* and the *staphylococcus aureus* were roughly dried on threads one centimetre long and then desiccated, a portion being placed in a vacuum, another portion in a desiccator over sulphuric acid, and a third in a closet; all were kept in the dark. The result of the desiccation under the three different conditions is as follows:

Bacillus typhi abdominalis:

Lived in a vacuum from December 30 until July 24, or 207 days. In a desiccator over sulphuric acid from January 3 until July 24, or 213 days.

In a closet from December 18 until July 24, or 229 days.

Bacillus coli communis:

Lived in vacuum from November 29 to May 30, or 183 days.

In a desiccator over sulphuric acid from January 3 until July 24, or 213 days.

In a closet from December 30 until May 30, or 152 days.

Staphylococcus aureus:

Lived in vacuum from November 29 until July 24, or 207 days.

In a desiccator over sulphuric acid from October 25 until April 19, or 178 days.

In a closet from February 13 until July 24, or 162 days.

It will be seen from these experiments that the organisms experimented on endure desiccation for five months, or more, without losing their vitality, and hence the slight evaporation which may have occurred in the insolation experiments, had probably no influence on the results.

It is evident that sunshine must exercise considerable influence in destroying bacteria on the surface of soil, streets, etc., exposed to its influence, but its action is almost confined to the surface, as appears from the results obtained by Esmarch in attempts to disinfect bedding and clothing by this agency. While the light from an incandescent electric lamp has little germicidal effect, that from a powerful arc lamp produces effects similar to those of sunlight, and it has been proposed to use this means to disinfect the walls of infected rooms. The bacillus of tuberculosis appears to be more quickly destroyed by light than the typhoid or the colon bacillus, being killed by exposure to simple diffused daylight in about a week,* and this fact should be borne in mind in advising measures to prevent the diffusion of this organism.

The investigations upon the typhoid and the colon bacillus referred to in this paper, were undertaken as part of a general scheme of inquiry to ascertain the agencies which tend to detroy the typhoid bacillus when it is introduced into a source of water supply, as, for example, into a running stream. An important part of this investigation relates to the influence of the common water bacteria, or of their products, upon the vitality of the typhoid bacillus.

This research was conducted as follows:

* Ueber bacteriologische Forschung: Vortrag in der ersten allgem. Sitzung des X internationalen Congress, 1890.

1. Forty-five varieties of bacteria found in the water of the Schuylkill river were used in the first experiment. Cultures of each organism were made on agar-agar and after attaining a luxuriant growth were sterilized, the reaction was taken, and the medium was again slanted. A set of these tubes was inoculated with the *bacillus typhi abdominalis* and a second set with *bacillus coli communis*.

The object of this research was to ascertain whether the two organisms would grow on media containing the products of the activity of water bacteria. The reaction was alkaline in every tube. The *bacillus typhi abdominalis* and the *bacillus coli communis* lived in every instance, some showing fairly luxuriant growths, while others were only transparent films.

2. In the second experiment, thirty-nine varieties of the water bacteria used in the first experiment were inoculated into tubes each containing 10 cc. of sterilized tap-water and 5 drops of bouillon. Two sets of tubes were made as before, one being inoculated with the *bacillus typhi abdominalis* and the other with the *bacillus coli communis*. To ascertain whether the two organisms under consideration would multiply in the presence of water bacteria, gelatine plates were made for twelve or more days. Both bacilli gave characteristic colonies with each of the water organisms, except two which had apparently an antagonistic effect upon their development. They were both members of the *subtilis* group. In other members of this group this peculiarity was absent.

The typhoid bacillus in several instances outlived its associate organism. In one instance a gelatine plate made from a tube of sterilized water inoculated with the typhoid bacillus and a water bacterium 160 days previously gave characteristic colonies of the *bacillus typhi abdominalis*.

3. To meet the objection that might be raised to the use of heat for the sterilization of the medium in which the water organisms had grown, the opinion having been advanced that some products of growth are either volatile or rendered inert by high temperatures, flasks each containing 70 cc. of bouillon were inoculated with water bacteria and incubated for from 15 to 20 days. The cultures were then filtered through porcelain, the reaction was taken, and the filtrate was run into sterilized tubes which were inoculated with the *bacillus typhi abdominalis* and the *bacillus coli communis* and then incubated. In each of the thirteen filtrates inoculated the bacilli grew and multiplied for at least four days.

John S. Billings.
Adelaide Ward Peckham.

CURRENT NOTES ON PHYSIOGRAPHY (I.).

INTRODUCTORY NOTE.

It is proposed to contribute to Science under the above title a series of notes and comments on recent investigations and current literature concerning physiography, or physical geography in its modern form. A brief statement of the field to be covered may be appropriate at the outset.

Following the plan introduced by Carl Ritter, and popularized in this country chiefly by Arnold Guyot, geography may be defined as the study of the earth in its relation to man. Some prefer to extend this relation to all forms of life. Physical geography may then be defined as the rational study of those features of the earth which must be understood in order to appreciate its relation to man. In deference to the opinions of the majority of the conference on geography, held in Chicago in Christmas week, 1892, physiography is taken as the name of this subject in its modern form, with particular reference to the rational study of the lands, where man dwells. Descriptive geography is an empirical study that hardly deserves a place in modern teaching. Political geography is undifferentiated history. Commercial geography is the elementary phase of economics. The distribution of plants and animals leads the way to botany and zoölogy; the chief value of this subject coming from the emphasis that it gives to those physical features and conditions of the earth that determine the distribution of life; when it is made a basis for the introduction of classification and terminology, it is misused, for these matters need deliberate study with a method and discipline of their own. The subjects of oceanography and meteorology involve considerations and disciplines so different in many respects from those which characterize the study of the lands that they fully deserve separate names and treatment; but their teachings must be frequently drawn on for use in physiography.

Contributions from many subjects, astronomy, physiology, botany, zoölogy, history and economics, are merged into a single elementary study—geography—in the earlier school years; all are expanded and separately treated in later school years; all deserve to be treated over again afterwards in the broader way characteristic of college teaching; and all include broad fields for investigation in the university.

Physiography being particularly directed to the study of the lands, must of necessity in its higher researches give due consideration to the more minute features of land forms and their development—subjects which recent writers name geomorphology and geomorphogeny—for the sufficient reason that a close understanding of the development of land forms greatly aids the observation, description and recognition of the forms themselves; and that the knowledge thus only to be gained of the forms of the land is essential as a preparation for the careful study of their relations to man and other inhabitants of the earth.

As thus explained, physiography is an outgrowth of geology; and geology, especially field geology, is a necessary preliminary discipline both for those who would undertake the higher study of physiography and for those who would reduce it to the simplest form of expression for early school use.

MEANING OF THE TERM, BASELEVEL.

SINCE the introduction of the term baselevel by Powell twenty years ago, its use has become popular but unhappily its meanings have not been well defined. A subdivision of the work that the word has been made to do now seems desirable. It should be restricted rather closely to its original meanings, and newer terms should be employed for its secondary meanings. Powell originally wrote: "We may consider the level of the sea to be a grand base level, below which the dry lands cannot be eroded, but we may also have, for local and temporary purposes other base levels of erosion, which are the beds of the principal streams which carry away the products of erosion." (Colorado River of the West, 1875, 203.) By using a few qualifying adjectives, there need be no confusion between general, local and temporary baselevels. When unqualified, the general baselevel, or sea level, should be understood.

When a region has been baselevelled (the verb being here made from the noun, after the ordinary English fashion), the surface thus produced is often spoken of as a 'baselevel.' For example, J. S. Diller writes: "It is evident that a general baselevel of erosion must have originated approximately at sea level. This is the only position in which a very extensive baselevel can originate. If we now find such a baselevel at a considerable elevation above the sea, its position furnishes evidence that since the baselevel was formed the country has been uplifted." (Chicago Journal of

Geology, II., 1894, 33.) Further on in the same article, he writes of the 'deformation of the baselevel.' Although the writer has repeatedly made a similar use of the term, it now seems doubtful if it should be used so freely; and some such word as peneplain might serve to replace this extension of the original meaning of baselevel. This is the more advisable, when it is considered how very seldom a region is reduced sensibly to baselevel; how generally a long eroded surface still retains some faint inequality of form which should be expressed in its name.

GEOMORPHOLOGY OF THE SOUTHERN APPALACHIANS.

The interpretation of the development of geographical features in accordance with the general theory of baselevelling has received two notable contributions during the past year. The first is by Hayes and Campbell on the Geomorphology of the Southern Appalachians (Nat. Geogr. Magazine, VI., 1894, 63). The authors recognize the widespread occurrence of more or less fully denuded peneplains at two levels, one of late Cretaceous, the other of late Tertiary date, thus extending the conclusions reached by others farther to the north. They then proceed to measure the amount of deformation that the peneplains have suffered by drawing contour lines upon them. It appears very clearly that the axes of elevation along which these old lowlands have been arched up, coincide closely with the Appalachian axis; thus adding two more dates to the many others at which this line has been the scene of deformation. The tilting of the surface of the deformed peneplains is regarded as of importance in determining the capture and diversion of certain streams by their rivals; this principle being further illustrated by Campbell in a separate article on 'Tertiary changes in the drainage of southwestern Virginia' (Amer. Journ. Science, XLVIII., 1894, 21).

GRADED RIVERS.

A river that ceased the active deepening of its valley is by various writers described as having reached its baselevel. Thus A. Winslow writes: "The streams of the prairie country . . . . . have, in large part, reached base level, and are developing meander plains." (Missouri Geol. Survey, VI., 1894, Lead and Zink deposits, 310.) H. Gannett figures a bit of the Great Plains of Colorado as 'near base level,' although the contour lines indicate altitudes of over 4000 feet. (Monogr. XXII., U. S. Geol. Survey, 1893, pl. viii.) Now it is true that streams which have ceased the active deepening of their valleys serve as *local* baselevels for their tributaries—as Powell's original definition stated; but it seems unadvisable to speak of these streams as themselves having reached baselevel; still less is the country which slopes down to them necessarily near 'baselevel.' If the term is used in so general a sense as this, then an important feature in the development of rivers will remain undistinguished by any special name, and the attention of readers will not be forcibly brought to it. It is well known that when a river has cut down its valley and reduced its velocity to such a value that its capacity to do work in transporting waste is just equal to the work that it has to do, any further change in the profile of the stream-channel can take place only as fast as a change in the amount of land-waste offered to the streams shall allow. If the amount of waste slowly decreases, as is commonly the case, the stream will slowly assume a flatter and flatter slope (except so far as the development of meanders may lengthen its course and thus retard the deepening of its valley). If an increase in the amount of waste takes place after equality of capacity and task is reached, as sometimes happens, then the stream must aggrade its valley for a time. If the climate of the region changes, a new slope may be called for. Of two regions, similar in all respects except that one is made of resistant rocks, and the other of weak rocks, the first will develop a stronger relief during its mature dissection than the second. The Great Plains of the West are often referred to as a region of considerable elevation, in which, however, the rivers are unable to cut deep valleys on account of the rapid disintegration of the tributary slopes, and the consequent necessity of maintaining steep-sloping channels in order that the streams may do their work of bearing the plentiful waste of the land to the sea.

All this series of considerations is confused if it is said that a river which has established an equality between its capacity and its task is 'at baselevel.' From whatever profile of slope it began to work on, it has developed a profile of equilibrium, as certain French writers would phrase it; or, following a suggestion by G. K. Gilbert (Chicago Journal of Geology, II., 1894, 77), it has graded its slope; it is a graded river; it is almost balanced between degrading and aggrading its valley, and most of its activity may be given to lateral sapping. No better English term than 'grade' has been suggested for the expression of this important idea.

GEOMORPHOGENY OF NORTHERN CALIFORNIA.

THE second contribution to the general subject alluded to above is by A. C. Lawson, in account of the Geomorphogeny of the coast of northern California (Bull. Dept. Geol., Univ. of Cala., I., 1894, 241–242), which students of this new-named subject will do well to consult. Although only the report of a rapid reconnoissance, the paper announces the determination of a well-marked, uplifted and dissected peneplain, in which a fully developed system of subsequent drainage is exhibited on an extensive scale. The district is recommended to students as an inviting field for further investigation. The author brings out the point that a constructional mass of resistant rocks will never at any stage of its denudation yield a topography that may be reached at certain stages in the denudation of a mass of weaker rocks; and he therefore suggests that in the accounts of topographic development, or geomorphogeny, a factor should be introduced indicative of the rate as well as of the stage of degradation of the region concerned.

THE ESSENTIAL PRINCIPLES OF BASE-LEVELLING.

THE results gained in the two papers mentioned above, and in many other similar articles, are based on the essential principles of baselevelling: Any region must in time be reduced to a nearly featureless peneplain close to sea level; during the progress of its denudation, the forms assumed follow a tolerably well defined sequence, depending chiefly on the structure of the wasting mass; the features and arrangement of the drainage lines are essentially systematic and not arbitrary in their development. A generally accepted corollary of these principles is that a surface of denudation, having faint relief and no control by structure, can be produced only close to its controlling baselevel; and that such a surface represents the peneplain stage, attained close to the end of the cycle of denudation in which it was developed. It is evident that if a plain of denudation can be produced at a considerable altitude above baselevel, and independent of structure, then the conclusions of various investigators regarding land movements, based on the occurrence of elevated, warped or faulted peneplains, must be critically revised. It therefore behooves those who accept and employ the doctrine of baselevelling to examine carefully any alternative hypothesis by which peneplains are explained independently of baselevels.

THE GEOGRAPHICAL EDUCATION OF OUR TOPOGRAPHERS.

Some engineers hold the opinion that it is not necessary for a topographer to have an understanding of the forms that he maps; it is sufficient for him simply to record what he sees without knowing its meaning. If all topographers could sketch with minute accuracy, if they all worked on a large scale and without limitation of time, they might perhaps manage to get along without an appreciative knowledge of the subject of their sketching. But the topographers by whom our maps are made cannot as a rule sketch with minute accuracy; and even if they could, their talent would be of little avail, for time could not be given to its use; moreover, maps of a scale large enough for minute accuracy are too expensive to undertake in so vast a country as ours. In many parts of the country the land is hardly worth as much per mile as it would cost to map it in an elaborate manner. Our maps must be made on a relatively moderate scale—seldom more than an inch to a mile; expensive detail cannot be permitted; and very slow work must give way to methods that will give results more rapidly. A great deal of our topographical work must be done by rapid sketching between measured points; the sketching must always be generalized; and every thing that will promote the production of good results from rapid and generalized sketching must be taught to the topographer.

Looking at the subject in this practical manner, there can be no question that an appreciative understanding of topographical features is of great value. Rapid work by a topographer who does not understand the country before him will produce an unappreciative portrait. Generalizations by a surveyor who does not understand the relations of the forms that he generalizes will produce an unsuggestive and inaccurate map. A good understanding of physiography should therefore be regarded as an essential qualification of a topographer; and schools of engineering should see to it that adequate teaching of this subject is provided for their students.

WINSLOW'S EXPLANATION OF THE MISSOURI PLAINS.

Such an alternative hypothesis is offered by A. Winslow in his recent report on the lead and zinc deposits of Missouri (Geol. Survey of Missouri, Vol. VI., 1894). He describes certain parts of southern Missouri as exhibiting broad expanses of nearly flat land. A 'prominent feature' of the district is 'the steepness of the hills adjacent to the stream valleys' (p. 306). Another part of the same region is a dissected plateau of carboniferous strata, terminating eastward in an irregular escarpment. The even inter-stream uplands of both plain and plateau are not regarded as of constructional origin, for the region has long been above sealevel; the possibility of either upland having once been a smooth peneplain of baselevel erosion is considered and rejected; and the following hypothesis is offered in its stead: "These prairie and plateau plains are primarily due to the fact that the slope of the surfaces has always been and continues slight. . . . . Consequently, the flow of the streams has been so sluggish that general atmospheric degradation has nearly kept pace with the corrasion of the streams and formation of the valleys. As a result, the whole surface has been denuded simultaneously. This condition is attributable, first, of course, to the gentleness of the original constructional slope; the horizontality of the stratification has helped to perpetuate it. . . . . . Secondarily as a factor in the production of these surfaces, it is probable that, where streams have corraded so slowly, broad flood plains have been developed at different levels at different times. Thus many flat stretches, which may be removed from the formative streams, are, perhaps, to be considered as of the nature of terraces marking the flood plains of a past stage of erosion" (p. 322, 323). Change of altitude of the region, or in other words, change of baselevel, is not referred to as essentially involved in the problem.

The plateau surface, sloping to the west and terminating eastward in an escarpment Carboniferous strata, seems to depend on the greater resistance of these strata. It might be called a structural plain; a stripped surface on which general denudation has hesitated by reason of the endurance of the exposed strata, although the streams have deeply trenched it.

With the prairie plains the case is different, for much of their area "is underlain by coal measure rocks, which are readily acted on by sub-aerial agents of erosion" (p. 323). If the streams of the region were not enclosed by steep-sided valleys, but wandered across the plains in channels hardly beneath the general surface level, then it might be admitted that the whole surface would waste away about as fast as the streams degraded their courses. But as the streams are in well-enclosed valleys, it does not seem logical to admit that the inter-stream plains can have wasted as fast as the valley forces. If the streams of the region even now distinctly incise its surface, all the more strongly must they have done so before long continued denudation had reduced its original altitude to its present altitude. The steep valley sides should long ago have been ravined, and the inter-stream plains should thus have been unevenly dissected. If this process had been long in progress, the region might already have reached or passed through the stage of most varied relief—topographical maturity; but it could not have attained an even surface distinctly above the level of its streams. Similarly, it does not seem admissible to suppose that streams, which are now running in rather narrow, steep-walled valleys, should ever, when still higher above baselevel, have had broad flood-plained valleys, beneath which they have incised the narrow existing valleys, yet without being prompted to this change of behavior by any change of altitude in the region.

A decision as to the origin of these plains must be left to workers on the ground; but opinion as to the sufficiency of the process suggested for their production may be formed by any one who has familiarized himself with the general principles of denudation here involved. In the writer's mind Winslow's hypothesis does not invalidate the generally current principles of the baseleveling theory.

GANNETT'S MANUAL OF TOPOGRAPHIC METHODS.

THE general principle that the topographer should be well trained in physiography is strongly affirmed in Gannett's Manual of Topographic Methods (Monogr. XXII., U. S. Geol. Survey, 1893, issued in 1894). The volume contains a concise account of the surveys thus far undertaken in the United States; an account of the map now in progress by the U. S. Geological Survey, this containing much of interest to the geographical reader; and a treatment of the more technical matters of astronomical determination of position, horizontal location, secondary triangulation, sketching, and office work. In the chapter on sketching, there is an interesting discussion of the origin of topographic forms, with illustrations taken from various map sheets in the Survey office; this discussion being introduced 'as an aid in the interpretation of the various topographic forms which present themselves' to the topographer. Here we read the sound statement that "it is in the matter of generalization that the judgment of the topographer is most severely tested. He must be able to take a broad as well as a detailed view of the country; he must understand the meaning of its broad features, and then must be able to interpret details in the light of those features. Thus, and thus only, will he be competent to make just generalizations" (p. 107).

THE UPLIFT OF THE EXISTING APPALACHIANS.

The origin of topographic forms has as yet received so small a share of attention from the greater number of field geologists and geographers, and the presentation of the problems involved has as yet gained so little attention from teachers in schools of higher grade that contributions to the subject from a man of Mr. Gannett's experience and qualifications are of great value. Yet in certain parts it seems to the writer that his plan of presentation is open to criticism. He states first that topographic features originate by uplift, by deposition and by erosion. Under the heading of uplift, he writes: "The ridges and valleys of the Appalachian region are the result of uplifts, with numerous sharp folds and faults, which raised at various angles an alternation of hard and soft beds, from which erosion has since carved the existing alternations of ridge and valley" (p. 109). In spite of the qualifications of a preceding paragraph, to the effect that forms produced by uplift are during and since their rise greatly carved by erosion, the reader can hardly acquire a correct understanding of the facts concerning the Appalachian ridges and valleys from Gannett's statement; nor can he easily acquire from the Appalachians an idea of the nature of forms produced by uplift with folding and faulting. Such forms can be illustrated best by the selection of young topographic districts, on which erosion has as yet made little advance. Our western country possesses many and excellent examples of this class. Furthermore, it is no more allowable to describe the Appalachian ridges and valleys as the 'result of uplifts, with numerous sharp folds and fault' than it would be to associate the fiords of Labrador with the ancient deformation of the old rocks of that region. The Appalachian uplifts with folds and faults have long ago been consumed; the uplift from which the existing ridges and valleys are carved was a broad arching of the region, without folding or faulting of perceptible measure. It is true that the up-arched mass possessed a structure given ages before by folding and faulting; but that more disorderly kind of uplift had little in common with the broad and even uplift of the region by which its present relief was initiated. The essay by Hayes and Campbell, already referred to, gives sufficient demonstration of this important conclusion.

A FRENCH OPINION.

The following abstract from an essay entitled '*L'age des formes topographiques*' by A. de Lapparent, the eminent geologist (Revue des questions scientifiques, Oct., 1894), expresses an opinion concerning the personnel of a topographic corps that is somewhat surprising as coming from France, where we had supposed that the propriety of the military control of official geographical work was unquestioned. De Lapparent writes in effect: The distraction of our professional geographers by the study of arbitrary political boundaries in the early part of this century would have been lessened if the work of detailed mapping had been left to men ready to interest themselves in the many questions provoked by the manifold forms of land relief. Unfortunately the reverse was done in decreeing that cartography should be exclusively a function of the department of war. Up to 1830 there was in France an excellent institution, that of the geographical engineers. Well prepared in the École polytechnique, the officers of this corps devoted themselves entirely to geodesy and topography. Thus occupied they came to have a lively appreciation of the relation between internal structure and external form. Truly, geology was at that time but little advanced, but this productive combination of two orders of studies must have been of mutual advantage, had not an always regrettable decision caused the suppression of the corps of geographical engineers, and the transfer of their duties to the officers of the army staff. Certainly there was no lack of capacity among the latter, but it was nevertheless a capital mistake to entrust a service essentially civil, and even scientific, to military officers who could not devote themselves exclusively to it. Consequently, even though the maps have been well made, there has been a slow advance of what may be called appreciation of topographique form (*l' intelligence du terrain*). Certain of the more sagacious geologists in vain showed how the meaning of topographic form is illuminated when it is studied in relation to internal structure; the divorce of 1830 continued to exercise its unlucky influence, and all the more because other nations, following the example of France, have for the most part identified topographical work with that of the national defense. But a reaction has gradually set in, and to this none have contributed more effectively than the Americans; and here the author goes on to pay a high tribute to the scientific results of our western surveys.

Accepting the correctness of the principles stated by de Lapparent, it follows that our topographers can succeed in their great work only when imbued with a truly scientific spirit. There is small likelihood of this spirit being generally attained so long as engineering schools give so little attention as at present to the study of the great subject on which their topographic art is to be exercised. For this reason, such works as Gannett's Manual are particularly welcome.

W. M. DAVIS.
HARVARD UNIVERSITY.

THE NEEDS OF METEOROLOGY.

To state a problem clearly is to contribute much towards its solution; to realize one's wants and make them known may bring the needed help; therefore I accept with pleasure an invitation to speak of the needs of meteorology.

Considered as a source of climatological statistics bearing on every branch of human activity, on land and sea, meteorology has been handsomely supported for a century by all governments and scientific organizations. This feature of our work is now carried on by the U. S. Weather Bureau and the State Weather Services with increasing thoroughness from year to year.

Considered as a system for the prediction of storms and weather for a day or two in advance, meteorology has received enthusiastic support by our own and all other nations. We are now doing about all that can be done by the mere utilization of the telegraph and weather map and the cautious application of general average rules, but we are still powerless in the presence of any unusual movement of the atmosphere. Indeed, I do not see that even our West Indian hurricanes are predicted any better to-day than they were in my 'Probabilities' of August, 1871.

Meteorologists can never be satisfied until they have a deeper insight into the mechanics of the atmosphere. Something more is needed than the most perfect organization for observing, reporting and publishing the latest news from the atmosphere. It is not enough to know what the conditions have been and are, but we must know what they will be, and *why so*. We must have a deductive treatise on the laws governing the atmosphere as complete and rigorous as the 'Celestial Mechanics' of La Place, and this will necessarily be a treatise on the application to the atmosphere of the general laws of force, or what is technically known as the dynamics and thermo-dynamics of gases and vapors. Such a work cannot be written now, nor when written can it be studied successfully unless accompanied by an introductory 'Laboratory manual of physics and hydro-dynamics.'

But the preparation of this latter work demands appropriate laboratory arrangements. I will, therefore, invert the order and say that further progress in meteorology demands a laboratory and the consecration of the physicist and the mathematician to this science. Something like this was started in 1881, by General Hazen, in establishing a 'Study Room,' but it was ruled out by the report of a committee of Congress, and since that day meteorology has more than ever looked to the universities for its higher development. The applications of climatology to geology, physiography, hygiene, irrigation and other matters have been developed, but meteorology itself, the most important and the most complex of all the physical sciences, still remains to be provided for.

The crying need of this science is a *home*, a domicile, a meteorological laboratory, and full recognition as a course in university study.

Without experimentation there is no true progress in the physical sciences.

Cleveland Abbe.

Washington.

CORRESPONDENCE.

A CARD CATALOGUE OF SCIENTIFIC LITERATURE.

Editor of Science, *Dear Sir:* The efforts which students of the Natural Sciences are constantly making to provide themselves with more complete summaries of the literature of their various departments all testify to the existence of a wide-spread feeling of dissatisfaction with the existing methods of cataloguing scientific papers and reporting upon the results of scientific research. That this dissatisfaction is felt by none more keenly than by those engaged in the work is shown by the appeal made last spring by the Royal Society to various universities and learned societies for advice as to the feasibility of maintaining by international coöperation a complete catalogue of current scientific literature.

The following circular of the Society, together with the reply of Harvard University to the same, will doubtless be of interest to your readers, and by opening the columns of your journal to a discussion of the subject you will not fail to elicit valuable suggestions with regard to the details of the plan.

In adopting the recommendations of the committee as printed below, the University Council voted "that the Secretary of the Council be instructed to transmit to the Royal Society a letter stating the opinion of this Council, that the expression 'scientific literature' as used in the above recommendation ought to receive a very broad interpretation."

Yours very truly,

H. P. Bowditch.

LETTER FROM THE SECRETARIES OF THE ROYAL SOCIETY.

The Royal Society,
Burlington House, March 22, 1894.

Sir: The Royal Society of London, as you are probably aware, has published nine quarto volumes of 'The Catalogue of Scientific Papers,' the first volume of the decade 1874–83 having been issued last year.

This Catalogue is limited to periodical scientific literature, *i. e.*, to papers published in the Transactions, etc., of Societies, and in Journals; it takes no account whatever of monographs and independent books, however important. The titles, moreover, are arranged solely according to authors' names; and though the Society has long had under consideration the preparation of, and it is hoped may eventually issue, as a key to the volumes already published, a list in which the titles are arranged according to subject-matter, the Catalogue is still being prepared according to authors' names. Further, though the Society has endeavored to include the titles of all the scientific papers published in periodicals of acknowledged standing, the Catalogue is, even as regards periodical literature, confessedly incomplete, owing to the omission of the titles of papers published in periodicals of little importance, or not easy of access.

Owing to the great development of scientific literature, the task of the Society in continuing the Catalogue, even in its present form, is rapidly increasing in difficulty. At the same time it is clear that the progress of science would be greatly helped by, indeed, almost demands, the compilation of a Catalogue which should aim at completeness, and should contain the titles of scientific publications, whether appearing in periodicals or independently. In such a Catalogue the titles should be arranged not only according to authors' names, but also according to subject-matter, the text of each paper and not the title only being consulted for the latter purpose. And the value of the Catalogue would be greatly enhanced by a rapid periodical issue, and by publication in such a form that the portion which pertains to any particular branch of science might be obtained separately.

It is needless to say that the preparation and publication of such a complete Catalogue is far beyond the power and means of any single society.

Led by the above considerations, the President and Council of the Royal Society have appointed a committee to enquire into and report upon the feasibility of such a Catalogue being compiled through international coöperation.

The committee are not as yet in a position to formulate any distinct plan by which such international coöperation might be brought about; but it may be useful even at the outset to make the following preliminary suggestions:—

The Catalogue should commence with papers published on or after January 1, 1900.

A central office or bureau should be established in some place to be hereafter chosen, and should be maintained by international contributions, either directly, that is by annual or other subsidies, or indirectly, that is by the guarantee to purchase a certain number of copies of the Catalogue.

This office should be regularly supplied with all the information necessary for the construction of the Catalogue. This might be done either by all periodicals, monographs, etc., being sent direct to the office to be catalogued there, or by various institutions undertaking to send in portions of the Catalogue already prepared, or by both methods combined.

At such an office arrangements might be made by which, in addition to preparing the Catalogue, scientific data might be tabulated as they came to hand in the papers supplied.

The first step, however, is to ascertain whether any scheme of international coöperation is feasible and desirable. The committee accordingly is desirous of learning the views upon this subject of scientific bodies and of scientific men.

We, therefore, venture to express the hope that you will be so good as, at some early opportunity, to bring the matter before the Harvard University and to make known to us, for the use of the committee, the conclusions arrived at concerning it.

Should the decision you report be in any way favorable to the scheme, may we further ask you to communicate to us, for the use of the committee, any suggestions which you may think it desirable to make; as to the best methods of inaugurating a scheme; as to the constitution and means of maintenance of the Central Office; as to the exact character of the work to be carried on there; as to the language or languages in which the Catalogue should be published, and the like?

We are, your obedient servants,

(Signed) M. Foster, *Secretary R. S.*
Rayleigh, *Secretary R. S.*
J. Lister, *Foreign Sec. R. S.*

REPORT OF THE COMMITTEE OF THE UNIVERSITY COUNCIL APPOINTED TO CONSIDER THE COMMUNICATION OF THE ROYAL SOCIETY RELATING TO A CATALOGUE OF SCIENTIFIC PAPERS TO BE MADE BY INTERNATIONAL COÖPERATION.

To the University Council of Harvard University:—

The committee of the University Council, to whom was referred the accompanying circular of the Royal Society, respectfully submits the following report:

The committee finds itself fully in sympathy with the desire of the Royal Society to improve the methods of cataloguing scientific literature, and is distinctly of the opinion that the establishment of such a catalogue, to be compiled through international coöperation, is both desirable and practicable.

To determine in what way this result can be best attained, it will be well to consider what are the defects of existing methods, and what are the requirements which an improved system may be reasonably expected to fill.

Bibliographical catalogues and indexes are generally defective in one or two ways. Either they present simply a list of titles which often convey an inadequate, and sometimes a misleading idea of the contents of the articles catalogued, or they appear, like the various annual reports, so long after the publication of the articles which are reported upon that they lose a great part of their value as guides to current literature. A third defect is common to all existing catalogues, viz., that of necessitating a reference to a number of separate volumes whenever the literature of several years is to be sought for.

It is evident that some form of *card catalogue* can alone remedy these defects, so that the practical question is: How can a card catalogue of current scientific literature be best established and maintained? The requirements of such a catalogue may be stated as follows:—

1. It should appear promptly—if possible, simultaneously with the book or article catalogued.
2. It should furnish an accurate description of the purport of the book or article.
3. It should be readily accessible to all persons interested in the literature catalogued.

It seems probable that these requirements may best be met by the coöperation of a central bureau with the various publishers and editors of scientific literature in issuing with each book and with each number of every periodical a set of cards of standard size and type, each card to exhibit for a book, or for a single article in a periodical:—

1. The name of the author.
2. The title of the book or article.
3. The date, place, and house of publication of the book, or the title, volume, and page of the periodical in which the article appears.
4. A brief statement, not to exceed eight or ten lines, to be prepared by the author himself, setting forth the general purport of the book or article, so as to furnish the necessary data for cross references.

Each card should be in duplicate to permit of arrangement according to subject or author, or both if desired, and additional cards should be issued whenever the character of the title necessitates cross references. A card when printed would present somewhat the following appearance: *

Calderwood, Henry. Evolution and Man's Place in Nature. Macmillan & Co., London and New York. 1893. pp. 349. sm. 8°.

Summary: ....

Gourlay, F. The Proteids of the Thyroid and the Spleen. Journal of Physiology. 1894. Vol. xvi. p. 23–33. Plate II.

Summary: ....

The dimensions and texture of the card should be determined by careful comparison of the cards already in use in the principal libraries of the world.

Space should be left at the top of the card for writing such words as may be desired for cross references. This could best be done by each person for himself, as there would necessarily be much difference of opinion as to the number and character of the cross references desired. Furthermore, subscribers of different nationalities would wish to catalogue the same subject under different headings, *e. g.*, an article on the spleen would be catalogued by a Frenchman under *rate* and by a German under *Milz*.

* The size is here reduced.

If thought desirable, the type used in printing the cards could be kept set up till the end of the year, and then, by arranging the material according to subjects, an annual report in book form could readily be published.

A central bureau charged with the work above outlined could very properly be established under the auspices of the Royal Society. In this central office subscriptions could be received from libraries and individuals for the cards relating to the articles published in certain journals, or to the literature of certain departments of science, and the subscriber would thus receive, in weekly instalments, a complete card catalogue of all the literature in his own line of work. The cards thus received could be arranged by each subscriber so as to form the sort of card catalogue best adapted to his own needs.

Although in this scheme the greater part of the work, including the printing of the cards, would be done in a central office, yet the coöperation of the publishers could not well be dispensed with; for from them must be obtained the summaries prepared by the authors, which form an essential feature of the scheme. No difficulty need be anticipated in obtaining such summaries; for it would be the interest of the writers to furnish them, and no one could prepare them so easily and correctly as the writers themselves.

A central office with this function would readily secure the coöperation of libraries and learned societies throughout the world; and to an undertaking thus endorsed the publishers of scientific literature would doubtless lend their aid, since they would find in it a means of advertising their business. The support of such an office could be provided for at the outset by international subscription; but it would doubtless in a short time become self-supporting, since portions of the total catalogue would be needed not only in every public library, but on the study table of every serious student in every department of science.

The above report is submitted not as an elaborated plan, but as a suggestion of the end to which effort should be directed. Your committee would further express the hope that some plan may be put into operation at an earlier date than the year 1900, the time suggested in the circular of the Royal Society.

In accordance with the views above set forth the committee respectfully recommends the adoption by the University Council of the following votes:—

1. That, in the opinion of the University Council, the establishment of a catalogue of scientific literature to be maintained through international coöperation is both desirable and practicable.

2. That a copy of this report be transmitted to the Royal Society as the suggestion of a way in which this plan may be successfully carried out.

3. That the Corporation be requested to contribute a suitable sum toward the carrying-out of this enterprise, provided the plan finally adopted by the Royal Society shall appear to the University Council to be practicable.

Henry P. Bowditch, *Professor of Physiology*, Chairman.
Frederick W. Putnam, *Peabody Professor of American Archæology and Ethnology.*
Nathaniel S. Shaler, *Professor of Geology.*
Edward C. Pickering, *Paine Professor of Practical Astronomy.*
John Trowbridge, *Rumford Professor and Lecturer on the Application of Science to the Useful Arts.*
William G. Farlow, *Professor of Cryptogamic Botany.*
Henry B. Hill, *Professor of Chemistry.*
Edward L. Mark, *Hersey Professor of Anatomy.*
William T. Councilman, *Shattuck Professor of Pathological Anatomy.*
Ira N. Hollis, *Professor of Engineering.*
Hugo Münsterberg, *Professor of Experimental Psychology.*
William F. Osgood, *Assistant Professor of Mathematics.*

June, 1894.

SCIENTIFIC LITERATURE.

Systematic Survey of the Organic Colouring Matters. By Drs. G. Schultz and P. Julius. (Translated and edited, with extensive additions, by Arthur G. Green, F. I. C., F. C. S., Examiner in Coal-tar products to the City and Guilds of London Institute.) London and New York, Macmillan & Co. 1894. 4°, pp. viii + 205. Price, $5.00.

The industry of the organic coloring matters has within a comparatively few years grown to enormous dimensions, and it is becoming difficult even for the specialist in organic chemistry to keep track of the new products. In this valuable book a carefully classified list is presented of 454 dye stuffs which have been patented, and many of these are now in extensive use. All of them are derived indirectly from coal-tar. Under each dye we find the common name, together with other names sometimes used; the scientific name; the empirical formula; the constitutional formula; the method of preparation; the year of discovery; the name of the discoverer; reference to the patents granted; behavior with reagents; shade and dyeing properties, and method of employment. The original German edition is so well known, and it has acquired such a high reputation that any words of praise for the book would be superfluous. The translator's work seems to have been done with care, and he has not only furnished a translation of the original, but brought the work up to date, that is to say, up to the date of publication, for it must be borne in mind that a book treating of organic coloring matters bears to the general subject somewhat the relation that an instantaneous photograph bears to the rapidly moving object which it attempts to represent.

The authors tell us that: "The average quantity of gas tar worked up per annum is given at 350,000 tons for England, and 530,000 tons for the whole world, whilst the quantity of coke-oven tar, though constantly increasing, probably does not at present exceed 50,000 tons. It may be expected, however, that with the more general introduction of electricity for lighting purposes and the consequent diminution of the supply of gas tar, the coke-oven tar will eventually become the main source of our aromatic hydrocarbons." To this it should be added that the increasing use of 'water-gas,' in this country at least, is decreasing the supply of coal-tar, so that the time is certainly approaching when it will pay to collect the tar from the coke-ovens.

The translator expresses the hope "that this work will be found valuable not only to the technical chemist, but also to the dyer, analyst, merchant, patent agent, etc., and in fact to every one concerned with the production, handling, or use of the coal-tar colours." His hope is undoubtedly well founded. He might have added the patent lawyers, many of whom have learned to rattle off their 'ortho,' 'meta,' 'para' with a facility that would put many a modest chemist to the blush. IRA REMSEN.

Elementary Lessons in Electricity and Magnetism. SYLVANUS P. THOMPSON. New York, Macmillan & Co. 1894. Pp. 628. Price, $1.40.

The first edition of this book appeared in 1881. It at once became immensely popular, and deservedly so, on both sides of the Atlantic. The author combined in a rare degree the three principal requisites for the preparation of a good text-book. He was himself a widely known scholar and investigator in the department of science specially treated; he was more than ordinarily accomplished in the art of exposition, and he was an experienced and successful teacher. His possession of these qualifications in undiminished magnitude is evidenced in the preparation of this new edition now offered to the public, which is the original work in plan, but entirely revised and largely re-written, with an enlargement of scope sufficient to embrace the important additions to the science which have been made during the past fifteen years. To enable this to be done without undesirable condensation, the size of the volume has been somewhat increased. Indeed, one of the larger merits of the plan of the book is to be found in the conscientious retention of the long known and well established principles and facts of the science, to neglect which for the newer and more novel developments is a temptation to which too many authors of text-books in physical science have yielded. While retaining all essential 'fundamentals,' Professor Thompson has found place for the presentation of all of the essentials of recent discovery, and while this has been done with conciseness it has also been done with that clearness and logical appropriateness for which the writings of this author are justly celebrated. The wonderful results of the study of alternating currents and alternating current machinery are well presented in this edition, as are recent advances in both theory and experiment due to Hertz, Fitzgerald, Boltzmann, Lodge and others. At the end is an excellent series of questions, classified as to the chapters of the books to which they refer, which cannot fail to add much to the value of the book in use, especially for those who study without an instructor. In fact, as an 'all around' elementary text-book in electricity and magnetism it will be difficult to find another in the English language that is superior or even equal to this.

T. C. M.

The Birds of Eastern Pennsylvania and New Jersey, prepared under the direction of the Delaware Valley Ornithological Club. By WITMER STONE. Philadelphia, 1894. 8°, pp. vii+185.

Eastern Pennsylvania has long been a favorite field for lovers of birds. Audubon, Wilson, Nuttall, Cassin, Peale, Woodhouse, Gambel, Bonaparte, Heerman, Haldeman, Ord, Baird and Trumbull may be numbered among the contributors to its ornithological literature. Aside from general works and special or local papers, three publications have been devoted to the birds of this particular area: (1) Barton's *Fragments of the Natural History of Pennsylvania;* (2) Trumbull's *Birds of East Pennsylvania and New Jersey;* (3) Witmer Stone's *Birds of Eastern Pennsylvania and New Jersey.* Barton's 'Fragments' is a rare folio printed in Philadelphia in 1799, and is something of a curiosity. Trumbull's list is a carefully annotated and attractively illustrated catalogue published in Glasgow, Scotland, in 1869, and reprinted in America. Stone's 'Birds of Eastern Pennsylvania and New Jersey' is a large octavo published by the Delaware Valley Ornithological Club in December, 1894. It is a thoroughly modern work, abounding in exact data and authorities, and based largely on the field observations of Mr. Stone and other members of the Delaware Valley Ornithological Club—evidently a very active organization. It is divided into two principal parts: An essay on the Geographic Distribution and Migration of Birds; and a Systematic Annotated List of the Birds of the region. To these are added a bibliography and an index. The chapter on Geographic Distribution is subdivided into general and local parts. The general part is weak, and in the references cited some of the more recent and important papers are overlooked. The local part is excellent and gives ample evidence of Mr. Stone's familiarity with the somewhat diverse physical and faunal characteristics of the region. Some idea of its scope may be had from the headings: The Maritime Marshes, the Pine Barrens, the Cedar Swamps, the Lowlands of Pennsylvania, the Delaware Valley, the Susquehanna Valley, the Interior Uplands, the Appalachian District, the Alleghany and Pocono Mountains. This part is accompanied by a curious colored map which might be termed a physico-faunal map of Eastern Pennsylvania and New Jersey.

The Canadian or Boreal element in the fauna is restricted in Pennsylvania to "the tops of the highest mountains and the elevated plateau region, where the deep hemlock forests, with their cool brooks and dense shade, still remain undisturbed. The passage from the Alleghanian to the Canadian zone is here, as a rule, remarkably distinct, as the more northern birds keep strictly to the virgin forest." The settlement of the region has proved particularly destructive to the Canadian species. It is melancholy to be told that "where the forest has been removed the Canadian species for the most part disappear, and judging from present indications, it would seem that this element in our fauna, which once undoubtedly extended over a much greater area than at present, may soon almost entirely disappear, as the lumbermen year by year encroach upon the forest tracts."

The chapter on *Bird Migration* is full of interest and replete with new information respecting the region studied.

In the Systematic part no less than 352 species are recorded on good evidence as occurring within the area embraced by the catalogue. A new departure is here introduced which more pretentious works would do well to follow. Instead of the much abused term '*Habitat*' the 'Breeding range' and 'Winter range' of each species are given. Mr. Stone is to be congratulated upon the distinction of being first to inaugurate this reform, which is bound to come into general use in the near future. Another improvement that might be made in all lists of birds is the transfer of accidental stragglers from the body of the work to a special list at the end. Since such extra-limital species form no part of the proper fauna of a region, why should they be included among the regular inhabitants? C. HART MERRIAM.

Visitor's Guide to the Local Collection of Birds in the Museum of Natural History, New York City. By Frank M. Chapman. 1894. 8°, pp. 100. 15 cents.

One of the best and most attractive local bird lists that has ever appeared in America has been recently issued from the American Museum of Natural History, New York. While it bears the misleading title *Visitor's Guide*, only a glance is necessary to see that it is much more. It is in reality a compact treatise on the birds known to occur within 50 miles of the great metropolis.

The author, Mr. Frank M. Chapman, prefaces the list proper by 12 pages of interesting and important matter respecting the physical and faunal aspects of the region, and the birds that are found there at different seasons. The area covered by the list is unusually rich in birds, no less than 348 species being recorded as occurring within its limits. This richness, as stated by Mr. Chapman, is due in part to the circumstance that two faunas—the *Alleghanian* (or eastern division of the Transition Zone) and *Carolinian* (or eastern division of the Upper Austral Zone) meet within its boundaries, and in part to the natural advantages of the region. "Our sea-coast, with its sandy beaches and shallow bays; our rivers, creeks and ponds, with their surrounding grassy marshes; our wooded hillsides and valleys; our rolling uplands and fertile meadows,offer haunts suited to the wants of most birds. Again, our coast-line and the Hudson River Valley form natural highways of migration regularly followed by birds in their journeys to and from their summer homes."

The paper is a model of its kind and should be in the hands of all interested in the birds of New York and vicinity. It is bountifully illustrated by cuts of birds borrowed from Coues' '*Key*,' to which are added several full-page plates of groups in the American Museum.

C. Hart Merriam.

Outline of Dairy Bacteriology. By H. L. Russell, University of Wisconsin. Published in Madison, Wisconsin, 1894. Pp. vi+186.

There is no better indication of the rather remarkable advance that has been made in recent years in bacteriological matters not connected with diseases than the publication of a text-book upon dairy bacteriology. That there should be demanded for classes in dairy schools a text-book describing the various phenomena connected with bacteria in their relation to dairy matters is rather surprising when we consider the fact that dairy bacteriology itself is the result of experiments of the last very few years. Prof. Russell has attempted in this little book of about 180 pages to give an outline of the present knowledge of the relation of bacteria to milk and all its products. The book is designed originally for his classes in a dairy school, and is, as its title indicates, only an outline, not involving any critical scientific discussions. As an outline, however, it is quite complete and the treatment is satisfactory. The book will be of use not only in dairy schools, but to all who are interested in matters connected with milk or butter supply. It will also be found useful to nurses and physicians who desire a knowledge of some of the recent discussions in connection with milk bacteriology and its relations to diseases.

H. W. C.

The nature and distribution of attraction-spheres and centrosomes in vegetable cells.—John H. Schaffner. Bot. Gaz. Nov. 1894.

The author studied centrosomes found in root tips of *Allium cepa* L., *Vicia faba* L., *Tradescantia rosea* L., also in the resting cells of the epidermis of *Allium cepa* bulb scales and in the walls of *Lilium longiflorum* ovaries. The usual methods for preparing and staining the material were adopted. In addition the author used a stain suggested by

Prof. Newcombe. It is called the iron-tannin-safranin stain and consists of the following solutions: 1, 1% aq. sol. of ferrous sulphate; 2, 5% aq. sol. of tannic acid; 3, alcoholic solutions of anilin-safranin; 4, aq. sol. of picro-nigrosin. The sections are placed for thirty to forty minutes in the iron solution, washed, then placed for the same period in the tannic acid solution; again washed and replaced for a few minutes in the iron sol. After washing again they are placed in the safranin for thirty minutes; then fifteen minutes in the picro-nigrosin. This method is said to give good results.

The special results of the investigations may be summarized as follows: (1.) Centrosomes and attraction spheres are present in non-reproductive as well as in reproductive vegetable cells. (2.) In phanerogams there are two of these bodies for each resting nucleus. (3.) When the nucleus prepares to divide, one or both of the centrosomes migrate to take their position at the poles of the future spindle. (4.) Subsequently they immediately begin to divide. The division is complete in the prophase of the mother nucleus. (5.) After their migration the spheres remain at the poles of the nuclear spindle and do not change their position until the beginning of the following division. (6.) Centrosomes are persistent.

One plate and a list of thirty-three valuable references accompany the article.

Albert Schneider.

NOTES AND NEWS.

THE ELIHU THOMSON PRIZE.

The Elihu Thomson prize of 5,000 francs has been awarded to Dr. Arthur G. Webster, of Clark University, Worcester, Massachusetts. The history of this prize is, briefly, as follows :—

In 1889 the City of Paris offered a series of prizes for the best 'electric meters,' it being required that certain conditions should be satisfied, to be determined by an exacting practical test. The first prize, 5,000 francs, was awarded to Professor Elihu Thomson, who submitted the well known Watt-meter devised by him. Wishing to encourage investigation of certain theoretical questions Professor Thomson donated the prize for the establishment of a new competition, the subjects to be considered and the prize to be determined by a committee which consisted of J. Carpentier, Hippolyte Fontaine, Hospitalier, Mascart, A. Potier and Abdank-Abakanowicz. Four subjects for investigation and discussion were selected, and it was announced that competing memoirs must be submitted on or before September 15, 1893. Four memoirs were submitted to the committee; one of these was written in German, one in French and two in English. The two latter, numbered respectively three and four, related to the same subject, namely, the determination of the period of electric oscillations. On examining the memoirs the committee reported that it 'considered memoir number four to be worthy to receive the prize established by Professor Elihu Thomson,' and expressed the hope that the author will be encouraged to continue his beautiful researches.

At the same time they express their regret that they have not available another prize of the same value which they would be glad to award to memoir number three. When their desire in this respect was made known, Professor Thomson and the French and English Thomson-Houston Electric Companies joined in offering another 5,000 francs, which was awarded to the author of memoir number three. On opening the sealed envelopes containing the authors' names, it was found that memoir number four, for which the first prize had been awarded, was prepared by Dr. Webster, and number three was the joint product of Oliver Lodge and Glazebrook.

The title of Dr. Webster's memoir was 'An Experimental Determination of the Period of Electric Oscillations.'

He is to be congratulated upon so signal a success, and it is especially gratifying that an American should have come out in the lead in competition with the two distinguished Englishmen who contested with him, and especially so as their work and his were upon the same subject.

ENTOMOLOGY.

DR. McCOOK is to be warmly congratulated on the successful issue of the third and final volume of his 'American Spiders and their Spinning Work,' which has appeared four years after the second volume. The author is more at home in his delineation of the outdoor world than in systematic work, with which this volume is mainly concerned, yet he has applied himself to this task with commendable zeal and success and describes 123 species and 30 genera. Apparently (as the table of contents curiously shows) he had intended to carry his work beyond the 'orb weavers,' but his courage or his time gave out as he saw his work grow to portentous dimensions. We have to thank him for thirty large and careful plates of spiders colored, besides a mass of structural details; they will greatly facilitate future study. The price of the complete work is now justly advanced to $50. Unhappily the title page is marked 1893, though the preface is dated in July, 1894, and the volume was not issued until December, 1894.

MR. AND MRS. PECKHAM have given us (Trans. Wisc. Acad., X) a new series of their admirable experiments with spiders in a paper on their visual powers and color sense; they "prove conclusively that Attidae see their prey (which consists of small insects) when it is motionless, up to a distance of five inches; that they see insects in motion at much greater distances; and that they see each other distinctly up to at least twelve inches"; they are guided by sight rather than by smell. The experimenters are further "of the opinion that all the experiments taken together strongly indicate that spiders have the power of distinguishing colors."

CERTAINLY the University of California Entomological Society has done a unique thing in issuing from Berkeley, Cal., as a Californian journal of entomology '*The Entomologists' Daily Post Card*' at $2.00 a year. A card of regulation size and color is printed on both sides in clear type, leaving a meagre space for an address. The number before us contains an editorial on note taking, part of a list of species in Edwards's last catalogue of butterflies, and a portion of a tabular key to the genera of Nymphalidæ. It is a curious venture.

IN a recent paper on the Siphonaptera (Proc. Bost. Soc. Nat. Hist., XXVI., 312–355) Dr. A. S. Packard gives an excellent resumé of published observations on the embryology, postembryonic history and anatomy and the adult structure of the fleas, adding new data from his own preparations and numerous figures. He is led to regard them as forming a distinct order standing nearer the Diptera than any other, but with many points of relationship to the Coleoptera.

HANSEN gives in English (Ent. tidskr.' XV., 65–89, pl. 2–3) an important paper on the structure and habits of Hemimerus, a Platypsylla-like insect infesting rats in Africa, and which had previously been studied only from dried material. Saussure in particular had published a long memoir upon it, founding upon it a new order Diploglossata from its possessing, as he thought, a second labium. Hansen shows that this does not exist (it is difficult to understand how the figures of Hansen and Saussure can have been taken from the same kind of insect) and he concludes that "Hemimerus belongs to the Orthoptera, constituting a separate family very closely allied to the Forficulina." He shows from his dissections that the insect is viviparous, bringing forth one young at a time.

THE COOLING OF HOSPITALS.

Dr. Morrill Wyman, of Cambridge, Massachusetts, has published in the Proceedings of the American Academy of Arts and Sciences, Vol. 30, page 482, an interesting paper giving the results of some experiments made in the Cambridge Hospital, in which the air admitted to the wards in warm weather was cooled by passing it through pipes in which cold water was circulating, these pipes being the same as those used for warming the air in the winter by the circulation of hot water. In one experiment the external temperature was at 83° F.; there was no wind and the patients were suffering from the heat. The temperature of the water as it entered the cooling pipes was 57 to 58 degrees. An electrical fan 36 inches in diameter, making five hundred revolutions a minute, forced about 10,200 cubic feet of the warm outer air through these pipes into the ward, which contained 21,000 cubic feet. In an hour the air entering the ward was at 71° F., and the comfort of the patients was manifestly improved. But the cooling surfaces were not only the ten cooling coils of 30 square feet each, but also the four walls of the air chamber beneath the ward, being about 3,300 square feet of surface, and it is estimated that the cooling power of the coils was about one-tenth that of the walls. A month later the heat of the external atmosphere was greater and the fan was more constantly in motion; the temperature of the air chamber had increased, and that of the water had risen to 70°. The quantity of water required for the circulation was large and expensive, and it was therefore shut off. But the same amount of ventilation was continued, the air passing through the air chamber. During the summer the ward temperature gradually rose until it differed but little from that of the open air. Nevertheless, the comfort given to the patients and nurses was immediate and decided, and there was a decided feeling of freshness and freedom of air.

Dr. Wyman points out that we can do little towards lowering the temperature of the air in hot weather in the volumes required for the ventilation of a hospital. It is a question of the rate of evaporation from the perspiring surface, which is governed in a great measure by the velocity of the air coming in contact with that surface, and this is a factor which by art it is possible to control. If we try to cool the air before it enters the ward, it must be remembered that air absolutely humid, when brought into contact with warmer air also saturated, will cool the latter, which will approach dew-point, and if its moisture is condensed into visible vapor will give out heat. "Evaporation consumes heat, condensation liberates heat." "To give comfort during the excessive heats of summer the sick require three or four times the air needed for satisfactory ventilation in winter. It required 400,000 cubic feet an hour for our sixteen patients, and yet while this large quantity was passing through the ward it was only known, except at the registers, by the accompanying sense of freshness and pleasant coolness; it was never felt as a draught."

"The experience of the Cambridge hospital leads to these two conclusions: first, that fresh air directly from the open, in the quantity and manner there supplied, can be made to give great comfort to the sick during the heats of summer; and, secondly, that previous cooling of the air so supplied is difficult and practically useless."

PITHECANTHROPUS ERECTUS.

Professor Marsh has contributed to the February number of the *American Journal of Science* an account illustrated by plates of the discovery by Dubois described in Science (January 11) by Professor Brinton. A writer in *Nature* (January 24) under the initials R. L. (Professor Lankaster) holds that the remains are human, the skull being that of a microcephalous idiot. Professor Marsh writes:—

" The brief review here given of the main facts relating to this discovery, together with the figures reproduced from the memoir, will afford the reader some idea of the importance of this latest addition to the known allies of primæval man, if not to his direct ancestry. Whatever light future researches may throw upon the affinities of this new form that left its remains in the volcanic deposits of Java during later Tertiary time, there can be no doubt that the discovery itself is an event equal in interest to that of the Neanderthal skull.

"The man of the Neander valley remained without honor, even in his own country, for more than a quarter of a century, and was still doubted and reviled when his kinsmen, the men of Spy, came to his defense, and a new chapter was added to the early history of the human race. The ape-man of Java comes to light at a more fortunate time, when zeal for exploration is so great that the discovery of additional remains may be expected at no distant day. That still other intermediate forms will eventually be brought to light no one familiar with the subject can doubt. Nearly twenty years ago, the writer of the present review placed on record his belief that such missing links existed, and should be looked for in the caves and later Tertiary of Africa, which he then regarded as the most promising field for exploration in the Old World. The first announcement, however, has come from the East, where large anthropoid apes also survive, and where their ancestors were doubtless entombed under circumstances favorable to early discovery. The tropical regions of both Asia and Africa still offer most inviting fields to ambitious explorers."

SOCIETIES AND ACADEMIES.

NEW YORK ACADEMY OF SCIENCES.

The section of Geology and Mineralogy of the New York Academy of Sciences met on Monday evening, January 21, and listened to a paper by Prof. R. S. Woodward, of Columbia College, on the *Condition of the Interior of the Earth*, of which the following is an abstract. The two envelopes of the earth, the atmosphere and the ocean are important factors in the problem of the interior, and yet we know less of the condition of the outer atmosphere than of the inner earth. The atmosphere's shape we can calculate, with some approximation to the truth, as an oblate spheroid, whose polar radius is 5.4 times the earth's radius, and whose equatorial radius is 7.6 times the latter. This shape is determined by centrifugal force and gravity. Its bulk is 310 times that of the earth, but its mass is only one-millionth that of the latter. If we speak of the latter as 6642×10^{18} tons we can get some conception of the mass of the atmosphere, and of its extreme tenuity in the outer portions.

Our inferences regarding the interior of the earth rest chiefly upon four facts, viz.

1. Its shape and size, which are known with great accuracy.

2. Its surface density, 2.6.

3. Its mean density, 5.58, which is probably accurate within two units in the second decimal place.

4. The precession ratio $\frac{C-A}{C}$, in which C is the moment of inertia of the earth with respect to the polar axis, and A is the moment of inertia with respect to an equatorial axis.

These facts limit the distribution of the earth's mass. The density of the mass must increase from the surface toward the center. Various laws of its increase have been proposed, of which that of Laplace seems to be on the whole the most plausible.

It is important to appreciate that the strata rest upon one another substantially as if fluid, because the arch of the crust is so flat. The compressive stress on any portion considered as a keystone is 30 times the crushing strength of steel, and 500–1000 times that of granite and limestone, whence it follows that the earth is practically in hydrostatic equilibrium. It also follows that the pressures in the interior are excessive, and that at the center the pressure is about 3,000,000 atmospheres. The earth is 'solid,' as the word is used by Lord Kelvin, that is, it has no cavities below a comparatively shallow depth. The explanations of the changes of latitude lately advanced and based on internal hollows in which loose matter rolls around are absurd. There is perfect continuity of matter, and there is only fluidity when for some local cause the pressure is somewhat relieved. As Major Dutton has shown, the transmission of vibrations from the centrum of the Charleston earthquake indicated a medium nearly as homogeneous as steel.

Geologists have had to account for movements of the crust, such as subsidence, elevation, crumpling, folding, etc. Two elementary forces are necessarily appealed to. The first is *Gravity;* the second that due to the *Earth's Internal Heat*. The idea of the earlier geologists that the earth cooled and contracted and hence caused the disturbances has been mostly relied on as an explanation, but for the last ten or fifteen years it has been felt to be insufficient. The idea of Babbage and Herschel that loaded areas, or areas of sedimentation, sink and crumple up the adjacent areas as mountains, tending thus to renew and perpetuate regions of upheaval, has also had believers. This has had its best formulation in the recent doctrine called *isostasy*, which regards the earth as a body in essentially hydrostatic equilibrium, and as balancing inequalities of pressure by subterranean flow. The speaker regarded this doctrine, however, as insufficient in that it furnishes no start and tends to run rapidly down. We need secular contraction to keep isostasy at work. The earth's internal heat is the great store of energy available for this purpose. How to explain the earth's internal heat is a hard and dark problem. The nebular hypothesis, first outlined in Leibnitz's Protogea has been most widely believed. The critical stage in this method of development came when convection ceased and the sphere was all at the same temperature, the stage usually called *consistentior status*. Then came the formation of a crust and the beginning of geological phenomena as usually discussed. The speaker had reason to question the reliability of the nebular hypothesis and whether the earth had ever been gaseous, etc. An origin for the globe and an explanation of its heat are perhaps as well to be found in the collision of meteoric bodies.

The time that has elapsed since the *consistentior status* has been an interesting subject for computations, and widely varying estimates have been made. Lord Kelvin in 1862, on very questionable data, placed the limits of geological phenomena at 20,000,000–400,000,000 years in the past. On the same line, Tait estimated 10,000,000, but it was doubtless true that in England the weight of Kelvin's authority had fettered geological thought in the last thirty years to too narrow limits of time, for no geologist of eminence had questioned his results. Yet within a month Lord Kelvin has raised his upper limit to a possible 4,000,000,000. All must appreciate that if the data are unreliable, the finest processes of mathematics will lead to no certain result.

The speaker concluded that to secular

cooling must be attributed the principal motive force. The main criticism raised against it is its insufficiency, but George Darwin has shown that as a cause it can be mathematically shown to be able to produce results at least of the same order as those observed. In the speaker's estimation it is probably sufficient, although the heat radiated is a very difficult thing to measure in a reliable way. Our data are all from the continents, and they have not been obtained in sufficient quantity. The oceanic areas are necessarily unobserved.

In discussion Professor Kemp stated that attention had been naturally been drawn to the interior of the earth in the endeavor to explain, first of all, the contrasts of the continental elevations and the oceanic abysses, and secondly, the crumplings, foldings and faults of mountainous regions. Herschel's explanation, while rational and simple on the face of it, is inapplicable because it is the area of sedimentation, subsidence and 'overloading' that later on is upheaved in the mountains, and this apparent contradiction is the great difficulty. He also referred to the measures of rigidity of the crust, to the remarkable localization of the yielding along narrow lines when it did come, and to its great effects and relatively short duration. He asked Professor Woodward also to touch on the slowing up of the revolution of the earth and the consequent readjustment of the spheroid to the loss of centrifugal force, an idea advanced some years ago by W. B. Taylor.

In reply Professor Woodward admitted that the questions were old and very difficult ones, and that for the mountains he had no explanation to advance. He spoke of the mountainous protuberances as measures of the rigidity, and yet this must be qualified by the statement that according to isostasy and to recent pendulum observations they appear to be somewhat lighter under the surface. As to the slowing up of rotation and loss of centrifugal force, the idea was an important and valuable one, but it did not appear to be sufficient to account for the results.

Professor Rees referred to the recent observations on changes in latitude made under his direction, and to certain factors that entered into the calculations which would throw light on the question.

Professor Hallock brought up the recent results of experiments on the gyration of liquids as bearing on the question and proving that a fluid set in rapid rotation continues to gyrate long after the enclosing vessel ceases. The curious results obtained at the Waterville arsenal in the great testing machine were also cited. The attempt was made to burst a cast iron cylinder by forcing into it, through a three-sixteenth of an inch hole, paraffine and tallow. But it was found that both these substances became, under high pressures, more rigid than steel and could not be driven through the hole.

Prof. Britton asked Prof. Woodward if the amount of heat radiated per annum could be quantitatively expressed, and in reply Prof. Woodward said it is computed from very meagre data to be enough to melt a layer of ice 5 to 7 mm. thick over the earth's surface. The chairman, Prof. R. P. Whitfield, in closing the discussion called attention to the fact that the submarine crumpling and upheaval were not well known nor often taken into account, and yet they probably far exceed all that we see on the continents.

The discussion will be continued at the meeting of the Section, February 18.

J. F. Kemp, *Recording Secretary.*

SCIENTIFIC JOURNALS.

AMERICAN JOURNAL OF SCIENCE, FEB.

Relation of Gravity to Continental Elevation: By T. C. Mendenhall.

Observations upon the Glacial Phenomena of Newfoundland, Labrador and Southern Greenland: By G. F. Wright.

Recurrence of Devonian Fossils in strata of Carboniferous Age: By H. S. Williams.

Constituents of the Cañon Diablo Meteorite: By O. A. Derby.

β - Bromvalerianic Acid: By J. G. Spenzer.

The Inner Gorge Terraces of the Upper Ohio and Beaver Rivers: By R. R. Hice.

The Glacial Land-Forms of the Margins of the Alps: By H. R. Mill.

Distribution of the Echinoderms of Northeastern America: By A. E. Verrill.

Lower Cambrian Rocks in Eastern California: By C. D. Walcott.

Pithecanthropus Erectus, Dubois, from Java: By O. C. Marsh. (With Plate II.)

Scientific Intelligence: Chemistry and Physics; Geology and Mineralogy; Botany; Miscellaneous; Obituary.

AMERICAN CHEMICAL JOURNAL, FEB.

Researches on the Complex Inorganic Acids: By Wolcott Gibbs.

Diazobenzene Aniline Chloride: By J. H. Kastle and B. C. Keiser.

On Imido-Ethers of Carbonic Acid: By Felix Lengfeld and Julius Stieglitz.

On Some Bromine Derivatives of Paraisobutyl Phenol: By F. B. Dains and I. R. Rothrock.

On the Action of Acid Chlorides on the Methyl Ether of Paraisobutyl Phenol: By F. B. Dains.

The Effect of Hydrolysis Upon Reaction-Velocities: By F. L. Kortright.

On the Influence of Magnetism on Chemical Action: By F. A. Wolff, Jr.

Reviews; Notes.

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*LABORATORY TEACHING OF LARGE CLASSES.**

TEACHING may be subdivided into two kinds: First, that which cultivates the faculties of the individual, increasing his ability to work for himself and enabling him to use his intellectual powers with confidence in the acquisition of knowledge. Second, that which disregards or takes for granted that he has these powers at his command and strives to increase his store of information. The first process is the improvement and building up of the intellectual forces by any means that will enable them to do their work thoroughly and correctly, and the second is practically, except in so far as it can be used in carrying on the first process, a load carried by the brain. Similar in fact, though not in kind, to the extra fats and extra growths, of all sorts carried by the body, it may sometimes be of advantage, and sometimes, when in unnatural proportion, a serious and perhaps even an injurious burden.

The cultivation of the original powers of the individual, of his whole mind, with, of course, proper regard for moral and physical well-being, which are, in my opinion, equally important and essential, is akin to the treatment by which a good teacher of athletics strives to improve the native strength of a pupil and give the muscles endurance and force, and which the young gymnast himself is taught how to use to the greatest advantage. This athletic training must go hand in hand with judicious feeding, and in the parallel processes of education similar objective training must go hand

*Annual discussion before the meeting of the American Society of Naturalists, Baltimore, December 27, 1894. The continuation of this discussion by Professors Bumpus and Ganong will be published in the next issue of SCIENCE.

in hand with a legitimate amount of information. It is striving against nature to throw a pupil wholly on his own resources and allow him to find his way alone. This effort not only wastes valuable time, but it is an attempt to return to primitive conditions and to produce unfavorable surroundings that do not exist at the present time. This kind of teaching is fortunately very rare nowadays, and usually an ideal that it is impossible to pursue consistently in actual practice.

The natural and right course is really followed by a very good teacher, and strives not only to exercise and train his pupil's faculties, but at the same time or at proper intervals furnishes information that will give needed nourishment and renewed strength to his power of doing pioneering work, if he be capable of this higher order of effort.

The happy combination of self-culture and sufficient intellectual meals is by no means easy, and they are mingled in due proportion only when the pupil can be employed for much the largest part of his time in the handling of objects or experiments under proper direction, which will enable him to build up by his own experience methods suitable for his own mind, or, failing that, to at least learn how original work has been done by others.

Laboratory teaching is an effort made now by all institutions to furnish proper facilities for practical instruction of this sort, and it is successful or the reverse, according to the proportions in which the system adopted deviates from the happy medium in which neither self-culture nor the administering of information is allowed to usurp the whole field.

This being a partial description of Laboratory teaching, it is a question in the minds of most naturalists whether it is in a strict sense applicable to large classes except under very exceptional conditions. In the first place, what is a large class; is it forty, eighty, or one hundred and sixty? Can the class be taken in sections or must it be handled as a whole? Can the instructor command laboratory facilities in the shape of rooms, tables, specimens or instruments, and materials for observation or experiment, and, above all, can he command assistants? All of these queries must be replied to in some shape by each instructor before it is possible for him to consider the subject from any practical point of view.

It is obvious that laboratory exercises and information must be individualized to be of the highest standard, and this could not be carried out fully by an instructor alone, except for a small class. There is also an obvious limitation of numbers due to the necessary limitation of facilities that can be offered by any institution, however well equipped. Even if an instructor had an enormous laboratory capable of accommodating a large class and money to employ the best of assistants, it is also obvious that the larger the class the further removed the members must be from personal contact with their teacher, and as individuals consequently less able to benefit by his experience and by his example, these two last being perhaps after all among the most important elements of good teaching.

Assistants come not only between the head teacher and his pupils, but where there are many minds there must be some strict system and set ways of doing the work, and more or less disregard of the peculiar needs of each individual. It is, however, evident that as long as this is recognized as a necessary evil, and the red tape of the system regarded in this light and not exalted into a fetish of productive virtue, a very large class may be kept at work through assistants, if they are allowed to have some individuality themselves and are taught to cultivate the same gift in some of their pupils. In such matters, however, one must speak from the fulness of his own experience, and I must leave this subject to those who have had experience in conducting large laboratories crowded with pupils.

My own experience has been with a few, unfortunately with very few pupils of the highest grade, and then, skipping all intermediate grades, next with pupils who have come to me uninformed or worse off in being burdened with undigested information. The first of course had almost unlimited time and ample facilities, and, therefore, do not come into consideration here.

My classes have varied from ten to five hundred, but unluckily the binding force of the conditions under which instruction was given did not vary in the same proportion. I have always been obliged to give lessons to the whole class at once, and the time has been invariably limited to comparatively few hours.

Under these somewhat difficult conditions it became necessary to adopt some system that would include, as far as practicable at least, the idea of self culture, so that the pupils would at any rate not be led into the belief that they knew how to handle and use a subject when they really had only acquired some information and the power to read about it more intelligently, and perhaps also the ability to recognize certain facts of which no educated man should be ignorant.

Permit me to exercise one of the usual privileges of every speaker and enlarge somewhat the boundaries of this discussion by asking you to consider Laboratory Teaching as but one branch of object teaching. We shall then be able to regard it from the point of view of its essential character and see more clearly its application to cases in which large classes must be dealt with in lecture rooms capacious enough to hold from eighty to five hundred or even more persons. It may then be said, that in proportion as a lecturer follows objective methods and clings to the habit of making his audience see, each for himself or herself, the objects he is talking about, in just the same proportion is he trying, at least, to educate them according to the ideal standards.

Some twenty-four years ago the Teachers' School of Science was begun in Boston and it became necessary to decide how the lessons should be conducted. To be faithful to the ideals of science and handle a class as large as could be comfortably seated in the lecture room of the Boston Society of Natural History was the practical problem, and secondarily how to do this so as to lead to the final adoption of natural history teaching in the public schools.

Two necessary conditions were assumed as the basis of the system adopted : first, the actual study of specimens, and second, the subsequent possession of these by the teachers. This system was inaugurated with a class of eighty, and was found to be practicable with five hundred persons, and succeeded as well as could be expected with such large audiences. The lecturers employed by the school, which subsequently came for the most part under the patronage of the Trustee of the Lowell Institute, Mr. Augustus Lowell, were instructed to conform to the requirements mentioned above, and the system has not been materially altered since the beginning, except in one of these requirements. Of late years it has not been deemed necessary to have very large classes, nor to distribute specimens in such profusion as during the earlier years in which hundreds of thousands had been given away. The details are very simple. Every person in the audience is furnished with a certain number of specimens. These are placed by assistants upon temporary tables opposite each chair before the lesson begins. The tables for large classes were of the simplest description, mere boards with a slight moulding to keep objects from rolling off. They were made in sections and were fastened to the floor by iron stanchions that could be unscrewed and removed after they were no longer needed. It occupied three men about one-half of a day to put them up and take them down and store them away from a lecture room accommodating five hundred persons. The seats in this room were the ordinary distance apart and were not constructed especially for the purpose of giving additional room for these tables. Similar tables have also been built and used successfully in several different lecture rooms.

The lecturer leads his hearers to observe with the specimens in hand certain facts, and he may if he chooses go far beyond these simple observations in his remarks, and he very often does this, but the specimens are dead weights upon his flights into the empyrean of fancy or theory. The objects are there; they demand constant attention, and the teacher cannot keep away from their consideration, nor can his audience lose consciousness that they are the subjects upon which the work is to be done.

The principal difficulty is to acquire the habit of carrying on the thread of the discourse, directing it to some definite morphologic point, or whatever the lecturer may choose, weaving the facts shown by the specimens into a demonstration of this point, and at the same time keep, the pupils at work upon the specimens to such an extent that most of them actually see the needed facts.

The field capable of being illustrated and taught in this way is necessarily limited, and there are in each branch of science certain series of facts requiring elaborate apparatus or rare specimens that cannot be used in sufficient numbers. Nevertheless, the limits in each department are not so narrow as one at first thinks, and the field covered grows continually broader in proportion to the ability and experience of the instructor.

The first expense is not large; the cost of the Geological and Mineralogical specimens was about ten dollars for each lecture, and for Botany about fifteen, and Zoölogy twenty to twenty-five for audiences of five hundred.

But before entering upon the second part of my subject, the application of this method to smaller classes, permit me to say that diagrams were used in order to direct the attention of the audience to the facts to be observed, and they were encouraged to make notes and sketches and instructed in the use of a cheap magnifier costing from sixty to seventy-five cents. The lecturer was allowed also to place objects similar to those in the hands of the audience upon his table and platform and on the tables in the body of the lecture room, and whenever practicable these were living representatives of the preparation.

It is needless to say to this audience that no claim is made here to the discovery of a royal road to knowledge. The system itself is an ancient one and was used before I was born by many persons. The habit of observing accurately cannot be formed by an hour or two of work on Saturday afternoon, even with the use of specimens. The method has, however, a valid claim to consideration in so far as it possesses great advantages over the subjective methods of the ordinary lecture, when illustrated solely by diagrams or stereopticon pictures, and its results are far more satisfactory.

All lessons or lectures away from the actual presence of the objects described or discussed throw the individual back upon his own mental processes, unless he already has experience and knowledge of the facts treated. Illustrations in the shape of diagrams or stereopticon pictures are substitutes of one dimension; they have the superficial attributes of length and breadth, but their apparent thickness and solidity are artistic shams. People who are taught in this way think afterwards in a weak subjective form. The objects depicted are present to them as pictures, not as things. The classes in drawing at the Lowell Institute were objectively taught and not permitted to draw from illustrations, but among the pupils there were often some that had had instruction according to this method. They were seated and placed so that no two persons got the same view of a cube mounted on a stand, in one of their lessons. Those that had had no training from copying flat illustrations tried to depict what they saw; those that had had this sort of training usually outlined the cube as they thought a cube ought to appear, giving it the conventional shape and aspect it had in their own minds. Whether seated on the floor or on chairs, or standing, in front or at one side, the cube almost invariably appeared on their sheets with the top side in perspective, whether they saw the top or did not see it from their station.

Able pupils carry away from our lessons much more than they can from lectures, however elaborately adorned with illustrations, and our results show that even the crudest efforts to observe facts with examples in hand lead often to a realization of the effectiveness of objective work and a desire for more culture in this direction.

These lectures to large audiences created gradually a demand for more precise and extensive instruction in some of our pupils, and this demand led to the giving of series of lessons on the same subjects to more limited numbers and extending over longer periods of time. These have lately taken the form of consecutive courses running through each winter for four years. We have just finished one on Geology, of this amount of time, about one hundred and thirty hours in all, and another in Botany, and have still another in Zoölogy and Paleontology, of about the same length, which will be finished this year.

In these classes numbering from thirty-six to forty-eight the lecturer treats the audience in much the same way as far as specimens are concerned, but having more time and more control over the pupils, he can do his work more effectually. Each person must have a note-book and magnifier; microscopes are furnished by the Society. The pupils are told that they must make notes, and must make sketches of the specimens. Those who state that they cannot draw are instructed to try and are shown that the quality of their drawing is not of so much importance as the employment of the eyes and mind in trying to draw the object before them. The act of trying to draw a specimen is not absolutely essential to the success of this method, but it is very helpful. It holds the pupil down to his work, keeps him constantly observing, and he soon learns to make approximately a good outline of the specimen, and then studies the details much more closely than he would otherwise do, if not making an effort to represent them on his sketch.

Different teachers have different ways of doing their work, but in general it may be said that those at present at work in the school follow this process more or less and also hold either examinations at stated intervals or have reviews in which the students are questioned with regard to what they have been studying and so on. One gentleman keeps a complete card catalogue of names and marks, so that he can follow accurately the exact course of each pupil back through the entire four years, and he holds no final examination, preferring to make his work perfect as he goes along.

Two of the teachers—there are only four in all—have constantly had the services of two assistants who helped to set out the specimens and clear the tables after the lessons were finished. These assistants were for the most part selected from the audience, and can generally be obtained in this way, either as voluntary laborers or for a very moderate compensation. These persons, under the direction of the teacher, help him to supervise the note making and microscopical observations of the pupils and help them to see and discuss with them the facts that they observe. A large part of every lesson is passed in the description and discussion of observations made on the specimens. The pupils are also encouraged to work independently in making connected studies and collections out of doors, and to embody the results in reports and actual collections presented at the final examinations or at the close of the term.

Field work is also carried on in connection with the laboratory lectures in mineralogy and geology, and it is proposed to do the same when opportunity offers in other branches. One can judge in part of the ability and attainments of classes by the examination papers and their note-books kept through the term, and the results in this direction have been highly satisfactory. I have not had time to gather any of these evidences as I had intended to do, and I shall have to ask you to take my word for it that these were more than creditable. I have brought a few placards of the courses and of the questions for the final examinations in two of the courses, which I have exhibited for your inspection.

The persons attending these lessons were all adults and mostly teachers in the public schools, but the same method has been found to be equally successful with classes of the Institution of Technology and Boston University, and no difficulty has been experienced in handling them in this way beyond what is usual with such pupils. Pupils of the Teachers' School of Science have also applied the same method to large classes of young people of both sexes in the public schools, and by covering less ground at each lesson succeeded with them also.

In certain subjects, such as Physical Geography, Chemistry and Physics, and Physiology, and so on, this method has a necessarily more limited application than in the branches enumerated above, but even in these departments it has been more or less used, directly by selecting the few experiments that could be actually made by individuals in the audience, and indirectly by showing others on the platform that could be repeated by them with apparatus that they could make themselves, or purchase with very small outlay.

Permit me in conclusion to repeat that it has been thoroughly tested with such classes of persons of all ages as have been described, but it has not yet, as far as I know, been applied to large classes of students in any university. If it has been applied to such classes by any one their experience is probably known to some persons in the audience, and I shall be glad to hear what the results have been. I am aware that our experience will probably be of real value only to those who have to deal with classes having at their command a limited number of hours and but little chance for laboratory work outside of the hours devoted to the lessons. Nevertheless, there are many who now lecture with illustrations, diagrams and the stereopticon, to whom I would with all deference suggest the possibility of adding to these specimens distributed among their pupils. And I further make bold to recommend that those who make partial use of text-books, as aids for the pupils to study and recite from, drop a part of these requirements and allow their pupils to substitute actual work on specimens done inside or outside of the class-room, collections made by themselves and so on. I also crave their permission to suggest one feature of our examinations which you will see mentioned on the cards I have displayed. This consists in placing before each pupil a set of test specimens which he is required to place in proper sequence as regards their mutual relations, to number, name, describe, and so on, in accordance with what he has been taught. I have myself a way of slipping into this set one object that the pupils have never seen, so far as I know their studies. The replies to this silent questioner frequently enable me to determine who are the best observers and most original thinkers, and very often point out clearly the difference between them and those who are merely the best students.

Whether this system is the best that can be devised or has only some praiseworthy feature, or is in reality but a poor substitute for a good one, I shall not pretend to decide. There are numbers of scientific teachers of great experience and learning present who have heard my arguments and must be our judges, but I think they will all indulgently agree that the teachers who have adopted and elaborated this method have tried to come as near to the ideals of objective work as the adverse circumstances of large classes and limited time would permit.

ALPHEUS HYATT.

BOSTON.

ORIGINAL RESEARCH AND CREATIVE AUTHORSHIP THE ESSENCE OF UNIVERSITY TEACHING.*

THAT which is most characteristic of the present epoch in the history of man is undoubtedly the vast and beneficent growth of science. In things apart from science, other races at times long past may be compared to the most civilized people of to-day.

The lyric poetry of Sappho has never been equalled. The epic flavor of Homer, even after translation, comes down to us unsurpassed through the ages. Dante, the voice of six silent centuries, may wait six centuries more before his mediæval miracle of song finds its peer.

* Inaugural Address by the President of the Texas Academy of Science, Dr. George Bruce Halsted, October 12, 1894.

The Apollo Belvidere, the Venus of Milo, the Laocoön are the glory of antique, the despair of modern sculpture. To mention oratory to a schoolboy is to recall Demosthenes and Cicero, even if he has never pictured Cæsar, that greatest of the sons of men, quelling the mutinous soldiery by his first word, or with outstretched arm, in Egypt's palace window, holding enthralled his raging enemies, gaining precious moments, *time*, the only thing he needed to enable him to crush them under his dominant intellect.

There is no need for multiplying examples. The one thing that gives the present generation its predominance is science. The foremost factor in modern life is science. All criticisms of the scope of life, of the essence of education, made before science had taken its present place, or attempting to ignore its prominence, are obsolete, as are of necessity any systems of education founded on pre-scientific or anti-scientific conceptions.

Unfortunately there are still some people so dull, so envious, so unscientifiic, so stupid as to maintain that the highest aim of a university should be the *training* of young men and young women, where they use the word 'training' in its repressive, inhibitive sense. The most profound discoveries of modern science unite in replacing this old 'training' idea of education by one immeasurably higher, finer, nobler. We now know that the paramount aim of teaching at every stage, and preëminently of the final stage, at the university, should be to *help* the developing mind, the developing character, the developing personality. Judicious, delicate, sympathetic *help* is now the watchword. Even horses and dogs worth owning are no longer 'broken;' they are 'gentled.'

What has brought about this glorious change? *Science*, the greatest achievement of human life, the one thing that puts to-day, the present, in advance of all past ages. Not only by having subjugated the forces of nature to the dominion of mind, but also by its intellectual influence, science is remodelling the life and thought of modern humanity.

Though science is the purest knowledge, yet even our estimate of knowledge has been changed by science. Mere acquirement is now considered an unworthy end or aim for endeavor. Action, production alone now receives our homage, now gives a life worth living; and, therefore, each must aim either at the practical application of his knowledge, or at the extension of the limits of science itself. For to extend the limits of science is really to work for the progress of humanity. This is a fitting crown to the sweet and symmetrical evolvement which true teaching aids—the unfailing spring of pure pleasure which it affords. The laws of physical, but, above all, of mental health, made clear by science, let every one realize how now our truest education stands ready to aid, to save, to satisfy endangered or craving bodies or minds. Nothing is more beautifully characteristic of young children than the desire to know the why and wherefore of everything they see. This natural spirit of inquiry needs only proper direction and fostering care to give us scientists. But no one can teach science who does not know it. For a teacher, however subordinate, to have the true informing spirit to vivify his book-knowledge, even of the very elements, it is found almost uniformly essential that he should have been in direct personal contact with some one of those great men whose joy it is to be able to advance the age in which they live, and lead on mankind to unexpected victories in the progressive conquest of the universe. But it is the highest function of a university to help the gifted young man on his way toward becoming one of these glorious creators, these men who make and who honor the age in which they live. A university should wish to feed the mental leaders of the next generation. For this nothing can take the place of contact with the living spirit of research, original work, creative authorship.

Without fostering and requiring such work of students and still more of all its professors, no institution can be a university of the first class. Intimate contact with a producer of the first rank is worth more than the whole world of so-called training by use of retailed convictions.

The most inspiring teacher must have known how to acquire conviction where no predecessor had ever been before him; to show others how to conquer new regions, he must himself have broken barriers for human thought. As Rector of the University of Berlin, Helmholtz said: "Our object is to have instruction given only by teachers who have proved their own power to advance science." There is no honest test or proof of scholarship or acquirement but production. The characteristic quality of all the highest teaching lies in the fact that it comes from a creator.

No more convincing demonstration of my thesis could be wished for than the work of Sylvester for America. On page 233, I., of his Höhere Geometrie, 1893, Felix Klein, as high an authority as any living, says: "Sylvester hat noch 1874 als 60 jähriger Mann den Mut gehabt an die Johns Hopkins University in Baltimore ueberzusiedeln und durch eine ganz specifische durch 10 Jahre fortgesetzte Lehrthätigkeit höhere mathematische Studien auf amerikanischen Boden zu initiiren."

The birth of higher mathematics in America will always date from Sylvester's advent at the Johns Hopkins. There and then with his mighty head he raised the whole western continent, and made it a worthy associate in the profoundest thought-life of our world. But few know that this epoch-making period was not Sylvester's first advent in the United States. The immortal glory now belonging to the Johns Hopkins University might have been anticipated by another, and with the very same instrument.

An adequate life of James Joseph Sylvester has never been written, and probably never will be while he lives. At Cambridge he was most impressed by a classmate of his own, the celebrated George Green, who had already then produced the remarkable Green's Theorem, and much of the work which still stands as a foundation stone in the edifice of modern electrical science. As Sylvester would not sign the thirty-nine articles of the Established Church, he was not allowed to take his degree, nor to stand for a fellowship, to which his rank in the tripos entitled him.

Sylvester always felt bitterly this religious disbarment. His denunciation of the narrowness, bigotry, and intense selfishness exhibited in these creed tests was a wonderful piece of oratory in his celebrated address at the Johns Hopkins University. No one who saw will ever forget the emotion and astonishment exhibited by James Russell Lowell while listening to this unexpected climax. Thus barred from Cambridge, he accepted a call to America from the University of Virginia.

The cause of his sudden abandonment of the University of Virginia is often related by the Rev. Dr. R. L. Dabney, as follows: In Sylvester's class were a pair of brothers, stupid and excruciatingly pompous. When Sylvester pointed out one day the blunders made in a recitation by the younger of the pair, this individual felt his honor and family pride aggrieved, and sent word to Professor Sylvester that he must apologize or be chastised.

Sylvester bought a sword-cane, which he was carrying when waylaid by the brothers, the younger armed with a heavy bludgeon.

An intimate friend of Dr. Dabney's happened to be approaching at the moment of the encounter. The younger brother stepped up in front of Professor Sylvester and demanded an instant and humble apology.

Almost immediately he struck at Sylvester, knocking off his hat, and then delivered with his heavy bludgeon a crushing blow directly upon Sylvester's bare head.

Sylvester drew his sword-cane and lunged straight at him, striking him just over the heart. With a dispairing howl, the student fell back into his brother's arms screaming out, "I am killed!" "He has killed me." Sylvester was urged away from the spot by Dr. Dabney's friend, and without even waiting to collect his books, he left for New York, and took ship back to England.

Meantime a surgeon was summoned to the student, who was lividly pale, bathed in cold sweat, in complete collapse, seemingly dying, whispering his last prayers. The surgeon tore open his vest, cut open his shirt, and at once declared him not in the least injured. The fine point of the sword-cane had struck a rib fair, and caught against it, not penetrating.

When assured that the wound was not much more than a mosquito-bite, the dying man arose, adjusted his shirt, buttoned his vest, and walked off, though still trembling from the nervous shock. Sylvester was made head professor of mathematics of the Royal Military Academy at Woolwich, a position which he held until the early period set by the English military laws for conferring the life-pension.

He thus happened to be free to accept a position at the head of mathematics in the Johns Hopkins University at its organization. With British conservatism, he stipulated that his traveling expenses and annual salary of five thousand dollars should be paid him in gold, and this fixed, he came a second time to America.

The fame of his coming preceded him, for by this time he was ranked by Kelland in the Encyclopædia Britannica as the very foremost living English mathematician. The only possible sharer of this proud preëminence was his life-long friend Cayley.

Appointed among the first twenty fellows at the organization of the Johns Hopkins University, and having an intense desire to study Sylvester's own creations with him, I became alone his first class in the new University. Sylvester gives in his celebrated address a graphic account of the formation of that first class as illustrating the mutual stimulus of student and professor.

The text-book was Salmon's Modern Higher Algebra, dedicated to Sylvester and Cayley as made up chiefly from their original work.

The professor broke every rule and canon of the Normal Schools and Pedagogy, yet was the most inspiring teacher conceivable. Every thing, from music to Hegel's metaphysics, linked into the theory of Invariants, combined with the precious personal data, and charming unpublished reminiscences of all the great mathematicians of the preceding generation.

Such a course in the creation of modern mathematics, with most precious, elsewhere unattainable, historic indications, will perhaps never be paralleled. It went on not only at the appointed hours, but the professor would send for his class at night, while at other times they took excursions together to Washington. The incidents of these two formative years, spent in most intimate association with one of the great historic personages of science, can never be forgotten. It was during this period that Sylvester founded the American Journal of Mathematics, and it is due to his particular wish that it was given the quarto form.

Then began a new productive period in his life, the astounding activity and marvelous results of which can be faintly estimated by consulting the pages upon pages taken up in the Johns Hopkins Bibliographia Mathematica, merely to enunciate the titles of the memoirs and papers produced. The very complete and profound historic and bibliographic account of the theory of Invariants given by Meyer in the Berichte of the deutsche mathematische Gesellschaft indicates very fairly Sylvester's final place in the history of that all-pervading subject. His original contributions to many other parts of the vast structure of modern pure analysis are of nearly as great weight.

Sylvester was completely of the opinion that no teaching for a real university can be ranked high which is not vitalized by abundant original creative work. He maintained that it was the plain duty of any mature man holding a professorship in a real university to resign at once if he had not already been copiously and creatively productive.

He believed that without unceasing original research and published original work there could be no real university teaching, and that any university professor who, without such a basis, pretended to be a good teacher, was, consciously or unconsciously, a selfish fraud.

On page 6 of his address delivered on Commemoration Day, 1877, he speaks of a university 'under its twofold aspect as a teaching body and as a corporation for the advancement of science.' He then continues; "I hesitate not to say that, in my opinion, the two functions of teaching and working in science should never be divorced.

"I believe that none are so well fitted to impart knowledge as those who are engaged in reviewing its methods and extending its boundaries . . . May the time never come when the two offices of teaching and researching shall be sundered in this University!"

This was spoken of the Johns Hopkins. Since then no university has voluntarily avowed an ideal not equally noble and exalted. Science, penetrating ever deeper, makes clear the conditions of progress, of true education, of finest teaching.

Only those who have produced can adequately fulfill its present motto: "I serve, I help." GEORGE BRUCE HALSTED.

UNIVERSITY OF TEXAS.

THE ARCHÆOLOGY OF SOUTHERN FLORIDA

THROUGH the investigations of Professor Jeffries Wyman, Mr. A. E. Douglass and lately of Mr. Clarence B. Moore, a large amount of accurate information about the mounds of central and southern Florida has been laid before the public. Especially noteworthy are Mr. Moore's explorations, which have been published with every desirable addition of maps, measurements and illustrations. They were conducted with a fidelity to the correct principles of mound excavation, which renders them models of their kind. The results were rich, instructive, often surprising, such as copper breastplates and ornaments, curiously decorated pottery, specimens of Catlinite, and little earthern images, very life-like, of the bear, squirrel, wildcat, and even the tapir, which latter had become extinct in Florida when the whites first explored it.

Nothing, however, which has been found in the mounds of Florida justify us in separating them as a class from other mounds in the Southern States; there is nothing in them 'extra-Indian,' as Mr. H. C. Mercer remarks in his review of the subject in the *American Naturalist* for January. He might have gone further and have said there is nothing extra-North American Indian. The pottery decoration does not reveal those arabesque designs which Mr. Holmes has pointed out in some of the more modern pottery of the Gulf coast, as indicating Caribbean or Antillean influence. If that arrived, its arrival was later than the construction of the older Floridian mounds.

But an obscurity certainly hangs over the ethnography of Florida at the period of the discovery.

A large part of the peninsula was peopled by a tribe whose language stood alone on the continent, the Timucuas, and which became extinct generations ago, though fortunately reserved in the works of a Spanish missionary, Father Pareja. They are described by the Spanish and French explorers of the sixteenth century as quite a cultured people, and at that time building mounds and erecting their houses upon them.

It is not certain that they extended to the extreme south, and therefore this portion of the peninsula is left blank on the linguistic map of the region. That some tribe of advanced culture occupied the territory about the Carlosahatchie bay is revealed by a curious discovery due to the distinguished antiquary and explorer M. Alphonse Pinart, which he communicated to the former publisher of SCIENCE. In examining a rare work by Father Francisco Romero, published at Milan in 1693, entitled *Llanto Sagrado de la America Meridional que busca alivio en los reales ojos de Nuestro Señor Don Carlos III.*, he found the statement that a chieftain called Carlos, who lived on the bay of that name on the southwest coast of Florida, came across to Havana in a small canoe to be instructed in the Christian faith and baptized. On returning, the authorities promised to send a missionary to his people, but neglected to fulfill their agreement.

"Some time afterward," says the writer, "they recieved a letter written with characters entirely different from ours, and with a strange ink. This letter was brought across by a fisherman, who translated it. He stated that the Floridian chief, Carlos, sent by it his respectful homage to the authorities, and complained bitterly that the missionary had not been sent to him."

The original, says the author, was subsequently taken to Spain and deposited in the library of the Duchess of Aveyro. M. Pinart adds that, from correspondence with the representatives of that family, he has reason to believe this original is still in existence.

Whether the 'writing' was the familiar pictography of the North American Indian, or allied to that higher form which prevailed in Mexico and Yucatan, may be decided by a sight of the document itself. At any rate, it is worth mentioning that this unknown people had a recognized system of recording ideas; and possibly investigations in the mounds of that locality may bring other specimens to light.

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THE EARLIEST GENERIC NAME OF AN AMERICAN DEER.

In September, 1817, Rafinesque published descriptions of two species of deer from Paraguay, which he named *Mazama bira* and *M. pita*.* The first was based on the *Gouazoubira*, the second on the *Gouazoupita*, of Azara. Both had been previously described by Illiger†; consequently the specific names fall. *Mazama bira* Raf. = *Cervus rufus* Ill.; *M. pita* Raf. = *C. simplicicornis* Ill. But the generic name *Mazama* antedates by many years the names *Subulo* ‡, *Passalites* §, *Coassus* ||, and even *Cariacus* ¶, and hence is the earliest generic name for any American deer, so far as known. Fortunately, the rules of nomenclature demand that the type be chosen from the species originally covered by the genus; it cannot be taken from those subsequently added by Rafinesque himself (in Am. Monthly Mag., Vol. I., p. 437, Oct. 1817; and Vol. II., p. 44, Nov. 1817). The type therefore must be one or the other of the two well known South American deer, *rufus* or *simplicicornis*, and may be restricted to the formr, which will stand as *Mazama rufa* (Illiger).

C. Hart Merriam.

*Am. Monthly Mag., Vol. I., No. 5, Sept. 1817, p. 363.

†Abhandl. K. Preuss. Akad. Wiss., Berlin (for 1811), 1815, p. 117.

‡ *Subulo* H. Smith, Griffith's Cuvier, Vol. V., 1827, p. 318.

§ *Passalites* Gloger, Hand- u. Hilfsbuch Naturgeschichte, 1, 1841, p. 140.

|| *Coassus* J. E. Gray, List. Mamm. British. Mus., 1843, pp. xxvii and 174.

¶ *Cariacus* Lesson, Nouv. Tableau Regne Animal, Mammif., 1842, p. 173.

JAMES OWEN DORSEY.

Rev. J. Owen Dorsey, Indian linguist, died in Washington, February 4, of typhoid fever. For over twenty years Mr. Dorsey was an enthusiastic student of aboriginal languages, chiefly those of the Siouan family. His acquaintance with these languages was so extended and his grasp of principles so strong as to render him one of the foremost authorities on Indian linguistics. Although numerous publications have been made under his name, the greater part of the material collected and created during his active career remains unpublished. Fortunately, this rich store of manuscripts is preserved, under the systematic arrangement of their author, in the Bureau of American Ethnology, with which Mr. Dorsey has been connected from its organization.

James Owen Dorsey was born in Baltimore, Maryland, October 31, 1848, and received his earlier education in local schools. He was remarkably precocious, reading Hebrew at the age of ten, and his vocal range and power of discriminating and imitating vocal sounds were exceptional. He entered the Theological Seminary of Virginia in 1867, was ordained a deacon of the Protestant Episcopal Church in 1871, and during the same year became missionary among the Ponha Indians, in what was then Dakota Territory. There he began systematic study of Indian language, myth and custom.

Among his publications are memoirs on 'Omaha Sociology,' 'Osage Traditions,' 'a study of Siouan cults,' 'Omaha dwellings, furniture and implements,' printed in the annual reports of the Bureau of American Ethnology; 'Omaha and Ponca letters,' a bulletin of the same bureau; and the 'Dhegiha language,' forming Volume VI. of the Contributions to North American Ethnology. In addition he edited a Dakota-English dictionary, and a volume on Dakota grammar, texts and ethnography, by the late Rev. S. R. Riggs, published in two volumes of the last named series. Numerous minor articles were published in different anthropologic journals. Mr. Dorsey was Vice-President of Section H of the A. A. A. S. in 1893, and at the time of his death was Vice-President of the American Folklore Society. In the absence of the President of this Society he presided over the annual meeting in Washington during the Christmas holidays, this being his last public work in science. W J M

DISCUSSION.

ON INDISCRIMINATE 'TAKING.'

IN many of the text-books which have of late appeared, and even in articles by some of the most renowned chemists, the verb 'to take' is frequently used in a way that is very annoying to teachers who are endeavoring to train students in brevity and exactness of expression. Pages could be filled with examples of bad style and verbosity that ill-accord with the clearness and brevity that are desirable, and that are supposed to characterize scientific literature. A few quotations from recent text-books will suffice to illustrate this particular case—that of indiscriminate 'taking.'

"Take a cylindrical porous jar, such as is used in a galvanic battery, close the open end, etc."

It were better to say, "close the end of a cylindrical porous jar, such as is used, etc."

Another example: "Take two flasks and connect them."

Better—"Connect two flasks," etc.

Another: "The method of experimenting adopted by Graham was to take a bottle or jar with a neck contracted somewhat and fill it to within half an inch of the top with the solution of the salt to be investigated."

Better—"The method . . . was to fill a bottle or jar with a somewhat contracted neck to within half an inch," etc.

Another: "If we take an iron tube closed at one end and connected at the other with a Sprengel pump and exhaust it completely."

This awkward form of diction often excites mirth in the class-room, as it gives unusual opportunities for double meanings.

"Take a pound of sugar and an equal weight of sulfuric acid." This would be a severe dose, even for a trained scientist.

The following is from a recent text-book: "Take a lump of chalk or sandstone, some very dry sand, a glass of water and a glass of treacle."

This might do for a bill of fare in a Chinese restaurant, but it is out of place in a scientific book.

"Take some white arsenic."—"Take a sedlitz powder,"—are the singular directions which preface two experiments in a book recently published by the Society for Promotion of Christian Knowledge in London.

If editors and teachers will pay more attention to this awkward use of the word 'take' they will incur the gratitude of a patiently suffering public.

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SCIENTIFIC LITERATURE.

The Life of Richard Owen. By his grandson, the REV. RICHARD OWEN, M. A. With the scientific portions revised by C. DAVIES SHERBORN. Also an essay on Owen's position in anatomical science.

By the RIGHT HON. T. H. HUXLEY, F. R. S. Portraits and illustrations. In two volumes. New York, D. Appleton & Co. 1894. Pp. 409, 393. $7.50.

The life of the great English comparative anatomist as told in these volumes was in many respects an ideal one. It is the old story of a self-made man, who, without the advantages of good preparatory schools, or of the university life at Cambridge or Oxford, by his own native ability and industry, as well as by his kindly disposition and social tact, rose to the highest scientific position in Great Britain, came to be the friend of some of England's leading statesmen, of her greatest poets and novelists; the recipient of marked favors from the Queen; living to see the completion of the magnificent natural history museum at South Kensington planned by himself, and dying at the great age of eighty-eight years, during sixty of which he published the long series of monographs and general works which form his most enduring monument.

This biography, as prepared by his grandson largely from Owen's letters and diary and those of his wife, even if it includes what may be thought to be many trivial details, gives what seems to us to be a most attractive and life-like sketch of the man. We see Owen, not only in his study at the College of Surgeons and afterwards at the British Museum, but also at his home in the little rambling thatched cottage in Richmond Park, presented him by the Queen. We also catch glimpses of his club life, of his success as an administrator, as a lecturer, as a literateur; we are given evidences of his fondness for art and music and the drama, as well as poetry, and accounts of his journeys over the continent and up the Nile.

It is a record not of a scientific recluse, but of one who had many outside interests, and who lived in touch with the best minds and the best thought of his time.

Richard Owen was born in 1804 at Lancaster, the son of a merchant. After leaving the grammar school, he was when sixteen apprenticed to a surgeon, and when twenty matriculated at Edinburgh University as a medical student. Six years after he became prosector to Dr. Abernethy in London and assistant curator of the Hunterian Collection at the College of Surgeons, and in 1856 was appointed superintendent of the Natural History collections of the British Museum, a position created for him and which he held until shortly before his death.

His first paper was published in 1830, and two years later his famous memoir on the pearly nautilus. This at once gave him a national and continental reputation as a comparative anatomist of the first rank. Huxley makes the generous claim, in referring to the work, that there is nothing better in Cuvier's 'Mémoires sur les Mollusques,' and he adds: "Certainly in the sixty years that have elapsed since the publication of this remarkable monograph, it has not been excelled, and that is a good deal to say with Müller's 'Myxinoid Fishes' for a competitor." Owen's last work (the list of the entire series of articles, monographs and general works embracing 647 titles) appeared in 1889. What a record! Sixty years of almost uninterrupted health, of unexampled productiveness, of accurate, painstaking, honest labor.

Owen's place in biological science, a science which has widened and deepened so immeasurably since the date of publication of his first great work in 1832, is not altogether easy to determine, but the task is much lightened by the appreciative and magnanimous essay by Professor Huxley on Owen's position in Anatomical Science, placed at the end of the biography.

Owen was called by some of his contemporaries 'the British Cuvier,' and this fairly well expresses his position. He may be said to have lived in the interregnum between the age of Oken, St. Hilaire and Cuvier, and the age of the modern school of morphologists. He made no special contributions to comparative embryology; he was guiltless of histology and of microscopic technique. His ideas and lines of thought and work were a fusion of Okenism and of the doctrine of correlation of organs taught by Cuvier, with perhaps a slight infusion of the transformationist school of France. Like some of the fossil forms which he restored with masterly skill and philosophic insight, he was in a sense a synthetic or prophetic type of naturalist. For example, he declined when asked to attack the 'Vestiges of Creation', rather sympathizing with the views put forth in that book; but also objected to become a loyal disciple of his friend, Darwin. He partially accepted the general doctrine of evolution; but though his views were vague and unformed, like many others perhaps in the period between 1850 and 1870, he probably felt that Natural Selection was not a sole, efficient cause, though believing in the orderly evolution of life by secondary law.

We find in this life no statement from Owen's own letters or journals regarding his attitude to the doctrine of natural selection. Either he was late in life somewhat indifferent, or he was guarded in speaking or writing of the matter. Certainly there are no grounds for the statement sometimes made that he showed outright 'hostility' to Darwinism, unless his Athenæum article be regarded as such. In Owen's evidence before Mr. Gregory's committee regarding the removal of the Natural History Collection to South Kensington his biographer tells us: "Owen made some interesting remarks concerning Darwin's work on the 'Origin of Species,' just published, which helps to strengthen the impression that he was at first much taken with the new views, and felt the same friendliness toward them as he had previously shown to the views expressed in the 'Vestiges of Creation.'" Owen remarks concerning the arrangement of the new museum: "With regard to birds, I must say that not only would I exhibit every species, but I see clearly, in the present plan of natural history philosophy, that we shall be compelled to exhibit varieties also. . . . . As to showing you the varieties of those species, or any of those phenomena that would aid one in getting at the mystery of mysteries, the origin of species, our space does not permit;" and again he replies to a question of the chairman: "I must say that the number of intellectual individuals interested in the great question which is mooted in Mr. Darwin's book is far beyond the small class expressly concerned in scientific research."

Owen's controversial papers, as well as his statements of scientific belief, were at times vague and a grain oracular, and were presented in a labored style, quite different from that of his letters and popular lectures, or even his work on Archetypes, the style of which has been characterized as 'clear and forcible.' Darwin in the well known reference to Owen's views in the Historical Sketch prefacing the sixth edition of the Origin of Species was, he says, 'completely deceived' by such expressions as 'the continuous operation of creative power,' and he was apparently unable to determine what his real opinions were, and was evidently piqued and disappointed that the great anatomist, his old scientific friend of many years, did not accept the doctrine of natural selection. On p. 91 his biographer states: "If not 'dead against' the theory of natural selection, Owen at first looked askance at it, preferring the idea of the great scheme of Nature which he had himself advanced. He was of the opinion that the operation of external influences and the resulting 'contest of existence' lead to certain species becoming extinct. Thus it came about, he supposed, that, like the dodo in recent times, the dinornis and other gigantic birds had disappeared. But he never, so far as can be ascertained, expressed a definite opinion on Darwinism."

It is well enough at this day, when the scientific world is of one mind as regards the truth of the evolution theory, to ascribe indifference and even 'hostility' to Owen, but we fail to see that this is quite just. For Owen, so far from attacking or minimizing the new plan of evolution invoked by Darwin, was even said by the editor of the 'London Review,' as Darwin tells us, in his own words, to have 'promulgated the theory of natural selection before I had done so.'

So strong a Darwinian as the acute and clear-headed Gray states, more fully and satisfactorily perhaps than Darwin, the position of Owen. In his 'Darwiniana' Dr. Asa Gray, who, writing in 1860, frankly confesses: "We are not disposed nor prepared to take sides for or against the new hypothesis," and yet who by his own studies and mental tendencies was 'not wholly unprepared for it,' thus humorously refers to Owen's views, published before the appearance of Darwin's book, "Now and then we encountered a sentence, like Prof. Owen's 'axiom of the continuous operation of the ordained becoming of living things,' which haunted us like an apparition. For, dim as our conception must needs be as to what such oracular and grandiloquent phrases might really mean, we feel confident that they presaged no good to old beliefs" (p. 88). Further on he writes: "Owen himself is apparently in travail with some transmutation theory of his own conceiving, which may yet see the light, although Darwin's came first to the birth. . . . . Indeed to turn the point of a pungent simile directed against Darwin—the difference between the Darwinian and the Owenian hypotheses may, after all, be only that between homœopathic and heroic doses of the same drug" (p. 102). Again, in 1873, he writes: "Owen still earlier signified his adhesion to the doctrine of derivation in some form, but apparently upon general, speculative grounds; for he repudiated natural selection, and offered no other natural solution of the mystery of the orderly incoming of cognate forms."

Finally we may quote from a letter of Darwin's (Life ii. p. 388), written in 1862 to Sir Charles Lyell: "I was assured that Owen in his lectures this spring advanced as a new idea that wingless birds had lost their wings by disuse, also that magpies stole spoons, &c., from a remnant of some instinct like that of the Bower bird, which ornaments its playing passage with pretty feathers. Indeed, I am told that he hinted plainly that all birds are descended from one."

From all that has been said it would seem to follow, from a perusal of these scattered fragments, that Owen was an evolutionist somewhat of the Lamarckian school; that he was not a Darwinian as such, not being fully persuaded of the adequacy of natural selection as the sole cause of all evolution. To this class certainly belong some naturalists and philosophers of the present day. But it should be added that Owen, in the latter part of his life, did not use the hypothesis or theory as a working one, as did some of the elder naturalists of his own period, as Lyell, Wyman, Leidy, etc. He was fifty-five years old when the 'Origin of Species' appeared, and either was not then prone to speculation, or had little leisure for it.

It must be granted that Owen, clear-headed and sagacious as he was, did not rise to the plane of that high quality of genius which opens up new lines of investigation. His was not an epoch-making mind of the quality of Lamarck or Darwin, in the field of evolution, nor of Müller, Von Baer, Rathke, and Huxley, the founders of modern morphology; nor of Koelliker or

Leydig, the founders of modern histology. He was a closet naturalist, made no collections with his own hands, was not a field paleontologist; and his travels were rather for health and recreation than for study or exploration. The vast collections which poured in upon him from South America, Australia and New Zealand, as well as from his own land, occupied his working hours and energies decade after decade, until the passing years left him stranded on the shores of a world of ideas and modes of cooking now subsiding beneath the incoming flood of modern methods and theories.

And yet, his philosophic grasp and suggestive mind exhibited in his treatment of the subject of parthenogenesis, in his essay on the subject which appeared in 1849, and in which he has, as Huxley states, anticipated the theory of germ-plasm of Weismann, are qualities of genius, and prove what he might have produced, had he received any training along the lines of embryology and cell-doctrines.

"Owen, in fact," says Huxley, "got no further towards the solution of this wonderful and difficult problem than Morren and others had done before him. But it is an interesting circumstance that the leading idea of 'Parthenogenesis,' namely, that sexless proliferation is, in some way, dependent upon the presence in the prolifying region, of relatively unaltered descendants of the primary impregnated embryo cell (A + B) is at the bottom of most of the attempts which have recently been made to deal with the question. The theory of the continuity of germ-plasm of Weismann, for example, is practically the same as Owen's, if we omit from the latter the notion that the endowment with 'spermatic force' is the indispensable condition of proliferation."

Owen's greatest works, those of most lasting value, in vertebrate zoölogy were, as pointed out by Huxley, besides his memoir on the anatomy of Nautilus, his work on Odontography, his papers on the anthropoid apes, on the aye-aye, on Monotremes, and on Marsupials, as well as on Apteryx, the great auk, the Dodo, and Dinornis, as well as Lepidosiren, while chief among his essays on fossil mammals were those on Mylodon, Megatherium, Glyptodon, etc. He also proposed the orders of Theriodonta (Anomodontia), Dinosauria, and Pterosauria, and as early as 1839, as Zittel states, "he began his long series of fundamental works which continued to appear for half a century, and which laid the foundation for all later researches on fossil reptiles." He also revised the classification of the Ungulates, his divisions of odd and even-toed Ungulates being well founded and generally accepted.

Unlike Cuvier and others, Owen labored without the aid of trained assistants; he did his own work unassisted. And here arises the question how far he was indebted to Cuvier for his methods of work. It is generally supposed and stated that Owen studied in Paris under Cuvier, and that "Cuvier and his collections made a great impression on Owen, and gave a direction to his after studies of fossil remains." But his biographer explicitly states that he only made a brief visit to Cuvier in July, 1831, and gives us the following account of his intercourse with the great French anatomist: "His rough diary, which he kept during his stay at Paris, seldom mentions the fossil vertebrate collection, and shows that his interviews with Baron Cuvier were for the most part of a purely social character. It notes, for example, that he attended pretty regularly Cuvier's soirées, held on Saturday evenings, and that he enjoyed the music. With the diary agree his letters. Both devote page after page to the sights and amusements of Paris. Owen, in fact, seems to have regarded this stay at Paris as an exceedingly pleasant and entertaining holiday. At the same time it is impossible to form a just estimate of Owen's work without taking the labors of Cuvier into account. Although Owen stands on ground wholly his own, he was ever willing to acknowledge the debt which he owed to Cuvier."

The name of Owen will ever be associated with those of Oken, Goethe, Spix, and Carus, or the school of transcendental anatomy. The discussion by Huxley of Owen's work on the archetype of the vertebrate skeleton is handled in his peculiarly trenchant and clear-minded way, and yet his criticisms are genial, just and broad. It should be remembered that Owen's work 'On the Archetype and Homologies of the Vertebrate Skeleton' appeared in 1848, over ten years before the appearance of the 'Origin of Species,' and at a period when many minds in the scientific world were tinged more or less deeply with the spirit of the German and French transcendental school of anatomy. As Huxley eloquently expresses it, "The ablest of us is a child of his time, profiting by one set of its influences, limited by another. It was Owen's limitation that he occupied himself with speculations about the 'Archetype' some time before the work of the embryologists began to be appreciated in this country. It had not yet come to be understood that, after the publication of the investigations of Rathke, Reichert, Remak, Vogt and others, the *venue* of the great cause of the morpology of the skeleton was removed from the court of comparative anatomy to that of embryology." He then adds: "It would be a great mistake, however, to conclude that Owen's labours in the field of morphology were lost, because they have yielded little fruit of the kind he looked for. On the contrary, they not only did a great deal of good by awakening attention to the higher problems of morphology in this country; but they were of much service in clarifying and improving anatomical nomenclature, especially in respect of the vertebral region."

As regards the vertebrate theory of the skull, perhaps the last word has not been said, if traces of vertebræ still, as is alleged, appear in certain of the sharks.

If Huxley by his destructive criticism has destroyed, or seemed to have destroyed this theory, the ghost is apparently not wholly laid. The more ideal constructive, German minds, as Gegenbaur and others, claim that the adult skull is in a degree segmented, as evinced by the serial arrangement of the nerves, as well as of the branchial arches. Though Wiedersheim states* that "the attempt to explain the adult skull as a series of vertebræ fails completely," adding, "it is a case of protovertebræ only," he says in a foot-note that Rosenberg has, however, shown that in a shark (*Carcharias glaucus*), "the portion of the cranium lying between the exit of the vagus and the vertebral column is clearly composed of three vertebræ." Gadow finds four vertebræ in embryos of Carcharias, while Sagemehl has found a somewhat similar modification in Ganoids. It would seem that the segmentation of the head observed in the embryo of vertebrates, and probably inherited from their vermian ancestors, has been obliterated in the adults by adaptation, but that traces may have survived in certain sharks and Ganoids.

Finally, it must be conceded that though it is the fashion of the younger men to characterize Owen as a comparative anatomist of the old school, and now quite overshadowed by the scientific leaders of the present generation, the kindly and discriminating judgment of the great English anatomist and essayist we have just quoted, will undoubtedly be sustained by many coming generations. Owen's place in natural science, in many respects an unique one, will be among the greatest anatomists of the first half of our century. His name will bridge over the gap between Cuvier, and the embryologists and morphologists

* Elements of the Comparative Anatomy of Vertebrates, p. 56.

of the second half of the nineteenth century.

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Heat; Light; Elementary Text-Books, Theoretical and Practical for Schools and Colleges: By R. T. GLAZEBROOK. 12 mo., about 220 pages each. New York, Macmillan & Co. Price $1.00.

These are recent volumes in the series of Cambridge Natural Science Manuals.

All American physicists are familiar with the previous excellent products of Mr. Glazebrook's pen in the line of text-books for laboratory and class-room, and will be interested in this new series which is intended to fill a place quite different from that for which his previous works were prepared. They are less extensive and more elementary. According to the author, they represent what has for some time constituted a practical course for medical students in the Cavendish laboratory. There has been much discussion, and there will continue to be much discussion for some time to come, as to the proper sequence of laboratory, text-book and lecture instruction in elementary physics. In the Cavendish laboratory the system adopted for this course, at least, seems to be that the instructor first presents a portion of the subject in the form of a lecture in which he illustrates, by the use of simple apparatus, and explains the theory of the experiments, deriving principles and numerical results, as far as possible, from the results of experiments actually performed. The members of the class then make the experiments, singly or in pairs, or occasionally in large groups, using the same, or similar, apparatus. The volumes contain descriptions of experiments and also theoretical principles and deductions, so that they constitute at once text-book and laboratory hand-book. At intervals throughout the work there will be found well selected collections of problems and examples, and a good set of examination questions at the end. The apparatus described is usually simple, and most of it could be made with simple materials by one having some technical skill of the right sort.

It is hardly necessary to say that the theoretical discussions and presentation of principles are, for the most part, clear and clean as far as they go.

In the 'Heat,' the first chapter has to do with its nature, and its relation to work or energy is concisely but clearly stated. In the second chapter the treatment of temperature and its measurement is unusually satisfactory, considering the limitations to which the whole work is subjected. It is to be regretted, however, that there is no mention of the hydrogen scale, since so many of the most important temperature measurements now depend upon it. Calorimetry is discussed quite thoroughly, with many practical illustrations, and in the chapters devoted to expansion several neat suggestions as to methods will be found. In the reference to the necessity for 'compensating' the effect of temperature on the balance wheel of a watch, it is erroneously implied that the principal reason for this grows out of the change in the dimensions, and consequently moment of inertia of the wheel due to change in temperature, while, as a matter of fact, it is the temperature change of the modulus of elasticity of the 'hair' or balance spring which makes nearly all the trouble. The volume ends with a brief but good chapter on the mechanical equivalent of heat.

In the volume on 'Light,' the geometrical treatment is used exclusively. There is a single brief reference to the physical nature of light, which is so thoroughly discussed in the author's volume on 'Physical Optics' published some years ago, but in the book under consideration the rectilinear propagation of a 'ray' is assumed and made the basis of the whole discussion. The chapters devoted to reflection from plane surfaces are excellent, and those in which refraction is treated are particularly thorough and good. The simpler geometrical treatment of lenses is very satisfactory; optical instruments and 'aids to vision' receive rather more attention (especially the latter) than is usual in books of this class. There are also a number of interesting and rather uncommon experiments and exercises combining the eye and lenses of various forms, by means of which many problems relating to vision are made clear. There is a chapter on the spectrum and color, with which the volume ends.

Both of these volumes can confidently be recommended for courses in secondary schools, or in colleges where a limited amount of elementary instruction in physics is required. T. C. M.

Electricity, One Hundred Years Ago and To-day. Edwin J. Houston. New York, W. J. Johnston & Co., Limited. 12mo., pp. 200.

This volume is built around or upon a lecture having the same title which was delivered in 1892. It was a historical discussion of the growth and development of electricity from the beginning (not one hundred years ago) to the present time. In preparing it for publication the author has increased its volume several times, and its interest and value proportionately by the addition of an extensive series of historical foot-notes. Many of these consist of long quotations from original authorities which would have been hardly suitable for a popular address, but which greatly enhance the worth of the address when printed. Some discussions of quite recent date are extensively quoted, and this volume includes, in comparatively small space, the results of much labor expended in the pursuit of exact information by reference to original papers. For this reason, if for no other, it will be welcome to all interested in the science of electricity or the art of its application. T. C. M.

Hygiene. By I. Lane Notter and R. H. Firth. London, Longmans, Green & Co. 1894.

This manual, of 374 pp. 8°, is a very concise and clear summary of what a non-professional, well educated man should know with regard to the general laws of health, the causes of disease, and the best means of combating the latter. Dr. Notter is the Professor of Hygiene in the Army Medical School at Netley, and Examiner in Hygiene in the Science and Art Department at South Kensington, and Dr. Firth is his assistant in each of these positions, hence this manual may be considered as a summary of the latest English teaching on this subject. In such subjects as heating and ventilation, house drainage, construction of buildings, hospitals, etc., its recommendations are adapted especially to the climate and customs of England, and the illustrations are solely of English appliances and methods, and this must be borne in mind by American readers.

Galton's grates, Tobin's tubes, Sheringham valves, Buchan's traps, etc., are not to be found in the market in this country, where other equally satisfactory appliances take their place.

It is not a book to be resorted to for thrilling and sensational quotations, but it will be found to give sound common sense advice upon the subjects of which it treats, and is commended to the readers of Science as a good manual of reference.

An Illustrated Dictionary of Medicine, Biology and Allied Sciences. By George M. Gould, A. B., M. D. Philadelphia, P. Blakiston, Son & Co. 1894. 4°, pp. 1633.

This is a very full and complete dictionary of medicine, printed clearly on good paper, and so bound that it will remain open at any page, a convenience not always found in books of reference. Some of the words proposed by the author are not accepted by good authorities, as for example, 'chemic' for chemical, 'physiologic' for physiological, and in this respect the work is sometimes misleading. In the attempt to give a complete list of the bacteria many names are given which would not be accepted by a bacteriologist, the list evidently having been prepared by some one not familiar with the subject. These, however, are minor details; the main fact about the work is that it is the most complete and practically useful single volume dictionary of medical terms in the English language, and as such it is commended to the readers of Science.

NOTES.

THE INTERNATIONAL ZOÖLOGICAL CONGRESS.

The following invitation has just been issued to the Third International Zoölogical Congress to be held in Leyden next September: "The first International Zoölogical Congress took place in Paris at the time of the International Exhibition of 1889. The second meeting was held in Moscow in 1892. There the resolution was passed that in September, 1895, this Congress would again meet in Leyden, the oldest University of the Netherlands. The Netherlands' Zoölogical Society has taken upon itself to make all the necessary arrangements for the reception and accommodation of the Congress. At the invitation of that Society, the undersigned request you to become a member of the International Congress and to attend the Leyden meeting. It appears probable that different questions, in which the interest of zoölogists in general, as well as those of specialists are involved, can be brought to a solution by mutual exchange of opinions on the occasion of such an international meeting. At any rate the way that will lead to such a solution may there be prepared. Moreover it is undoubtedly a distinct advantage to become personally acquainted with representatives of Zoölogical Science from different parts of the world. As soon as you shall have expressed your sympathy with the above stated aims of the International Zoölogical Congress we shall be glad to be allowed to append your name to a more general invitation directed to all zoölogists and morphologists, which will be brought before our fellow-workers by the aid of different periodicals. We venture to add that even in case of your not being able to attend the proposed Congress you will favor us with the expression of your sympathy with the movement. Pray to be so kind to send your answer to Dr. P. P. C. Hoek, Secretary of the Netherlands Zoölogical Society at Helder, Holland."

The invitation is signed by about one hundred naturalists in different parts of the world, including the following from this country: A. Agassiz, E. D. Cope, E. L. Mark, O. C. Marsh, H. F. Osborn, W. B. Scott and C. O. Whitman.

THE TESTING OF ELECTRICAL STREET RAILWAYS.

The expenditure and distribution of power on electrical street railways has formed a subject of investigation on a somewhat extensive scale, and for a number of years past, by the departments of Sibley College, Cornell University. In the issue of the *Sibley College Journal* for January, Mr. James Lyman, formerly of Yale University, now engaged in special work of this character in the graduate department of the College, summarizes some of the most important results thus collated. In the performance of the work of investigation, parties are sent out, sometimes to the number of ten or a dozen, including the experts in charge and their student-assistants, divided into squads, assigned each to its special part of the work, the electricians to the measurement of current, the electrical engineers to the handling of the dynamos and electric 'plant,' the mechanical engineers to the testing of engines and boilers, and each individual to that work which he can best direct or with regard to which the experience will prove most fruitful.*

The records of the Sibley College laboratories are thus peculiarly rich in data of this kind. The first case quoted is that of the trial of the Rochester, N. Y., street railway plant by Dr. Bedell, in 1891. The road has about twenty miles of track, and very easy gradients. The traction demanded 1.4 *E. H. P.* per ton, at 6.5 miles average speed, efficiency of line was 90 per cent., that of the station 64.8 per cent., and there were needed, at the engines, 2.4 *I. H. P.* per ton, 20 *I. H. P.* per car. The Buffalo plant was tested in 1892, under the responsible direction of Messrs. Wood and Palmer. The average power demanded was 1.76 *I. H. P.* per ton. The Ithaca street railway was tested in 1894, and is important as illustrating work on heavy gradients, averaging about nine per cent., a maximum occurring at twelve or thirteen. The traction coefficient was found to be 40 pounds, per one per cent. of gradient and per ton. In a level country, the estimate for power to be provided at the station is put at 2.5 I. H. P. per ton of car and load, the number of cars on the line averaging about ten. If averaging twenty, the figure becomes 2.2.

* As many as a dozen indicators and numerous volt and ammeters, dynamometers, special condensing apparatus, scales for weighing coal and water, and similar test apparatus are often supplied by the College, the resources of which are gauged, in a way, by the fact that it furnishes a large part of its graduating classes of late years, numbering about a hundred, with all the instruments needed in work of investigation in their graduating theses; which theses are usually accounts of such work. Its working 'plant' includes fifteen steam engines, seven gas engines, some fifty gauges and a still larger number of steam engine indicators.

THE MINNESOTA ACADEMY.

THE Minnesota Academy of Natural Sciences has, in addition to its 'Bulletin,' instituted a new series of publications termed 'Occasional Papers.' It is intended that in this series shall be published researches of considerable importance. Vol. I., No. 1, which has recently appeared, contains 'Preliminary Notes on the Birds and Mammals collected by the Menage Scientific Expedition to the Philippine Islands,' by Frank S. Bourns and Dean C. Worcester.

ANTHROPOLOGY.

UNDER the title of 'Notes on Primitive Man in Ontario,' by David Boyle, there has been printed in Toronto, by order of the Legislative Assembly, as an appendix to the report of the Minister of Education, Dr. G. W. Ross, a pamphlet of about 100 pages, containing much instruction concerning the aboriginal tribes of that province. Mr. Boyle has been for many years the efficient curator of the valuable Ethnological Museum of the Canadian Institute. This monograph comprises many pictures of the native implements of stone, clay, bone, horn, shell and copper in that museum, and will be useful to ethnologists for purposes of comparison.

Tsetsaût is the Tsimsián name of a small tribe recently discovered on Portland Inlet, British Columbia, 54° 50′ Lat., which consists at present of twelve Indians only. They live on the proceeds of hunting and fishing and originally spoke a Tinné or Athapaskan dialect, which is evidenced by the fact that two of their number still remember words of it, though the rest speak the Nass dialect of the Tsimsián Indians surrounding them. Even the original Tinné name of the tribe is no longer remembered. Dr. Franz Boas studied the tribe during the later months of 1894, and also discovered another remnant of the same linguistic family, the Tinné, which lives in the vicinity. He favors, somewhat, the theory that Haida, Tlinkit and Tinné are related to each other, and that after a more thorough study the three will be found to form one and the same linguistic family. Dr. Boas' discovery is remarkable for this reason, that the great Tinné family is almost exclusively an inland nation, and has pushed its branches to the ocean only at two places, viz., in Southern Texas (Lipans) and in Southwestern Oregon (Rogue Rivers), contiguous to the northwest coast of California, where little Tinné tribes have settled also.

Alaska. This name was originally applied only to the narrow peninsula situated at the southwest extremity of the Alaska Territory. It is a corruption of alákshak, *mainland, continent*, a term of the Eastern dialects of the Ale-ūt language. The name of *Unalashka Island* contains the same word, for it is contracted from ángun alákshak, 'to the west of the mainland.' Ángun, *west*, also enters into the composition of Unángun, a division of the Ale-ūt people, which is reducible to un, *people*, and ángun, *west*. (From notes by Ivan Petróff.)

The Department of Anthropology, University of Chicago, has just published Bulletin 1—*Notes on Mexican Archæology*, by Frederick Starr. A full description is given of the ruins of an interesting 'painted house' at San Juan de Teotihuacan. The walls were decorated with pictures, in a variety of colors, representing warriors and religious personages. The designs are reproduced in a series of a dozen cuts. Some notes are also given regarding Mitla and Monte Alban. Paintings from a wall at Mitla are reproduced in full size.

It is the intention of the University to publish Bulletins in this Department from time to time as fresh material is secured.

ZOÖLOGY.—THE MAMMALS OF FLORIDA.

Mr. Frank M. Chapman has recently published a list of the Mammals known to inhabit the State of Florida (Bull. Amer. Mus. Nat. Hist. vi. pp. 333-346). He gives in all, the names of 53 species and sub-species. Aquatic species are excluded. The largest forms are the Virginia deer, the black bear, the puma and the wolf. The last-named is approaching extinction. The beaver is believed to occur in the Chipola River.

The sole West Indian form is a leaf-nosed bat (*Artibeus carpolegus*), and this is believed to be only an accidental visitant. The house-rat of Florida is the white-bellied roof rat (*Mus alexandrius*) rather than the Norway rat. F. W. T.

GEOLOGY.

At a meeting of the Council of the Michigan Academy of Sciences, Messrs. A. C. Lane and I. C. Russell were appointed a committee to present to the Legislature a plan for a topographical survey of Michigan. The plan to be proposed will be in coöperation with the U. S. Geological Survey and the preparation of a map similar to the maps of Massachusetts, Rhode Island and Connecticut, recently compiled at the joint expense of the States named and the U. S. Geological Survey.

Professor J. E. Todd, State Geologist of South Dakota, has just issued his first report. It is entitled 'South Dakota Geological Survey, Bulletin No. 1: A Preliminary Report on the Geology of South Dakota.' In this volume the present state of knowledge concerning the geology of the State is presented briefly and in a form that is acceptable to the intelligent citizen as well as to the specialist. The report is an octavo of 172 pages, and it is accompanied by several plates and a geological map of the State.

The committee appointed by the members of the Johns Hopkins University to mature a plan for securing a permanent memorial of the late Professor George Huntington Williams are able to announce that subscriptions have been received of a sufficient amount to procure a portrait in oil, which will soon be completed and presented to the University. The artist selected is Mr. Robert G. Hardie, of New York.

ENTOMOLOGY.

In a paper read to the K. Böhm. Gesellschaft der Wissenschaften on November 23d last, Dr. Anton Fritsch, of Prag, announced the discovery in the Permian beds of Bohemia of the larval cases of a caddis-fly. This is the first indication of the existence of insects with a complete metamorphosis in paleozoic times, unless the doubtful fragments found by Dathe in Silesian culm are to be regarded as shards of beetles, or the passages found in certain carboniferous woods are to be credited to coleopterous larvæ. It is to be hoped that Dr. Fritsch will amply illustrate these remains in his great work now in progress on the Fauna der Gaskohle Böhmens.

GENERAL.

Professor Warburg, of Freiberg, has been called to Berlin as the successor of Kundt.

Professor Kulz, of Marburg, known for his researches in physiological chemistry, died on January 16.

Macmillan & Co. announce a translation by Dr. A. C. Porter, of the University of Pennsylvania, of the *Lehrbuch der Botanik*, by Strasburger, Noll, Schenck and Schimper.

The St. Petersburg Academy of Sciences has recently made some changes in the system of publishing papers communicated to it. In September, 1894, it commenced the publication of a monthly number, under the title *Bulletin de l' Académie Impériale des Sciences*, which serves as the organ of the three classes of the Academy. This *Bulletin* is intended to include the *procès-verbaux* of the meetings, annual reports of scientific researches, reports on prizes conferred by the Academy, notes on the work of the museums, &c. In addition to notices of this kind, the *Bulletin* will contain short scientific papers. The *Mémoires de l'Académie Impériale des Sciences* will form in future the second means of publication. It will be divided into two independent series, dealing respectively with the physico-mathematical section of the Academy's papers, and the historical and philological section. The publication of the *Mélanges, tirés du Bulletin*, has been discontinued.—*Nature.*

An International Congress on Childhood will be held in Florence in the spring of 1895. Among the questions to be discussed are the physical, moral and mental elevation of children, children's hospitals, the care of deaf-mute and blind children up to the time of their admission into an educational institution, care of poor and abandoned children, reformatories, and vagabondage in its relation to childhood.—*N. Y. Medical Record.*

SOCIETIES AND ACADEMIES.

A. A. A. S. MEETING, 1895.

At a special meeting of the Council, held on January 26th, it was decided to postpone the proposed meeting in San Francisco. An invitation from Springfield, Mass., to hold the meeting of 1895 in that city, was accepted. The date of the meeting was fixed as follows: Council meeting, Wednesday, August 28th, at noon; General Sessions, Thursday, August 29th, at 10 a. m.

Special efforts will be made by the officers of the sections to prepare programmes for the sections in advance of the meeting and for this purpose members are requested to send abstracts of their papers, as early as possible, to the Permanent Secretary, or to the Secretaries of the Sections.

F. W Putnam, *Permanent Secretary.*

Salem, Mass., Jan. 30, 1895.

NEW YORK ACADEMY OF SCIENCES; SECTION OF ASTRONOMY AND PHYSICS, FEB. 4.

Professor W. Hallock showed *a new*

photographic method of comparing the rate of vibration of two tuning forks. The forks are so clamped that a prong of each is held in front of a manometric capsule. The forks are bowed and the flames photographed as described in the Physical Review, Vol. II., p. 305, 1875. The vibrations are then counted in the wavy line on the negative. The accuracy in ordinary work is about two or three-tenths of a wave per second.

The second paper was by Prof. J. K. Rees on the *Penumbræ of sun-spots* as shown in Rutherfurd's photographs, with especial reference to the discussion at the December meeting of the Royal Astronomical Society. Professor Rees called the attention of the Section to the remarks made by the Rev. F. Howlett on presenting to the Royal Astronomical Society of London three volumes of sun-spot drawings. This set of volumes contains drawings made during a period of thirty-five years, and shows minute details in regard to the forms and changes of solar spots. The Rev. Mr. Howlett stated that his main object in continuing the series had been to test the theory put forth by Professor Wilson, of Glasgow, in the latter part of the last century. Wilson's theory claimed that the penumbra in a spot shelves down toward the umbra; and that the portion of the penumbra nearest the sun's centre will, therefore, grow narrower and narrower, due to perspective, as the sun-spot reaches a point nearer and nearer to the limb. Mr. Howlett claimed that his drawings showed that the Wilsonian theory was not borne out by his observations as recorded in his drawings.

He made bold to say that, instead of the penumbra of the spot possessing shelving sides sloping down toward the umbra, the penumbra shows a convex surface in a curve conformable to the general contour of the solar surface. He remarked that he had not himself witnessed a single case of certain notching of the limb.

Professor Rees exhibited on the screen a series of fine photographs of the solar surface taken by Mr. Rutherfurd with his photographic telescope (13 inches diameter of object glass, 11 feet of focal length) during the years 1870–1871. Attention was called to the appearance of the penumbral regions of the spots which showed conclusively that the penumbra was, as a rule, eccentric with respect to the umbra. Spots were pointed out near the centre of the sun where the penumbral marking was deficient on, sometimes the west side, then on the east side, sometimes on the north side and sometimes on the south side. Spots were also indicated which showed, when near the limb of the sun, the penumbral region wanting on the side farthest from the centre and well developed on the side toward the centre. So far as these photographs showed, there was no doubt that the Wilson theory did not completely explain the various phenomena.

Professor Rees also showed some pictures of sun-spots taken by Mr. C. A. Post, of New York City, exhibiting the non-central character of the umbra with respect to the penumbra. Mr. C. A. Post, of New York City, then threw on the screen some photographs of the sun and moon that he had taken.

He also exhibited a series of strikingly beautiful lantern slides made from photographs of lightning flashes.

Professor M. I. Pupin described his *new form of automatic vacuum-pump* (see Am. Journ. Sci., Vol. 39, 1895, p. 19). An extremely ingenious device utilizes an ordinary vacuum pump (water pump) to raise mercury for the Sprengel pump. Little mercury is needed and the whole is continuous in its action.

INDIANA ACADEMY OF SCIENCE.

The Indiana Academy of Science met at Indianapolis, December 27–28, 1894, with W. A. Noyes, of the Rose Polytechnic of

Terre Haute, as President, and C. A. Waldo, of De Pauw University, as Secretary.

The Academy was well attended by the leading scientists of the State.

After the ordinary preliminary business, the body continued in general session, and listened to the reading of nine papers on general scientific topics.

The Academy then met in two sections, Physico-Chemical and Biological. In the former section, 28 short papers were read, and in the latter 51. The papers indicated that much work had been done during the past year in the various lines of scientific investigation.

The reports from the directors of the Biological Survey of Indiana were encouraging, showing that every effort was being put forth to accomplish this survey as quickly as possible and in a satisfactory manner. A resolution was passed requesting the Legislature of the State to print and distribute the proceedings of the Academy. This expense has always been borne by the Academy, but in view of the fact that the State is reaping the benefits it should assume the expense.

The Spring meeting will be held at the Wyandotte Cave, in Crawford county.

Following is a list of the papers:

Address by the Retiring President,—Lavoisier. W. A. Noyes.

GENERAL SUBJECTS.

1. *Some Facts in Distribution of Gleditschia Triacanthos and Other Trees:* Ernest Walker.

2. *Propagation and Protection of Game and Fish:* I. W. Sharp.

3. *Anthropology; the Study of Man:* Amos W. Butler.

4. *A New Biological Station and its Aim:* C. H. Eigenmann.

5. *Transmission of Impressions in Spinal Cord:* G. A. Talbert.

6. *Does High Tension of Electric Current Destroy Life:* J. L. Campbell.

7. *The Surdue Engineering Laboratory since the Restoration:* Wm. F. M. Goss.

8. *Method of Determining Sewage Pollution of Rivers:* Chas. C. Brown.

9. *Psychological Laboratory of Indiana University:* W. L. Bryan.

PHYSICO-CHEMICAL SUBJECTS.

10. *Interesting Deposit of Alumina Oxyhydrate:* G. W. Benton.

11. *Observations on Glacial Drift of Jasper County:* A. H. Purdue.

12. *Concerning a Burial Mound Recently Opened in Randolph County:* Joseph Moore.

13. *Reversal of Current in the Toepler Holtz Electrical Machine:* J. L. Campbell.

14. *A Florida Shell Mound:* U. F. Glick.

15. *Note on Rock Flexure:* E. M. Kindle.

16. *The Alternate-Current Transformer with Condenser in one or both Circuits:* Thomas Gray.

17. *Elastic Fatigue of Wires:* C. Leo Mees.

18. *A Warped Surface of Universal Elliptic Eccentricity:* C. A. Waldo.

19. *Accurate Measurements of Surface Tension:* A. L. Foley.

20. *Effect of the Gaseous Medium on the Electrochemical Equivalent of Metals:* C. Leo Mees.

21. *Some new Laboratory Appliances in Chemistry:* H. A Huston.

22. *Volumetric Determination of Phosphorus in Steel:* W. A. Noyes and J. S. Royse.

23. *Action of Ammonia upon Dextrose:* W. E. Stone.

24. *Action of Zinc Ethyl on Ferric Chloride and Ferric Bromide:* H. H. Ballard.

25. *The Sugar of the Century Plant:* W. E. Stone and Dumont Lotz.

26. *Camphoric Acid:* W. A. Noyes.

27. *Action of Potassium Sulfhydrate upon Certain Aromatic Chlorides:* Walter Jones and F. C. Scheuch.

28. *A New Phosphate:* H. A. Huston.

29. *Dip of the Keokuk Rocks at Bloomington, Ind:* Edward M. Kindle.

30. *Structural Geologic Work of J. H. Means in Arkansas:* J. C. Branner.

31. *Wave Marks on Cincinnati Limestone:* W. P. Shannon.

32. *Correlation of Silurian Sections in Eastern Indiana:* V. F. Marsters and E. M. Kindle.

33. *Some New Indian Fossils:* C. E. Newlin.

34. *Extinct Fauna of Lake County:* T. H. Ball.

35. *Strepomatidæ of the Falls of the Ohio, with their Synonymy:* R. Ellsworth Call.

36. *Streams of Southeastern Indiana, with List:* H. M. Stoops.

37. *The Swamps of Franklin County:* H. M. Stoops.

BIOLOGICAL SUBJECTS.

38. *Water Cultures of Indigenous Plants:* D. T. MacDougal.

39. *Working Shelves for Botanical Laboratory:* Katherine E. Golden.

40. *New Apparatus for Vegetable Physiology:* J. C. Arthur.

41. *Collections of Plants made during the Year:* M. B. Thomas.

42. *The Flowering Plants of Wabash County:* A. B. Ulrey and J. N. Jenkins.

43. *Revision of the Phanerogamic Flora of the State:* Stanley Coulter.

44. *Report of Progress of the Botanical Division of the State Biological Survey:* L. M. Underwood.

45. *Value of Seed Characters in Determining Specific Rank in the Genus Plantago:* Alida M. Cunningham.

46. *Additions to the Fish Fauna of Wabash County:* W. O. Wallace.

47. *Notes on the Reptilian Fauna of Vigo:* W. S. Blatchley.

48. *Preliminary List of Birds of Brown County:* Edward M. Kindle.

49. *The Birds of 1893:* Amos W. Butler.

50. *Some Notes on the Blind Animals of Mammoth Cave, with Exhibition of Specimens:* R. Ellsworth Call.

51. *The Batrachians and Reptiles of Wabash County:* W. O. Wallace.

52. *On the Occurrence of the Whistling Swan (Olor columbianus) in Wabash County:* A. B. Ulrey.

53. *Birds of Wabash County:* A. B. Ulrey and W. O. Wallace.

54. *Birds Observed in the Sawtooth Mountains:* B. W. Evermann and J. T. Scovell.

55. *Animal Parasites Collected in the State during the year 1894:* A. W. Bitting.

56. *Angling in the St. Lawrence and Lake Ontario:* Barton W. Evermann.

57. *Indiana Mammals:* Amos W. Butler.

58. *Mimicry in Fishes:* W. J. Moenkhaus.

59. *Variation in Leuciscus:* C. H. Eigenmann.

60. *The Redfish of the Idaho Lakes:* B. W. Evermann and J. T. Scovell.

61. *Observations upon Some Oklahoma Plants:* E. W. Olive.

62. *Rediscovery of Hoy's White Fish or Moon-eye (Argyrosoma hoyi):* Barton W. Evermann.

63. *Saxifragaceæ of Indiana:* Stanley Coulter.

64. *The Range of the Blue Ash:* W. P. Shannon.

65. *Plant Products of the U. S. Pharmacopœa* (1890): John S. Wright.

66. *Noteworthy Indiana Phanerogams:* Stanley Coulter.

67. *Methods of Infiltrating and Staining in oto the Heads of Vernonia:* E. H. Heacock.

68. *Embryology of the Ranunculaceæ:* D. M. Mottier.

69. *Certain Chemical Features in the Seeds of Plantago Virginiana and P. Patagonica:* Alida M. Cunningham.

70. *Root System of Pogonia:* M. B. Thomas.

71. *Salt-rising Bread:* Katherine E. Golden.

72. *An Increasing Pear Disease in Indiana:* L. M. Underwood.

73. *Notes on the Florideæ:* Geo. W. Martin.

74. *Measurement of Strains Induced in Plant Curvatures:* D. T. MacDougal.

75. *The Stomates of Cyas:* Edgar W. Olive.

76. *The Buckeye Canoe of 1840:* W. P. Shannon.

77. *Embryo-Sac of Jeffersonia Diphylla:* Frank M. Andrews.

78. *Cell Structure of Cyanophyceæ:* Geo. W. Martin.

79. *Some Notes on the Amoeba:* A. J. Bigney.

80. *Variations of the Polyporus lucidus:* L. M. Underwood.

81. *Preliminary Account of the Development of Etheostoma Cæruleum:* A. B. Ulrey.

82. *Embryology of the Cupuliferæ:* D. W. Mottier.

83. *Embryology of the Frog:* A. J. Bigney.

84. *Variation in Etheostoma:* W. J. Moenkhaus.

85. *Blood Corpuscles of very Young Human Embryo:* D. W. Dennis.

86. *Poisonous Influences of some Species of Cypripedium:* D. T. MacDougal.

87. *Development of Sexual Organs of Cymatogaster:* C. H. Eigenmann.

88. *The Vegetation House as an Aid in Research:* J. C. Arthur.

89. *The Proposed New Systematic Botany of North America:* L. M. Underwood.

SCIENTIFIC JOURNALS.

ASTROPHYSICAL JOURNAL, FEB.

On a Lens for Adapting a Visually Corrected Refracting Telescope to Photographic Observations with the Spectroscope: James E. Keeler.

Schmidt's Theory of the Sun: E. J. Wilczynski.

A Cloud-Like Spot on the Terminator of Mars: A. E. Douglass.

Preliminary Table of Solar Spectrum Wave-Lengths. II.: H. A. Rowland.

Photographic Observations of Eclipses of Jupiter's Satellites: Willard P. Gerrish.

The Arc-Spectra of the Elements. II. Germanium: H. A. Rowland and R. R. Tatnall.

Comparison of Photometric Magnitudes of the Stars: Edward C. Pickering.

The Spectrum of δ Cephei: A. Belopolsky.

Minor Contribution and Notes; Reviews; Recent Publications.

NEW BOOKS.

Butterflies and Moths. W. Furneaux. London and New York, Longmans, Green & Co. 1894. Pp. xiv+358. $3.50

Elements of Astronomy. George W. Parker. London and New York, Longmans, Green & Co. 1894. Pp. 236. $1.75.

Steam and the Marine Steam-Engine. John Leo. London and New York, Macmillan & Co. 1894. Pp. xiv+196. $2.50.

Memoir of Sir Andrew Crombie Ramsay. Sir Archibald Geikie. London and New York, Macmillan & Co. 1895. Pp. x+397. $4.00.

Meteorology. Thomas Russell. London and New York, Macmillan & Co. 1895. Pp. xxiii+277. $4.00.

The Supremacy of the Spiritual. Edward Randall Knowles. Arena Publishing Co. 1895. Pp. 61.

The International Beginning of the Congo Free State. Jessie Siddall Reeves. Baltimore, Johns Hopkins University Press. 1894. Pp. 106.

Report of work of the Agricultural Experiment Stations of the University of California; Being a Part of the Report of the Regents of the University. Sacramento, 1894. Pp. 506.

The Cause of Warm and Frigid Periods. C. A. M. Taber. Boston, Ellis. 1894. Pp. 80.

Electrical Engineering for Electric Light Artisans and Students. W. Slings and A. Brooker. London and New York, Longmans, Green & Co. New and Revised Edition. 1895. 8°, pp. vii+753. 346 illustrations.

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Subscriptions (five dollars annually) and advertisements should be sent to the Publisher of Science, 41 East 49th St., New York, or Lancaster, Pa.

*ON DR. WILLIAM TOWNSEND PORTER'S INVESTIGATION OF THE GROWTH OF THE SCHOOL CHILDREN OF ST. LOUIS.**

Dr. Porter's investigations on the growth of the school children of St. Louis claim particular attention, as the author opens a number of new problems and proposes new methods of inquiry. His conclusions are far-reaching and have a close relation to the method of treatment of a number of questions. It is the importance of these investigations, which are based on very extensive material, which induces me to subject the author's methods to an examination.

Dr. Porter's scheme of measurements is based largely upon that used by Dr. H. P. Bowditch in his investigations in Boston, and on the one which I used in the collection of data in Worcester, Mass. To these the measurements of girth of chest and of strength of grasp are added. It must be regretted that Dr. Porter determined the age of the child at its nearest birthday,

*1. The Physical Basis of Precocity and Dullness. (Transactions of the Academy of Science of St. Louis, Vol. VI., No. 7, March 23, 1893.)

2. The Relation between the Growth of Children and their Deviation from the Physical Type of their Sex and Age. (Ibid., Vol. VI., No. 10, November 14, 1893.)

3. Untersuchungen der Schulkinder in Bezug auf die physischen Grundlagen ihrer geistigen Entwicklung. (Verh. d. Berliner Gesellschaft für anthropologie, 1893, pp. 337–354.)

4. The Growth of St. Louis Children. (Transactions of the Academy of Science of St. Louis, Vol. VI., No. 12, April 14, 1894, pp. 263–380; republished in Quarterly Publications of the American Statistical Association, N. S., No. 24, Vol. III., December, 1893, pp. 577–587.)

5. The Growth of St. Louis Children. (Ibid., Nos. 25, 26, Vol. IV., March–June, 1894, pp. 28–34.)

while heretofore all investigators determined the age in full years. There exists, therefore, a difference of half a year between the period of Dr. Porter's tables and all others which makes a comparison difficult.

Dr. Porter bases all his discussions on the assumption that all series of observations of children of any given age are probability curves, and he illustrates this point by a detailed discussion of the observations on stature of eight-year-old girls. In connection with this subject he discusses the meaning of the probable deviation, of the mean, and of the average value of the series. Although he employs both the mean and average values, he evidently inclines toward the use of the former. I will not dwell at length upon the fact that whenever the curve is really a probability curve the average is a better value than the mean, because it is more accurate, nor on the other fact that the mean deviation gives a more constant value than the probable deviation, and is therefore the better value, as both considerations have little practical bearing, although I consider them of importance from a theoretical point of view.

It may be granted for a moment that the curves are probability curves. Then there remain two objections to Dr. Porter's values. The one is that the difference in numbers of individuals observed for each year is not taken into consideration. This difference in numbers has the effect that the average age of all individuals whose nearest age is six years will be a little more than six years. These corrections amount to more than 3% of the annual growth, during the first and last years to even more. It affects the annual rate of growth of stature to the amount of several millimeters, the weight to the amount of half a pound.

Furthermore, Dr. Porter makes a linear interpolation for determining the mean, while the general curve ought to be taken into consideration. The determination of the 50% point of a series ought to be based on the values found at two points, at least, on each side. The same may be said of the interpolation for all the other percentile grades. The corrections made necessary by these two causes are not great, but sufficient to make all the millimeters and tenths of kilograms inaccurate.

A more important objection is based on the fact that the observed curves are *not* probability curves. In examining Dr. Porter's curve for stature of girls of 8 years of age (paper No. 4, p. 286), it will be seen that in the first part of the table the differences between theory and observation are all positive, while in the second part they are, with one exception, all negative. When the curves of stature, weight, span of arms, height sitting, girth of chest for girls from 12 to 15 years of age, and for boys from 14 to 18 years of age are consulted it will appear that the asymmetry is still more marked. Dr. Porter himself quotes at length Dr. Bowditch's remarks on this asymmetry (Ibid., p. 298), and calls attention to the difference between mean and average. These constantly occurring differences and their regular distribution are the very best proof that the curves under consideration are not probability curves. If this is the case, neither the average, nor the mean, nor the most frequent value represent the type of the age to which the curve refers. This can be determined only by a detailed examination of the causes of the asymmetries.

I have stated at a former time (Science, Vol. XIX., May 6, May 20, 1892) what I believe to be the cause of this asymmetry, and I will revert to this subject after the discussion of one of Dr. Porter's most fundamental deductions.

He concludes, from his data, that the basis of dullness is deficient physical development; that the basis of precocity is an unusually favorable physical development.

His method has been to compare the measurements of all children of a certain age attending various grades of schools. He found that those in the lower grades were inferior in their measurements to those attending the higher grades. He expresses this result in the following language (No. 1, p. 168): "Precocious children are heavier, and dull children lighter, than the mean child of the same age. This establishes a basis of precocity and dullness." I believe that the method of investigating this point is not free of objections. It would, indeed, be a serious accusation against the teachers of St. Louis if they should entirely disregard the effects of physical development in grading their pupils. However crudely this may be done, it is certainly done to a limited extent. Sickly children who stay out of school for a great portion of the term will lag behind; vigorous ones will advance more rapidly. Be this the case or not, the fact remains that children who are physically more vigorous accomplish a greater amount of mental work. But I do not believe that Dr. Porter's wording of the phenomenon conveys the correct interpretation. I should prefer to call the less favorably developed grade of children retarded, not dull; and these terms are by no means equivalent, as a retarded child may develop and become quite bright. In fact, an investigation which I had carried on in Toronto with the same object in view, but according to a different method, gives just the reverse result. The data were compiled by Dr. G. M. West, who found that the children pronounced by the teacher as bright were less favorably developed than those called dull by their teachers. Furthermore, I do not believe it is correct to say that the facts found by Dr. Porter establish a basis of precocity and dullness, but only that precocious children are at the same time better developed physically; that is to say, the interesting facts presented by Dr. Porter prove only that children of the same age who are found in higher grades are more advanced in their general development than those who are found in lower grades. Dr. Porter has shown that mental and physical growth are correlated, or depend upon common causes; not that mental development depends upon physical growth.

This brings me back to the question of the cause of the asymmetries of the observed curves. According to the above interpretation of Dr. Porter's results (which is merely a statement of the observed facts), we must expect to find children of a certain age to be on different stages of development. Some will stand on the point corresponding exactly to the age, while others deviate from it. This was the assumption which I made in the paper quoted above, when trying to explain the asymmetries of the curves, and I consider Dr. Porter's observations a very strong argument in favor of my theory, which is briefly as follows:

When we consider children of a certain age we may say that they will not all be on the same stage of development. Some will have reached a point just corresponding to their age, while others will be a little behind, and still others in advance of their age. Consequently the values of their measurements will not exactly correspond to those of their age. We may assume that the difference between their stage of development and that belonging to their exact age is due to accidental causes, so that just as many will be less developed as further developed than the average child of a particular age. Or, there will be as many children on a stage of development corresponding to that of their age plus a certain length of time as corresponding to that of their age minus a certain length of time.

The number of children who have a certain amount of deviation in time may be assumed to be arranged in a probability curve, so that the average of all the children will be exactly on the stage of development belonging to their age.

At a period when the rate of growth is decreasing rapidly, those children whose growth is retarded will be further remote from the value belonging to their age than those whose growth is accelerated. As the number of children above and below the average of development are equal, those with retarded growth will have a greater influence upon the average measurement than those whose growth is accelerated, therefore the average value of the measurement of all the children of a certain age will be lower than the typical value, when the rate of growth is decreasing; higher than the typical value when the rate of growth is increasing. This shows that the averages and means of such curves have no meaning as types. I have shown in the place quoted above, how the typical values can be computed, and also that for stature they differ from the average up to the amount of 17 mm.

These considerations also show clearly that the curves must be asymmetrical. Supposing we consider the weights of girls of thirteen years of age, the individuals composing this group will consist of the following elements: girls on their normal stage whose weight is that of the group considered, advanced girls, and retarded girls. In each of these groups which are represented in the total group in varying numbers, the weights of the individuals are probably distributed according to the laws of chance, or according to the distribution of weights in the adult population. What, however, will be the general distribution? As the rate of increase of weight is decreasing, there will be crowding in those parts of the curves which represent the girls in an advanced stage of development, and this must cause an asymmetry of the resultant general curve, which will depend upon the composition of the series. This asymmetry does actually exist at the period when the theory demands it, and this coincidence of theory and observation is the best argument in favor of the opinion that advance and retardation of development are general and do not refer to any single measurement.

Futhermore, the increase in variability until the time when growth begins to decrease, and its subsequent decrease, are entirely in accord with this theory. I have given a mathematical proof of this phenomenon in the paper quoted above (Science, May, 1892). Dr. Porter has called attention to the same phenomenon in his paper of November, 1893, but I believe his formulation is not sufficiently general, nor does he give an interpretation of the phenomenon which may be explained as follows: The probability of a child not being in the stage of development corresponding to its age follows the laws of chance. With increasing age the mean deviation from the normal type must increase. Assuming that at the age of four years, .5 year represents the mean deviation, then a certain number of children will be in the stage of development corresponding to 3.5 and 4.5 years. At the age of sixteen years the mean deviation will probably be one year, and just as many children would be on the stages of fifteen and seventeen years as there were of the four-year old children on the stages of 3.5 and 4.5 years. The absolute amount of growth (in girls) from fifteen to seventeen years is less than from 3.5 to 4.5, so that for this reason a decrease in variability must be found at the time when the rate of growth begins to decrease. On the other hand, the difference between individuals which will finally become tall or short, increases with the increase of growth, so that the combined effect of these counteracting causes will be a maximum of variability at the period preceding puberty. Dr. Porter's formulation of the phenomenon (No. 2, p.

247) that "the physiological difference between the individual children in an anthropometric series and the physical type of the series is directly related to the quickness of growth" does not quite cover the phenomenon.

It will be seen from these arguments that the very natural supposition that some children develop more slowly than others is in accord with all the observed facts. It was necessary to prove this in some detail, because the further interpretations made by Dr. Porter largely hinge upon this point.

These conclusions are based on the assumption that "the type at a certain deviation from the mean of an age will show the same degree of deviation from the mean at any subsequent age; for example, a type boy in the 75 percentile grade at age 6 will throughout his growth be heavier than 75 per cent. of boys of his own age." (No. 4. p. 293.) This assumption which I have criticised on a former occasion (SCIENCE, Dec. 23, 1892, p. 351), is most decidedly incorrect, and with it fall all the conclusions in regard to the growth of tall children and short children.

We know a number of facts which show plainly that the assumption is incorrect. It has been shown in Dr. Bowditch's tables that Irish children are shorter than American children. If the position of the American child is expressed in percentile grades of the whole Boston series, and that of the Irish child in the same manner, it will be seen at once that they diverge more and more with increasing age. Pagliani's measurements of Italian children and my own of Indian tribes of different statures bring out the same point still more strongly.

I think the error underlying the assumption that the average children retain their percentile rank can be shown best in the following manner: We know by means of observations the distribution of measurements for certain ages. If the assumption is made that the same children remain on the average in the same percentile grade a certain very complex law of growth follows. We may invert this reasoning by saying: Only if the assumption of a certain very complex law of growth is made can the same children remain in the same percentile grade. For any different law of growth they would change from one grade to another. There is no inherent probability in this law; on the contrary, it was quite unexpected and surprising when first promulgated. As a matter of fact, three factors condition the rate of growth: hereditary influences, the preceding life history of the individual and the average conditions during the period under consideration, and it is quite unlikely that these factors should always be found to stand in such a relation as to result in general stability of percentile grades.

As the facts disprove this assumption, and as the cause of the asymmetries remains entirely obscure under it, while they can be fully explained in all their details by the theory advanced before, I cannot acknowledge that the conclusions reached regarding the growth of tall and short children are correct.

On pp. 339–348 of his fourth paper Dr. Porter makes a valuable suggestion regarding the practical application of measurements to the determination of the stage of development of individuals. His proposal is to compute the distribution of weight, chest, girth and others correlated to various heights. Then all children are under the suspicion of being abnormally developed who differ much from the standard values. Dr. Porter assumes the narrow limits of the probable deviation as the limits of normal variability. It may be a question where these limits ought to be drawn, but there can be no doubt that this method is much better than the one applied in our gymnasia, namely, that the individual is expected to be in all his measurements on the same percentile grade. This latter method is based on a quite erroneous theory of the proportions of the body. Dr. Porter's method is also better than that based on single measurements, as it points out abnormal proportions, not simply abnormal size. It is necessary, however, to bear in mind the one restriction that many measurements are not closely correlated with stature, but have different correlations. This is the case with girth of chest, strength of squeeze and many others. Therefore their correlation to stature will not give more satisfactory results than the study of the single measurements alone. It will certainly be of great use to school hygiene to subject all children whose proportions are abnormal to a medical examination, but it will not be possible to determine by means of the measurements what individuals are retarded in growth and what are advanced, as Dr. Porter suggests, except in very exceptional cases. The correlation between any two measurements is so slight that a great many cases which are normal for one year are also quite normal for the preceding and following years at least. This is also shown by the fact which is apparently so contradictory, that children of a certain height are the heavier the older they are (according to Bowditch), but that also children of a certain weight are the taller the older they are.

Finally, I must say a word in regard to Dr. Porter's objection to the combination of measurements taken in different cities. It is, of course, true that the results in various cities depend upon the composition of the population and its geographical and social surroundings. If we knew all these factors and their influences it would be necessary to sub-divide the series of each city into numerous divisions. As we do not know the exact influence of these factors, we must endeavor to take as our basis a general curve, including as many individuals as possible of the same population but under a diversity of conditions and compare the curves determined by certain factors with them. It is, therefore, perfectly correct to compute the growth of American children from data collected in various cities, provided each city is given its proper weight according to the number of children measured. The more cities and villages are included in such a combination, the more nearly we shall get the curve representing the growth of the American child. By comparing the general curve with the ones obtained in different cities we can investigate the causes which produce the difference between the individual curves and the general curve. We know that nationality, occupation, social status have a considerable influence. I have found that first-born children exceed later-born children in size. The effect of all these causes can be studied by comparing the individuals representing each group of factors with the general population.

FRANZ BOAS.

NEW YORK.

LABORATORY TEACHING OF LARGE CLASSES IN BOTANY.*

THE great increase in the size of the classes in Elementary Botany during late years in Harvard College has forced their teachers to the development of some system for their efficient and economical management in the Laboratory. Under the guidance of Professor Goodale there has been worked out the plan upon which are based the recommendations made in this paper; indeed, what I have to say is little more than a description of the system in use there during the last year I was connected with it, *i. e.*, 1892–'93. My observations are, therefore, based not upon theory alone, but upon the results of trial and selection.

*Read before the American Society of Naturalists, Baltimore, Dec. 28, 1894.

The conditions which had to be faced were these: A class, numbering towards, and in one year over, 200 men, and likely in the future further to increase, composed of beginners ignorant of how to study *things*, comes in for a course in General Botany, extending from the middle of February to the first of June, in all some fifteen weeks. There are two regular weekly lectures. The Laboratory work cannot for academical reasons exceed an average of four hours per week, and for practical reasons it must be confined to the hours 11–1 and 2–5 Tuesday, the same hours on Thursday, and 9–1 Saturday, *i. e.*, only 14 hours in the week are available. The normal seating capacity of the Laboratory is 75, but the supply of dissecting microscopes and boxes for students' utensils, books, etc., is enough for over three-fourths of the class.

I give thus fully a statement of the conditions at Harvard, because they illustrate in kind, though perhaps in unusually favorable degree, the difficulties which in more or less modified form must be faced in all large colleges providing elementary laboratory instruction, and to which an efficient system of laboratory management must be adapted. These conditions may be classified for purposes of discussion as follows:

1. The classes are too large for individual teaching by the instructor.

2. Laboratory hours must be adjusted to other academic work, to insufficient accommodations and sometimes to yet other considerations.

3. Many students of diverse attainments must be taught how to work and to think scientifically, and must be kept progressing together through the stages of a logically-graded course.

4. Large quantities of special material must be provided at an unfavorable season.

I have placed first what all admit to be the greatest drawback to large laboratory classes, but one which seems inseparable from our unwieldy colleges, *i. e.*, the impossibility of individual knowledge of and contact with his students by the instructor. That this kind of teaching, this diagnosis of each case and fitting of proper treatment to it, is the only good kind, and that no development of methods or systems, or of leadership of the whole class by one man, can replace it, is pedagogically so axiomatic that the instructor should here take his stand squarely and insist that his students shall have it, if not from him directly, then from competent assistants trained by him. I regard this as the first great essential in the laboratory teaching of large classes—competent assistants.

The source of supply of such assistants is not far to seek; they should come from amongst the advanced students who have been through the course and who intend to make teaching a profession. Any college with elementary courses large enough to need such a system as we are discussing must have advanced students in proportional numbers, and skillful management of the real advantages of the position should give the instructor his choice from among them. In Harvard College the supply has always exceeded the demand; the prestige, experience and money attaching to the positions make them attractive to the best men.

The assistants having been thus selected, it is essential to place each in full charge of a section which he keeps without change to the end of the term, in order that he may come to know well and teach well each individual. These sections should never exceed thirty men, and twenty-five is a much better, and twenty the best number. The instructor himself will, of course, visit the laboratory constantly, but he will do far better to go about among the men generally than to take a section himself. Moreover, great freedom should be allowed to the judgment of each assistant in the details of his teaching. There must be, to be sure, a uniform plan of study for the course, but the carrying-out of the plan in details should be left to the assistant, who should be held responsible for his results rather than his methods. It is very desirable, or perhaps, I should say, necessary, to hold weekly meetings of the assistants at which the coming laboratory topics are discussed, uniform ways of treating difficult or morphologically debatable questions agreed upon, and pedagogic advice given, the latter, as I have found, always eagerly received and acted upon. In this way, in conjunction with the weekly guides to be mentioned below, all desirable uniformity of treatment can be secured.

It is necessary, and indeed, good policy as well, to pay the assistants; the amount will vary according to the general scale of expenditure in vogue in the particular college. One dollar an hour may be considered fair, perhaps the maximum that it is needful to pay.

Let us consider secondly how conflicting hours may be adjusted to insufficient accommodations, and to the need of bringing each man always under the same assistant. The solution of this often appalling problem can be found only in this: the instructor must claim for his Laboratory work equal rank with any other college exercises, make the choice of hours, or rather sections, as wide as possible, and require students to work exactly in these sections or else remain out of the course. The size of the sections must be limited partly by the number an assistant can manage, partly by the seating accommodation of the laboratory. Thus a room of fifty to sixty seats can accommodate two sections at once. The hours should be arranged so as to give at least two hours of consecutive time; the best arrangement for a four-hour-a-week course is to have each section meet in two-hour periods at the same hours on two different days. Thus a section meeting 11–1 Tuesday would meet 11–1 Thursday. No student should be allowed to break hours and come in different sections if it can possibly be avoided. To arrange the students in sections, each should be asked to hand in, at the opening of the work, his preference and his second choice. The great majority can be assigned to their preference, only a few, selectable by lot, need to be placed in their second choice in order to adjust the sizes of the sections. In order to prevent all confusion, we have found it very useful to give each student a card stating the number of his section, of his seat, of his microscope, of his box and the name of the assistant, and to check off for each section on blue-print plans of the laboratory and lists of instruments, etc., the numbers as assigned. By this plan successive sections may use the same seats and instruments without confusion and each come always under its own assistant.

We have next to notice how the labor and confusion of getting the sections to work may be minimized and the time of the assistants economized for the higher grades of their teaching work, and how the sections may be kept progressing uniformly. The beginner (and for that matter the most advanced of students), when a new topic is placed before him, has no idea of what he is to study about it, of what is important and what is not, of the nomenclature he is to employ. The questions "what am I to do with it?" "what do you want me to do next?" dreadful as they sound, are yet natural enough. If these questions can be answered for each student without reference to the assistant it is an immense gain, and they can be answered by a printed guide or synopsis of the week's work supplied each week to each student. These should be arranged upon the approved plan in use in the many excellent laboratory manuals, *i. e.*, they should indicate the points which it is needful to study, suggest some idea of their relative importance, give needed bits of information now and then, and in general supply just enough data to allow the student to work by himself to correct conclusions. But an ordinary laboratory manual is not sufficient, for a great value of these weekly guides is that they fit the exact material to be used, the state of advancement of the class, and the logical course laid out by the instructor, which cannot be the same as that of anybody else's manual. These guides may also be made to supply botanical terms, always upon the good pedagogic principle of making the student feel the need of a term before supplying it, and then offering it not as a term with a definition, but as a definition or description which can be expressed in a single word. The effect of these guides upon the order and rapidity of work is remarkably great, and they enable one assistant to teach a much larger section than is possible without them.

It is also of very great value to the laboratory work to have the lectures accompany, and actually, as they do theoretically, supplement it. This is practically possible, though perhaps not always convenient. The most logical course (to be briefly described immediately) that I have been able to develop in my few years' teaching does allow the lectures to keep with and supplement the laboratory work throughout the term. Laboratory study must always be the study of a few type forms ; the correlation of the data thus gained, their bearing upon general principles and their relation to the science as a whole must be the function of the lecture, and this is the better performed when the latter follows as closely as possible upon the former and while it is still fresh in mind. A few minutes at the beginning of each lecture devoted specially to the topics of the laboratory work just past, and its relation to what is to come, has been found to be very profitable.

We come finally to our fourth and last problem, how can good materials be provided in the winter to such large classes? A college which has abundant greenhouses hardly needs to ask this question. What remarkable results may be obtained in providing large quantities of material from small space is shown by Mr. B. M. Watson's work at the Bussey Institution in supplying material to the classes at Harvard. For those less fortunately situated, its solution is to be found in so arranging the course that materials available in the markets or easily grown, come first, and are gradually replaced by out-door materials as the season advances. Happily the most logical plan of treatment for a general course in Botany lends itself exactly to this procedure. Experience has shown that with elementary classes it is desirable to consider plant life as a cycle, which may best be broken for study at the seed. If now the structure and morphology of the seed be the first topic in both Laboratory and Lectures, and its development into the young plant the second, and if then the plant-organs leaf, root, stem, flower and fruit be treated in succession, we are in both brought back to the starting point, the seed. If, moreover, in the lectures, the full biology and physiology of each organ be considered along with its anatomy and morphology and as determining these, then are the topics not only treated in the most logical and instructive fashion, but the lectures and laboratory work may be kept together, the one truly supplementing the other; and the topics are taken up in the order which allows material best obtainable in winter to come first, gradually giving place to that which the spring offers. The seed, always obtainable, comes first, then follow germinating embryos and young plants easy to grow in wardian cases in class rooms or at very small cost in the nearest greenhouse. Leaves may be obtained from the same greenhouse, from evergreen shrubs out of doors or even bought in the markets, as celery, cabbage, etc. Roots may likewise come from the markets, stems and buds abundantly from the trees out of doors, and towards spring the latter may be forced to open in warm rooms. Far too little use is made of these easily obtained materials. By the time the vegetative organs have been studied the first Apetalæ will be in bloom, and if the students have been properly taught to use eyes and hands the Apetalæ will present no difficulties; later come other wild flowers, and all is easy.

Allow me, in conclusion, to sum up the points of this paper. In the laboratory teaching of large classes, the first essential is a recognition of the fact that nothing can replace individualism in teaching and that a sufficient number of assistants should be employed. These assistants must be intending teachers, given some pedagogic instruction, supplied with a uniform plan of work, but left very free in the details of their modes of reaching the students. Classes should be divided into sections with fixed hours and containing not more than thirty men, over each of which one assistant has entire charge until the end of the term. As an aid to uniformity of plan and to answer the innumerable legitimate questions which arise in laboratory work, as well as to supply technical nomenclature, weekly printed guides, fitted to the exact work being done, should be supplied to each student. Lectures and laboratory work should be kept together and follow such a course that the vegetative organs upon which material is at all times available should be studied in the winter, and the reproductive organs in the spring or summer.

So much for a general plan; each teacher must vary it in adaptation to his own needs. W. F. Ganong.

Smith College.

MAGNETISM AND THE WEATHER.

Much time has been devoted to the study of magnetic and meteorologic observations with the hope of establishing a definite connection between the two. The results thus far have been almost entirely negative, although a connection has been found with auroras, and the diurnal range of air pressure is now believed to be a thermo-electric phenomenon, allied to the diurnal range in the swings of the magnetic needle. There are certain well established facts that have been ascertained regarding magnetism that almost always stand at the base of all such investigations, although it is admitted that magnetic phenomena are extremely complex, and those of the weather are far more so.

1. The three principal magnetic conditions or fluctuations are as follows: (a) The diurnal change due to some combined solar and terrestrial action. (b) Magnetic storms, which are peculiar and sharp disturbances, generally originating in the sun. These often occur at three or four successive rotations of the sun.

(c.) A gradual change in magnetism from one day to the next. These are quite singular, and have been studied more than any other conditions in the hope of establishing some relation with our weather.

2. In studies of magnetism strenuous and long continued efforts have been made to establish a regular recurring period depending upon the rotation of the sun. It is easy to see that if there were such regular period its discovery would be of the profoundest significance. The results of such studies, however, have been far from satisfactory. It is known that sunspots have a different period of rotation, according as they are near or far from the equator, and this fact is enough to show the extremely dubious nature of an attempt to fix on any definite period for recurring solar effects. It is not at all surprising that more than a

score of periods have been determined from 25.5 to 27.5 days, depending somewhat upon the data employed and upon the method of its manipulation. It is very certain that, if any one will take the 'horizontal force,' for example, and arrange the observations in intervals of 26 days (the best thus far found) he will quickly find that, judging from the disturbed days, there is absolutely no fixed interval. These disturbed days would seem the very best material for such studies, as they are very definite. These days will occur for three or four rotations most beautifully, but after that the disturbance disappears and no more will appear along that line for a score of rotations.

In the same way one will very quickly find, in using the data and leaving out the disturbed days, that there is absolutely no recurring period of 26 days or any fraction of that interval. Sometimes by grouping ten rotations one will find a fairly good fluctuation, but the very next group of ten rotations will make 'hodge podge' of the previous group. This would seem an extremely important point to settle, as months have been devoted to fruitless efforts in trying to determine such a period.

3. The fluctuations under (c) above are simultaneous over the whole earth, as has been shown by the records at Batavia, India, Los Angeles, Cal.; St. Petersburg and Tiflis, in Russia; Vienna, Austria; Washington, D. C., and Zikawei, China. One is struck at once by the wonderful regularity of these fluctuations over the whole Northern Hemisphere. Making allowance for the difference of time and for disturbed days, the fluctuations are found to be exactly the same at each station, and the record at a single station will answer perfectly for comparison with any supposed related meteorologic phenomenon.

4. After thirteen years of study and careful discussion I am satisfied that the pressure of the air, or perhaps the fluctuations of the dew point, are by far the best to use for determining a possible connection with magnetism. I am also perfectly satisfied that, except in the cases specified above, there is no direct relation between magnetic and meteorologic phenomena, and this is also the outcome of the exhaustive studies in England and on the continent. I am also satisfied that there is an indirect relation, but the phenomena are so extremely complex that it has proven impossible to determine it up to the present.

5. In all studies of this character, and in all attempts at determining coincidences between such phenomena, one will always find a most valuable check by cutting up the long list of rotations into groups of 7, 10 or 14 rotations in each. If these separate groups do not show a thread running through them, or fluctuations common to all and continually recurring, he may be satisfied that there is nothing in it. There is a peculiar and well-nigh unaccountable fascination in arranging and summing groups of figures in the hope that something may come, but continuous effort will show that there is something back of it all which is not understood, and no headway can be made by direct comparisons.

H. A. HAZEN.

FEBRUARY 1, 1895.

SIMILAR INVENTIONS IN AREAS WIDE APART.

As a contribution to the much disputed question of the occurrence of similar inventions in areas wide apart, I desire to call the attention of readers to the device for weaving of which I have found abundant examples in the Pueblo country, in the New England States, and in Finland.

The apparatus consists essentially of a small rectangular frame-work in which are placed a series of perpendicular slats perforated in the middle. It has the appearance of a grating of small bars about one-sixteenth of an inch apart, and each bar is pierced in the middle. In fact, all of these are the harness of a small loom used in weaving tape, braid, garters, belts and the like.

Among the old-time families of New England, this apparatus is set up by taking a ball of twine or thread which is to constitute the warp, and walking around a number of chairs placed at a distance from one another as many times as there are to be threads in the warp. This coil is then cut apart, one end tied together in a knot, and the separate threads of the other end passed through the holes of the slats and between them. This apparatus is worked by lifting and depressing this frame as the weft shuttle is passed backward and forward by the hand. At each turn the weft is beaten home by the harness, the lower end of which is held between the knees, by the shuttle, or by the hand.

In a Zuñi example in the Museum set up by Mr. Cushing, the weaver sits upon the ground, having the far end of the warp fastened to some part of the building, and the proximal end attached to a stick forming part of a belt. The very same process employed by the New England woman is also in vogue among the Pueblos. By lifting and depressing the frame which is simply a couple of parallel sticks to which split reeds are tied, having holes burnt through the center, the weaver is able to pass the shuttle stick backward and forward.

When the Pueblo woman wishes to make short garters she uses the soles of her feet as a resting place for the little bar to which the far end of the weft is attached. Her shuttle is a stick on which the weft yarn is wound.

The Finnish harness is carved from a single block of wood, the upper and lower borders being somewhat cylindrical and the upright bars carved like little slats from the solid piece. These are perforated exactly after the manner of the New England examples.

I learn by inquiring at the Patent Office in Washington, that in Belgium a patent has recently been issued for an improvement on this style of weaving apparatus.

I leave the question open as to the amount of contact between the Fins, the New England housewife and the Pueblo woman. It is easy enough to account for the dispersion of this apparatus among the white people of Europe, and thence among the Fins and the New England farmers. The only question for us to inquire into now, is, where did the Pueblo woman learn to weave after this fashion?

Dr. Matthews tells me that the Navajo do not use this frame, but make their belts by means of a harness similar to that which they employ in making their blankets. It is also a question where and how the Navajos learned to set up a loom so much like those found among the primitive European weavers. It is a fact that the Aino employ precisely the same apparatus as do the Navajo.

O. T. Mason.

Washington.

THE SOCIAL SENSE.

All persons thrown intimately with children from about four years of age and later may serve psychologists by making detailed observations of what may be called '*chumming*' on the part of children and youth. By 'chumming' is meant all instances of unusually close companionship voluntarily made, 'platonic affection,' personal influence one over another when this influence is limited more or less to one person, and when the relationship is stronger than ordinary and is shown in any unusual or remarkable ways, such as bearing punishment for or with the other, moping or becoming very unsocial when separated. Cases of boys chumming with boys, and girls with girls, are especially valuable; and of older persons of the same sex. Similar observations are needed on cases of marked or unreasonable *antipathy* of one child to another.

The object of the inquiry is to get light on the growth of the child's social sense, what it is that attracts and repels him most in others. To this end observations on the following points are especially desired by the writer.

In every case of chumming or antipathy:

1. (a) Ask the child A why he loves or dislikes the child B. Take down the answers in full. (b) Repeat the question once a week for six weeks at least, if the phenomenon continues.

2. (a) Observe what A imitates most in B, and (b) whether he imitates the same actions or qualities in others besides B. (c) Note whether what A imitates in B is more prominent in B than in other persons.

3. (a) Observe how far A shares his toys, property, food, pleasures, etc., with B more than with other children. (b) Ask him why he gives his things to B. (c) Observe whether this keeps up if B does not reciprocate.

4. (a) Observe any cases in which A is willing to suffer for or with B. (b) Whether he will fight for him, or defend him with words (give details of actions or words of defense).

5. Observe whether B figures largely in A's dreams (a) by noting any speech aloud when sleeping, and (b) by asking A frequently what he dreamed about the preceding night (being careful not to suggest B to him in any way).

6. State all the details of the relation between A and B especially. (a) Do they see each other oftener than they do others? (b) Do they sit together in school? (c) Do they room or sleep together? (d) Have they any common infirmity or fault (stammering, defective vision, stooping, deceitfulness, &c.)? (e) Have they ever been punished or disgraced together in school or at home?

7. Give (a) what is *known* (not mere impressions) of the disposition of each; (b) the length of time they have shown the liking or antipathy.

8. In case of the breaking off of the liking or antipathy (a) note all the facts which lead to it. (b) Question each child as to why he has ceased to like or dislike the other.

9. When the relation is mutual make the same series of observations with the second child, B, as with the first, A (as given above).

10. Give the number of companions of each child reported on: (a) Number of brothers and sisters, and their ages and places of residence with or away from the child reported on. (b) Amount of time per day which the child spends with other children in school and on the street, etc.

11. Make special note of any unusual occurrences or action, showing the affection or antipathy, which are not covered by this schedule.

N. B. All observations should cover as many of these enquiries as possible, yet observations of some of them only should still be sent in. All observations should be carefully arranged under the headings of the schedule, *i. e.*, by the numbers, letters, etc., in order to secure correct classification. All reports and enquiries should be addressed to the undersigned at Princeton, N. J., and should bear the name and address of the sender plainly written. All names, personal details, etc., are strictly confidential, except when special consent to the contrary is given in further correspondence.

J. MARK BALDWIN.

PRINCETON.

AMERICAN STUDENTS AT THE NAPLES ZOÖLOGICAL STATION.

In another number of Science the steps leading to the establishment in 1892 of the United States Table at Naples, by the Smithsonian Institution are described. Upon behalf of Harvard College, Prof. Alexander Agassiz subscribed for a second table at the same time. Mr. Willian E. Dodge, of New York, has recently visited the Station, and has offered to contribute $250 a year for three years toward a third American Table. In response to this offer Dr. Anton Dohrn has sent to Mr. Dodge the following interesting letter, giving a complete history of American work at the Naples Station up to the present time:

"When I established the Station I had a correspondence with Professor Louis Agassiz, who greatly applauded my plans, but at the time was not in a position to establish any relations with us. In a later letter he told me that he had also begun to work in the same direction, having procured a sum of money and a suitable locality in Penikese Island, where he would try to establish a school of marine biology. In the year 1881 Professor Whitman, now of Chicago University, came to Naples, on his return from Japan, where he had been professor at Tokio for two years, and asked for permission to work in the Zoölogical Station. Although there was no American Table for him I offered him hospitality, and he remained for six months. Half a year later came Miss Nunn, availing herself of the table of the University of Cambridge, also for six months. In 1883 Dr. Sharp, from Philadelphia, spent two months at the Bavarian Table. In the same year the first American Table was engaged by Williams College for one year, and this table was first occupied by Prof. E. B. Wilson, now at Columbia College, for six months, and was engaged later by Professor Clarke, of Williams College, but owing to sickness he postponed coming until the year 1884. In 1885 the table was subscribed for one year by the University of Pennsylvania, and was occupied first by Dr. Dolley and later by Dr. Patten. Dr. Patten was also received for six months as our guest.

"All my efforts to secure the coöperation of other American colleges proved unsuccessful, and again the American naturalists took advantage of the English and German Tables. Dr. Cobb, of Massachusetts, occupied the British Association Table for two months. Mr. Norman, of Indiana, occupied the Hamburg Table; Mr. Ward, from Troy, the table of the Grand Duchy of Baden, and Mr. Kaufman one of the Prussian Tables. This was in the spring of 1891, when Major Davis first visited Naples and became acquainted with the state of things. He immediately offered, in a most generous way, to engage a table for his countrymen, and asked me not to admit any more Americans to the European tables. His table was immediately occupied by Dr. Russell, a botanist, who worked here during four months; by Miss Platt, of Boston, for three months, and again in the second year by Professor E. B. Wilson, then of Bryn Mawr. Dr. Corning, also an American, but occupying the post of assistant in Prague University, came upon one of the Austrian Tables, and Dr. Bashford Dean, of Columbia, upon the Bavarian Table, while the Davis Table was occupied by others. In the year 1893 the Davis Table was occupied by Dr. Field, of Baltimore, and Dr. Parker, from Harvard College. In the meantime Dr. C. W. Stiles, of Washington, who had paid a short visit to the Zoölogical Station in 1891, led a movement for the establishment of more direct official relations between American institutions and the Zoölogical Station, and finally upon the unanimous recommendation of the Society of American Naturalists, the secretary of the Smithsonian Institution entered into a contract for an American table for three years. Almost at the same time Professor Agassiz engaged a table for Harvard College for three years. Both of these tables are in demand by so many investigators that they still do not cover the needs of American students. In fact, there have always been more American occupants than tables, and I receive them willingly as guests. Dr. Fairchild, of Washington, Dr. Wheeler, of Chicago, and Professor Bumpus, of Brown University, occupied the Smithsonian Table in 1893–94, while Mr. Rice, of Washington, occupied the Harvard Table in 1894. In 1894–5 Dr. Murbach, of Berkeley, occupied the Smithsonian Table, while Dr. Child and Professor Ritter, of the University of California, occupied the Harvard Table. At the same time Professor Hargitt, of Syracuse University, and Professor Gardiner, of the University of Colorado, were received as guests. At the present time Professor Morgan, of Bryn Mawr, and Professor Leslie Osborn, of the University of Indiana, are occupying the Smithsonian Table, and Dr. Nutting and Professor Reighard are soon expected to arrive.

"These twenty-nine American naturalists have already profited by the Zoölogical Station, and many more would have come had arrangements been made earlier and on a larger scale. In comparison with European states, I may state that Germany rents eleven tables, Italy nine, Austria-Hungary three, England three, Russia three (which were discontinued this year, but are going to be continued). Spain has had three, which have been for a time discontinued, but will most likely be re-established. Holland, Belgium, Switzerland and Roumania have each one table. I entertain the hope that France and the Scandinavian kingdoms will subsequently secure tables. I am glad to say that the Zoölogical Station is quite capable of giving them all the full benefit of its complete arrangements."

This letter places before American zoölogists in the most direct and convincing manner the importance, not to say obligation, of remedying the past infringement upon the hospitality of the broad-minded director of the famous station. The Smithsonian table and the Harvard table should now be supplemented by a third, and it is to be hoped that some means will be found of adding $250 to the generous subscription of Mr. Dodge and securing this end.

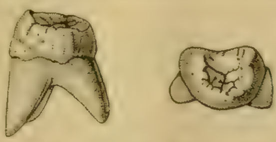
HENRY F. OSBORN.

CORRESPONDENCE.

PITHECANTHROPUS ERECTUS.

MR. EDITOR: In SCIENCE of January 11, p. 47, Dr. D. G. Brinton reviews under the title 'The Missing Link Found at Last,' Dr. E. Dubois' Memoir on *Pithecanthropus erectus*. Dr. Brinton, while accepting the dental apparatus to be of the simian type, acknowledges that the skull is like the famous Neanderthal man, and that the femora are singularly human. Professor O. C. Marsh (Silliman's Journal, February, 1895, p. 144) calls *Pithecanthropus* an 'ape-man.' In another place he alludes to it as a 'large anthropoid ape.' A communication signed 'R. L.,' presumably Richard Lydekker, appeared in 'Nature,' January 24, 1895; the ground is taken that the femur of *Pithecanthropus* is 'actually human;' that the skull 'can belong to no wild anthropoid;' and that the molar may 'perfectly well be human.'

It thus appears that differences of opinion are already being entertained, respecting the validity of *Pithecanthropus*. I have ventured to make a contribution to the subject,



since I quite agree with 'R. L.' The single tooth preserved (see the accompanying cut) is the third upper molar. It possesses two divergent roots. Contrary to what one expects, the smaller part of the crown forms the outside (buccal), and the larger the inside (palatal) surface. Du Bois thus describes the tooth on the assumption that the broader of the two roots represented two other confluent roots. If the broader half of the crown were outside (as it appears to be from the figure) the identification of the tubercles on the grinding surface would be easy. As it is, it is difficult, if not impossible, to name the cusps. The tooth must be classified as irregular and degenerate. I am in the habit of naming such teeth, crater-like, since all sides of the crown are uniformly higher than the centre, and the sides of the single valley are much fissured. We often meet with such teeth in man, but so far as I know they have not been seen in apes.

The tuberculation in the gorilla for the third molar is complete; the fourth cusp (hypocone), while rudimentary, is distinct. In the chimpanzee, according to Owen, the third molar is tritubercular, but in a specimen in the Academy of Natural Sciences of Philadelphia, it shows distinctly the rudiment of a hypocone. In the orang the third molar is distinctly quintitubercular, the fifth cusp being developed in the commissure between the mesocone and the hypocone.

The tooth of *Pithecanthropus* is larger than any human tooth with which I am familiar. The following table will place its measurements in harmony with ape and human teeth.

	Length.	Width.
Pithecanthropus,	11.3 mm.	15.3 mm.
Gorilla,	14.1 "	13.5 "
Orang,	12 "	13 "
Chimpanzee,	10 "	10 "
Native of Australia, (1)	10 "	13 "
" " " (2)	10 "	14 "
" " Sandwich Islands,	10 "	13.5 "

In Owen's Odontography the gibbon is seen to possess a molar of length 6 mm. and width 7.5 mm.; but even here the form of the tooth is quite unlike that of *Pithecanthropus*, being tritubercular with a rudimental hypocone. The tooth, unlike that of any anthropoid ape examined, is wider than long. The proportion of the width in comparison to the length is much the same as in the third molar of the human subject. The great size of the tooth and the possession of three roots, forming two diverging root-stems are distinguishing characters, but they are not simian. Some allowance must be made for the great variability in the shape of the third upper human molar.

Respecting the calvarium, I note in the view of the vertex a median elevation apparently over the interfrontal suture. This is often met with in the human skull, but so far as I know is never seen in the skull of the ape. The recession back of the external orbital process differs only in degree from that seen in man. The femur is indubitably human. HARRISON ALLEN.

PHILADELPHIA, Feb. 14, 1895.

THE ELIHU THOMSON PRIZE.

THE EDITOR OF SCIENCE: Your transatlantic contemporary, *Nature*, has from its beginning enjoyed a large support among scientific men of the United States. It is so well conducted, and combines in so unusual a degree freshness and reliability, that it it is almost indispensable, and Americans continue to renew their subscriptions annually, in spite of the very general feeling and not infrequently expressed opinion that, on the whole, it is not now and never has been quite fair or just in its treatment of American science and scientific men.

An illustration of this is to be found in a recent number (January 31, 1895) which is so striking as to deserve attention. On page 324 will be found a note in reference to the recently announced award of the Elihu Thomson Prize (see this journal, page 190). It is a most ingeniously constructed account of the award made by the Paris Committee, the preparation of which must have cost the writer no small effort. So skilfully, however, are the words selected and the phrases arranged that, to one unfamiliar with the facts, the note appears to be a simple and straightforward statement that in declaring the award the Committee announced that it had found two memoirs of equal value and that it was decided to award a prize of 5000 francs to each, the collection of the additional money being the cause of the delay in the publication of the decision of the Committee. In the account of the affair in a recent number of SCIENCE it was pretty clearly stated that the memoir prepared by an American, Dr. Webster, of Clark University, had been adjudged by the Committee to be worthy of first place.

In order that every reader may be able to decide this matter for himself, the following quotations from the report of the Committee are submitted: Memoir 3 was that to be the work of Messrs. Oliver Lodge and R. T. Glazebrook, and No. 4 was that of Dr. Webster.

"Le n° 3 est consacré à la vérification de la formule donant la périod des décharges oscillantes d'un condensateur. C'est un travail considérable, accompagné de plusieurs photographies et dans lequel l'auteur a cherché, au moyen de calculs approfondis, à évaluer toutes les corrections inhérentes à l'emploi de sa méthode.

"La vérification n'est qu'approachée ; le principe de la méthode pourrait donner lieu à quelques critiques, le circuit de la décharge se fermant périodiquement par une étincelle qui introduit des perturbations impossibles à prévoir.

"Le mémoir n° 4 port sur le même sujet, étudié par une méthode nouvelle dans ses détails, qui a permis à l'auteur d'atteindre et de mesurer des périodes de quelques cent-millièmes de seconde. L'influence des principales causes d'erreur paraît très atténuée, bien qu'il reste encore quelques doutes sur l'influence de la capacité inhérente à la bobine de self-induction. La formule a été vérifiée a 1 pour 100 prés. Le temps a fait défaut à l'auteur pour compléter ses recherches en variant les conditions de ses expériences."

And then the following award from the 'procès-verbal' of the Commission :

"La Commission estime que le mémoire n° 4 est digne de recevoir le prix établi par le Professeur Elihu Thomson ; elle espére que ce témoignage encouragera l'auteur à continuer ses belle recherches.

"'Toutefois elle regrette de ne pas avoir à sa disposition deux prix d'égale valeur qu'elle serait heureuse d'attribuer aux mémoires n° 3 et n° 4.'"

A literal translation of the above, as a fair statement of its meaning is, perhaps, too much to look for in the columns of 'Nature,' but it is a pleasure to assure Messrs. Lodge and Glazebrook, whose names are 'household words' in every corner of this country, that their reputation is not such as to need bolstering by any oblique methods. M.

SCIENTIFIC LITERATURE.

Monographic Revision of the Pocket Gophers, Family Geomyidæ (exclusive of the Species of Thomomys). By DR. C. HART MERRIAM. North American Fauna, No. 8. Washington, Government Printing Office. 1895. 8vo, pp. 258, pll. 18, with 4 maps and 71 cuts in text.

In this memoir Dr. Merriam has produced an admirable piece of monographic work, setting a standard that may well be aimed at by other workers in the treatment of similar groups. The family Geomyidæ, or the Pocket Gophers, has hitherto been regarded as consisting of the two genera *Geomys* and *Thomomys*, only the first of which is here treated. It is a distinctively North American group, ranging from the dry interior of British Columbia and the plains of the Saskatchewan to Costa Rica. The regions occupied respectively by the two groups, however, do not to any great extext overlap, *Thomomys* occupying in the United States the area west of the Great Plains, and the *Geomys* group the region between the Mississippi River and the eastern base of the Rocky Mountains, with outlying representatives in northern Florida and the contiguous portions of Alabama and Georgia. In Mexico *Thomomys* ranges over the peninsula of Lower California and a large portion of the interior of Mexico, which latter region it shares with numerous forms of the *Geomys* group, now broken up by Dr. Merriam into no less than nine genera. These collectively not only occupy a large part of central and southern Mexico, but extend as far southward as Costa Rica.

In respect to material Dr. Merriam has been especially fortunate, having availed himself of opportunities at his disposal as Chief of the Division of Ornithology and Mammalogy of the United States Department of Agriculture, to bring together material from a wide area and in an abundance scarcely dreamed of by any previous monographer of the group. Of the one thousand specimens thus rendered available for study, over two hundred are from Mexico and Central America, from which area the specimens previously handled by investigators could be counted on the fingers of the two hands. Hence not only has the known area inhabited by these animals been greatly extended, but the harvest of specimens has yielded novelties not previously suspected to exist.

Only about one-half of Dr. Merriam's excellent memoir is given to the systematic descriptions of the genera and species, the first hundred pages being devoted to the generalities of the subject—habits, function and structure of the cheek pouches, food, sexual and individual variation, geographical distribution, etc., about 15 pages—and to chapters on the morphology of the skull (30 pages) and the dental armature (36 pages). Nearly seventy of the text figures and six plates relate to the structure of the skull and teeth, this profusion of illustration greatly facilitating a clear comprehension of the points discussed in the text, and forming a most important feature of the work.

In coloration, size and in external details generally, the species of Geomyidæ are very much alike. There are, however, large forms and small forms, between which there is a wide difference in size, and also forms that are normally plumbeous instead of the usual shade of yellowish brown, but in general, even for the discrimination of species, resort must be made to structural details of the skull and teeth, which often afford characters of importance where external differences are nearly inappreciable. The range of variation in cranial and dental characters is so great, in these animals which look so much alike externally, that Dr. Merriam has felt justified in separating the old genus *Geomys* into nine groups which he thinks should rank as genera, 'several of which' he says, 'are of supergeneric value.' These genera are *Geomys*, *Pappogeomys*, *Orthogeomys*, *Cratogeomys*, *Platygeomys*, *Orthogeomys*, *Heterogeomys*, *Macrogeomys* and *Gygogeomys*. While these are apparently natural groups, doubtless taxonomers will differ as to whether all are entitled to full generic rank.

In 1857 Baird recognized seven species of *Geomys*, of which six retain place in Merriam's list. In 1877 Coues, in his monographic revision of the genus, admitted five. During the last two years others have been described, raising the number currently admitted in 1894 to sixteen. To this number Dr. Merriam here adds twenty-one, raising the total of species and sub-species to *thirty-seven!* Only the genera *Geomys* and *Cratogeomys* are represented in the United States; the former, with seven species and five sub-species, scarcely extends across our southern border; the latter, with seven species and one sub-species, is mainly Mexican, one speciês, however, ranging northward over southeastern New Mexico and northwestern Texas. *Macrogeomys* is known only from Costa Rica; *Heterogeomys* and *Orthogeomys* occupy separate areas in southern Mexico and Guatemala; *Pappogeomys*, *Platygeomys* and *Zygogeomys* occur in central and western Mexico, the latter being known only from a very restricted area in the State of Michoacan.

The chapters on the Morphology of the Skull and the Dental Armature bring into strong relief many points in relation to changes of structure, due to age and growth, which have heretofore been only lightly touched upon, and especially the influence of the masseter muscle upon the general shape of the skull in adult life. The facts here presented may well be studied with care and profit by students of not only the mammals of to-day, but of the extinct forms as well. The skull is considered not only as a whole, but its individual bones are treated in detail, with cuts showing the skull sectionized, and young skulls in comparison with old ones of the same species. The memoir thus illustrates some of the best work and the tendencies of the 'new school' in recent mammalogy. In fact, no similar group of mammals has before been treated in such exhaustive detail, or from a morphological standpoint, or with such admirable profusion of illustration.

J. A. ALLEN.

AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK.

The Planet Earth. RICHARD A. GREGORY, 16 mo, pp. 108. Macmillan & Co., New York. Price 60 cents.

This little book is called 'An Astronomical Introduction to Geography.' In the preface the reader is promptly informed that in class books on Astronomy and Geography the subject of the earth considered as a planet is treated inadequately and unscientifically. The author expresses his hope that his treatment, which, by inference, is both adequate and scientific, may be the means of reviving the 'Observational Astronomy of pre-telescopic times.' Just why the telescope should be tabooed, or why it is less 'scientific' than strings with beads strung on them, does not clearly appear. It is quite evident, however, that the author wishes to restore what is sometimes called the 'historical' method of presentation and instruction, according to which the student is expected to traverse the path along which mankind has slowly toiled in order to reach conclusions which in the present state of our knowledge are often quickly attained by perfectly logical processes. There is, also, generally involved in this method, the erroneous assumption that a student can, in the short time available for his training in science and scientific methods, re-discover for himself all the great facts and principles which are the fruit of ages of intellectual activity, if only he has a few simple appliances at hand and is started in the right direction. This is a very large error, and it is not desirable to pursue it farther at this point. Admitting, therefore, and no one will venture to deny this, that much can be learned by a proper study of the apparent motions of the heavenly bodies, and that young people should be led to make such study before finishing or even beginning their study of the earth, as it is presented in the so-called unscientific treatment in Astronomy and Geography, it is yet extremely doubtful if the book now under consideration will be of real value to them.

The first chapter, which forms a considerable part of the whole, is devoted to 'the constellations.' The continued fixedness of the North Star at one point in the sky is established by a quotation from Shakespeare, but there is an intimation later that the distinguished poet was possibly a little weak in his Astronomy. The author is very fond of bolstering up quite generally accepted scientific theories by poetic quotations, and even in the case of the Law of Gravitation, against which there can hardly be said to be any serious rebellion at the present time, he finds it desirable to repeat that bit of nonsense beginning,

"The very law that moulds a tear,"

for the existence of which not even poetic license furnishes excuse.

In the discussion of the size and mass of the earth, as elsewhere, great unevenness is shown. On one page is a diagram of a complicated piece of triangulation by the British Ordnance Survey, including the base-line on Salisbury Plain, and on that opposite is one explaining angular measure and terrestrial latitude by opening the legs of a pair of compasses. In the discussion of latitude there are many errors, and a beginner will be greatly helped by not reading it. There is a good deal about the Zodiac, with incidental references to 'mansions in the sky' and the emotions with which the first men witnessed the first Setting of the Sun, 'to whom he was dead,' together with a brief account of how their hopes were buoyed up and their fears calmed by the appearance of the 'Evening Star.' See wood cut on opposite page representing Venus shining upon a rural scene, including a village of at least twenty houses, a church with a tall spire tipped with a cross, and calming the fears of a farmer driving a yoke of oxen drawing a cart on which is probably a half ton of hay or grain or something of the sort. This is a marvellous development for a single day. At this point more poetry appears, and the rigorously scientific treatment is enhanced in value by numerous references to Lucifer, Apollo, etc., etc.

To illustrate the phases of Venus, which, by the way, hardly belong to pre-telescopic astronomy, the author shows a picture in which a lamp represents the sun, and a comely young woman with quite-up-to-date leg-of-mutton sleeves is represented as standing in four positions, in front of, behind, on the right and on the left of the luminary as viewed by the reader. Unfortunately it has been thought necessary to represent this young lady as looking squarely at the sun in all of the four positions, and thus what is intended to simplify the explanation of one phenomenon proves to be much more effective in establishing a very erroneous conclusion respecting another. And this is not the only happening of this kind in the barely one hundred pages of the book. To one who only 'skims' through it, it is reminiscent of the days of a quarter or half century ago, when 'Astronomy and the Use of the Globes' was a favorite subject in young ladies' seminaries. A more careful examination shows, however, that it is not so harmless as might at first appear, and although it unquestionably contains some good features it is quite safe to predict that the 'inadequate and unscientific' treatment of the subject found in good, modern text-books of Astronomy and Geography will continue, for the present, to receive the confidence of both instructors and students.

T. C. M.

Biological Lectures Delivered at the Marine Biological Laboratory of Wood's Holl. 8vo, 242 pp. Boston, Ginn & Co. 1894.

In no way, short of an actual sojourn at the Wood's Holl Laboratory, is it possible to secure a better idea of the scope and character of the opportunities afforded by this institution than by the perusal of this series of selected lectures. Wood's Holl is at once the 'finishing school' of the American biological student, and the rallying point for trained investigators. Its biological laboratory affords advantages which are each year more widely appreciated, and one has but to glance over the titles of the papers listed in the appendix to the volume under consideration, to be impressed with the scientific vigor which characterizes both its staff and pupils.

The ten lectures for 1894 bear the following titles: I. 'The Mosaic Theory of Development,' by E. B. Wilson. II. 'The Fertilization of the Ovum,' by E. G. Conklin. III. 'On Some Facts and Principles of Physiological Morphology,' by Jacques Loeb. IV. 'Dynamics of Evolution,' by J. A. Ryder. V. 'On the Nature of Cell-Organization,' by S. Watasé. VI. 'The Inadequacy of the Cell-Theory of Development,' by C. O. Whitman. VII. 'Bdellostoma Dombeyi Lac,' by Howard Ayres. VIII. 'The Influence of External Conditions on Plant Life,' by W. P. Wilson. IX. 'Irrito Contractility in Plants,' by J. Miurhead McFarlane. X. 'The Marine Biological Stations of Europe,' by Bashford Dean.

Of these papers more than one-half are concerned in a presentation of the results of modern research into the activities of the living cell, and it would be difficult to direct a student to any one volume from which he might gain a clearer idea, or find a more satisfactory discussion, of the present condition of theory and established fact concerning the cell state. Prof. E. B. Wilson strikes the key-note of the motive which runs through the book when he calls attention, on the first page, to the remarkable change of front which has taken place during recent years respecting the germ-layer theory—namely: (*a*) the growing recognition of the inadequacy of a theory of development which practically ignores the pregastrular stages of the ovum; and (*b*) the tendency to resume the attempts of Brücke and others to formulate a pre-organization theory which should account for the evident organization of the cell, by the postulation of primary elements, or bearers of cell qualities; the 'physiological units' of Herbert Spencer, the 'gemmules' of Darwin, the 'Micellæ' of Nägeli, the 'plastidules' of Elsberg and Haeckel, the 'inotagmata' of Th. Engelmann, the 'pangenes' of De Vries, the 'plasomes' of Wiesner, the 'idioblasts' of Hertwig, the 'biophores' of Weismann, and finally the 'idiosomes' of Whitman, in which may be found 'the secret of organization, growth and development.'

The tendency in modern biology is, in other words, to rob the cell of its leadership in the phenomena of organization, and to regard it as but a 'biotome,' life epoch, or form-phase; correlated with a series of visible cell-aggregates (organs and tissues) on the one hand, and to another series of invisible aggregates of diminishing complexity, which terminate finally in protoplasmic molecules variously designated, as indicated above. These living molecules are pointed out as the foundation of organization, and the protoplasmic molecule, the 'Specifische Bildungstoffe' of Sachs, as the 'essential archetectonic element;' furnishing a common basis for every grade of organization, but 'subject to a regenerative and formative power existing as one and the same thing throughout the organic world' (Whitman). The prevailing thought of the book seems to be expressed by Ryder in the conviction "that experimental investigation in embryology will make no solid progress until all such conceptions as gemmules, biophores and idiosomes are abandoned," and in the dictum of Loeb that "all life phenomena are determined by chemical processes." We are asked to concur in the admission that "the phenomena of life are ultimately physical in their nature and are to be treated in detail as physical problems."

We may derive from these essays a notion of the drift of biological thought in the immediate future which will undoubtedly throw much light on the behavior of protoplasm through the investigation of its molecular relations, its surface tensions, vortex movements, chemotropism, chemotaxis, polarity, etc.; but many will doubt whether this treatment of life phenomena 'as purely physical and chemical problems' will do away with the conception of some anagenetic or organic growth force, some bathmic energy, such as is assumed by Cope in his consideration of the 'Origin of Structural Variations.'

That physical and chemical influences tend to *locate* growth force is becoming more and more evident, from such studies as we have presented to us in these lectures, and in recent researches like those of Bütschli on 'Protoplasm and Microscopic Forms,' Loeb on 'Physiological Morphology,' and Vaughan, Halliburton and others on the 'Nucleins.' There is no reason to doubt that surface tensions may lie behind all protoplasmic movements; that polarity, gravity, geotropism, heliotropism or thermotropism may determine the direction of growth, and that osmosis, metabolism, or the presence of nuclein may explain the ability of cells to utilize the pabulum within their reach, but the explanation seems, somehow, to be inadequate.

Notwithstanding the brilliant achievements of experimental science, the oracular dicta of the modern priests of monism or materialistic empiricism carry little conviction. One turns away with a sense of dissatisfaction and a lingering doubt whether mechanism and organism are after all identical. Haeckelismus has by no means proven itself infallible, and the reading of these lectures will be much more interesting to many, from the fact that here and there are to be found wide differences of opinion on fundamental questions; while along with the assurance that certain present statements must be regarded as axiomatic; long established theories are shown to be inadequate; long discarded theories are resuscitated and presented, rehabilitated and disguised. The moneron no longer stands in its integrity as the material basis and starting point of life. The student of the cell finds himself confronted with a microcosm, not with an ultimate unit of life, and is puzzled to know whether he may account for this complex organism by differentiation from some homogeneous *Anlage* or rudiment, or whether nucleus and cytoplasm represent dissimilar organisms, which 'by mutual adaptation have given rise to a third organism, in which each of them serves as organ to the whole.'

As the facts of particulate inheritance have led to a rehabilitation of the old theory of incasement, preformation or pangenesis, it seems not improbable that having traced 'the secret of organization, growth and development' beyond the cell to certain 'ultimate elements of living matter,' 'idiosomes,' or protoplasmic molecules, and bearing in mind that these living molecules must have a complex atomic organization, inasmuch as 'function presupposes structure,' we find ourselves forced to ask what determines the upbuilding of atomic aggregates combining the physical and chemical complexity essential to the phenomena of growth and evolution. In reply we are presented with a prepotent 'plastic power' (Schwann); a 'regenerative and formative power, one and the same thing throughout the organic world' (Whitman); this is probably the 'formative impulse' of Schleiden. Cope (*loc. cit.*) refers us to 'a special form of energy known as growth energy or Bathmismel.' In what way does this 'plastic power,' 'formative impulse' and 'growth energy' differ from the 'vital force' of Planck, Schelling, Schopenhauer and other philosophers? The physiological morphologist has carried us back to living protoplasmic molecules varying greatly, and which he finds himself able to direct somewhat in their future combinations, as the chemist handles radicles and proximate principles; but President Schurman has long since pointed out that there is a 'fundamental contrast between the initial variations and the subsequent means of their preservation'; for example, between modifying organisms and originating idiosomes and 'that where science stops, philosophy begins.'

It is to this lothfulness to directly admit that Czolbe was right in saying: "The power of organisms cannot be explained by the planless and formless physical and chemical activities;" that Schurman refers in saying: "This jugglery with causality, as though in time everything could be got out of almost nothing, is the besetting sin of Darwinists." CHARLES S. DOLLEY.

PHILADELPHIA.

Aero-therapeutics or the Treatment of Lung Diseases by Climate. By CHARLES THEODORE WILLIAMS. London and New York, Macmillan & Co. 1894. 8°, pp. 187.

This is a good book by a competent authority, being the Lumleian lectures for 1893, by Dr. Williams, who is the senior physician to the hospital for consumptives at Brompton, and the late President of the Royal Meteorological Society. It includes a discussion of those factors and elements of climate which bear directly upon human health, and is especially full upon the subject of atmospheric pressure and its variations, and on the effects of high altitudes upon cases of consumption.

The effects of such altitudes as are usually resorted to for curative purposes depend in part upon the rarefaction and increased diathermancy of the atmosphere, and in part upon the change in habits, exercise and food which is made when becoming a resident of such a resort. One of the most definite effects produced by diminished atmospheric pressure upon the healthy animal organism is an increase in the number of the red corpuscles of the blood, which has been shown by Viault and Eggar to occur in man to the amount of 16 per cent. in the course of three or four weeks. Mountain races usually have large chests, comparatively great activity of the respiratory organs, and great power of endurance for walking. They are usually remarkably free from scrofula and consumption, which is probably due to absence of overcrowding and to their comparatively great amount of out-door life, which greatly lessen the chances of their becoming infected with the tubercle bacillus. The sending of consumptives to high altitudes is a method of treatment which has come into vogue within the last thirty years, Davos and St. Moritz being the first of this class of health resorts to attract special attention. Dr. Williams concludes that this mode of treatment is most effective in recent cases of consumption, that at least six months', and in many cases two years', stay is desirable, and that it produces great improvement in about 75 per cent. of the cases, and a cure in about 40 per cent. One chapter of the book is devoted to the high altitudes of Colorado and their climates, and is based on the author's personal observations. The greater part of the surface of this State is over 5000 feet above the sea level, and some of the most beautiful parks are above 7000 feet in altitude, the atmosphere is dry and clear, and there is sunshine the year round, all of which are important factors in the treatment of consumption. Physicians will find Dr. Williams' comments upon the importance of these great mountain plateaus and parks, as a location for consumptive patients in the first stages of their disease, to be interesting and valuable.

PHYSICS.

On the Voluntary Formation of Hollow Bubbles, Foam and Myelin Forms by the Alkaline

Oleates, together with Related Phenomena, Especially those of Protoplasm. G. Quincke. Wiedemann, Ann. 1894. Vol. 53, p. 593.

This article is a continuation of Prof. Quincke's investigation published in 1888 (Weid., Ann., Vol. 35, 1888, p. 562, et seg), and a reply to the criticisms which his article provoked. It gives the results of elaborate investigations upon the phenomena observable upon mixing various soaps, oils and water, and traces them to surface tension and allied forces. Some very interesting suggestions are given upon the similarity of some of the resulting appearances, with the arrangement of the heavenly bodies in space, and a strong likeness is shown between some of these peculiar bubbles with very thin, solid walls formed in such mixtures, and some of the formations in plant cells. The observations also go far toward explaining the motions sometimes observed in cells, which would seem to be due to the same forces as produce those peculiar motions of a drop of oil upon water.

On the Comparison of High Range Mercury Thermometers of Jena Glass 59III, with the Air Thermometer at temperatures between 300° and 500° C. By Alfons Mahlke. (Wied. Ann. 1894. Vol. 53, p. 965.)

Contains a very careful determination of the apparent co-efficient of expansion of mercury in Jena glass 59III, and demonstrates the availability of mercury thermometers made of this glass for the measurement of temperatures up to 500° C. (900° Th). William Hallock.

On the Units of Light and Radiation. By A. Macfarlane, D. Sc., LL.D. A paper read before the American Institute of Electrical Engineers, 16th January, 1895. (Abstract.)

The author shows that the difficulty experienced in defining and denoting the different ideas commonly expressed by the word 'candle' is due to the want of a name for the unit of solid angle; and suggests the word *steradian*, which has already been used for that purpose.

He considers the different physical ideas in the general subject of radiation, and shows the appropriate expression for the unit of each. With this system of radiation units he compares the system of units of light recently proposed by M. Blondel, and shows that the light system ought to be parallel to, not identical with, the radiant energy system. Finally he discusses M. Hospitalier's proposed symbols for light quantities.

GEOLOGY.

Report on the Bevier Sheet, by C. H. Gordon and others. Arthur Winslow, State Geologist, Mo. Geol. Surv. 1894.

This is the second of a series of detailed reports on areal geology in Missouri. The main feature is a carefully prepared and well executed topographic and geologic map, which includes portions of Macon, Randolph and Chariton counties, an area of about 250 square miles. This map is on a scale of $\frac{1}{62500}$ and the topography is shown by contours of 20 feet interval. The topographic base was executed by Messrs. C. H. Gordon, C. F. Marbut and M. C. Shelton. On the map are shown the horizon lines of the coal beds and the distribution of the geological formations, as well as the location of coal pits, drifts and drill holes. It is accompanied by a sheet of columnar and cross-sections, which give details of the geology. In the accompanying text, Mr. Gordon describes the physiography, including the topography, drainage, soil, forestry, etc., and the stratigraphic and economic geology. The Quaternary geology is reported on by Prof. J. E. Todd, and the distribution of the clays and shales by Mr. H. A. Wheeler, E. M., who were employed as specialists and whose reports on these subjects for the whole State are in process of preparation. J. D. R.

NOTES AND NEWS.

A. A. A. S. TABLE AT WOODS HOLL LABORATORY.

IN joint session of Sections F and G, the following resolutions of the Committee of the A. A. A. S., on a table at the Marine Biological Laboratory at Woods Holl, Mass., were offered by Dr. S. H. Gage for adoption by the Sections:

The Sections of Zoölogy and Botany (F and G) request that the Association continue its subscription of $100 for an investigator's table at the Marine Biological Laboratory at Woods Holl, Mass.

The two Sections in joint Session also make the following suggestions for the award and government of the table subscribed for by the Association:

1. That the table shall be known as the A. A. A. S. table.

2. That the award of this table shall be entrusted to a committee of five, consisting of the vice-president and secretary-elect of each Section (F and G), and of the director of the Marine Biological Laboratory (at present C. O. Whitman).

3. Any fellow or member of the A. A. A. S. shall be eligible for appointment to the table. (An applicant for membership in the Association will be considered as a member, and therefore eligible.)

4. Applications for the table are to be made to the permanent secretary, who shall forward them to the senior vice-president of Sections F and G, seniority being determined as in § 11 of the Constitution, *i. e.*, according to continuous membership.

5. That the holders of the Association's table are expected to give proper credit for the use of the table in all published results of investigations carried on at the table.

[The grant for the table was made by Council.]

GENERAL.

PROFESSOR T. H. MORGAN and Professor Herbert Osborn have been awarded the Smithsonian Table at the Naples Zoölogical Station for periods lasting until October 8, 1895. After that date the table will be vacant and applications for it may be addressed to Professor Langley, Secretary of the Smithsonian Institution.

LORD ACTON succeeds the late Professor Seeley in the professorship of modern history at the University of Cambridge.

PROFESSOR W. W. CLENDENIN, of the State University of Louisiana, has been appointed geologist in charge of a survey of the State.

DR. LOMBARD, known for his writings on climatology, died at Geneva on January 22, in his ninety-second year.

ACCORDING to *The American Naturalist*, Mr. R. T. Hill, of the U. S. Geological Survey, is in Panama, and Dr. H. C. Mercer, of the University of Pennsylvania, is in Yucatan.

THE New York Assembly has passed a bill appropriating $1,175,000 for the purchase of a new site, and the erection of buildings for the College of the City of New York.

THE American Museum of Natural History has applied to the Legislature for $500,000, for an addition wing, which would complete the southern front of the building.

THE Arizona Legislative Assembly has presented a memorial to Congress, requesting that the district in Apache county covered with trunks of petrified trees be withdrawn from entry with a view to preventing destruction and injury until the district has been made a public park.

THERE have been so many requests for copies of Prof. Charles S. Minot's article in the *Popular Science Monthly* for July, 1893, entitled 'The Structural Plan of the Human Brain,' that the article has been reprinted and copies may now be obtained at twenty cents each, from Mr. Charles B. Wormelle, 6 Menlo Street, Brighton District, Boston, Mass.

SOCIETIES AND ACADEMIES.

MICHIGAN SCIENCES AND ACADEMIES.

After some discussion and correspondence, a preliminary meeting was called at the State University in Ann Arbor, last June, and an organization effected. The following officers were elected to serve for the first meeting which was held in connection with that of the State Teachers' Association, December 26–27, in the State Capitol at Lansing :

President—W. J. Beal.

Vice-President—J. B. Steere.

Secretary and Treasurer—F. C. Newcombe.

Additional Members of the Executive Committee—W. B. Barrows, I. C. Russell.

At the close of the meeting very nearly an even hundred members were enrolled.

A very complete constitution and by-laws were adopted. One of the main features of the Society is to proceed systematically with a State biological survey. The State will be asked to publish the transactions, and to furnish some aid toward conducting field work.

Three vice-presidents were elected who are to act as chairmen of committees on Botany, Zoölogy and Sanitary Science. Doubtless other vice-presidents for other work may be elected at the next annual meeting.

An informal field meeting will be held in May or June.

Those in attendance were much pleased with the first program as carried out in Lansing, and are showing much enthusiam regarding future work. The objects of the Society, as now stated in the constitution, are the investigations in Agriculture, Botany, Zoölogy, Sanitary Science, Archæology and kindred subjects, but may include other departments when workers are ready to enter the field.

The present officers are:

President—Bryant Walker.

Vice-President—Frederick C. Newcombe.

Vice-President—Jacob E. Reighard.

Vice-President—Henry B. Baker.

Secretary—G. C. Davis.

Treasurer—E. A. Strong.

The program was as follows:

WEDNESDAY, 1:30 P. M.

1. *Call to order and introductory remarks by the President.*
2. *Report of the Executive Committee.*
3. *Determination of the hour for Election of Officers, and for Other Business.*

PRESENTATION OF PAPERS.

1. *The Mammals of Michigan:* Dr. J. B. Steere.
2. *The Birds of Michigan:* Prof. D. C. Worcester.
3. *Additions to the Flora of Michigan:* Mr. C. F. Wheeler.
4. *The Cryptogamic Flora of Michigan:* Mr. L. N. Johnson.
5. *Work of the Michigan Fish Commission:* Dr. C. A. Kofoid and Prof. H. B. Ward.
6. *The Michigan Lepidoptera:* Dr. R. H. Wolcott.

WEDNESDAY. 7:30 P. M.

7. *Our Society and a State Survey:* Prof. W. J. Beal.
8. *Practical Benefits of Bacteriology:* Prof. F. G. Novy.
9. *Simian Characters of the Human Skeleton:* Prof. W. H. Sherzer.
10. *Date and Development of Michigan Archæology:* Mr. Harlan I. Smith.
11. *Some Notes on the Michigan Coat of Arms:* Prof. W. J. Beal.
12. *Teaching Botany in Winter:* Prof. W. J. Beal.

THURSDAY, 9:00 A. M.

13. *Flora of Michigan Lakes:* Prof. Chas. A. Davis.
14. *Michigan Lepidoptera:* Dr. R. H. Wolcott.
15. *Review of our Present Knowledge of the*

Molluscan Fauna of Michigan: MR. BRYANT WALKER.

16. *Distoma Patalosum; A Parasite of the Crayfish:* MR. C. H. LANDER.
17. *Bacteria and the Dairy:* PROF. C. D. SMITH.
18. *Tendencies in Michigan Horticulture:* MR. A. A. CROZIER.
19. *Futile Experiments for the Improvement of Agriculture:* DR. MANLY MILES.

THE ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA.

PROFESSOR DANIEL G. BRINTON is giving a course of six lectures, entitled *A Survey of the Science of Man*, on Mondays, January 28, February 4, 11, 18, 25, and March 4, 1895, in the Lecture Hall of the Academy. The lectures are:

1. *The Physical Faculties of Man.*
2. *The Mental Faculties of Man.*
3. *The Social Faculties of Man*
4. *The Artistic Faculties of Man*
5. *The Religious Faculties of Man.*
6. *The Progress of the Race.*

GEOLOGICAL SOCIETY OF WASHINGTON.

FEB. 13.

Discussion of Field Methods: (1) How do you determine the Thickness of Strata? Symposium opened by MR. G. K. GILBERT. General discussion is invited.

Rapid Section Work in Horizontal Rocks: MR. M. R. CAMPBELL.

Newly Discovered Dyke near Syracuse, N. Y.: MESSRS. N. H. DARTON AND J. F. KEMP.

WHITMAN CROSS, *Secretary.*

PHILOSOPHICAL SOCIETY OF WASHINGTON.

FEB. 16.

Biographical Sketch of James Clarke Welling: MR. J. HOWARD GORE.

Biographical Sketch of Robert Stanton Avery: MR. L. D. SHIDY.

Biographical Sketch of Garrick Mallery: MR. ROBERT FLETCHER.

The Central American Rainfall: MR. MARK W. HARRINGTON.

WILLIAM C. WINLOCK, *Secretary.*

FORTNIGHTLY SCIENTIFIC CLUB IN THE UNIVERSITY OF MINNESOTA.

Jan. 19, 1895.

The Vivisection of Plants: MR. D. T. MACDOUGAL.

Is Man Woman's Equal? The Zoölogist's answer and some of its consequences: PROFESSOR H. F. NACHTRIEB.

Feb. 2, 1895.

The Departure of the Ice Sheet from Lake Superior and the more Eastern Laurentian Lakes: MR. WARREN UPHAM.

Some Things People Ought to Know About Micro-Organisms: DR. CHAS. N. HEWETT.

Feb. 16, 1895.

The Detection of Star Motions in the Line of Sight: PROFESSOR J. F. DOWNEY.

The Constitution of Matter: DR. G. B. FRANKFORTER.

SCIENTIFIC JOURNALS.

THE AMERICAN NATURALIST, FEB.

The Philosophy of Flower Seasons, and the Phænological Relations of the Entomophilous Flora and the Anthophilous Insect Fauna: (Illustrated.) CHARLES ROBERTSON.

Insanity in Royal Families; A Study in Heredity: ALICE BODINGTON.

The Significance of Anomalies: THOMAS DWIGHT, M. D., LL. D.

Editor's Table; Recent Literature; Recent Books and Pamphlets.

General Notes; Geography and Travels; Mineralogy; Petrography; Geology; Botany; Zoölogy; Entomology; Embryology; Archæology and Ethnology; Microscopy: On a New Method of Entrapping, Killing, Embedding and Orienting Infusoria and other very small Objects for the Microtome. (Illustrated.)

Proceedings of Scientific Societies; Scientific News.

THE JOURNAL OF THE AMERICAN CHEMICAL SOCIETY, FEB.

A Modified Arrangement of the Elements Under the Natural Law: F. P. Venable.

The Determination of Potash in Kainite: Rudolph de Roode.

The Oxidation of Organic Matter and the Decomposition of Ammonium Salts by Aqua Regia, in Lieu of Ignition, in the Determination of Potash in Fertilizers: Rudolph de Roode.

On Certain Phenomena Observed in the Precipitation of Antimony from Solutions of Potassium Antimonyl Tartrate: J. H. Long.

An Examination of the Atmosphere of a Large Manufacturing City: Charles F. Mabery.

A New Form of Water-Oven and Still: Lewis William Hoffmann and Robert W. Hochstetter.

The Determination of Nickel in Nickel-Steel: E. D. Campbell and W. H. Andrews.

The Volumetric Determination of Phosphorus in Steel and Cast Iron: W. A. Noyes and J. S. Royse.

The Contribution of Chemistry to the Methods of Preventing and Extinguishing Conflagration: Thomas H. Norton.

The Action of Organic and Mineral Acids Upon Soils: Harry Snyder.

New Books.

THE JOURNAL OF GEOLOGY, JAN.–FEB.

The Basic Massive Rocks of the Lake Superior Region. IV.: W. S. Bayley.

A Petrographical Sketch of Ægina and Methana. Part II.: Henry S. Washington.

Lake Basins Created by Wind Erosion: G. K. Gilbert.

On Clinton Conglomerates and Wave Marks in Ohio and Kentucky: Aug. F. Foerste.

Glacial Studies in Greenland. III.: T. C. Chamberlin.

Studies for Students:

Agencies which Transport Materials on the Earth's Surface: Rollin D. Salisbury.

Editorials; Publications; Notes.

THE AMERICAN GEOLOGIST, FEB.

George Huntington Williams: John M. Clark. (Portrait.)

The Geological History of Missouri: Arthur Winslow.

A New Cretaceous Genus of Clypeastridæ: F. W. Craigin.

Further Observations on the Ventral Structure of Triarthrus: C. E. Beecher.

The Second Lake Algonquin: F. B. Taylor.

Editorial Comment.

Review of Recent Geological Literature.

Recent Publications.

Correspondence.

Personal and Scientific News.

THE BOTANICAL GAZETTE, FEB.

New or noteworthy Compositæ from Guatemala: John M. Coulter.

A preliminary paper on Costaria, with description of a new species: De Alton Saunders.

Notes on our Hepaticæ. III: Lucien M. Underwood.

The flora of Mt. Mansfield: W. W. Eggleston.

Briefer Articles.

Editorial; Current Literature; Open Letters; Notes and News.

NEW BOOKS.

Missouri Botanical Garden. Fifth Annual Report. St. Louis, Mo., Board of Trustees. 1894. Pp. 166.

The Great Ice Age. James Geikie. New York, D. Appleton & Co. 1895. 3d Edition. Pp. xxviii+850. $7.50.

The Pygmies. A. de Quatrefages. Translated by Frederick Starr. New York, D. Appleton & Co. 1895. Pp. xiv+255. $1.75.

Annals of the Astronomical Observatory of Harvard College. Vol. XXXII., Part I. Investigations in Astronomical Photography. William H. Pickering. Cambridge, Mass., the Observatory. 1895. Pp. 115.

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Subscriptions and advertisements should be sent to SCIENCE, 41 N. Queen St., Lancaster, Pa., or 41 East 49th St., New York.

CURRENT NOTES ON ANTHROPOLOGY (IV.)

THE SIGNIFICANCE OF VARIATIONS IN THE HUMAN SKELETON.

IT is a little odd that two papers on the same subject, with almost the same title, prepared independently at the same time, should agree in defending a new view of the significance of variations and anomalies in organic forms.

The one of these is that which I read before the American Association for the Advancement of Science in August last, published in the American Anthropologist for October, entitled *Variations of the Human Skeleton and their Causes;* the other was the Shattuck Lecture, delivered before the Massachusetts Medical Society by Dr. Thomas Dwight, Professor of Anatomy at Harvard University, with the title, *the Range and Significance of Variation in the Human Skeleton.*

The two papers, although drawing their material from wholly independent sources, and reasoning along different lines, reach quite the same conclusion, to wit: That variations, which in the human skeleton resemble forms in lower animals, are not to be interpreted as 'reversions' or 'atavistic retrogressions,' but that other laws should be invoked to account for them, such as nutrition, mechanical action, etc.

Dr. Dwight adds the following significant words: "The opinion is growing daily stronger among serious scholars that if man's body came from a lower form it was not by a long process of minute modifications, but by some sudden, or comparatively sudden transition."

This is the opinion which, under the name *heterogenesis,* I have defended for many years (see my *Races and Peoples,* pp. 80, 81). It has lately received strong support from some of Bateson's admirable studies in variation.

THE ANTIQUITY OF MESOPOTAMIAN CULTURE.

At a recent meeting of the Oriental Club of Philadelphia, Dr. J. P. Peters, whose researches among the ruins of the valley of the Euphrates are well known, mentioned his observations on the deposition of alluvium by the river as a chronometer for measuring the antiquity of some ruin-mounds. The deposits from the known date of Alexander's conquests display marked uniformity; and taking the depths of these as a standard, the foundations of Ur (the 'Ur of the Chaldees' of Genesis, the modern Muchair) and of Erichu (the modern Abu-Shahrein) must have been laid about seven thousand years B. C.

This venerable antiquity, however, appears quite modern compared to that assigned the same culture in some calculations laid before the Académie des Inscriptions by M. Oppert last summer. They had reference to the established beginnings of the Sothiac cycle and the Chaldean Saros, or recurrent cycles of eclipses. His argument was that the former dated from an observation of the cosmical rising of Sirius visible to the naked eye. This could occur only at an eclipse of the sun at its rising; and this he figured was upon a Thursday, August 29, in the year 11,542 before Christ! And as it was visible only south of latitude 26°, the locality of the observation he fixes for various reasons at the island of Tylos, the modern Bahrein, in the Persian Gulf. Truly, this is a *tour de maître* in archæology which makes one dizzy!

DIVISIONS OF THE STONE AGE.

A useful broadside, about twenty inches square, presenting succinctly the subdivisions of the Stone Age, was published last year by M. Philippe Salmon in the *Bulletin de la Société Dauphinoise d' Archæologie et d' Ethnologie.* The three periods it presents are the palæolithic, the mesolithic and the neolithic. These are subdivided into epochs, six in all, each characterized by the products of definite stations, peculiar industries, climate and fauna. As a synopsis of the accepted data, from the best French authorities, the scheme merits high praise.

The position of the mesolithic division takes the place of the 'hiatus,' which figures in the works of Mortillet and others as an unexplained time of transition between the rough and polished stone ages. Salmon, however, claims that no such gap exists. He quotes, for instance, the station of Campigny, near the lower Seine, and Spiennes, in Belgium, as proofs that the peoples and the culture of the earlier and ruder epochs progressed steadily, without important breaks, up to the full bloom of the neolithic generations. The importance of such a generalization, if it could be established, would be great; for, working back from historic to pre-historic times, there is no doubt but that the neolithic nations of central and western Europe were of Aryan speech, and Salmon's argument would carry this mighty stock in lineal line to the preglacial fishermen in the valley of the Somme.

THE TEACHING OF ANTHROPOLOGY.

In a little pamphlet which I published in 1892, entitled 'Anthropology as a Science and as a Branch of University Education,' a plan was suggested by which this science could be introduced into our universities as one of the optional branches for the doctorate of philosophy, and its importance as a department of the higher education was emphasized.

The subject has been taken up lately in Germany with gratifying interest. In the 'Globus' for October, 1894, Professor Friedrich Müller, of Vienna, warmly advocates that a chair representing anthropology should be recognized as a proper addition to the faculty of a great university; and a few weeks later, in the same journal, the question was discussed by Dr. Rudolf Martin, of the University of Zurich. The latter agrees that anthropology properly takes its place in the faculty of philosophy; but his division of the science is open to doubt. He would class all its branches under two groups: those relating to, 1. physical anthropology; and, 2. psychical anthropology, or 'ethnology.' Under the latter, he includes pre-historic archæology; and not seeing very clearly where in such a scheme ethnography would come in, he takes the short cut of leaving it out altogether! This is a serious omission, as in many respects descriptive racial and tribal anthropology alone offers the indispensable raw material on which to build up a true science of man. His opinon, that at least two instructors, one for the physical and one for the psychical side, are desirable, will, of course, commend itself; but each should at the same time be well versed in the side which he does *not* teach.

GUATEMALAN ANTIQUITIES.

UNDER the sensational title 'An American Herculaneum,' a writer, M. X. West, in '*La Nature*,' November 3, describes the site of an ancient city, three kilometers from Santiago Amatitlan, Guatemala. His story is that at a depth of five or six meters, under a mass of volcanic cinders and tufa thrown out by some sudden eruption, there have recently been discovered the remains of a village with all the appurtenances of its daily life, finely decorated pottery, stone implements and images, the foundations of its buildings, and blocks bearing inscriptions in unknown characters. More astonishing is the statement that along with these were cups of graceful shape of glass, sometimes colored. This casts serious doubt on the whole narrative, unless 'voclanic glass,' *i. e.*, obsidian, is intended, as nowhere on the American continent had glass-making been discovered by the natives; and, indeed, it is very doubtful if at any point they had reached the art of glazing pottery.

At the Madrid Exposition, in 1892, the Lake of Amatitlan figured as the locality where an extraordinary seal, Egyptian in appearance, and some other probable frauds were found. No doubt it was the center of a high native culture, that of the Zutuhils, a Mayan tribe; and there seems to be also some modern adepts at present in the vicinity, whose skill should admonish the collector to be wary in investing in articles of that *provenance*.

AN EXCELLENT INTRODUCTION TO ANTHROPO-GEOGRAPHY.

THE various relations which his geograpical surroundings bear to man in his personal, social and national life constitute the almost new science of 'anthropo-geography,' to which Professor Ratzel, of Leipzig, has lately contributed a standard work. In this country it has received little attention from educators since the time of Professor Guyot, whose 'Earth and Man' was creditable for its period. The more modern opinions and results have been admirably summed up in a little volume written by Professor Spencer Trotter, of Swarthmore College, under the title 'Lessons in the New Geography' (Boston, D. C. Heath and Co., 1895). In the compass of 182 pages the author presents, in succinct language, suitable to the student and the general reader, the relations which have existed between the distribution of land and water, the climates of the various zones and the plants and animals which they produce, to the life and development of the human species. He then proceeds to define the recognized types or races of men, and to point out their distribution when they first became known. The book closes with observations on commerce and the progress of discovery, and various tables of statistical information.

Whether as a text-book in schools and colleges, or as a trustworthy and lucid exposition of the subject for general reading, this volume merits cordial commendation, and should awaken a wider interest in the attractive topics which it discusses.

THE OROTCHI TARTARS.

An entertaining description of this tribe is given from Russian sources in the 'Journal of the China Branch of the Royal Asiatic Society,' Vol. XXVI. (Shanghai, 1894). It is a member of the Tungusic stock, and is situate along the eastern coast of the continent, from 42° to 52°; but the pure types are found only toward the northern limit. They are small in stature, a man five feet four inches in height being considered tall. The women average six inches shorter than the men. Their bodies are thick set and muscular, and their power of endurance remarkable. Like all the other pure blood tribes in Eastern Sibera, they are steadily diminishing, either through intermixture of blood or through new diseases introduced by foreigners.

Their boats are rude, but they manage them skillfully, which is the more necessary, as none of them knows how to swim, and when a craft capsizes its occupants infallibly drown. This ignorance is owing to two causes: the coldness of the water at most seasons, and their invincible repugnance to cleanliness. They are adepts in making garments of the bowels and skins of fishes, from which they are sometimes called 'the fish-skin Tartars.' They are also handy with tools.

Their religion is ostensibly that of the orthodox Greek Church; but really their ancestral Shamanism is as strong as ever. The residences of the Shamans are denoted by sticks or poles planted in front of them, carved to resemble animals, like the Totem poles of the north-west coast. Their chief divinities make a triad, being Boa Anduri, 'spirit of the sky;' Temu Anduri, 'spirit of the sea,' and Kamtchanga Anduri, 'spirit of the mountains.' They indulge in violent religious frenzies, in which they speak in unknown tongues. One woman was unable to talk in her own for two months after such a spell.

THE FUTURE OF THE COLORED RACE IN THE UNITED STATES.

This momentous question has been made the subject of careful investigation by a physician of Savannah, Dr. Eugene R. Corson. His essay is published in the 'Wilder Quarter-Century Book,' a well deserved memento issued by the pupils of Dr. Burt C. Wilder, of Cornell University, at the expiration of his first quarter century of teaching.

Dr. Corson regards the relative mortality of the two races, white and colored, in the United States as 'the pith of the whole matter;' and, therefore, addresses his special attention to this. From his own observations and the census statistics, he concludes that the pure blacks have in our country a decidedly higher mortality than the whites; more die in childbirth, they are more susceptible to disease, they succumb more quickly, they are prone to bacillar diseases in a higher degree, and their alleged exemption from malaria is not generally true. The hybrids between the two races he pronounces less fertile and less viable than either. "Miscegenation is a reducing agent, chemically speaking."

From these considerations, which he advances, backed by large testimony, he reaches the comforting conclusion that there will be no 'war of races' among us; that the blacks will gradually fade out or become absorbed in the white population; and this in such a manner as not to deteriorate it.

THE PRE-HISTORIC TRIBES OF THE EASTERN UNITED STATES.

In the *Archiv für Anthropolgie*, for November, 1894, Dr. Emil Schmidt undertakes to gather together the fragmentary facts which cast light on the population of the Mississippi Valley and Atlantic slope of the United States at a date anterior to that of the tribes found there resident by the first explorers. He presents the question temperately and free from the fantastic notions which one generally anticipates in this investigation. His results may be briefly stated.

Beginning with the 'mound builders,' he points out numerous reasons for considering them the immediate ancestors of the present Indians; going further into their identification, he decides that the ancestors of the Cherokees were the mound builders of the Ohio Valley. The original seat of the Huron-Iroquois family he locates north of the Great Lakes, and that of the Algonquian family somewhere to the south of Hudson's Bay, where the Crees are still found speaking a pure and ancient dialect. These two mighty stocks moved slowly southward, driving the mound builders from the Ohio, and penetrating into Virginia. There they met the Dakotas, whom they destroyed, except the small tribes of the Tuteloes and Catawbas. The Gulf States were peopled by the Muskoghean tribes from the south-west. The debated question whether there was a 'rough stone' or palæolithic age in the United States, he answers, from the evidence before him, in the negative.

GALTON'S METHOD OF ISOGENS.

Mr. Galton is fertile in the application of new methods to anthropologic data. In a recent article in the *Journal of Statistics* he applies the method in use among meteorologists to define lines of equal barometric pressure, to data of natality. His so-called 'isogens' are analogous to the *isobars* of the weather maps. They are lines of equal birth-rate forming a constant derived from the two variables, the age of the father and that of the mother.

By this ingenious and simple process he reaches some curious results. One is the unexpected law of natality, "That the sums of the ages of the parents are constant; in other words, that the birth-rate is determined by the joint ages of the father and mother. The difference between the ages of the two parents is of no account whatever in nine-tenths of the total number of marriages." Only in the obvious case where the wife is older than the husband and is approaching the limit of the child-bearing age, is this law at fault. Another odd fact developed by this method is that a woman approaching somewhat closely the limit of the child-bearing age, say about thirty-five or thirty-eight, is more fertile with a man of her own age than with one who is younger; though it is admitted certain social reasons may help to this result.

Like all of Mr. Galton's articles, this one will be found admirably presented and well worth study.

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CURRENT NOTES ON PHYSIOGRAPHY (II.).

SIXTH INTERNATIONAL GEOGRAPHICAL CONGRESS.

The Sixth International Geographical Congress is to be held in London from July 26th to August 3d, 1895, under the auspices of the Royal Geographical Society. An invitation circular has lately been issued, stating the general plan of the Congress, the conditions under which tickets of membership can be obtained, the program of subjects for discussion, and a most comprehensive list of honorary officers, honorary general committeemen, and committees in charge of various divisions of the subject proposed for discussion. An extended exhibit of geographical materials will be held in connection with the Congress, which altogether promises to be a most attractive reunion. The invitation circular can be had from the Secretary, Royal Geographical Society, 1 Saville Row, London, W. A representative American attendance is highly desirable.

NATIONAL GEOGRAPHIC MONOGRAPHS.

A recent number of the 'National Geographical Magazine,' as well as a circular distributed by the American Book Co., New York, announces the early preparation of a series of geographical essays under the above title, prepared by various experts and addressed particularly to the public school teachers of this country. The intention of this series of monographs is to present accurate and properly correlated information upon the geography of our country, in simple, untechnical language, and with good illustrations, in such form that it may be practically useful in supplementing the ordinary teaching of physical geography. They are to help supply the teacher with that background of knowledge that is so essential to good teaching. They will not replace any existing text-books, but in time, as the number of monographs increases, they will certainly be freely drawn on by text-book makers. They deserve prominent mention in Science, for although reduced to as simple form as possible, the names of the authors announced are a guarantee that the monographs will be essentially scientific in character. Their appearance will be watched for with interest.

GEOGRAPHICAL PRIZES.

The National Geographic Society announces as a subject for a competitive prize essay in 1895: 'The River Systems of the United States.' The essays must not exceed two thousand words in length, and will be received only from those public schools whose intention to compete is announced not later than May, 1895. The essays must be composed entirely by scholars. They must be written by the end of the school year, 1894–'95, and submitted to the Society not later than July 15th next. The geographical gold medal of the Society will be awarded to the best essayist of the country; the second best will receive a certificate of honorable mention. The best essayist of each State will receive a certificate of proficiency from the committee on awards. This committee consists of General A. W. Greely, Professor T. H. Mendenhall and Superintendent W. B. Powell. Further information concerning the competition may be had from the Society by addressing its Secretary in Washington, D. C.

NEWELL'S REPORT ON AGRICULTURE BY IRRIGATION.

Much physiographic material is gathered in the harvest fields of other subjects. A good opportunity for physiographic gleaning is Newell's 'Report on agriculture by irrigation in the western part of the United States at the eleventh census' (1890), recently issued. In California, where irrigation has attained greater importance than in any other State, the advantageous arrangement of the canals and ditches is in many cases peculiarly dependent on the aggraded alluvial fans that the streams from the Sierra have so often built out from their canyons on emerging upon the open valley plain. The fans of Kings and Kern rivers are the best illustrations given of this kind. The abrupt slopes of the San Bernardino mountains in the southern part of the State are cut by deep narrow valleys from which the waste is strewn in great alluvial fans of unusual height and radius. Newell shows these to be of much importance in their relation to agriculture, but, as if to illustrate the backward condition of geographical terminology, and the slow penetration that the few terms already invented make among practical engineers, he calls these well-formed fans by the vague term, 'great masses.' "The debris, consisting of sand, gravel and bowlders, has been piled in great masses at the points where the streams enter upon the lower plains." If it were not for the earlier account of these huge fans by Hilgard (Bull. Geol. Soc. Amer., iii, 1891, 124) they could hardly be recognized here. In Arizona we read that the irrigating streams are largely supplied by rains induced by the enforced ascent of the winds when they encounter the precipitous and ragged fault scarp, where the great plateaus rise out of the lower desert plains. In Idaho a great expanse of dissected country, where the rivers have cut down deep valleys, cannot be irrigated without expensive engineering operations; but farther up the Snake River, "where the streams have not yet succeeded in cutting through the lava," the river water can be distributed over the plain with comparative ease. 'Yet' is a most expressive word for the geographer. The whole report is full of suggestive examples for extract and quotation.

BAYS AND FIORDS REGARDED AS SUBMERGED VALLEYS.

Early writers generally ascribed bays and fiords to the destructive action of the sea, or to local dislocation. Esmark, about 1826, was perhaps the first to ascribe much importance to ice as an agent in making the Norwegian fiords; a suggestion that was afterward carried to an extravagant extreme. Dana, on returning from the Wilkes expedition, introduced the idea that fiords are drowned valleys; but whether the erosion of the valleys was done by 'river work alone, or more or less by glaciers,' must be determined by local study. In the present view of the problem, glacial erosion is almost by general consent reduced to a moderate measure; it is chiefly the fiord basins that are now attributed to ice action, while fiord valleys are regarded by nearly all observers as of preglacial origin as ordinary land valleys, afterwards submerged. Bays, like Chesapeake and Narragansett, are commonly regarded as resulting from the submergence of wide river valleys, modified by glacial erosion or deposition, if in glaciated regions. This modern view is lately reënforced in an article by Professor Shaler (Evidences as to change of sea level, Bull. Geol. Soc. Amer., vi., 1885, 141–166), in which various reëntrants of our coast, such as Chesapeake and Narragansett bays, the fiords of Maine, and the numerous depressions which break the northern part of the continent into a group of islands, are all ascribed wholly or chiefly to the submergence of stream-worn lands. The general problem of submergence seems, however, hardly so simple as 'to indicate a progressive subsidence of a somewhat uniform nature' along the Atlantic coast from Mexico to near the pole. The possibility of numerous subordinate and discordant oscillations in different parts of the coast is wide open; and while in a general way it may be said that our eastern coast has been depressed, it does not follow that the depression was synchronous throughout, as it must have been if its cause were a movement of the sea floor; hence a preference for this 'hypothesis of Strabo' hardly seems warranted. The submergence of our southern coast may now be going on, while the northern coast may be at present rising, but not risen enough to correct an earlier and greater submergence. This would make diverse continental movements the essential cause, and displacement of the sea floor only secondary.

GEOLOGIC ATLAS OF THE UNITED STATES.

Although primarily of geological interest, the several folios of this great atlas now issued are important to geographers from the accurate and succinct accounts that they give of topographical features. The topographical sheets alone are very instructive; but their value is greatly increased when accompanied by explanations that have been prepared by trained observers who have been all over the ground, examining the forms of the surface as the expressions of internal structures. From the sheets in eastern Tennessee we may learn of the two peneplains that there give local illustration of wide-spread Appalachian forms. On the Livingston sheet, Montana, there is a fine illustration of one of the many extinct lake basins now drained through a steep-walled gorge, in a way so characteristic of the northern Rocky Mountains. With the Placerville sheet, in the California Sierra, the text tells of the reduction of the mountain belt to gentle slopes before the eruption of the great Neocene lava flows by which many of the older valleys were broadly filled; and of the deep canyons cut by the displaced rivers since the mountain belt has been upheaved with a westward slant. The plan of liberal distribution of these folios ensures that they will reach a wide variety of readers. They will be welcomed by many workers: students, teachers and investigators; geographers, geologists and economists.

GEIKIE'S GREAT ICE AGE.

The third edition of this important work has been lately issued (New York, Appleton, 1895). Although distinctly a geological treatise, not written from the geographical point of view, it contains numerous pages of physiographic interest, for many glacial deposits are so young as still to preserve essentially their constructional form; hence the account of moraines, drumlins, rock-basins, and so on, are of immediate geographical value. The general subject of glacial erosion is hardly treated with the fulness that the many discussions it has given rise to would warrant; and the explanation of rock-basins does scanty justice to the opinions of many Swiss geologists who look on ice action as a secondary process compared to a gentle warping of pre-existent valleys. The extract from Wallace's paper, defending the glacial excavation of rock-basins, would imply that that author was not acquainted with the numerous lakes of dislocation in our western territory. For American readers the two chapters and the several maps by Chamberlin will prove attractive.

W. M. Davis.

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LABORATORY TEACHING OF LARGE CLASSES—ZOÖLOGY.*

If the large and increasing attendance at our summer schools, and the publication of many books and the reports made by those dealing in scientific apparatus, can be taken as an index, the amount of zoölogical teaching is very rapidly increasing, and the conduction of large classes is a problem of considerable importance.

A class of college students numbering twenty or twenty-five, and conducted by one officer, is a large class and, even with a favorably equipped laboratory, is quite as large as a single teacher should attempt to carry. Of course, if a certain number of assistants can be engaged, a larger number of students can be directed, though this is virtually the establishment of so many subclasses.

One of the first conditions for successful zoölogical instruction is that of immediate environment. To crowd a score or more of katabolic youth into a small, miserably-lighted room, and compel them to breathe the fumes of stale alcohol for two or three hours, is to invite failure. Each student should have a table to himself where there is good light, and where he feels a certain amount of proprietorship. It should be so located that he is not tempted to carry on a clandestine parasitism, or even a symbiotic

*A paper read before the American Society of Naturalists at the Baltimore meeting, December 28, 1894.

existence with his neighbors. He should be provided with instruments, drawing and dissecting, that are his own, and these ought not to be handed down from class to class, broken and rusty and inheriting mutilations from a long line of ancestors. Each table should be provided with a drawer or locker in which towel, dissecting tray, books, notes, etc., can be safely kept, and any disposition towards untidiness should be censured.

I do not think that the best dissecting material obtainable is any too good for the college student. An advanced worker, or one of a small class, may perhaps profitably examine poorly prepared material, but nothing can more effectually dampen the enthusiasm of an instructor than to see a student *pour* from carapace to the dissecting dish the only too appropriately named 'soft-parts' of a crab or lobster. There is everywhere an abundance of good laboratory material, if the teacher will only exercise a little activity and foresight. With the numerous preserving fluids, and with alcohol free of duty, the student should have perfectly preserved material, unless living forms are available.

The compromise that is often made between the lecture and laboratory, by the mere exhibition of specimens or the passing of specimens from hand to hand during the lecture, is slipshod and dangerous. Such a display may come off once or twice a month, and if carefully conducted is of considerable value, but if occurring frequently there is bound to be a most unfortunate sameness in the style of presentation. The average student who has carefully dissected the cranium of the cat or sheep will take away with him a better understanding of the mammalian skull than he who has viewed acres of diagrams or handled, for a moment, the skulls of all the typical vertebrates.

In certain laboratories it is considered good form to prohibit, or at least to discourage, the free consultation of books of reference by the laboratory student. Pictures and diagrams, illustrating the animals under discussion, are supposed to poison the adolescent mind and should only be kept in the inner recesses of the professor's study, where he may occasionally retreat for a few moments of silent communication, after having been floored by a poser from one of his students. In my opinion, the student should be given every possible aid; there should be books galore; charts and diagrams should be conspicuous upon the wall; and fine dissections, made possibly by advanced students, anatomical preparations and models should be freely displayed upon the reference table. Prof. Howes, in his admirably equipped laboratory in London, has placed upon a ledge, running nearly around the room, a series of most beautiful dissections. In America these are too often hidden away in cases, and I fail to understand why the best of such material is placed in our museums, ostensibly for the education of the public, but actually to the sacrifice of the interests of the student.

Speakers at earlier meetings of this Society have, I think, not over-estimated the educational value of drawing, but we should be very careful that the permission to diagrammatize is not interpreted as permission for free-hand carelessness. The drawings should be carefully prepared; outline, composite pictures of the material studied.

It is unfortunate that we must introduce the microscope into our large class of 'zoology students.' The question of first expense, for every student must have an instrument, is a serious one, and then there is the time lost in giving a course in optics. Here, however, a little forethought will prevent much waste of the precious time actually appropriated to zoölogy. It is well to have one or two extra instruments in reserve, to use in case of accident, and there should be an abundance of the material studied. I feel that one should be cautious in appropriating large time to the process of killing, staining, and other matters of pure technique, and especial care should be taken lest the disease of 'microtome-mania' become epidemic. The microtome is an instrument for the advanced worker and the investigator, but it is no uncommon thing to see a student, yielding to the blandishments of the instrument, cutting sections by the yard, when a few questions will reveal shameful ignorance of the gross anatomy of the animal imbedded.

One is inclined to think that the enthusiasm of the student is the proper index of the work accomplished. But it is not, at least not always. The course in zoölogy should be a course in zoölogy, and the student, certainly of the elementary class, should not be allowed to take alluring short cuts to histology, embryology and advanced morphology. There is a vast amount of microscopical work that can and ought to be done in our large classes. At Brown University the work of an entire term is upon the cat. The material is easy to procure; the organs are large, and I think the time of fifty students well spent. A critical study of other vertebrates should use up the two remaining terms of the year. The turtle and the snake very fairly represent the reptilian phylum; the latter, aside from popular prejudice, is a most satisfactory animal for the laboratory. I think it is a mistake not to more generally provide Elasmobranch material for the college student. When skate and 'dog-fish' can be so readily procured and so easily preserved, every zoölogical laboratory should have an abundance.

And now let me mention a condition, and the one upon which success with large classes most directly depends, viz., order and system. Though the members of our class are not all free and equal, as Americans they must be treated as such. The work of a certain day must be planned for the class as a whole, and not for individuals of the class. All students should have, at the beginning of the session, the same equipment, the same material, and matters of neatness should be enjoined upon all alike. The water in the dissecting trays must be frequently renewed, organic refuse must be disposed of, the tables must be kept dry, the instruments should not be allowed to soak in the bottom of the pan, or the pencil used as a probe. The table should not be smeared with blood, fat and alcohol. There should be a place for everything, and 'systematic zoölogy,' in the sense of order, should everywhere prevail. It is much easier for the student to become indifferent to the orderly side of zoölogy than it is for him to acquire respect for the cleanly.

A definite syllabus, placed upon the board, or laboratory outlines, one on each table, must be used. The latter can be prepared by the teacher and struck off with a cyclostyle or hectograph, and they are of immense help. The student knows what to do and when and how to do it. Extra paragraphs may be added for those who work more rapidly; though quality and not quantity should be the end.

The teacher, with his eye upon the whole class, must go from table to table, quizzing here and helping there. He must be ready to dissect mutilated specimens and reproduce lost parts instanter; and thankful is he, if not too frequently he is constrained to follow that motto placed by Professor Agassiz so conspicuously at Penikese: "Do not be afraid to say, I do not know."

I must beg your forbearance while I say a few words in regard to the large zoölogy classes in our secondary schools. It is my opinion that laboratory classes, conducted along the lines which we have just mentioned, are not at the present time to be too strongly urged for the common schools. There are very few teachers who have had proper training for this kind of work, though the number is happily on the rapid increase. The 'hard parts' of the lower animals, starfish, urchins, molluscs, crustacea, insects, etc., offer ample opportunity for elementary zoölogical work, but it seems to be hardly advisable to largely recommend the dissecting of mammals by the average class, though I think the isolated parts, eye, bones of the ear, the tongue, heart, brain, etc., can be properly and very profitably used. Elaborate outfits of dissecting instruments are not here necessary, though one or two microscopes are desirable. In the secondary school there is a splendid opportunity for the cultivation of the observational powers, by comparing the external characters of animals; by observing habits; how the bird breathes; how it involuntarily grasps the branch; the adaptation of structure to use in the feet of waders, scratchers and singing birds; the structure of the scale and feathers, and claws; the pneumaticity of the bones; the preening of the feathers; the dull coloring of the female; the shapes and colors of eggs and any peculiar nesting habits. It is all wrong for a child to think that zoölogy can only be learned over a dissecting dish. The fundamental principles of biology, the theory of adaptation, protective coloring, protective and aggressive mimicry, distribution, degeneration, parasitism and development can all be illustrated to and understood by the school-child who has never held a scalpel.

The school-room already has its plants; it should also have its local collection. The children make most enthusiastic and active collectors. It is not necessary that the teacher should be qualified to give off-hand the sesquipedalian scientific name of each and every insect that is brought to the school. A far better goal is reached when the student is taught to recognize homologies, to place grasshoppers, katydids and crickets together, to have a separate apartment for butterflies and moths, and another for beetles, etc. Perhaps certain students may be interested in the molluscan fauna of the neighborhood and others may choose to collect cocoons. (I recently read in one of the ubiquitous anti-vivisection papers that the lung of the pond-snail is provided with most beautiful rows of minute horny teeth. Early observations would not only correct such aberrations, but would secure a familiarity with natural phenomena which would give that philosophical training that is often so lamentably lacking in our educated classes.) The child is delighted with the movements of aquatic animals. Aquaria should be in every school. There are hundreds of animals to be collected in any pond or stream, and how easy is it to here find themes for written exercises and models for drawing!

The zoölogy of the secondary school should not be merely an isolated subject of study. It is not attractive to some, and knowledge cannot be forced upon unwilling minds; but it can be unconsciously absorbed in solution. Zoölogy then should enter into the reading, the writing, the spelling, the arithmetic; geography is stupid without it, and the history of human progress is but distribution with the consequent 'struggle for existence' and the 'survival of the fittest."

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*NOTES ON THE BIOLOGY OF THE LOBSTER.**

Reproduction.—After hatching a brood in May, the female usually molts and afterwards extrudes a new batch of eggs. In

*This paper was read before the Society of Morphologists, Baltimore, December 28th.

The following observations are from part of a prolonged investigation of the habits and development of the lobster, undertaken for the U. S. Fish Commission. The detailed work, now ready to go to press, will be published in the Fish Commission's Bulletin. It will contain a full presentation and discussion of the habits and general life-history of the adult lob-this case egg-laying follows close upon copulation. Sometimes a female is impregnated immediately after the old eggs are hatched and before the molt occurs. A second copulation is then necessary for the fertilization of the eggs. Occasionally the seminal receptacle of a lobster is found loaded with sperm a year before the eggs are due.

Laying of Eggs.—Much confusion has surrounded this subject because of the lack of continuous observation throughout the year. The facts seem to be as follows: The majority of lobsters capable of spawning lay eggs in July and August. About 20 to 25 % extrude their eggs at other times, it may be in the fall, winter or spring. During a period of seven consecutive months five traps were kept set in the harbor of Wood's Holl, Mass., December 1st, 1893, to June 30th, 1894, and visited daily. In all 168 egg-lobsters were taken; 44, or 25.6 % of the number, bore eggs which had been laid in the fall and winter.

I have tabulated 51 lobsters coming from different parts of the coast of Maine, having external eggs which had been laid out of the usual season of July and August. In one case at Matinicus Id., Maine, February 4th, the eggs had been extruded but a few hours, and the yolk was unsegmented. Another from York Id., Maine, November 15th, had eggs in a late state of segmentation of the yolk. Still another from Brimstone Id., Maine, January 27th, had eggs in the nauplius stage. At Wood's Holl, in 1889 to 1893, the recorded observations (over 300 in all) show that the greatest number of eggs are laid in the last two weeks of July, the whole period lasting from June 16th to August 31st. Data from the Maine coast (129 observations) indicate that the greatest number spawn in the first two weeks of August.

ster, and the habits of the larvæ and young during their period of immaturity. The history of the larva and the structure and development of the reproductive organs will be fully described, and the development of the embryo will also be reviewed. The work is illustrated by 54 full-page plates, many of which are executed in colors or reproduced from photographs, and by 40 figures in the text.

The spawning period of lobsters in the extreme north is said to last from July 20th to August 20th in Newfoundland. July and August are the months commonly assigned for the spawning in Prince Edward Island.

Number of Eggs Laid and Law of Production.—In the course of the work of lobster-hatching at the Station of the United States Fish Commission at Wood's Holl, it becomes necessary to remove the eggs from a large number of lobsters. These are carefully measured and the number deduced by simple calculation. I have tabulated the number of eggs laid in 4,645 lobsters measuring from 8 to 19 inches. In examining the column of averages one is struck by the fact that a ten-inch lobster bears twice as many eggs as one eight inches long; that a twelve-inch lobster bears twice as many as one ten inches long. It is therefore suggested that in early years of sexual vigor there is a general law of fecundity which may be thus formulated; the number of eggs produced by female lobsters at each reproductive period varies in a geometrical series; while the lengths of lobsters producing these eggs vary in an arithmetical series. If such a law prevails we would have the following:

Series of lengths in inches:

(1)	(2)	(3)	(4)	(5)	(6)
8 :	10 :	12 :	14 :	16 :	18

Series of eggs:

5,000 : 10,000 : 20,000 : 40,000 : 80,000 : 160,000.

An examination of the table shows how closely the first four terms of this series are represented in nature, and that when the 14–16-inch limit is reached there is a decline in sexual activity. Yet the largest number of eggs recorded for lobsters of this size show that there is a tendency to maintain this high standard of production even at an advanced stage of sexual life.

A graphic representation of the fecundity of the lobster tells more forcibly than words or figures can do how closely it conforms to the law just enunciated. The curve which we obtain is the wing of a parabola; the curve of fecundity is parabolic up to the fourth term, where the ratio of production is distinctly lessened. The largest female lobster, carrying the largest number of eggs, was obtained at No Man's Land, June 9th, 1894. It was sixteen inches long and carried one pound of eggs, estimated to contain 97,440. It is safe to assume that the average number of eggs laid by a lobster eight inches long is not above 5,000. The large lobster just mentioned, on account of the incumbrance of its eggs, was unable to fold its 'tail,' which suggests the explanation of the rudimentary condition of the first pair of swimmeretts. If these appendages were of the average size the large number of eggs which would naturally adhere to them would make folding of the abdomen impossible, and it is by folding the 'tail' that the egg-bearing lobster so successfully protects her eggs and eludes her enemies.

Period of Incubation.—Summer eggs which are laid in July and August are ordinarily hatched in June, after a period of from ten to eleven months. Nothing is known about the hatching of fall and winter eggs. The majority of the eggs which are hatched at Wood's Holl complete their development in June.

That young are hatched at other times is certain, and we should expect this to be the case from the variations which occur in the time of ovulation. Captain Chester in 1885 hatched some eggs at Wood's Holl Station on the 8th of November and the following days, the temperature of the water varying from 54.3 to 56 degrees Fah. Some lobsters were hatched early in February in 1889 at the hatchery of the Fish Commission Station at Gloucester, Mass. The water was very cold, and it was estimated that as many as 10,000 lobsters were hatched.

Period of Sexual Maturity. Lobsters become mature when measuring from 7½ to 12 inches in length. Very few under 9 inches long have ever laid eggs, while but few have reached the length of 10½ inches without having done so. The majority of female lobsters 10½ inches long are mature. Anatomical evidence shows that the period at which lobsters become mature is a variable one, extending over several years.

Frequency of Spawning. The adult lobster is not an annual spawner, but produces eggs once in two years. This is proved by the anatomical study of the reproductive organs, and confirmed by the percentage of egg-bearing lobsters which are annually captured. In a total catch of 2,657 lobsters, December 1st to June 30th, 1893 and 1894, the sexes were very nearly equally divided, and about one-fifth of the mature females caught bore external eggs. The catch off No Man's Land in 1894 amounted to 1,518 lobsters; 93.5% were females, and 63.7% carried eggs. When these results are averaged it is found that about one-half of the females carried eggs, as would be the case if they spawned every other year. Ehrenbaum is, without doubt, mistaken in supposing that the lobster does not breed oftener than once in four years (*Der Helgolander Humer, ein Gegenstand deutscher Fischerei.* Aus der Biologischen Anstalt auf Helgoland, 1894.

Gastroliths. Gastroliths are known only in two Macroura, the lobster and crayfish, and were observed in the lobster for the first time, and recorded by Geoffroy, the Younger, in 1709. Though a differentiated part of the cuticle, they are not cast off in the molt, but are retained and dissolved in the stomach. Their structure in the lobster, consisting of hundreds of small spicules, makes the solution of them possible in a very short time.

The gastroliths have been supposed to possess great medical properties and to perform a variety of functions, the most common and accepted belief being that they play an important part in the provision of lime for the hardening of the new shell. The small quantity of lime which they contain, however, not more than one one hundred and twenty-sixth of that of the entire shell, according to an analysis recently made by Dr. Robt. Irvine, shows that this is relatively unimportant. Fragments of lime furthermore are always at hand, and are frequently eaten by the soft lobster, shortly after ecdysis, in the adolescent stages at least. It is more likely that the gastroliths are the result of excretion of lime which is absorbed from parts of the shell to render molting possible, and that their subsequent absorption in the stomach is a matter of minor importance.

Rate of Growth.—Larvæ increase in length at each molt (stages 2 to 10) from 11 to 15.84%, or on the average about 13.5% (measurements from 66 individuals). The increase in the young at each molt agrees quite closely with that seen in the adult, where the increase per cent. in ten cases was 15.3%. Allowing an increase per cent. at each molt of 15.3—probably not excessive for young reared in the ocean—and assuming the length of the first larvæ to be 7.84 mm. we can compute approximately the length of the individual at each molt.

Length at 10th molt	28.23 mm.		
" " 15th "	57.53	"	
" " 20th "	117.24	"	
" " 25th "	258.90	"	(9.5 inches.)
" " 30th "	486.81	"	(19.1 inches.)

According to this estimate a lobster two inches long has molted 14 times; a lobster 5 inches in length, from 20 to 21 times; an adult from 10 to 11 inches long, 25 to 26 times; and a 19-inch lobster, 30 times. These estimates do not, I believe, go very far astray. We see them practically verified up to the tenth molt.

The time interval between successive molts is the next point to consider. Here the data are very imperfect. How long is the three-inch lobster in growing to be six inches long? Probably not more than two years and possibly less. This is supported by the observations of G. Brook. We therefore conclude that a ten-inch lobster is between four and five years old, with the highest degree of probability in favor of the smaller number.

Francis H. Herrick.
Adelbert College.

THE NEWARK SYSTEM.

In an article in a recent number of Science* Professor C. H. Hitchcock again objects to the use of 'Newark' as a group name in geology. This article is essentially a republication of a portion of a paper by the same author, which appeared in the American Geologist in 1890† in criticism of an article of mine in the same journal,‡ in which reasons were presented for reviving the use of Newark as a name for a certain system of rocks.

I replied§ to Professor Hitchcock's objections and criticisms, and showed conclusively, as I believe, that the term referred to has precedence over all other names applied to the system in question, which do not imply correlation. In his recent article Professor Hitchcock does not so much as mention my rejoinder; but is of the opinion that the considerations presented in his earlier paper 'would have been sufficient to convince any one, looking at the subject judicially and impartially, of the inadequacy

*Vol. 1, New Series, Jan. 18, 1895, pp. 74–77.
†Vol. 5, 1890, pp. 197–202.
‡Vol. 3, 1889, pp. 178–182.
§Am. Geol., Vol. 7, 1891, pp. 238–241.

of the name Newark to special recognition.' On the other hand, I am of the opinion that my reply should have silenced opposition. There is, thus, a radical difference of opinion between us. There is also a question of fact involved. Has Newark priority as a group name? This is a simple historical question that almost any one can decide from the documentary evidence. In the papers described in the following foot-note* I have presented or referred to all of the evidence known to me bearing on the question.

In Professor Hitchcock's recent article there are many statements that have no relation to the matter under discussion, since they refer to usages of later date than the introduction of the term Newark. No legitimate arguments are advanced that are not in the former paper, and as these have all been answered, there is nothing left for me to do but to follow my opponent's example and republish my reply to his five-year-old criticism.

My paper in the American Geologist for April, 1891, reads as follows:

"IN a brief paper on the Newark system published in this journal [Am. Geol.] about two years since,† I proposed a revival of 'Newark' as a group name for the reddish-brown sandstones and shales and associated trap rocks of the Atlantic coast region, which had previously been quite generally referred to the Triassic and Jurassic. A long list of names was presented that had been used to designate the rocks in question; nearly all of which implied correlation with European terranes, ranging from the Silurian to the Jurassic. The advisability of adopting a name that did not indicate relationship with distant formations was also pointed out. The first name on the list referred to which met this requirement was 'Newark group,' proposed by W. C. Redfield, in 1856. That this was a group name, intended to indicate the entire formation, is shown by the language used. Redfield's words are:

"I propose the latter designation [Newark group] as a convenient name for these rocks (the red sandstone extending from New Jersey to Virginia) and to those of the Connecticut valley, with which they are thoroughly identified by foot-prints and other fossils, and I would include also the contemporaneous sandstones of Virginia and North Carolina."*

As stated in my previous paper, the term 'group' has been adopted by the International Congress of Geologists in a wider sense than was implied by Redfield. I therefore suggested that 'system' should be substituted instead. Before offering the suggestion I made what I believe to have been an exhaustive examination of the literature relating to the terrane in question, and concluded that Redfield's name had precedence over all other names that had been used which did not imply correlation.

The term Newark system has recently been adopted by several geologists, in accordance with my suggestion, and up to the present time but one voice has been raised against it. In an article on 'The use of the terms Laurentian and Newark in geological treatises,' published in this journal, † Prof. C. H. Hitchcock has formulated five objections to its acceptance. These will be considered in the order in which they were presented.

First. It is claimed that 'An essential feature of a name derived from a geograph-

*The Newark System, Am. Geol., Vol. 3, 1889, pp. 178–182.

Has 'Newark' priority as a group name, Am. Geol., Vol. 7, 1891, pp. 238–241.

The Newark System, U. S. Geol. Surv., Bull. No. 85 (Correlation Papers) 1892.

†Vol. 3, 1889, pp. 178–182.

†Am. Geol. 5, April, 1889, p. 251.

* Am. Jour. Sci., 2d ser. 1856, Vol. 22, p. 357; also in Am. Assoc. Adv. Sci., Proc., Vol. 10, Albany meeting, 1856, p. 181.

† Vol. 5, 1890, pp. 197-202.

ical locality is that the terrane should be exhibited there in its entirety or maximum development;' and that the territory about Newark, N. J., does not meet these requirements for the Newark system.

Without dissenting from the wisdom of the rule proposed, although a large number of exceptions could be found to it in the best geological memoirs, I wish to state from my own knowledge that the region about Newark may be taken as typical of the terrane named after that city. The characteristic reddish-brown standstones and shales are there well exposed, and in the neighboring Newark mountains the associated trap rock occurs in sheets of great thickness. This statement is sustained by Prof. Hitchcock's own words, a little farther on in the paper cited, where he says, "the New Jersey terrane possesses the distinguishing features of the Trias quite as well as the one in New England."

That *Passaic* would have been a better name, as Prof. Hitchcock suggests, is perhaps true, but the one before us was definitely selected and has priority.

Second. It is stated by Prof. Hitchcock that the name 'Connecticut or Connecticut River sandstone has priority over Newark,' and was used by several geologists before Redfield's proposal in 1856, 'though none of them had proposed it as a geological term.' The admitted fact that no one had used the name referred to as a geological term, relieves me of the necessity of showing that Redfield's name has priority.

In the writings of the older geologists among whom Prof. Edward Hitchcock will always take the first rank as an investigator of the sandstones of the Connecticut valley, the terms 'Connecticut sandstone,' or 'Connecticut River sandstone,' were used in the same sense as the coördinate term I have just employed, *i. e.*, as a geographical designation; just as they might have referred to the granite of Massachusetts without any intention of proposing a group name. The fact that the older geologists, and among them Prof. Edward Hitchcock, spoke of the Newark rocks of New England under definite group names, implying correlation, is sufficient evidence that they did not recognize the value of an independent name.

Third. It is stated that Prof. J. D. Dana adopted the name proposed by Redfield, in his lectures, but did not use it in his subsequent writings. Prof. Dana's reasons for this course have never been published, and so far as it is a precedent—happily precedents have less weight in geology than in some other professions—it indicates that we should first use the name Newark and then abandon it for other names implying indefinite correlation with distant terranes.

Fourth and *Fifth.* While it is admitted that the terrane under discussion is quite as well represented in New Jersey as in the Connecticut valley, it is claimed that the latter having been studied first, should have furnished the group name. I fully agree with Prof. Hitchcock in this, and could add several other group names which to my taste might be improved, but the author of a geological name, like the palæontologist who describes a new fossil, is entitled to priority. To attempt to introduce a new name for a group of rocks already sufficiently well designated, would only bring confusion, similar to that produced by the great variety of names implying correlation that have already been used for the Newark system." ISRAEL C. RUSSELL.

UNIVERSITY OF MICHIGAN.

DEATH OF GEORGE N. LAWRENCE.

THE veteran ornithologist, George N. Lawrence, died at his home in New York City, Jan. 17, 1895, at the age of 89 years. He was born in New York, Oct. 20, 1806. His wife, to whom he had been married more than sixty years, died only five days earlier.

Mr. Lawrence was one of the most careful and prolific of American ornithologists. The list of his published writings * contains 121 titles, the earliest of which appeared in 1844, the latest in 1891. The period of his productive activity thus covered nearly half a century. He was an active contemporary of all American ornithologists from Audubon and Nuttall to the younger writers of the present day. 'Baird, Cassin and Lawrence' are classic names in ornithology—names associated in joint authorship in Baird's great work on the birds of North America, published in 1858. For nearly fifty years Baird and Lawrence, then the foremost authorities on American birds, were warm personal friends, and on more than one occasion accomplished, by hearty coöperation, what neither could have done alone. It should not be forgotten that their arduous labors paved the way for the refinement of detail that characterizes the bird work of to-day.

Baird busied himself chiefly with the birds of the United States, Lawrence chiefly with those of tropical America. Lawrence described more than 300 new species from the West Indies, Mexico, Central and South America. One genus and twenty species were named in his honor—tokens of respect and esteem—by American and European naturalists.

Baird and Lawrence lived under widely different conditions. Baird led an active official life, burdened with the cares and responsibilities of three great institutions, two of which, the National Museum and Fish Commission, were his own creation; he was constantly overworked and died prematurely at the age of sixty-five years. Lawrence led a quiet, retiring life, far away from the public eye, and died at the ripe age of fourscore years and nine. Still, the two had many traits in common; both were plain and unassuming, kind and thoughtful in their family relations, and ever ready to extend a helping hand to those, however young, whose tastes led them to the study of birds. In looking back over the twenty-five years that have passed since I first enjoyed their acquaintance, my mind constantly recurs to the kindly words of encouragement and advice that shaped my early course as a naturalist, and the friendships that followed will always live among my most cherished memories.

C. HART MERRIAM.

* The Published Writings of George Newbold Lawrence, by L. S. Foster. Bull. U. S. National Museum, No. 40. 1892.

SCIENTIFIC LITERATURE.

A Treatise on Hydrostatics. By ALFRED GEORGE GREENHILL, Professor of Mathematics in the Artillery College, Woolwich. Macmillan & Co., London and New York. 16mo, pp. viii+536.

The science of hydrostatics, originating with Archimedes, is now more than twenty centuries old. It is, in many respects, one of the most perfect and satisfactory of the sciences. This fact, however, arises from the simplicity of the phenomena with which hydrostatics has to deal rather than from anything like continuity of progress during its lengthy history. Indeed, as regards purely hydrostatical principles, we are not very greatly in advance of Archimedes. Our superiority over him is due, first, to an immensely enlarged capacity, through the developments of mathematics, for the application of those principles; and, secondly, to the exploration of the much larger and more interesting domain of hydrodynamics, of which, in fact, hydrostatics is only a special case.

The work of Professor Greenhill treats hydrostatics from the modern point of view. He does not hesitate to cross the border for an excursion into hydrokinetics whenever desirable or essential, although some might infer from the title of the book that such excursions are avoided. The scope and character of the work may be best inferred from the following paragraphs of the preface:

"The aim of the present Treatise on Hydrostatics is to develop the subject from the outset by means of illustrations of existing problems, chosen in general on as large a scale as possible, and carried out to their numerical results; in this way it is hoped that the student will acquire a real working knowledge of the subject, while at the same time the book will prove useful to the practical engineer."

"In accordance with modern ideas of mathematical instruction, a free use is made of the symbols and operations of the Calculus, where the treatment requires it, although an alternative demonstration by elementary methods is occasionally submitted; because, as has well been said, "it is easier to learn the differential calculus than to follow a demonstration which attempts to avoid its use."

Too much stress cannot be laid on this remark with regard to the rôle of the calculus in applied science. We are coming now, after two centuries, to realize clearly that the use of the calculus has become general in all higher investigations, not because the pure mathematicians have so desired, but because the phenomena of nature demand for their interpretation such an instrument of research.

The book is a mine of interesting and useful information, and must become one of the standards for students, teachers and engineers. The principles are illustrated by a wide variety of good examples, many of which are drawn from practical applications. Special attention is given to the problems of flotation and stability of ships, and to problems arising in naval architecture. The theory of the various hydrostatic instruments, including the hydrometer, the barometer and the gas thermometer, is worked out quite fully. A chapter is devoted to pneumatics, and another to pneumatic machines. There are also chapters on capillarity, hydraulics, the general equations of equilibrium, and on the mechanical theory of heat. In short, the work is a very comprehensive one. Few books contain more information per page, and few abound to such an extent in historical references.

The exposition of the author is in general clear and logical, though occasionally an important principle is announced without due warning. Thus, Bernoulli's theorem appears without demonstration on p. 467 in the chapter on hydraulics. It would have been more in accord with the admirable spirit of the book, we think, if the author had given in that chapter the general equations of fluid motion, and thence deduced Bernoulli's theorem, even if this enlargement had required a change in the title of the work.

Some obscurity arises here and there from the author's habit of condensation. Thus, on p. 458 we read, "so that the attraction of pure gravitation on a plummet weighing W g is WG dynes, where G denotes the acceleration of gravity." Of course, the expert would quickly see that Wg means W grammes, but the average engineer will not commend such economy.

The book has a good, but not quite good enough, index. For example, the unusual words *barad* and *spoud* are occasionally used by the author. Their meaning is plain from the context, in most cases, to the specialist, but the general reader would not get any light on these terms from the index; for it does not contain the word *spoud*, while it refers for *barad* to a page on which this word does not occur.

These faults, however, are small ones, and such, moreover, as are well-nigh inseparable from the first edition of a book so full of sound knowledge as this one.

R. S. WOODWARD.

Eine Discussion der Kräfte der chemischen Dynamik. 3 Vorträge von Dr. Ludwig Stettenheimer. H. BECHHOLD. Frankfurt, 1895. 6 Marks.

This pamphlet of 85 pages is certainly revolutionary in character, as the author proposes to abandon some of our fundamental conceptions of chemistry, and to deal with the subject purely mechanically. Chemistry, according to the author, is the mechanics of the smallest bodies, as Astronomy is the mechanics of the largest, while Physics is a connecting link between the two.

In chemistry we have to deal with matter, with equilibria, and with forces. In chemical reactions energy is set free, and we know in many cases its mechanical expression in calories. All the groupings and unions which we express in our chemical formulæ do not necessarily have their counterpart in the substances themselves, but are only conditions of equilibrium, not general but special cases of equilibria. The molecule ceases to be a fundamental conception. Chemistry of to-day is a molecular chemistry, but we must now give up this conception, and, in the place of the molecular or chemical compound, we must introduce, as in astronomy, a 'system,' a 'chemical system.' Atoms combine to form groups due to the action of the various forces, but why not have these groups go on combining until we have something which can be perceived by the senses? If the same force of attraction which binds the atoms together also causes the groups to unite, what conditions the limits of the molecule? In a substance like potassium oxide we do not know whether two or several molecules are combined, but why may not hundreds, thousands or all the molecules be combined? This does not conflict with Boyle's law, since we may regard a gas as if it were only one molecule and having no inter-molecular spaces.

A system composed of a few well-defined atoms and groups is termed a molecular system. These combined systems, and not the ordinary molecules, represent conditions of equilibria. In the second chapter considerable space is given to the consideration of equilibria, both stable and unstable, and the third and last is devoted to the conditions of union in the solid, liquid and gaseous states. As a result of these considerations, the author concludes that chemical forces differ in no wise from mechanical, but that everything points to a mechanical interaction between the smallest particles of matter.

While scientists are always ready to consider new ideas which will lead to wider generalizations, yet it is always a fair question to ask whether a given suggestion will accomplish this. In the present case it seems quite proper to consider whether all the chemical evidence of the existence of atoms and groups forming definite units, called molecules, has been taken into account. If so, then will this method of regarding chemical phenomena enable us to advance further or faster than that involving atoms and molecules? It can be safely predicted that chemists will be somewhat adverse to giving up conceptions upon which their whole science is built, at least until something more than abstract ideas are offered in their place, something about which they can think definitely and clearly, and which will suggest new lines of work. It is doubtful whether the work published by Dr. Stettenheimer will meet with pronounced success in removing these conceptions of atoms and molecules from chemistry, since they have proved so fruitful in the past, and seem to meet the demands of most of the working chemists of to-day.

The book, while clearly printed, contains a remarkably large number of typographical errors. Nearly a full page of corrections is given, yet the reader will encounter many mistakes in the text which form no part of this tabulated evidence of careless proof-reading.

The reviewer has found it difficult to give a satisfactory account of the contents of the second and third chapters in a short review, so that those who may wish to follow the author's applications of his fundamental ideas must read the original.

H. C. Jones.

Johns Hopkins University.

Text-book of Organic Chemistry. By A. Bernthsen. 2d English Edition, translated by G. McGowan, from the 4th German Edition. London, Blackie & Son. New York, D. Van Nostrand. 1894.

The general excellence of this work is indicated by its reception both in German and in English speaking countries. Four German editions in six years have been found necessary, and the second English edition will probably be even more extensively used than the first. The present book is a work of about 575 pages, fifty more than the previous edition, and occupies a position between the elementary and the encyclopedic text-book. As stated in the preface, the descriptive part is condensed as far as possible, and special emphasis put upon summarizing the characteristics of each class of compounds. There are frequent valuable tables of the principal properties of important classes of compounds. The subject-matter is treated in a way showing the intimate knowledge of the literature to be expected from a chemist like Bernthsen, though it seems strange that he makes no reference to American periodicals, but seems content to use the often imperfect abstracts in the foreign journals. The fourth German edition was published in 1893, and the subject is well brought to that date. A point would have been gained, and the value of the book greatly enhanced, had the translator brought to the date of publication of the English editions at least those chapters which treat of classes of compounds on which important work was done in 1893 and 1894. I refer particularly to the sugars, terpene, etc. The translation is good, though sometimes too literal. Many German expressions have crept in, and do not make the matter any clearer. In the text, formulæ of substances are frequently used instead of names. It would be better to use names only, but if P_2S_5 is used in one place because it occupies less space than Phosphorus Pentasulphide, it should be used always, and the one should not appear on one page, and the other a few pages further on. On the whole the work is well adapted to the needs of those American colleges in which organic chemistry can receive the time and attention it deserves. With it a mature student can easily get a good working knowledge of the subject. For undergraduate work, as carried on in most of our colleges, a less ambitious course, thoroughly given and embodying the use of a smaller text-book, seems desirable.

Felix Lengfeld.

University of Chicago.

Systematische Phylogenie der Protisten und Pflanzen. Ernst Haeckel. Jena, 1894; Pp. 400.

Prof. Ernst Haeckel, of Jena, has recently begun an extensive work on the systematic evolution of animal and plant life. It is to be in three parts, the first of which has just appeared as the '*Phylogeny of the Protista and the Plants.*' The second part, on the phylogeny of invertebrates, and the third part, on that of vertebrates, are also promised during the present year. In the present volume the author outlines his plan and presents in the opening paragraphs the main data upon which his phylogenetic trees are based, namely, the three branches of natural science, palæontology, ontogeny, or the life history of individuals, and morphology. The work as a whole is in

Haeckel's most attractive popular style, and is divided and subdivided into titles and headings, thus making it delightful for reading and reference.

The first organisms, he imagines, were Monera, or '*Probionten*,' which were small homogeneous plasma particles with no anatomical structure. Life activity here was limited to mere assimilation and growth, and where the latter exceeded a certain limit of cohesion of the constituent plasm the organism split into two parts and thus formed two organisms. This was the beginning of reproduction and of inheritance. The homogeneous protoplasm of these Monera was an albuminate arising from a mixture of water, carbonic acid and ammonia. The origin of life, therefore, is little more than this particular combination of inorganic parts at a certain period. While it is probable that the Monera were widely created at this period, the atmosphere, temperature, etc., being in the proper condition, it is not probable that they have been produced spontaneously since then. Haeckel states the stages in this creation as follows: 1st. Nitro-carbon compounds were formed by the synthesis and reduction of various acids and salts. The composition was about the same as that of albumen. 2nd. The albumen molecules with water formed crystalline, but as yet microscopically invisible, molecules. 3rd. These albumen groups arranged themselves in definite ways and formed microscopically visible plasma granules. 4th. These plasma granules had the power to assimilate food, a chemical change, and to grow, and at the limit of cohesion to divide and form new ones. These homogeneous plasma granules were *Monera*.

All of this, however, is hardly new to the readers of Haeckel. The greatest novelty of this work lies in his radical views as to the re-classification of animals and plants. He first separates them on the old lines according to their mode of nutrition. Plants are essentially formative organisms and have the power by the thermal energy derived from the sun's rays to change inorganic into organic combinations, taking up carbon dioxide and throwing off oxygen. Animals, on the other hand, are just the reverse; with them the chemical energy of combinations is reduced to heat and motion. It follows that plants must have been the first forms of organisms on the earth, because they only are able to transform by the energy of the sun's rays inorganic substances into organic. Animals were developed secondarily from the plants by a process of parasitism. That is, some of the plants began to absorb and assimilate parts of other plants, thus changing from an inorganic, carbon-dioxide diet to an organic mode of nutrition. This process of nutrition-change, known as *metasitism* (*metasitismus*), is familiar in certain of the higher plants which have acquired the power to absorb solid nutriment, for example, the insectivorous plants. Haeckel derived the original name then from the original plant by a mere change in nutrition. Metasitism plays a most important part in the new theory, and in this book is given more importance than it has hitherto received.

From this original homogeneous substance the several parts of the cell, as it is known to-day, were derived by a process of differentiation. Certain parts of the plasm, by reason of their position, became adapted for the acquisition of food, while the internal parts, unable to take in food, gradually assumed reproductive functions, and in time came to have a certain definite form; thus arose the cell nucleus. The outer portion of the cytoplasm, in addition to its nutritive function, gradually acquired a protective function also, and membranes were differentiated. Later, by a process of incomplete cell division, colonies of these simple cells were formed, and from these the higher cell aggregates were derived by a process of division of labor. Haeckel supposes, from the almost universal appearance of nuclei in cells, that this differentiation, into nucleus and cytoplasm, must have taken place at a comparatively early period, and that all of the forms of life which have a nucleus must have been derived from one early nucleated type, for he is a firm believer in the inheritance of acquired characters.

The primitive plants, from which all of the organic world has been derived, are called 'Probiontes' or archephyta. From this primitive stem, which was non-nucleated type, and composed of absolutely homogeneous protoplasm without indications even of the 'micellæ,' of Hertwig, or the 'Schaumplasma,' of Butschli, were given off the primitive nucleated plant types of the Flagellata in one direction, and the primitive non-nucleated animal (Moneran) types in another. In addition to these two derivatives there was a third, which represents the original chlorophyl bearing plant. These were the *Cyanophyceæ* or Chromaceæ, in which the chlorophyl is not in the form of small plates, but exists as a diffuse coloring matter within the cell. From these forms, which also were non-nucleated, the Bacteria arose by a process of metasitism.

In the primitive plant types of Flagellata the nuclei have not acquired a distinct differentiation, but remain absolutely homogeneous (*i. e.*, not divisible into nuclein, paranuclein, etc.), and therefore represent the first and most primitive forms of nuclear differentiation. These are not derived from the Monera or non-nucleated animal types, but come directly from the primitive plant type or the Archephyta. He gives the name Mastigota to these early flagellated plant cells which belong to the class of Palmellaceæ, and from them he derives all of the higher plants and animals, the latter arising polyphyletically by the process of metasitism. The rise of the higher animals and of man is traced in a direct line down to these primitive plants. The first step in the scale is the origin of the animal Flagellates by change in the method of nutrition and consequent loss of chlorophyl or allied bodies. Then comes the formation of colonies and gradual division of labor until the highest type of protozoon organization is attained. This type is represented by the form *Catallacta*, which is thus the connecting link between the protozoa and metazoa. Volvox occupies a similar position in the phylogeny of the higher plants in their relations to the protophyta.

In general it may be said that this part of the 'Systematische Phylogenie' is a revision of the earlier views of Haeckel. The one essentially new feature is the division line which he makes between plants and animals. This border line has been the subject of contention between zoölogists and botanists for ages, and now he proposes to form a hard and fast distinction. The dividing line is the ability of the organisms, whatever they may be, to form chlorophyl or similar bodies, and thus to derive nourishment, in conjunction with solar energy, from inorganic substances. This, as may readily be supposed, makes havoc with our existing classifications, and the changes will be accepted, if ever, only after much contention. For example, the Fungi (Chytridiaceæ, Zygomycetes and Ovomycetes) are taken from the vegetable kingdom and transferred to the animal, and with them the Saccharomycetes (yeast) and the Bacteria. The latter he claims have absolutely no connection with the fungi—"*Indessen beruht diese Auffassung nur auf der Macht der dogmatischen Tradition und nicht auf welchem rationellen Urtheil*"—is Haeckel's forcible way of representing this position.

On the other hand, many of our so-called Protozoa are taken into the Protophyte division of plants. All forms which have coloring matter in the form of chlorophyl, and are, therefore, holophytic in their mode of nutrition, are transferred to the vegetable kingdom. The greatest drafts are upon the group of Flagellata, which are so often provided with chromatophores. He does not take the Radiolaria, however, with their 'yellow cells,' probably for the reason that they are symbiotic forms. This will probably be the sticking point in such a classification, for even if the dividing principle be admitted, the difficulty will ever be to decide, in these low forms, what is true chlorophyl formation and what symbiosis. The discoveries of Famintzin and Entz show that in many of the lower forms the presence of chlorophyl is due to minute plant cells which live independently of the animals with which they are associated. Before the classification can be complete it must be determined for each form whether the chlorophyl is a symbiotic plant or a natural product.

GARY N. CALKINS.

GEOLOGY.

Kansas River Section of the Permo-Carboniferous and Permian Rocks of Kansas. CHARLES S. PROSSER. Bulletin Geol. Soc. America, Vol. 6, pp. 29–54. 1894.

In the above paper Professor Prosser considers the historic section of the Upper Paleozoic rocks as exposed along the upper course of the Kansas River. As is well known, the early geologists of the State engaged in a most animated controversy over the correlation of the geological formations of this region. Although the investigations of Meek, Hayden, Hawn and Swallow began more than thirty-five years ago and were vigorously conducted for a number of years, still the subject was not settled, and many of the points at issue between the disputants are still open for decision.

The author describes various typical geological sections as exposed in the steep bluffs of the Kansas river and its tributaries, giving the distinctive geological characters and fossils of the various divisions. In connection with this description, there is a complete review of the previous geological work, followed by a chart of tabulated sections, on which the correlation of the early geologists is indicated.

Possibly the most interesting fact in the paper to a geologist familiar with the region, is the statement that the Cottonwood and Manhattan limestones are the same. This limestone, which is the most valuable stone in the State for construction, has been extensively used, and the author states that he has traced it across the country from Cottonwood Falls, on the Cottonwood River, to Manhattan, on the Kansas River. Another interesting fact in reference to the stratigraphical geology is the correlation of the buff, magnesian limestones near Fort Riley with those of Florence, in the Cottonwood Valley.

In conclusion, it is stated that this is only a preliminary paper and that the writer has in hand the preparation of a report in which a full description of the formations of Central Kansas will be given, with the distribution of their fossils and their general correlation.

NOTES AND NEWS.

FORESTRY AND ECONOMIC BOTANY.

THE steady increase of interest in forestry matters, so desirable and essential, has recently become evident in many ways, especially in the Eastern States. New York, Pennsylvania and New Jersey have taken long strides in the right direction in the shape of much needed legislation; and the establishment of forestry journals for the promulgation of knowledge respecting the nature and value of our native trees is a step that will receive commendation from thoughtful people everywhere. The South Jersey Woodmen's Association has shown wisdom in securing an official organ through which they may increase the scope of their influence. The first number of 'The New

Jersey Forester' (May's Landing, New Jersey), contains valuable and interesting articles from the pens of well known scientists. The editor, Mr. John Gifford, Special Forestry Commissioner of New Jersey, reviews recent forestry legislation in that State, and discusses the causes and effects of forest fires in the southern interior of New Jersey; B. E. Fernow, Chief of the Forestry Division, Washington, D. C., presents the extent and aims of the forestry movement in the United States. An illustrated article on 'The Periodical Ciceda, or Seventeen Year Locust,' by Professor John B. Smith, State Entomologist, New Brunswick, N. J., is followed by interesting contributions on 'The Evil Effects of Drifting Sands Along the Jersey Coast,' by Professor Charles S. Dolley, President of the American Association for the Advancement of Education, and on 'The Colony of Russian Refugees at Woodbine,' by H. L. Sabsovich, Superintendent of the Baron Hirsch Colony. The example set by this publication, both in its purpose and the high standard presented in this first number is one which may well be followed by State forestry associations throughout the country.

All teachers of botany and lovers of trees, especially those interested in forestry agitation, will welcome the excellent charts issued by Miss Lewis. The execution of the drawings of leaves and acorns in Chart No. 1 is all that could be desired, accurate and finished in every detail, as might be expected of anything coming from the pencil of one so expert, and so widely and favorably known as a thorough botanist and skilled artist. Miss Lewis, a member of the Academy of Natural Science of Philadelphia and a teacher of long experience, has done much to promote a love for natural history and to encourage its being taught in our schools, and she is to be congratulated upon her latest contribution to this good work. Teachers have only to see the charts to insure the introduction of the same into the class-room.

Stiles and Hasall announce in a recent number of the Veterinary Magazine the discovery of a new species of intestinal fluke (*Distoma tricolor*) in the Cotton-tail rabbit (*Lepus sylvaticus, Bachman*) and in the Northern hare (*L. americanus Erxleben*).

A new and serious enemy to pear trees has recently been discovered in New Jersey by Dr. John B. Smith. It is a flat-headed borer (*Agrilus Sinnatus* Ol.), a species common in Europe, and was imported into a nursery in Union county, N. J., not more than ten years ago. It is already widespread in that State, probably occurring also in New York. The last number of 'Insect Life' (Vol. VII., No. 3) contains an illustrated article on this pest.

M. G. V. Berthoumieu, in the first number of the current volume of Annales de la Société entomologique de France, has begun the publication of what bids fair to be a very complete monograph of the *Ichneumonidæ* of Europe.

E. A. Smyth, Jr., of the Virginia Agricultural College, has recently examined the stomach contents of a large number of hawks and owls, with the result that he is able to show that the good offices of many of these birds by far overbalance any occasional instance of ravages upon the poultry yard. The 'trim and dauntless little sparrow hawk' is found to be a very active enemy of caterpillars, grasshoppers and other insects, as well as of the ubiquitous English sparrow, and to deserve protection at the hands of all farmers.

The Cornell University Agricultural Experiment Station Bulletins for December, 1894 (Nos. 78, 80), just issued, treat respectively of 'The Quince in Western New York,' a subject of considerable interest, inasmuch as quinces are more extensively grown in that district than anywhere else in North

America; and of 'The Variety and Leaf-Blight of the Strawberry.' The publications of the Cornell Station excel in beauty of illustration much of the material issued by similar institutions.

ENTOMOLOGY.

In a recent and excellently illustrated memoir (Musæum Dzieduszyckianum, iv–Lemberg) on the insect fauna of the petroleum beds of Boroslow, Galicia, Lemnicki describes no less than seventy-six coleoptera, of which nineteen are regarded as identical with living European insects, while the others find their nearest allies in boreal Europe, Asia and America. As only four species are identical with those found by Flach at Hösbach, Bavaria, in beds looked upon as Lower Pleistocene by Flach, and since the Hösbach coleoptera as a whole show far less boreal affinities than those of Galicia, Lemnicki thinks the Hösbach fauna must be considered Middle Pleistocene and the Galician Lower Pleistocene.

RUSSIAN SCIENCE NOTES.

The Jubilee-book issued by the University of Kasan in commemoration of the Lobachévsky centenary has already reached a very large circulation. His compatriots are pushing the non-Euclidean geometry.

N. P. Sokolov has just issued at Kiev (University Press) a pamphlet of 32 pages (large 8vo) entitled 'The significance of the researches of N. I. Lobachévsky in geometry.'

Volume VI. of the second series of the Bulletin of the physico-mathematical society of Kasan, pp. 18–41, contains an interesting contribution by W. Sichstel on the fundamental theorems of spherical geometry.

Two books on America have lately been published in Russia. One is by Witkowsky, a scientist sent by the Russian government to study geodetic work in the United States. The other is published by a Russian, now resident in Los Angeles, who has been more than ten years in America, and has here amassed a fortune. He is a fervid republican, and writes under the *nom-de-plume* Tverski.

The well-known and justly admired writer Korolenko, ranked by the Russians second only to Tolstoi of living authors, was during 1893 in America, and is about to issue his impressions of travel. This book, because of the high reputation of the author, is awaited with keen interest.

George Bruce Halsted.

THE COLD SPRING HARBOR LABORATORY.

The A. A. A. S., at its Brooklyn meeting, made two appropriations to aid research in biological laboratories. One at Wood's Holl, of which notice was given in our last number, and one at the Cold Spring Harbor Laboratory; concerning which the following is the wording of the vote:

"That $100 be granted to Franklin W. Hooper in behalf of the Biological Laboratory at Cold Spring Harbor, to be devoted to defraying the expense of original research; he nature of this to be approved by a committee selected by the Council."

The committee appointed consists of the Vice-Presidents-elect of Section F. and G., viz.: Prof. D. S. Jordan and Prof. J. S. Arthur.

Applications are to be sent to Prof. F. W. Hooper, Brooklyn Institute of Arts and Sciences, or to Prof. H. W. Conn, Wesleyan University, Middletown, Conn.

WASHINGTON LECTURES.

The Series of Saturday Lectures, complimentary to the citizens of Washington, will be continued during the season of 1895, under the joint auspices of the Anthropological and Geological Societies. Two courses have been provided for, each so arranged as to give a logical introduction to the science treated.

The addresses will be delivered in the Lecture Hall of U. S. National Museum, 4:20 to 5:30 P. M., on the dates specified. Citizens of Washington and their friends are cordially invited to attend.

The anthropologic course will comprise: (1) an exposition of the elements of anthropology by the President of the Anthropological Society, and (2) somewhat more detailed expositions of the different branches of the science of man by representatives of the four sections of the Society. The geologic course, which is provisionally arranged, will comprise an exposition of the growth of North America from the most ancient geologic period to the present time, illustrated by maps showing various stages in continental development.

ANTHROPOLOGY.

February 23.—*What is the Science of Demology?* Major J. W. Powell.

March 2.—*Human Growth:* Dr. Franz Boas.

March 9.—*The Founding of Sociology:* Vice-President Lester F. Ward.

March 16.—*The Progress of the Scientific Method:* Vice-President W J McGee.

March 23.—*The Growth of Arts:* Vice-President Frank Hamilton Cushing.

GEOLOGY.

March 30.—*The Continent in Algonkian Time:* Prof. C. R. Van Hise.

April 6.—*The Continent in Cambrian and Silurian Time:* Hon. Charles D. Walcott.

April 13.—*The Continent in Devonian Time:* Marius R. Campbell.

April 20.—*The Continent in Cretaceous and Tertiary Time:* G. K. Gilbert.

April 27.—*The Continent in Glacial and Recent Time:* Prof. William B. Clark.

A PROPOSED NATIONAL UNIVERSITY.

Representative Hainer, of Nebraska, has introduced a bill to establish the University of America, in which each State, Territory and Congressional District shall be entitled to an equal proportionate number of students, chosen by means of open competitive examinations. Instruction in all the branches of all departments of knowledge is to be given, and facilities furnished for scientific and literary research and investigation. The government of the University is to be vested in a board of twenty regents.—*Evening Post.*

GENERAL.

John Murray has published the report of the Oxford meeting of the British Association edited by the Assistant Secretary, Mr. G. Griffith. In addition to the address of the President, Lord Salisbury, and those of the Vice-Presidents of the several sections, there are printed in full eight papers by special invitation of the general committee, among them Professor Langley's *On Recent Researches in the Infra Red Spectrum.* The other papers are given in abstract or by title only. Four hundred pages, half the volume, are taken up by the reports of committees and investigators, previously appointed.

Dr. Hermann Weber, a Fellow of the College of Physicians in London, gave last December £2,500 for the purpose of founding a prize to be given triennially for the best essay on tubercular consumption. The competition is open to writers in all countries.

The Vienna Academy of Sciences has received by the will of Josef Treitel 800,000 florins to be used for the advancement of astronomy.

Professor Weldon is announced to discuss *Variation in Animals and Plants* at the second of the special meetings of the Royal Society.

M. Guignard has been elected a member of the Section of Botany of the Paris Academy of Sciences, succeeding M. de Chartre.

DR. F. N. SCHMITZ, Professor of Botany in the University of Greifswald, died on January 28, at the age of 44.

THE University of Wisconsin has begun the publication of series of bulletins in Philology and Literature, in Science, in Engineering, and in Economics, Political Science and History. The numbers so far issued are: *On the Speed of the Liberation of Iodine in Mixed Solutions of Potassium Chlorate, Potassium Iodide and Hydrochloric Acid*, by Herman Schmidt. *Track*, by L. F. Loree. *Some Practical Hints in Dynamo Design*, by Gilbert Wilkes. *The Steel Construction of Buildings*, by C. T. Purdy. *The Evolution of a Switchboard*, by Arthur Vaughan Abbott. *The Geographical Distribution of the Vote of the Thirteen States on the Federal Constitution, 1787–8*, by Orin Grant Libby.

THE J. B. LIPPINCOTT CO. announce *Suggestions to Hospital and Asylum Visitors*, by Dr. John S. Billings and Dr. Henry M. Hurd, and *A Text-book of Chemistry*, intended for the use of pharmaceutical and medical students, by Professors Samuel P. Sadtler and Henry Trimble, of the Philadelphia College of Pharmacy.

GINN & Co. announce *The Religions of India*, by Edward Washburn Hopkins.

D. APPLETON & Co. announce *The Story of the Stars*, by G. F. Chambers, as the first volume in a new series of 'Useful Stories.' This series includes *The Story of the Earth*, by H. G. Seeley; *The Story of the Primitive Man*, by Edward Clodd; *The Story of the Solar System*, by G. F. Chambers. The same publishers announce a translation of Max Nordau's *Entartung*.

SOCIETIES AND ACADEMIES.

THE NEW YORK ACADEMY OF SCIENCES.

THE Section of Geology and Mineralogy, on February 18, listened to papers of which the following are abstracts: Heinrich Ries described the geology and petrography of the 'Harrison Granite' of Westchester county, N. Y. This forms an elongated belt, principally in the town of Harrison, on Long Island Sound, and is in the midst of the mica schists, which Dr. F. J. H. Merrill regards and has recently mapped as metamorphosed representatives of the Hudson River stage. The granite contains both hornblende and biotite and is really a granite-diorite. It is all more or less gneissic, and shades from a coarsely laminated variety with many 'Augen' of feldspar, in the central portion, to decidedly schistose varieties at the border. Evidences of crushing and many curious inclusions in the feldspar are abundant.

In discussion, J. F. Kemp cited the many intrusive bosses of granite all along the north shore of the Sound from Stony Creek, Conn., to Niantic, R. I. The results of observations as yet unpublished, on those in Rhode Island, were given and a few notes on their mineralogy.

G. F. Kunz followed with a paper on the 'Minerals used for the Assyrian, Babylonian and Sassanian Cylinders, Seals, etc.,' which was illustrated by many specimens and lantern slides. An abstract of the paper, which will be printed in full in the Transactions of the Academy, is as follows:

The seals that date from 4000 B. C. to 2500 B. C. are cylinders, a form that is thought to have been suggested by the joint of a reed. Nearly all depict animals without other ornamentation. They were made of black or green serpentine, conglomerate, diorite, and often of the central whorls of the large conchs from the Persian Gulf. From 2500 to 600 B. C. the cylindrical shape continues, but, in addition to the animals, from one to six rows of cuneiform characters appear. Variously colored chalcedony, (especially a blue variety), brick red ferruginous quartz and red hematite are also used. Up to this time the carving was done with a sapphire point, but in the fifth century wheel-work begins to appear. In the sixth century B. C. cylinders begin to be partially replaced by cone-shaped seals, and by the scaraboid forms introduced from Egypt. From the third century B. C. to the third century A. D. the seals become lower and flatter, and finally graduate into rings, mostly with Persian or Sassanian characters. Although in part made from the stones of the neighboring hills, yet rarer materials begin to appear—evidently obtained by trade with Egypt and other countries more or less remote.

In addition to the minerals mentioned above, the following are recognized: clear, pellucid quartz, amethyst, agates of various colors, lapis-lazuli from Bodakshan in Turkestan, amazon-stone, possibly of Egyptian origin, calcite, green and white and in the form of various marbles, aragonite, gypsum, syenite and jade.

It is hoped that further study may enable us to trace these minerals to their original localities with greater certainty.

J. F. Kemp, *Recording Secretary.*

SCIENTIFIC JOURNALS.

THE JOURNAL OF THE AMERICAN CHEMICAL SOCIETY, MARCH.

The Synthetic Food of the Future: Harvey W. Wiley.

The Determination of Phosphoric Acid: H. Pemberton, Jr.

On the Estimation of Sulphur in Pyrites: G. Lunge.

Improvement in the Manufacture of Acetone: E. R. Squibb.

Report of Committee on Atomic Weights, Published During 1894: F. W. Clark.

Coloring Matter in the California Red Wines: W. D. Bigelow.

The Penetration Machine—An Explanation: H. C. Bowen.

Notes: Argon.

AMERICAN JOURNAL OF PSYCHOLOGY, JAN.

Comparative Observations on the Indirect Color Range of Children, Adults, and Adults Trained in Color: Geo. W. A. Luckey.

Minor Studies from the Psychological Laboratory of Cornell University: Taste Dreams: E. B. Titchener. *On the Quantitative Determination of an Optical Illusion:* R. Watanabe, Ph. D. *The Cutaneous Estimation of Open and Filled Space:* C. S. Parrish.

The Daily Life of a Protozoan; A Study in Comparative Psycho-physiology: C. F. Hodge, Ph. D., and H. Austin Aikins, Ph. D.

Minor Studies from the Psychological Laboratory of Clark University: A Study of Individual Psychology: Caroline Miles. *The Memory After-Image and Attention:* Arthur H. Daniels, Ph. D. *On the Least Observable Interval between Stimuli addressed to Disparate Senses and to Different Organs of the Same Sense:* Alice J. Hamlin. *Notes on New Apparatus:* Edmund C. Sanford.

On the Words for 'Anger' in Certain Languages; A Study in Linguistic Psychology: A. F. Chamberlain, Ph. D.

A Laboratory Course in Physiological Psychology; The Visual Perception of Space: Edmund C. Sanford.

Proceedings of the Third Annual Meeting of the American Psychological Association at Princeton.

Psychological Literature.

NEW BOOKS.

History of Chemistry. F. P. Venable. Boston, D. C. Heath & Co. 1894. Pp. viii + 157.

Mental Development of the Child and the Race. Methods and Processes. James Mark Baldwin. New York and London, Macmillan & Co. 1895. Pp. xvi+496. $2.60.

Qualitative Chemical Analysis of Inorganic Substances as Practiced in Georgetown College, D. C. New York, Cincinnati, Chicago, American Book Company. 1894. Pp. 61.

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*THE PLANT INDIVIDUAL IN THE LIGHT OF EVOLUTION.**

THE PHILOSOPHY OF BUD-VARIATION, AND ITS BEARING UPON WEISMANNISM.

I.

WHILST the animal and vegetable kingdoms originate at a common point and are not clearly distinguishable in a number of the lower groups or organic beings, they nevertheless diverge rapidly and they finally become very unlike. I believe that we shall find that this divergence into two coördinate branches of organic nature is brought about by the operation of at least two fundamentally distinct laws. There is a most unfortunate tendency, at the present time, to attempt to account for all phenomena of evolution upon some single hypothesis which the observer may think to be operative in the particular group of animals or plants which he may be studying. For myself, I cannot believe that all forms of life are the results of any one law. It is possible that all recent explanations of evolution contain more or less truth, and that one of them may have been the cause of certain developments, whilst others have been equally fundamentally important in other groups of organisms. If I were a zoölogist, and particularly an entomologist, I should hold strongly to the views of Lamarck; but, being a horticulturist, I must accept largely, for the objects which come within the range of my vision, the principles of Darwin. In other words, I believe that both Lamarckism and Darwinism are true; and, in this connection, it is significant to observe that Lamarck propounded his theory from studies of animals, whilst Darwin was first led to his theory from observations of plants. I am willing to admit, also, at least for the sake of argu-

*Address before the Biological Society of Washington, Jan. 12, 1895.

ment, that Weismannism, or the Neo-Darwinian philosophy, may be true for some organisms, but it is wholly untenable for plants.

There is one feature of this difference between the animal and the plant to which I wish to call your attention on this occasion. It is the meaning of individuality in the two. I must say, at the outset, that when I speak of a plant or an animal I refer to those higher forms which the layman knows by these names, for it is not my purpose to discuss the original causes of divergence so much as those phenomena of individuality which are most apparent to the general observer. The animal may be said to have complete autonomy. It has a more or less definite span of life. It grows old and dies without having been impaired by decay, and the period of death may have no immediate relation to environment. It has a definite number of parts, and each part or organ is differentiated and performs one function, and this function serves the whole animal and not the organ itself. If any part is removed the animal is maimed and the part cannot be supplied, and the severed portion has no power to reproduce either itself or the animal from which it came. The only means by which the animal can multiply is by a union of sexes. The plant, on the contrary, has no perfect or simple autonomy. It has no definite or pre-determined proximate span of life, except in those instances when it is annual or biennial, and here duration is an evident adaptation to environment. The plant frequently dies as the result of decay. It has not a definite number of parts, and each part of the plant may perform a function for itself, and the part may be useful to the remainder of the plant or it may not. One part is like what all other parts are or may be. If one portion is removed the plant may not be injured; in fact, the plant may be distinctly benefited. And the severed portion may not only have the power of reproducing itself, but it may even reproduce an organism like that from which it came. In other words, plants multiply both with and without sex. Potentially, every node and internode of the plant is an individual, for it possesses the power, when removed and properly cared for, of expanding into what we call a plant, and of perfecting flowers and seeds and of multiplying its kind.

Those of you who are botanists now recall the contention of Gaudichaud concerning the plant unit or phyton. He proposed that the leaf, with its connecting tissues, is the vegetable individual and that the plant is a colony of these individuals. Gaudichaud offered this theory as an explanation of the morphology and physiology of plants, and the hypothesis really has no place in the present discussion; but, inasmuch as I have borrowed the word which he proposed for the plant unit, it is no more than fair that I should explain his use of it; and this explanation may serve, incidentally, to illustrate some of the problems of individuality to which I shall recur. Gaudichaud, while recognizing that a cell which develops into a bud is itself an individual, nevertheless considered that the leaf, with its dependent tissues, represents the simple vegetable unit. Each of these units has an aerial or ascending part and a radicular part. The ascending part has three kinds of tissues or merithals—the stem merithal, petiolar merithal and the limbic merithal. Now, each phyton fixes itself upon the trunk or upon an inferior phyton, in the same manner as a plant fixes itself in the soil, and, sending its vascular threads downwards between the bark and the wood, is enabled to support itself upon the plant colony; and, at the same time, the extension of these threads produces the thickening of the stem, and the superposition of phytons increases the height of the plant. This mechanical theory of the morphology of plants was not original with Gaudichaud, but he greatly enlarged it and gave it most of its historic value, and, what is more to our purpose, he used the word phyton, which, in lieu of a better one, I shall use as a convenient expression for that asexual portion of any plant which is capable of reproducing itself. Gaudichaud's fanciful hypothesis was not completely overthrown until the exact studies of Von Mohl upon the vegetable cell established a rational basis of morphology and physiology.

What I wish now to show is that the evolution of the vegetable kingdom cannot be properly understood until we come to feel that the phyton, or each portion of the plant, which, when removed, has the capability of reproducing itself and its parent, is in reality a potential autonomy. In doing this I shall not forget that the plant also has an individuality as a whole, but as this feature is quite aside from my argument and is the conception of the plant which is everywhere accepted, I shall necessarily confine my remarks to the individual life of the phyton. The mere fact that the phyton may reproduce itself is not the most important point, but, rather, that each part of the plant may respond in a different manner or degree to the effects of environment and heredity. Before proceeding to this matter, I should say that there is no doubt about the capability of every plant to be propagated asexually. It is true that all plants have not been so propagated, but there is every reason to suppose that the gardener can acquire the requisite skill to grow oaks and hickories from cuttings were it worth his while to do so. At present there are cheaper modes of multiplying these plants. But certain pines and spruces, which do not seed under cultivation, are propagated by cuttings, and the tissue of these trees is as little adapted to such use as that of any plants with which I am acquainted. The fact that plants are not grown from cuttings does not prove that they cannot be so propagated, for we know that the essential structure of all of them is very similar, and that each node and internode—or each phyton—does or may produce branches and flowers and seeds when it is borne upon its parent plant. And I should remind you that those plants which are not readily multiplied by cuttings are generally propagated by grafting, which, for illustration, amounts to the same thing, for we only substitute the stock of another plant for the soil. Plants of the most various kinds are readily multiplied by graftage. Even tuberous herbaceous stems, which are not commonly associated with the art of the grafter, unite with ease. One of the latest investigators in this field is a Frenchman, Daniel, and his conclusions upon the physiology of grafted plants show that the physiological modifications in these plants are largely such as arise from physical causes, showing that the parts still preserve their essential autonomy.

Now, if every plant varies in the number of parts, or phytons, of which it is composed, it follows that this number must be determined by agencies which act immediately upon the given plant itself. We all know that the number of these parts is determined very largely by environment. A dozen plants springing from the same capsule may vary immensely in the numbers of their branches, leaves and flowers, and this variation is generally obviously correlated with amount of food, amount of space which the plant is allowed to occupy, and other physical conditions which affect its welfare. But we not only find that no two plants have the same number of parts, but that no two branches in the same plant are alike. One part grows longer, one more erect, one has greener leaves, one bears more fruit. So, too, there may be different forms of flowers on the same plant, a subject to which Darwin has devoted an entire volume. We know, also, that this variation amongst the sisterhood or colony of branches is determined by very much the same conditions which determine variation in independent plants growing in soil. I believe that the primary and most important determinant of this variation is the variation in food supply, the same which Darwin believed to be the most potent factor in the origination of variations in general. That branch or phyton which receives the most food, because of its position or other incidental circumstance, is the one which grows the largest, has the heaviest and greenest leaves, and, in the end, is the most fruitful. I use the word food to designate not only the supply of nutriment which is derived from the soil, but also that obtained from the air and which is most quickly and thoroughly elaborated in the presence of the brightest sunlight. Thus the uppermost branches of the tree, whilst farthest from the root, are generally the strongest, because they are more freely exposed to light and air and their course is least impeded. Many branches in the interior of tree tops are undoubtedly parasites upon the plant colony, taking from it more than they return.

If the number of the plant units is determined by circumstances peculiar to that plant, and if there is variation amongst these units in any plant, then it follows that there must be struggle for existence between them. And this struggle differs from the conflict between independent plants in the complex battle for life only in the circumstance that it is more intense or severe, from the fact that the combatants are more closely associated. There are weak branches and strong branches, and the survival of the fittest is nature's method of pruning. The strong terminal branch, shooting upwards towards air and sunlight, makes the bole of the tree, whilst the less fortunate or side branches perish and fall. The leaf surface of any tree or large plant is always pushing outwards towards the periphery, which is only another way of saying that the anterior branches die. I often find fruit growers who refuse to prune their trees because they believe it to be unnatural, while at the same time their tree tops are full of dead limbs, every one a monument to the stupidity of the owner!

Now, the effect of this struggle for existence allows of mathematical measurement. Each bud should produce a branch or a cluster of fruit. A seedling peach tree may be two feet high the first year, producing thirty leaves, and in every axil a bud. Each of these buds should produce a branch, which should again produce thirty buds. The third year, therefore, whilst the tree is only six or eight feet high, it should have 900 branches, and in the fourth year 27,000! Yet a peach tree twenty years old may not have more than 1,000 branches! That is, many millions of possible branches have been suppressed or have died. I once made an actual observation of such a battle and counted the dead and wounded. A black cherry tree came up near my door. The first year it made a straight shoot nineteen inches high which produced twenty-seven buds. It also sent out a branch eight inches long which bore twelve buds. The little tree had therefore enlisted thirty-nine soldiers for the coming conflict. The second year twenty of these buds did not grow. Nineteen of them made an effort, and these produced 370 buds. In two years it made an effort, therefore, at 409 branches, but at the close of the second year there were only twenty-seven branches upon the tree. At the close of the third year the little tree should have produced about 3,500 buds or branch-germs. It was next observed in July of its fourth year, when it stood just eight feet high; instead of having between 3,000 and 4,000 branches, it bore a total of 297, and most of them were only weak spurs from one to three inches long. It was plain that not more than twenty, at the outside, of even this small number could long persist. The main stem or trunk bore forty-three branches, of which only eleven had much life in them, and even some of this number showed signs of weakness. In other words, in my little cherry tree, standing alone and having things all its own way, only one bud out of every 175 succeeded in making even a fair start towards a permanent branch. And this struggle must have proceeded with greater severity as the top became more complex, had I not put an end to its travail with the axe!

II.

I am now ready to say that I believe bud-variation to be one of the most significant and important phenomena of vegetable life, and that it is due to the same causes, operating in essentially the same way, which underlie all variation in the plant world. As some of you may not be familiar with the technical use of the term, I will explain that a bud-variety is an unusual or striking form or branch appearing upon a plant; or, as Darwin put it, bud-variation is a term used to "include all those sudden changes in structure or appearance which occasionally occur in full-grown plants in their flower-buds or leaf-buds." A classical example is the origination of the nectarine from a branch of a peach tree; and one often hears of Russet apples upon a certain branch of Greening apple tree, of weeping, variegated or cut-leaved shoots on otherwise normal trees, or of potatoes that 'mix in the hill.' Now, this matter of bud-variation has been a most puzzling one to all writers upon evolution who have touched upon it. It long seemed to me to be inexplicable, but I hope that you will now agree with me in saying that it is no more unintelligible than seminal variation of plants, for I have already shown that there is abundant asexual variation (of which bud-variation is itself the proof), and that this variation takes place as readily when the phyton is growing upon a plant as when it is growing in the soil. The chief trouble has been, in the consideration of this subject, that persons have observed and recorded only the most marked or striking variations, or those which appear somewhat suddenly (although suddenness of appearance usually means that the observer had not noticed it before), and that they had therefore thought bud-variation to be rare and exceptional. The truth is, as I have said, that every branch or phyton is a bud-variety, differing in greater or lesser degree from all other phytons on the same plant. These differences, even when marked, may arise in every part of the parent plant, as on stems aerial and subterranean, from bulbs and tubers, or even from the adventitious buds of roots; and the characters of these varieties are as various as those originating from seeds. The nurseryman knows that branches differ amongst themselves, for he instructs his budders to cut buds only from the top-most shoots of the nursery rows in order that he may grow straight, vigorous trees; and every farmer's boy knows that the reddest and earliest apples grow on the uppermost branches, and his father will always tell him that he should never select cions from the center or lower part of a tree. Every skilful horticulturist will tell you that the character of the orchard is determined very largely by the judgment of the propagator in selecting cions. To select out the extreme forms of these variations and to attempt to explain bud-variation by them is exactly like selecting the extreme types of seminal variations, and, by ignoring the lesser ones and the intermediates, to attempt to build thereon a theory of the variation of plants. If you ask me why it is that the nectarine was produced upon a branch of a peach tree I will answer that nectarines have also been produced from peach seeds. The answer to one answers the other. It is true that bud-variations, if we use that term, as we logically must, to denote all variations between phytons, are commonly less marked than seed-variations, but this is only because the conditions of origin and environment of the phyton are less varied than those of the seedling. The phytons originate from one parent, not from two; and they all grow in very like conditions. But I am convinced that, when we consider the plant individual in the light of evolution, the bugbear of bud-variation vanishes.

A good proof that bud-variation and seed-variation are one in kind is afforded by the fact that selection can be practiced for the improvement of forms originating by either means. Darwin was surprised, as he says, to "hear from Mr. Salter that he brings the principle of selection to bear on variegated plants propagated by buds, and has thus greatly improved and fixed several varieties. He informs me that at first a branch often produces variegated leaves on one side alone, and that the leaves are marked only with an irregular edging, or with a few lines of white and yellow. To improve and fix such varieties he finds it necessary to encourage the buds at the bases of the most distinctly marked leaves and to propagate from them alone. By following, with perseverance, this plan during three or four successive seasons a distinct and fixed variety can generally be secured." This practice, or similar ones, is not only well known to gardeners, but we have seen that nature selects in the same manner, through the operation of the same struggle for subsistence which Darwin so forcibly applied to all other forms of modification. Once given the three fundamental principles in the phylogeny of the phyton, the variation amongst themselves, the struggle for existence, the capability of perpetuating themselves—an indisputable trinity—and there can no longer be any doubt as to the fundamental likeness of the bud-variety and the seed-variety.

Yet I must bring another proof of this likeness to your mind. It is well known that the seedlings of plants become more variable as the species is cultivated; and it is also true that bud-varieties are more frequent and more marked in cultivated plants. Note, for example, the tendency of cultivated plants to bear variegated or cut-leaved or weeping shoots, and the fact that the colors and doubleness of flowers often vary greatly upon the same plant. Many of our best known roses, carnations, chrysanthemums, violets and other garden plants originated as bud-sports. This fact is so well known that critical gardeners are always on the alert for such variations. In any house of 200 roses, all grown from cuttings, the grower will expect to find more than one departure from the type, either in color or freedom of bloom or in habit of plant. Every gardener will recall the 'sporting' tendencies of Perle des Jardins rose, and the fact that several commercial varieties have sprung from it by bud-variation. As early as 1865 Carrière gave a descriptive list of 154 named bud-varieties, and remarked at length upon their frequency amongst cultivated plants. This fact of greater bud-variability under cultivation was fully recognized by Darwin, and he regarded this as one of the strongest proofs that such variation, like seed-variation, is "the direct result of the conditions of life to which the plant has been exposed."

In order to extend the proofs of the essential ontogenetic likeness of bud and seminal variations, I will call to your remembrance the fact that the characters of the two phytons may be united quite as completely by means of asexual or graft hybridism as by sexual hybridism. I do not need to pursue this subject, except to say that we now believe that graft-hybrids are rare and exceptional chiefly because the subject has received little experimental attention. Certainly the list given by Focke, and the anatomical researches of Macfarlane, show that such hybrids may be expected in a wide variety of subjects and with some frequency. It is now stated positively by Daniel, as the result of direct experiment, that the seeds of cions of certain cultivated herbs which are grafted upon a wild plant give offspring which show a marked return to the wild type. I should also add that the breaking up of seminal hybrids into the characters of either parent may take place, as Darwin has shown, through either seed- or bud-variation. You are all no doubt aware that hybrids generally tend to revert to the types from which they sprung, and this sometimes occurs even in hybrid offspring which is propagated exclusively by buds or cuttings.

Still another proof of the similarity of bud-varieties and seed-varieties is the fact that the seeds of bud-varieties are quite as likely to reproduce the variety as the seeds of seed-varieties are to reproduce their parents. Darwin and others have recorded this seminal transmission of bud-sports. "Notwithstanding the sudden production of bud-varieties," Darwin writes, "the characters thus acquired are sometimes capable of transmission by seminal reproduction. Mr. Rivers has found that moss-roses [which are bud-varieties] generally reproduce themselves by seed; and the mossy character has been transferred by crossing from one species to another." This general fact that bud-sports may reproduce many of their essential acquired characters by seeds is so well grounded in the minds of gardeners that the most critical of them make no distinction, in this respect, between varieties of bud and seed origin when selecting parents for making crosses. And if we can prove the similarity of bud and seed variations by showing that both bear the same relation to transmission of characters by means of seedage, we can demonstrate it equally well by the converse proposition—that both bear the same relation to the perpetuation of their features by cuttings. Some seed-varieties will not 'come true' by cuttings, and there are also some bud-sports which will not, as every gardener of experience knows. I will cite a single case of 'sporting' in bud offspring. One winter a chance tomato plant came up in one of my greenhouses. I let it grow, and it bore fruit quite unlike any other variety which I ever saw. There was no other tomato plant in the house. I propagated it both by seeds and cuttings. I had two generations of cuttings. Those taken directly from the parent plant, 'came true' or very nearly so; then a lot of cuttings from these cutting-grown plants was taken, making the second asexual generation from the original seedling. While most of the seeds 'came true,' few of these second cuttings did, and, moreover, they 'sported' into several very unlike forms—so much unlike that I had both red and yellow fruits from them. In respect to transmission of characters, then, bud- and seed-varieties are alike, because either class may or may not transmit its marks either by seeds or buds.

Finally, let me say, in proof of the further similarity of bud- and seed-variations, that each class follows the incidental laws of external resemblance which pertain to the other class. For instance, there are analogous variations in each, giving rise to the same kinds of variegation, the same anomalies of cut and colored foliage, of weeping branches, party-colored fruits and the like; and the number of similar variations may be as great for any ameliorated plant in the one class as in the other. The most expert observer is not able to distinguish between bud-varieties and seed-varieties; the only way of distinguishing the two is by means of the records of their origins, and because such records of any varieties are few we have come to overlook the frequency of bud-variation and to ascribe all progressive variability in the vegetable kingdom to seeds or sex.

Whilst it is not my purpose to discuss the original sources of bud-variations, I cannot forbear to touch upon one very remarkable fact concerning reversions. It is a common notion that all bud-varieties are atavistic, but this position is untenable if one accepts the hypothesis, which I have here outlined, of the ontogenetic individuality of the phyton, and if he holds, at the same time, to the transforming influence of environment. It is also held by some that bud-varieties are the effects of previous crossing, but this is controverted by Darwin in the statement that characters sometimes appear in bud-varieties which do not pertain to any known living or extinct species; and the observations which I am about to recite also indicate the improbability of such influence in a large class of cases. The instances to which I call your attention are, I think, true reversions to ancestral types. Those of you who have observed the young non-blooming shoots of tulip-tree, sassafras and some other trees will have noticed that the leaves upon them often assume unusual shapes. Thus the leaves of sassafras often vary from the typical oval form to three-lobed and mitten-shaped upon the strong shoots. There are the most various forms on many tulip-trees, the leaves ranging from almost circular and merely emarginate to long-ovate and variously lobed; all of them have been most admirably illustrated and discussed recently by Holm in the proceedings of the National Museum. Holm considers the various forms of these liriodendron leaves to be so many proofs of the invalidity of the fossil species which very closely resemble them. This may be true, for there are probably no specific names of organisms founded upon so fragmentary and scant material as those applied to fossil plants; and yet I cannot help feeling that some of these contemporaneous variations are reversions to very old types. I was first led to this opinion by a study of the sports in ginkgo leaves, and finding them suggestive of Mesozoic types. "This variation in leaf characters," I wrote at the time, "recalls the geologic history of the ginkgo, for it appears to be true that leaves upon the young and vigorous shoots of trees are more like their ancestors than are the leaves upon old plants or less vigorous shoots, as if there is some such genealogical record in leaves as there is in the development of embryos in animals." Subsequent observation has strengthened my belief in the atavistic origin of many of these abnormal forms, and this explanation of them is exactly in line with the characters of reversions in animals and in cultivated plants. It would, of course, be futile to attempt any discussion of the merits of the specific types proposed by palæobotanists, but in those cases, like the ginkgo, where the geologic types are fairly well marked, constant and frequent, and where the similar contemporaneous variations are rare, there is apparently good reason for regarding contemporaneous forms as fitful recollections of an ancient state; and this supposition finds additional support in the ginkgo, because the species is becoming extinct, a fact which also applies to the tulip-tree, which is now much restricted in its distribution. I am further reinforced in this view by Ward's excellent study of the evolution of the plane-tree, for, in this instance, it seems to be well determined that the geologic type has fairly well marked specific characters, and the auricular or peltate base upon contemporaneous leaves, which records the connection between the two, is sufficiently rare to escape comment. Various writers have remarked upon the similarities of these occasional leaves to geologic types, but, so far as I recall, they regard them as remnants or vestiges of the ancient types rather than as reversions to them. There is this important difference between a remnant and a reversion. A remnant or rudiment is more or less uniformly present under normal conditions, and it should give evidence of being slowly on the decline; whilst a reversion is a reappearance of wholly lost characters under unusual or local conditions. Now, my chief reasons for considering these sports to be reversions is the fact that they occur upon the sterile and verdurous shoots, the very shoots which are most likely to vary and to revert because they receive the greatest amount of food supply, as Darwin has shown to be the case with independent plants. And I am therefore able to make still another analogy between phytons and plants, and to illustrate again the essential sameness of bud-variations and seed-variations.

III.

I now wish to recall your attention more specifically to the subject of asexual variation. I have shown that no two branches are alike any more than are any two plants. I have also cited the frequent occurrence of differences so marked that they are called bud-varieties or sports. Carrière enumerated over 150 of them of commercial importance in France, and, as nearly as I can estimate, there are no fewer than 200 named horticultural varieties grown at the present moment in this country which had a like origin. It is also known that there are a number of species in which seeds are practically unknown, and yet which run into many varieties, as the pineapple, banana and bread-fruit; and note, if you will, the great variations in weeping willows, a tree which never fruits in this country. In our gardens there are three or four varieties of the common seedless 'top' onion, and I have been able, by treatment, to vary the root of the horse-radish, a plant which rarely, if ever, produces viable seeds in this climate; and there are variable seedless plants in our greenhouses. I might also cite the fact that most fungi are sexless, so far as we know, and yet they have varied into innumerable species. You will be interested in a concrete case of the apple. The Newtown Pippin, which originated upon Long Island, New York, has been widely disseminated by graftage. In Virginia it has varied into a form known as the Albemarle Pippin, and a New York apple exporter tells me that it is a poorer shipper than the Northern Newtown and is not so long-keeping. In the extreme Northwestern States the Newtown, while it has not been rechristened there, is markedly unlike the Eastern fruit, being much longer and bearing distinct ridges about the apex. Finally, in New South Wales, the ridges are more marked and other characters appear, and the variety is there known as the Five-crowned Pippin. This is not an isolated case. Most Northeastern varieties of apples tend to take on this elongated form in the Pacific Northwest, to become heavy-grained and coarse-striped in the Mississippi Valley and the Plains, and to take other characteristic forms in the higher lands of the South Atlantic States. This asexual variation is sometimes very rapid. An illustration came directly under my own observation (and upon which I have once reported) in the case of the Chilian strawberry. Within two years this plant, growing in my garden, varied or departed from its wild type so widely as to be indistinguishable from the common garden strawberry, which has been regarded by many botanists to be specifically distinct from the Chilian berry. This remarkable departure, which has enabled me, as I believe, to reconstruct the evolution of the garden strawberry, was one in which no seedling plants were concerned. If all the common garden strawberries owe their origin to a like source—as I cannot doubt—then we have here a most instructive case of sexless evolution, but one in which the subsequent generations reproduce these characters of sexless origin by means of seeds.

This asexual modification is not confined to domesticated plants. Any plant which is widely distributed by man by means of cuttings or other vegetative parts may be expected to vary in the same manner, as much experiment shows; and if they behave in this way when disseminated by man they must undergo similar modification when similarly disseminated by nature herself. I need only cite a few instances of habitual asexual distribution of wild plants to recall to your attention the fact that such means of distribution is common in nature, and that in some cases the dispersion over wide areas is quite as rapid as by means of seeds; and some plants, as various potamogetons, ceratophyllums and other aquatics, are more productive of detachable winter buds and other separable vegetable organs than they are of seeds. The brittle willows drop their twigs when injured by storms of ice or wind, or by animals, and many of these cuttings take root in the moist soil, and they may be carried far down streams or distributed along lake shores; the may-apple and a host of rhizomatous plants march onward from the original starting point; the bryophyllum easily drops its thick leaves, each one of which may establish a new colony of plants; the leaves of the lake-cress (Nasturtium lacustre) float down the streams and develop a new plant while they travel; the house-leeks surround themselves with colonies of off-shoots, the black raspberry travels by looping stolons, and the strawberry by long runners; the tiger-lily scatters its bulb-like buds, and all bulbiferous plants spread quite as easily by their fleshy parts as by seeds. Now all these vegetative parts, when established as independent plants, produce flowers and good seeds, and these seeds often perpetuate the very characters which have originated in the asexual generations, as we have seen in the case of many bud-varieties; and it should also be remarked that these phytons usually transmit almost perfectly the characters acquired by the plant from which they sprung. Or, to put the whole matter in a convenient phrase, there may be, and is, a progressive evolution of plants without the aid of sex.

Now, where is Weismann's germ-plasm? One of the properties of this material—if an assumption can receive such designation—is its localization in the reproductive organs or parts. But the phyton has no reproductive parts; or, if it has them, they are developed after the phyton has lived a perfectly sexless life, and possibly after generations of such life, in which it and its progeny may either have remained comparatively stable or may have varied widely, as the circumstances may have determined. If any flower, therefore, contains germ-plasm it must have derived it out of the asexual or vegetative or soma-plasm. And I will ask where the germ-plasm is in ferns. These plants are fertilized in the prothallic stage, and one brief sexual state is all that the plant enjoys, after which the sex-organs die and wholly disappear. The fern, as the layman knows the plant, is wholly asexual, and the spores are as sexless as buds; yet these spores germinate and give rise to another brief prothallic or sexual stage, and if there is any germ-plasm at all in these fleeting sexual organs it must have come from the sexless spores. It is interesting to note, in this connection, this bud-variation is as frequent in ferns as in other plants. Or, if the Weismannians can locate the germ-plasm in all these instances, pray tell us where it is in the myriads of sexless fungi! There is no such thing as continuous localization of germ-plasm in plants. Weismann himself admits that the germ-plasm must be distributed in 'minute fraction' in all 'somatic nuclei' of the begonia leaf, because that leaf is capable of giving rise to new plants, by means of cuttings, and all the plants may produce good flowers, which, if they are sexual at all, are so only by virtue of containing some of this elusive germ-plasm. There is no other way for these plants to get their germ-plasm, except from the somatic leaf from which they came. It would seem that this admission undermines the whole theory of the localization of the germ-plasm in plants, for one exception in the hypothesis must argue that there are others. But not so! There are no insurmountable difficulties before the Weismannians. It is the begonia which is the exception, for it is abnormal for plants to propagate by any such means! The answer which has been made to this statement is that very many plants are propagated asexually by horticulturists, and that all plants can probably be so propagated if there were any occasion for the effort. This answer is true; but the philosophical answer is that every phyton is an autonomy, and that the mere accident of its growing on the plant, in the soil, or in a bottle of water, is wholly aside from the point, for wherever it grows it lives at first a sexless life, it has an individuality, competes with its fellows, varies to suit its needs, and is capable, finally, of developing sex.

Another fundamental tenet of Weismannism is the continuity of the germ-plasm, the passing down from generation to generation of a part or direct offspring of the original germ-plasm. Now, if there is any continuity in plants, this ancestral germ-plasm must be inextricably diffused in the soma-plasm, as I have said, for every part or phyton of these plants, even to the roots and parts of the leaves, is able to produce sexual parts or germ-plasm. And if this germ-plasm is inextinguishably associated with every cell of the plant body, why does it not receive and transmit all incident impressions upon the plant? Why should acquired characters impress themselves upon the soma-plasm and not upon the germ-plasm when this latter element is contained in the very nuclei, as Weismann admits, of somatic cells? If the theory of the continuity of the germ-plasm is true for plants, then acquired characters *must* be transmitted! The only escape from this position is an arbitrary assumption that one plasm is impressionable and that the other is not; and, now, that we can no longer relegate the germ-plasm to imaginary deep-seated germ-cells, such an assumption is too bold, I think, to be suggested.

The entire Weismannian hypothesis is built upon the assumption that all permanent or progressive variation is the result of sexual union; but I have shown that there is much progressive variation in the vegetable kingdom which is purely asexual, and, for all we know, this type of modification may proceed indefinitely. There is no doubt of the facts; and the only answer to them which I can conceive the Weismannian to make is that these progressive variations arise because of the latent influence of ancestral sexual unions. In reply to this I should ask for proofs. Hosts of fungi have no sex. I am not convinced but that there may be strains or types of some species of filamentous algæ and other plants in which there has never been sexual union, even from the beginning. And I should bring, in rebuttal, also, the result of direct observation and experiment to show that given hereditable asexual variations are often the direct result of climate, soil or other impinging conditions. As a matter of fact, we know that acquired characters may be hereditary in plants; if the facts do not agree with the hypothesis, so much the worse for the hypothesis. Unfortunately, the hypothesis is too apt to be capable of endless contractions and modifications to meet individual cases. I sometimes think that we are substituting for the philosophy of observation a philosophy of definitions.

I have, therefore, attempted to show :

1. That the plant is not a simple autonomy in the sense in which the animal is.

2. That its parts are virtually independent in respect to (a) propagation (equally either when detached or still persisting upon the parent plant), (b) struggle for existence amongst themselves, (c) variation, (d) transmission of their characters, either by means of seeds or buds.

3. That there is no essential difference between bud-varieties and seed-varieties, apart from the mere fact of their unlike derivation; and the causes of variation in the one case are the same as those in the other.

4. That all these parts are at first sexless, but finally may or may not develop sex.

5. That much of the evolution of the vegetable kingdom is accomplished by wholly sexless means.

There is, then, a fundamental unlikeness in the ultimate evolution of animals and plants. A plant, as we ordinarily know it, is a colony of potential individuals, each one of which, save the very first, is derived from an asexual parent, yet each one may, and usually does, develop sex. Each individual is capable also of receiving a distinct or peculiar influence of the environment and struggle for existence, and is capable, therefore, of independent permanent modification. It is not possible, therefore, that there is any localization or continuity of a germ-plasm in the sense in which these conceptions are applied to animals; nor is it possible for the plant as a whole to make a simple functional adaptation to environment. If there is a continuity of germ-plasm in plants this element must of necessity be intimately associated with every particle of the plant body, even to its very periphery, and it must directly receive external impressions; and this concept of Weismann—the continuity of the germ-plasm—becomes one of the readiest means of explaining the transmission of acquired characters. All these conclusions prove the unwisdom of endeavoring to account for the evolution of all the forms of life upon any single hypothesis; and they illustrate with great emphasis the complexity of even the fundamental forces in the progression of organic nature.

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CURRENT NOTES ON PHYSIOGRAPHY (III.).

WOODWARD'S SMITHSONIAN GEOGRAPHICAL TABLES.

'The average geographer,' to whose needs Professor Woodward has attempted to suit the recent volume of Geographical Tables issued by the Smithsonian Institution, should certainly feel highly complimented by this tribute to his quality. The volume contains, among many other matters, tables of coördinates for the projection of polyconic maps, lengths of a degree on parallels and meridians at different latitudes, areas of latitude-and-longitude, quadrilaterals of different dimensions and at different latitudes, adopted dimensions of the earth's spheroid, value of gravity at the earth's surface, and salient facts of physical geodesy. The latter heading includes the area of the earth, of oceans and continents, and the average heights of continents and depths of oceans, taken from Helmert's Geodäsie. For areas the continents are given 51,886,000, and the oceans 145,054,000 square miles. The mean depth of the oceans is placed at 3,440 meters. The mean heights of the continents are given as follows: The earlier results of Humboldt's, still often quoted, and the later ones of Penck (Morphologie der Erdoberfläche, 1894) being added for comparison.

		Humboldt.	Helmert.	Penck.
Europe,		205	300	330 m.
Asia,		351	500	1010
Africa,		—	500	660
Australia,		—	250	310
America,	North,	228 } 285	410	650 } 650
	South,	346		650
All Continents,		308	440	735

The increase in the values of the latter measures is probably an approach to the truth, for early explorations frequently gave too much emphasis to narrow mountain ranges, and too little to broad plateaus.

A. AGASSIZ ON THE BAHAMAS.

A RECONNOISSANCE of the Bahamas and of the elevated reefs of Cuba, made by A. Agassiz in the winter of 1893, affords material for a Bulletin of 200 pages with 47 plates and many figures in the text, lately issued by the Museum of Comparative Zoology at Harvard College. The author is emphatic in rejecting the sufficiency of the Darwin-Dana theory of submergence in explaining the features of great limestone banks. The Bahamas consist of low hills of æolian limestone, "formed during a period of rest, during which the great beach of the then existing reef constantly supplied fresh material to be changed by the surf and the winds into sand for the heaping up of sand dunes. They could not be formed in a district of subsidence unless the subsidence was slower than the rate of growth of the corals, which is not the case in the Bahamas, as the reefs of to-day, even when they come to the surface, are not the sources from which the material for the great dunes of the Bahama Islands is derived" (p. 184, 185). At present the dunes are disappearing before the action of the sea. The conclusion of the reconnoissance seems to be that the great limestone banks are chiefly formed as 'marine limestones,' accumulating 'at great depths by accretion;' and that in the West Indies "wherever coral reefs occur, and of whatever shape, they form only a comparatively thin growth upon the underlying base" (p. 177). The text, with its figures, supplemented by maps and plates, gives an excellent idea of the geographical features of the region and of their evolution.

SPENCER'S RECONSTRUCTION OF THE ANTILLEAN CONTINENT.

PROF. MARCEL BERTRAND, of the École des Mines and the Geological Survey of France, has published an account of certain faint deformations of northwestern France, in which he interprets the inequalities in the floor of the English channel as the result of faint anticlinal and synclinal movements (Bull. Soc. Géol. France, xx., 1892, 118); thus implying that neither erosion nor deposition has been of significant measure in shaping the channel floor. Prof. J. W. Spencer takes almost the other extreme, and interprets certain inequalities of the ocean floor of the Antillean region, even to depths of twelve or fifteen thousand feet, as the results of river erosion during a not remote time when the entire region is supposed to have had a much greater altitude than at present (Bull. Geol. Soc. Amer., vi., 1895, 103–140); thus implying that no other processes than river erosion can account for the inequalities that he has traced. It must be concluded from these contrasted arguments that the forms of the sea floor are not yet so well understood as those of the land; because the facts are much less accurately known under than over sea level, because only form and not structure can be determined by soundings, and because the forms of the sea floor have received relatively little study. Where two specialists reach conclusions so unlike, it is difficult for others to choose between them; and for the present there will probably be some hesitation in adopting the teachings of the one or the other. With much interest aroused in the facts brought forward, and with all willingness to look on the continents as unstable, it is difficult to believe that they have suffered changes so great as Spencer announces, not only in the uplift of the Antillean region, but in the deep depression of the axis of Central America, and in the denudation of the (inferred) great banks or continental shelf along the Windward Islands. The strongest proof will be demanded before vertical movements of two miles and a half can be accepted; and we fear that most readers will take refuge in a verdict of 'not proved.'

HISTORY OF THE ST. JOHN RIVER, NEW BRUNSWICK.

AN article on the 'Outlets of the St. John river,' by G. F. Matthew (Bull. Nat. Hist. Soc., New Brunswick, xii., 1894, 43–62), concludes that this river has been built up by contributions from three other systems, whose lower parts are now to be seen in the Restigouche, Miramichi and Petitcodiac. The evidence of this conclusion is derived from the geological structure of the country, beginning as far back as the Huronian time; the three rivers whose upper basins now belong to the St. John having been defined as basins of deposition in more or less remote geological periods. Thus the St. John river has attained its present magnitude by the breaking of mountain or hill barriers which once separated its three river systems, and is not a simple valley of continuous growth like the Mississippi (p. 55). The difficulty of accepting Dr. Matthews' conclusion as the only solution of the history of the St. John does not lie in any objection to the geological history of the region and its several basins of deposition, as far as stated, but in the omission of sufficient consideration of what has happened in the region since it became a land area. It has long been subject to subaërial erosion. During this time it has in all probability been variously warped and otherwise moved with respect to its base-level. Its rocks are of diverse resistance, and hence there may have been repeated opportunities for diversion and rearrangement of river courses during the long life of the region as a land area. While admitting that several geological basins of great antiquity are now drained by a single river, it does not necessarily follow that this river is an immediate descendant of the rivers which at one time or another drained the separate basins. The actual St. John river may once have been larger than now; its neighbors may have gained drainage area from it instead of losing drainage area to it; but these possibilities are not considered.

THE ORIGIN OF THE MISSISSIPPI.

THE reference to the Mississippi in the previous paragraph brings up an oft-encountered implication of simple history in the development of this great river, against which there is much evidence. A similar implication is found in a recent State Survey Report, where it is stated that, as a result of continental evolution at the close of the Carboniferous period, the drainage of the Ohio region was turned southward "into the great Mississippian bay, which then washed the shores of the new-born continent as far north as the mouth of the Ohio river" (Geol. Coastal plain of Alabama, 1894, 11). It is found again in the 'Story of the Mississippi-Missouri,' where the Mississippi at the close of the Appalachian revolution is described as heading somewhere in the Minnesota-Wisconsin region, and flowing southward to its mouth somewhere near the present city of St. Louis, whence a deep gulf extended southward to the present Gulf of Mexico (Amer. Geol. iii., 1889, 368). While the southward-flowing streams of the Wisconsin-Minnesota highlands are probably of ancient origin, the southward course of the Mississippi between Tennessee and Arkansas seems to have been initiated not at the close of the Appalachian revolution, but long afterwards in Cretaceous time. The Appalachian revolution formed the mountains of Arkansas, as well as those of the Alleghany belt. The similarity of structure is so great that a trans-Mississippian extension of Appalachian growth may be reasonably assumed, as has been pointed out by Winslow (Bull. G. S. A., ii., 1891, 231). The existence of a bay, from the Gulf of Mexico northward towards St. Louis, is very improbable as a result of the Appalachian revolution; an east and west constructional mountain belt is a more likely product; and not until this mountain belt was well denuded to a peneplain did a later deformation depress it transversely, admitting the Cretaceous waters northward across it, and thus first forming the Mississippi embayment. Probably in part at the same time, and to a greater extent in later time, the denuded peneplains to the east and west were raised towards their present upland altitude, and as a result of this elevation the existing valleys and lowlands were opened in them during some part of Tertiary time. With these later elevations we may associate the uplift of the filled embayment and the southward growth of the Mississippi as a river. This view of the origin of the Mississippi embayment and of the date of the southward discharge of Mississippi drainage was first published by L. G. Westgate (Amer. Geol. xi., 1893, 251), as a result of conference with L. S. Griswold, who had then recently completed his investigation of the novaculite region of Arkansas.

THE CHUNNENUGGA RIDGE AND THE BLACK PRAIRIES OF ALABAMA.

IT is, perhaps, too much to expect that the origin of the physiographic features of a region should always receive due attention in a geological report along with the origin of its strata; yet there is no other place so appropriate for the official publication of physiographical discussions. It therefore occasions regret to find so little account of the origin and meaning of the Chunnenugga ridge and the Black prairies of Alabama in the elaborate report on the Geology of the Coastal Plain lately published by the Survey of that State. "The Chunnenugga ridge is made in great part by alterations of hard limestone ledges and bands of indurated sands of the Ripley. . . . . It overlooks the low trough of the black prairies of the Rotten limestone towards the north with somewhat precipitous slopes in that direction, while its descent towards the south is much more gentle" (p. 356). It is manifest that the ridge with its inland-facing escarpment and the denuded inner lowland are typical features of a certain stage in the denudation of a coastal plain that consists of more and less resistant strata; the drainage of the lowland being chiefly gathered by subsequent streams that have been developed along the strike of the beds, and discharged by consequent streams which maintain transverse valleys through the enclosing ridge or upland. This general relation of form and drainage is so often repeated on coastal plains that its occurrence in Alabama deserves mention as a local example of a general physiographic feature; just as the Cretaceous strata on which it is developed deserve mention as local examples of a widespread geological formation.

W. M. DAVIS.
HARVARD UNIVERSITY.

THE NEW YORK MEETING OF THE ASSOCIATION OF AMERICAN ANATOMISTS.

THE Seventh Annual Session of the American Anatomists was held in the Medical Department of Columbia College, 437 West 59th Street, New York City, December 28 and 29, 1894.

The Association was called to order Friday, December 28th, by the President, Dr.

Thomas Dwight, in a few introductory remarks.

The report of the Secretary and Treasurer was read and accepted.

The Executive Committee recommended for election to membership the following names, and, on motion, the gentleman were elected :

1. Dr. F. J. Brockway, Assistant Demonstrator of Anatomy, Columbia College, New York City.

2. Dr. W. A. Brooks, Jr., Assistant in Anatomy, Harvard Medical School.

3. Dr. Franklin Dexter, Demonstrator of Anatomy, Harvard Medical School.

4. Dr. B. B. Gallaudet, Demonstrator of Anatomy, Medical Department of Columbia College, New York City.

5. Dr. R. H. Gregory, Jr., Demonstrator of Anatomy, St. Louis Medical College.

6. Dr. C. J. Herrick, Acting Professor of Biology, Denison University, Granville, Ohio.

7. Dr. P. C. Hunt, Assistant Demonstrator of Anatomy, Columbian Medical College, Washington, D. C.

8. Dr. Woods Hutchinson, Professor of Anatomy, Medical Department, University of Iowa.

9. Dr. W. P. Mathews, Demonstrator of Anatomy, Medical College of Virginia, Richmond.

10. Dr. Eugene A. Smith, Professor of Anatomy, Niagara University, Buffalo, N. Y.

11. Dr. P. Y. Tupper, Professor of Anatomy, St. Louis Medical College.

The Executive Committee, while not recommending affiliation with the Society of Naturalists, suggested that, as a rule, the Association should meet at the same time and place. This suggestion was discussed by Drs. Wilder, Spitzka, Dwight and Lamb, and was then adopted.

Dr. Wilder, from the Committee on Anatomical Nomenclature, reported progress. He also stated that Professor Stowell had resigned from the Committee.

The report of the Committee on Anatomical Material was called for. In the absence of the Chairman, Dr. Mears, Dr. Dwight reported progress.

The Committee on the Anatomical Peculiarities of the Negro also reported progress.

Dr. Huntington was elected to the vacancy on the Executive Committee, caused by the retirement of Dr. Spitzka.

The following papers were then read :

1. 'The best arrangement of topics in a two years' course of Anatomy in a medical school.' Dr. Gerrish. Discussed by Drs. Huntington, Baker, Wilder, Bevan, Allen, Shepherd, Lamb and Dwight.

2. 'History of the Development of Dentine.' Dr. Heitzmann.

3. 'On the Value of the Nasal and Orbital Indices in Anthropology.' Dr. Allen. Discussed by Drs. Wilder, Huntington and Dwight.

4. 'Loose characterizations of vertebrate groups in standard works.' Dr. Wilder. Discussed by Drs. Baker, Dwight and Allen.

5. 'The comparative anatomy of the cerebral circulation, with an exhibition of a series of anomalies of the circle of Willis.' Dr. Leidy. Read by title in the absence of the author.

6. 'Convolutions of the hemispheres of Elephas Indicus.' Dr. Huntington. Discussed by Drs. Wilder and Baker.

An inspection of the Medical Department of Columbia College was made in the evening, under the conduct of Dr. Huntington.

On Saturday, the 29th, the President appointed Dr. Gerrish to fill the vacancy upon the Committee on Anatomical Nomenclature, caused by the resignation of Professor Stowell.

The reading of papers was resumed:

7th paper. 'Classification of the tissues of the animal body.' Dr. Baker. Discussed by Drs. Heitzmann, Wilder, Dwight and Lamb.

8. 'Anomalies—Their significance.' Dr. Dwight.

9. 'Some muscular variations of the shoulder girdle and upper extremity, with especial reference to reversions in this region.' Dr. Huntington.

10. 'Some anomalies of the brain.' Dr. Wilder.

11. 'The correlation between specific diversity and individual variability, as illustrated by the eye muscle nerves of the Amphibia.' Professor C. Judson Herrick.

The discussion on papers 8 to 11, inclusive, was then opened by Dr. Baker, and continued by Dr. Shepherd (who illustrated his remarks with specimens), Dr. Wilder, Dr. Lamb (who also showed a specimen), Dr. Huntington, and concluded by Dr. Dwight.

Dr. Wilder exhibited a brainless frog and made remarks thereon.

On motion, the thanks of the Association were tendered to the College, and particularly to Dr. Huntington, for their hospitality.

The following members were present at some time during the session: Allen, Baker, Bevan, Bosher, Dwight, Ferris, Gerrish, Hamann, Heitzmann, C. J. Herrick, Huntington, Lamb, Moody, Shepherd, Spitzka, Weisse, Wilder. Total, 17.

CORRESPONDENCE.

A CARD CATALOGUE OF SCIENTIFIC LITERATURE.

EDITOR OF SCIENCE—*Dear Sir:* Your invitation to open in the columns of SCIENCE a discussion of the projected Catalogue of Scientific Literature to be prepared by international coöperation, the claims of which were presented in your issue of February 15, affords me a welcome opportunity to fall publicly into line with a great movement that I believe destined to prove of the highest importance to scholarship. As a few of your readers are aware, I printed privately, last summer, a brief circular advocating a similar enterprise. At the time of doing so I was at an out-of-the-way spot in the country, where it was impossible to exchange inspirations, except by post, with friends whose interest in the scheme might have been counted upon; but upon canvassing the subject in my own mind I became so convinced that the learned world was in sore straits in this matter, and that the way out was clear, that I felt sure I should presently discover that other restive spirits were beginning to agitate in the same direction. Little did I expect, however, to meet with so conspicuous and agreeable a confirmation of my premonition as came to me several weeks after the issuance of my circular (though dated before it), in the printed report of the Harvard committee, which has now appeared in SCIENCE. (The original communication of the Royal Society I have seen for the first time, through your editorial courtesy, in the proof sheets of SCIENCE.)

Although several of the suggestions contained in my own little circular were promptly outgrown by me, it may appear not inappropriate, on the principle of comparing small things with great, to reproduce here the contents of this highly aspiring but wholly unpretentious little document:

UNIFORM CARD MEMORANDUM INDEX.

The accompanying slip (size $2\frac{1}{4}$x$3\frac{1}{2}$ inches, 5.7 x 8.9 centimetres), designed to be cut out and filed alphabetically in the manner of a card catalogue, is printed as a tentative specimen of a projected **Uniform Card Memorandum Index**, and is herewith privately submitted to representatives of a few of the leading universities, learned societies and publication agencies, with a view to securing influential approval of the general plan, together with useful suggestions and criticisms as to its practical application. It is proposed that all the universities, learned societies and high-class periodicals of the world should coöperate (from January, 1895) in the production of such a uniform *memorandum index*, by publishing, as a supplement (or appendix, *or both*) to every number of their original publications, a brief slip-digest of the contents of each article—or even of important portions of each article, as may appear to be warranted. These supplements could be easily prepared (the digests being furnished in all or in most cases by the authors themselves), would be inexpensive both in their original form of publication and as separate slips, and would incalculably facilitate both the distribution and the classification for instant reference of all the newest results of discovery and research. Those interested in such a project are earnestly requested to communicate on the subject, before September 15, with the undersigned.

The specimen slip read as follows:

KINETO-PHONOGRAPH. PHONO-KINETOGRAPH. PHONO-KINETOSCOPE.
Edison, Thomas A., Invention of the Kineto-phonograph.
Century Magazine, June, '94, p. 206.
"In the year 1887 the idea occurred to me that it was possible to devise an instrument which should do for the eye what the phonograph does for the ear, and that, by a combination of the two, all motion and sound could be recorded and reproduced simultaneously. This idea, the germ of which came from the little toy called the Zoetrope, and the work of Muybridge, Marié and others, has now been accomplished, so that every change of facial expression can be recorded and reproduced life size. The Kinetoscope is only a small model illustrating the present stage of progress, but with each succeeding month new possibilities are brought into view, etc., etc."

The above circular, though sent to but comparatively few persons, called forth a gratifying number of 'adherences' and of valuable suggestions. In particular, the president of one of the American universities famous for activity in research and in the promulgation of knowledge undertook to have furnished, with the official *imprimatur*, summaries of the contents of all the publications of his university.

The necessity of entrusting the organization of the enterprise to a great central bureau that would command universal confidence early became manifest, and an informal communication on the subject was addressed to one of the officers of the Smithsonian Institution at Washington, who wrote in response: "I heartily favor the idea. When you have the matter in shape to make a formal proposition I shall have much pleasure in recommending it to the Secretary."

Meanwhile, from correspondence and conference with numerous scholars, various points involved in the success of the enterprise have grown in distinctness. The problem of utilizing more effectively the ever-increasing mass of accumulated, scattered and current contributions to knowledge can no longer be shirked. The time is ripe for instituting widely concerted action for recovering mastery of the situation. The various efforts hitherto directed to this end have done great service; but they have been devised almost exclusively to meet the requirements of reference and circulating libraries in their relations to broad classes of readers, rather than to serve the immediate needs of the individual scholar engaged upon a learned specialty.

All productive scholars, it would seem, must have devised or adopted for their personal use some form of *index rerum*, some mode—systematic or unsystematic—of note making. It is safe to say that very many such scholars have adopted for this purpose the general idea of the alphabetical card index, the merits of which are at present almost universally recognized. The scholar of Anglo-Saxon race is fast becoming as wedded to, and as dependent upon, his reference slips as the German scholar has long been silently devoted to his *Zettel* or the French savant to his *fiches*. It now remains for the Anglo-Saxon, with his openness to new applications of old ideas and the proverbial genius of his race for practical devices, to bring the power of the printing-press, as well as of scholarly coöperation, to bear upon the problem of multiplying indefinitely the benefits of the private card index.

Just here I should like to emphasize a consideration that is unexpressed, though latent, in the masterly report of the Harvard committee. This is, that such a card catalogue as is there projected, if based upon a wise choice in the *size of card* adopted, would render it possible for every member of the rapidly recruiting army of those employing the card system for private notes to incorporate his own manuscript or type-written cards and the printed cards (pertaining to his own specialty) of the coöperative index into one homogeneous whole, ever-growing, ever abreast of the latest research. This consideration it was, with all the possibilities and problems of administration it opens up, that held the mind of the writer under a spell of fascination for almost a week of vacation leisure. For be it noted that the blessings of the proposed coöperative card index are to flow directly into the lap of the individual scholar, seated at his own desk in his private sanctum, enabling him to discard (not inappropriate word) to the limbo of the great libraries everything that does not directly concern him, while filing within reach of his finger-tips absolutely everything (pardon the optimism of an enthusiast) that he may intimately desire.

How can so Utopian a consummation be most speedily attained?

Let universities and colleges, and all manner of learned institutions and societies, at once appoint committees similar to the Harvard committee (though of course not limited to the natural and physical sciences, since the project of the Royal Society will form only a portion of the great undertaking), to accomplish three preliminary objects:

1. To arouse an intelligent and earnest interest in the subject.

2. To induce the Smithsonian Institution to assume the American leadership of the movement.

3. To convince publishers—primarily the publishers to the respective institutions concerned—of the importance of printing, on slips of the standard size, No. 33, of the American Library Bureau (7½x12½ cm., 3x5 in. approximately), summaries of their current publications for distribution as publishers' announcements. This size of slip is already widely in use, both publicly and privately, and may well prove to be of the dimensions ultimately adopted by the authorities of the projected international index. A beginning of these publishers' announcements has already been made by Messrs. D. C. Heath & Co., at the personal request of the present writer, and has been favorably submitted to the attention of the Secretaries of the Royal Society by Professor Bowditch, chairman of the Harvard committee. Other leading American publishers have heartily favored the idea of these card announcements and have promised to introduce them into use.

Columbia College has within a few days appointed, through its University Council, a committee to further the interests of the proposed International Coöperative Catalogue of Scientific Literature.

Yours very truly,

HENRY ALFRED TODD.

COLUMBIA COLLEGE, March 2, 1895.

PITHECANTHROPUS ERECTUS.

EDITOR OF SCIENCE—*Sir*:

In my letter of February 14th occur two expressions which need amendment. For the phrase 'divergent roots,' p. 240, 1st col., first line, read 'divergent root stems;' and for the phrase 'is wider than long,' p. 240, 2d col., fifth line, read 'is much wider than long.' Yours truly,

HARRISON ALLEN.

PHILADELPHIA, March 4th, 1895.

SCIENTIFIC LITERATURE.

Electrical Engineering, for Electric Light Artisans and Students. By W. SLINGO and A. BROOKER. New and revised edition, London, 1895. Longmans. Price, $3.50.

The object of this work is to cover general electrical engineering, and, taken as a whole, it is probably the most successful attempt yet made in this direction. The demand for a satisfactory general treatment of the applications of electricity is a very large and important one, and anything which supplies this demand is more than welcome. It is very doubtful whether any single work is ever likely to be published which will completely set forth the numerous and rapidly developing branches of electrical science and industry. Nothing short of an encyclopædia of many volumes could be expected to accomplish this result. A general discussion of the most important principles and uses of electricity, particularly if it is not attempted to cover all branches, is a far more practicable problem, as the success of this volume demonstrates.

A work of this kind, however, is somewhat limited in its scope, since it is not intelligible to the ordinary untechnical reader, and is not of much use to the professional electrical engineer, who requires a more thorough and detailed study of each subject than is possible in a general treatise. This work would therefore be suited to one who had a certain amount of technical knowledge but who was not a specialist in electricity, for example, a mining or mechanical engineer, or a young man who had received a certain amount of electrical education at a technical or trade school and who wanted to learn more by his own efforts. It would also be useful as a text-book wherever a general course in electrical engineering is given. But in the opinion of the reviewer, a general treatment running from one subject to another is not the best way to educate electrical engineers of the highest type. This requires a careful and special study of each branch, aided by lectures and laboratory work, and the text-books should be entirely devoted to one subject, or, in fact, several books, each devoted to a small part of any one branch, is often preferable.

The authors of this book have had considerable experience as teachers and also the advantage of correcting and extending the contents of the first edition, which appeared in 1890, with the result that the new edition is well arranged and expressed and in most cases is brought reasonably well up to date. The first six chapters are devoted to general principles, units and methods of measurement. The next six chapters contain a treatment of dynamos and motors which is very satisfactory, considering the limitation of space. Transformers, secondary batteries, arc and incandescent lamps, are also well explained; but the last chapter, on 'Installation equipment, fittings, etc.,' is very meagre and the least satisfactory portion of the book. In fact, the principal criticisms would be that each element or device is explained as a separate thing, and no methods for combining these into systems are given. Nevertheless, it is a fact that the general design and arrangement of electrical apparatus is fully as important as the merits of each particular element. For example, the laying-out of a central station, or even a small isolated plant, determines its success or failure fully as much as the quality of the individual dynamos, lamps, or other particular parts of the plant.

The various systems for transmitting and distributing electric power, which is probably the most important branch of electrical engineering, are barely touched upon. In short, we may say that electrical engineering in its broadest sense is not covered, and probably was not intended to be covered, by this work. The subjects of electro-chemistry and electro-metallurgy, which now appear to be on the eve of important development, are not discussed. Telegraph and telephone apparatus and methods are not even mentioned.

These omissions, which are doubtless intentional and probably necessary, indicate that a complete treatise on electricity and its applications is almost an impossibility.

A few mistakes are noted; for example, on page 17, the International Ohm, adopted at the Chicago Electric Congress of 1893, is defined in terms of a column of mercury 106.3 centimetres in length and one square millimetre in cross section, whereas, the statement actually adopted was 'a column of mercury at the temperature of melting ice, 14.4521 grammes in mass, of a constant cross-sectional area and of the length of 106.3 centimetres.' This was intended to be exactly equivalent to a cross-section of one square mm., but it was put in this form because mass is more easily and accurately determinable than cross-section. Another somewhat serious mistake, since it is fundamental, is the statement on page 18, that specific resistance is 'the resistance of any particular substance as compared with the resistance of a piece of some other conductor, such as silver, both being of unit dimensions.' As a matter of fact, specific resistance, which is a very important term, is the resistance in ohms of a unit volume, and is entirely independent of any particular standard substance. The use of the term 'magnetic resistance,' on pages 219 to 221, is open to objection, since the term 'reluctance' is now almost universally employed to distinguish this quantity from electrical resistance.

Taken as a whole, however, the errors are not numerous, and the work is recommended as a text or reference book for those who desire to learn the principles, general construction and action of the various kinds of electrical machinery and instruments, with the exceptions already noted.
F. B. CROCKER.

PHYSIOLOGICAL PHYSICS.

On the Spontaneous Heating and Ignition of Hay. BERTHELOT. Ann. Chim. Phys., 7, 2. p. 430. 1894.

The author finds that poorly dried hay may ignite when the rise in temperature is only to 140° C. (280° Fh.). The evolution of heat necessary for this rise of temperature is due to the absorption of oxygen in spite of the interrupted sprouting, which will only take place when the hay is quite wet. The chemical process involving this absorption of oxygen may continue until the hay is thoroughly dry.

Druck und Arbeitsleistung durch Wachsende Pflanzen. W. PFEFFER. Abh. d. Math.-Phys. Kl. der K. Sachsicher Gesellschaft der Wiss., 20. p. 235. 1893.

Mr. Pfeffer investigated very carefully and ingeniously the pressure exerted by parts of plants in growth, and found, for example, that a root point could exert a pressure of 10–15 atmospheres. He ascribes these forces to osmotic pressure, and criticises the view concerning the growth of the cell-wall, which ascribes it to simple plastic expansion.

La Lumière Physiologique. R. DUBOIS Rev. gén. des Sciences, 5. p. 415 and p. 529. 1894.

Part first contains a review of light emitting organisms, and a description of the organs involved. In part second the author treats the subject of the emission more thoroughly, describing the character of the light radiated, and finds that the brightest Pyrophorus radiates 1, 4×10^{-7} calorie in ten minutes.

The author summarizes his extensive investigations as follows:

Neither a perfect organ nor a perfect cell is necessary for the coming and going of the light. The cell produces the photogenic substance which, once formed, may light or not, according to the conditions surrounding it.

They must fulfill the conditions necessary for life, must contain oxygen and water, and have a suitable temperature.

The light (luminous energy) is found to be 90% of the total energy radiated.

Dubois made a fluorescent substance from the blood of Pyrophorus, which, like that from the animal itself, lost its peculiar property on being treated with weak acetic acid and regained it on treatment with ammonia.

All the causes which excite or destroy the activity of the protoplasm have a similar effect upon the production of the physiological light.

The production of light depends upon the change of living protoplasmic granulations into the condition of lifeless crystalline matter.

It is to be remembered that the secretions of *Orya barbarica* are acid, thus in this case excluding the explanation of Radziszewski.

WILLIAM HALLOCK.

MATHEMATICS.

*The Principles of Differentiation in Space-Analysis.** By A. MACFARLANE, D. Sc., LL. D.

According to Hamilton the differentiation of a function of a quaternion presents novel difficulties due to the non-commutative character of a product of quaternions. There is in general no derived function, and it is necessary to define the differential in a new manner. Under certain conditions there is an analogue to Taylor's Theorem, but it is very complex, and no use is made of it. Hamilton does not differentiate the general transcendental functions, but only these functions restricted to a constant plane.

The author shows that these anomalies are true of products of vectors, but not of functions of versors. In versor analysis there is a derived function, satisfying a generalized form of Lagrange's definition; and Taylor's Theorem takes on a form similar to that in ordinary analysis, only the order of the two quantities must be preserved. Let x and h denote two versors, then

$$f(x+h)=f(x)+f'(x)h+\tfrac{1}{2}f''(x)h^2+, \text{etc.},$$

provided the order of the x and h be preserved throughout.

The author finds the derived functions of various transcendental functions in space. He also shows that there are two essentially different meanings of $\sqrt{-1}$; one, when made definite, means a quadrant of rotation round a specified axis; while the other has no reference to direction, but distinguishes the area of a hyperbolic angle from the area of a circular angle. He also remarks that the theory of functions must be imperfect, because it is based upon a complex number which is restricted to one plane; no account is taken of the two essentially different meanings of $\sqrt{-1}$, and the idea of the versor is not distinguished from that of the vector.

* A paper read before the meeting of the American Mathematical Society, January 26, 1895. (Abstract.)

METEOROLOGY.

Neudrucke von Schriften und Karten ueber Meteorologie und Erdmagnetismus.

Dr. G. Hellmann, of Berlin, has undertaken the republication of certain old and rare writings relating to meteorology and terrestrial magnetism which have an important bearing on the history and development of these sciences. Very rare or typographically interesting works are printed in facsimile. Each reprint is preceded by an introduction, containing a general description of the book and its author. Although facsimile publications generally are so dear that only connoisseurs are able to buy them, yet, owing to the aid of the German Meteorological Society and its Berlin branch, the reprints are offered at a relatively low price by A. Asher & Co., Berlin. A few copies may also be had of A. L. Rotch, Blue Hill Observatory, Readville, Mass., at the publishers' prices. Each year one or two of the reprints will be issued, but the whole number will not exceed twelve. The following have already appeared:

No. 1. *Wetterbuechlein von wahrer Erkenntniss des Wetters.* REYNMAN, 1510. 41 pages introduction and 14 pages facsimile. Price 6 M. = $1.50.

This is the oldest printed meteorological work in the German language and was very popular, having 34 editions in seventeen years. Nevertheless, it is now so scarce that hardly thirty-six copies can be found.

No. 2. *Récit de la Grande Expérience de l'Equilibre des Liqueurs.* BLAISE PASCAL. Paris. 1648. 10 pages introduction and 20 pages facsimile. Price 3 M. = 75 cents. This little work is of the greatest importance for the history of physics, meteorology and physical geography, since it furnishes proof of the existence of atmospheric pressure, and forms the basis of measurements of altitudes with the barometer. But three copies of the original are known to exist.

No. 3. *On the Modification of Clouds.* LUKE HOWARD. London. 1803. 9 pages introduction and 32 pages facsimile with three plates. Price 3 M. = 75 cents.

This was the first successful attempt at a cloud nomenclature on which all later schemes are based. The first edition of the original work is very rare.

A. L. ROTCH.

NOTES AND NEWS.

ENTOMOLOGY.

IT IS well to draw attention to two admirable brief illustrated papers published last year by Ch. Janet on *Myrmica rubra*, one on the morphology of the skeleton and especially of the postthoracic segments (Mém. Soc. Acad. de l' Oise, xv.), the other on the anatomy of the petiole (Mém. Soc. Zool. France, 1894). We regret we have not space for a full analysis of each, but they will be found of great interest to morphologists and hymenopterists. The clear illustrations are pretty sure to find their way into text-books.

The annual presidential address before the Entomological Society of London by Capt. H. J. Elwes is on the geographical distribution of butterflies and deals largely with those of North America.

Dr. Ph. Bertkau announces that his health obliges him to give up the admirable annual review of entomology which has appeared in the *Archiv für naturgeschichte* since 1838 under different editors — Erichson, Schaum, Gerstaecker, Brauer and Bertkau. Entomologists are under great obligations to Dr. Bertkau for the excellence of his summaries, their completeness and the promptness with which they have appeared. A still prompter method of rapid publication in all branches of biology is now being planned, which is at the same time a practical combination of all the current reviews —a consummation devoutly to be wished and helped forward.

M. Emile Blanchard was retired November last from the chair of entomology at the Jardin des Plantes, on account of age; his first entomological paper was published nearly seventy years ago; his successor has not yet been announced.

Fire has committed ravages with our entomologists this winter. Mr. J. G. Jack lost his library and collection in Jamaica Plain by the destruction of the building in which they were kept; Prof. C. H. Tyler Townsend lost his valuable dipterological library (nearly complete for America and very full for Europe) by the burning of the warehouse at Las Cruces, N. Mex., while he was absent for a few weeks at Washington; and now comes news that Rev. C. J. S. Bethune's school at Port Hope, Ont., has been burnt to the ground. His loss is estimated at eighty thousand dollars.

GENERAL.

AMONG the articles of scientific interest in the popular magazines for March are the following: Hermann von Helmholtz; Thos. C. Martin—*Century.* The World's Debt to Medicine; John S. Billings—*The Chautauquan.* Weather studies at Blue Hill; Raymond L. Bridgman—*New England Magazine.* Heredity; St. George Mivart—*Harper's Magazine.* The Direction of Education; N. S. Shaler—*Atlantic Monthly.*

PROFESSOR CARHART will deliver the address at the dedication of the Hale scientific building of the University of Colorado, on March 7th. His subject is *The Educational and Industrial Value of Science.*

THERE will be held at Vienna between the months of January and May, 1896, an historical exhibition intended to bring under view the social and industrial condition of the country at the beginning of the century.

Arrangements have been made that will probably ensure the union of the Astor Library, the Lenox Library and the Tilden Endowment. This would supply New York with a Library whose property is valued at $8,000,000.

A Committee of the English House of Commons has been appointed to consider changes in the system of weights and measures.

Mr. Charles D. Walcott has been awarded the Bigsby Medal of the Geological Society of London.

Lord Rayleigh is delivering a course of six lectures on *Waves and Vibrations* at the Royal Institution of London. On April 5th he will lecture on 'Argon.'

The Massachusetts Horticultural Society invites subscriptions for the erection of a monument in honor of the late Francis Parkman.

Dr. Kossell, of Berlin, has accepted a call to the Professorship of Physiology at Marburg.

Professor C. L. Doolittle, of Lehigh University, has been called to the chair of Mathematics in the University of Pennsylvania, and Mr. A. P. Brown has been appointed Assistant Professor of Geology and Mineralogy.

Professor John B. Clarke, of Amherst College, has accepted a call to a professorship of Political Economy in Columbia College.

Dr. D. Hack Tuke, editor of the *Journal of Mental Science*, and well known for his writings on insanity, died in London, on March 5th, at the age of sixty-eight.

Mr. J. W. Hulke, President of the Royal College of Surgeons of England, died recently at the age of sixty-five. He was eminent as a surgeon and especially as an ophthalmologist.

Mr. Hyman Montague, known for his writings on numismatics, died in London on the 18th of February, at the age of fifty-one.

Professor Lauth, the eminent Egyptologist, died at Munich, on February 11th, at the age of seventy-three.

The death is announced, at the age of eighty-five, of Sir Henry Rawlinson, the eminent Assyriologist.

Macmillan & Co. announce two works on Physical Geography, by Prof. Tarr, of Cornell University—one an elementary and the other an advanced text-book. The same publishers announce: *Louis Agassiz, his Life, Letters and Works*, by Jules Marcou.

SOCIETIES AND ACADEMIES.

BIOLOGICAL SOCIETY OF WASHINGTON, FEB. 23.

Mr. F. E. L. Beal read a paper on the food habits of woodpeckers, based on the examination of more than 600 stomachs. He found that the Hairy and Downy woodpeckers (*Dryobates villosus* and *pubescens*) feed chiefly on insects, most of which are harmful species. They also eat wild fruits and seeds. The food of the flicker (*Colaptes auratus*) consists largely of ants. Two stomachs contained each more than three thousand ants, and these insects formed 45 per cent. of all the stomach contents examined. The Flicker also ate other noxious insects and some wild fruit, such as dogwood berries and wild grapes. The Red-headed woodpecker (*Malanerpes erythrocephalus*) feeds largely on insects, all of which are harmful species except a few predacious beetles. The vegetable food of the Redhead comprises wild fruits and some corn and cultivated fruit. The Yellow Bellied woodpecker, or Sapsucker (*Sphyrapicus varius*), is the only one in which the vegetable food exceeds the animal. It feeds largely on the inner bark and sap of trees, and also on insects. More than two-thirds of the latter in the stomachs examined were ants.

Dr. C. Hart Merriam, commenting on this paper, said that one result of the study of birds' stomachs by the Division of Ornithology and Mammalogy of the Department of Agriculture had been to show a wider range of food than previously suspected. Each bird has its favorite foods, but when these fail it is usually able to find something else on which it can subsist. Furthermore, the food of most species varies in different localities and at different times of the year, so that the examination of a series of stomachs, however large, from a single locality is utterly insufficient to furnish a reliable index to the range of food of the species. Thus, while the 600 stomachs of woodpeckers examined by Professor Beal failed to show a single beech-nut, it is nevertheless true that in northern New York beech-nuts form, during winters following 'nut years,' the principal article of food of three of the five species mentioned.

Mr. L. O. Howard remarked that it had been queried whether or not ants were more injurious than beneficial, and stated that as harborers of aphids and mealy-bugs they indirectly cause much damage, and are to be considered on the whole as decidedly injurious. He gave an interesting illustration of the manner in which ants had placed colonies of mealy-bugs on the artificially enlarged foliar nectar glands of certain Liberian coffee trees which had been placed in the hot-house of the Department of Agriculture.

Mr. F. A. Lucas described the general structure of the tongue of woodpeckers, noting the great difference between the tongue of the sapsucker (*Sphyrapicus*) and of most woodpeckers. In the sapsucker the tongue was of moderate length and margined for some distance back from the tip with hair-like bristles, some standing out, others directed backward, thus forming a brush for securing syrup. In the other woodpeckers examined, the tongue was excessively long and armed towards the tip with a few sharp, reverted barbs, an arrangement which seemed admirable for extracting grubs from holes in trees.

Mr. B. E. Fernow, in closing the discussion, said that he was glad to see the rehabilitation of the woodpecker, a bird which, once considered very beneficial, had been latterly condemned as injurious, while the evidence now presented seemed to be in its favor.

Mr. F. A. Lucas exhibited some Abnormal Feet of Mammals, saying that abnormalities in the way of digits could be mostly grouped under three heads, duplication of digits, irregular additions to the number of digits, the extra ones budding out from the others, and increased number of digits due to reversion. The latter he considered to be the rarest of the three, most of the extra digits of polydactyle horses being simply cases of duplication, as in the specimen shown. The feet of a pig exhibited illustrated the irregular addition of digits, while two feet of a three-toed cow were thought to be cases of reversion. Feet of an old and young llama illustrated the transmission of abnormalities.

Mr. M. B. Waite gave notes on the flora of Washington and vicinity, which were the result of his own collecting. Two species were added to the flora, namely: *Floerkia proserfinacoides*, Willd. (already published), and *Kyllingia primila*, Michx.

Selaginella rupestris, Spring, which had not been found for many years, was rediscovered at Great Falls. New localities were given for a number of rare plants. Attention was called to some spurious and doubtful additions to the local flora. The tendency of some of the botanists to include in the flora cultivated plants or plants escaped from cultivation which do not properly belong there was criticised, as was also the practice of publishing plants in the lists of additions without seeing specimens and depositing them in some accessible collection.

F. A. LUCAS, *Secretary*.

NEW YORK ACADEMY OF SCIENCES, FEB. 11. BIOLOGICAL SECTION.

The following papers were presented:

The Occurrence and Functions of Rhizobia. DR. ALBERT SCHNEIDER. A discussion of the discovery of the adaptability of rhizobia to other plants than leguminous. Some conclusions based on investigations carried on at the Illinois experiment station were given to show that it is probable that rhizobia may be so modified as to grow in and upon roots of gramineous plants (ex. Indian corn).

An Undescribed Ranunculus from the Mountains of Virginia. PROF. N. L. BRITTON.

On the So-called Devil's Corkscrews of Nebraska. DR. J. L. WORTMAN. A visit to the locality during the past summer had enabled him to study many problems in connection with their occurrence, which tend to throw considerable light upon their nature. The formation in which they occur was positively identified as the Loup Fork division of the upper Miocene, which is a true sedimentary deposit. The Diamonhelix occurs in a stratum of from 50 to 75 feet in thickness always standing vertically, and their tops are not confined to any one level. They vary much in size and character, but so far as observed always present the spinal twist. The fact that they occur in true sedimentary rocks, that their tops occupy many levels, together with the lack of evidence to show that there was any disturbance of level during the time the sediment was being laid down, was considered to totally disprove the theory that they represent the burrows of animals, which has been so extensively held in explanation of their curious nature. The invariable presence of plant cells, together with other facts, leads to the conclusion that they very probably represent the remains of roots or stems of some gigantic water plant.

The excretory System of Clepsine and Nephelis. DR. ARNOLD GRAF. The results of H. Bolsius have proved to be erroneous. The different parts of the nephridium are classified as follows: (1). *Infundibulum*, consisting in Nephelis of six bilobed ciliated cells, in *Clepsine* of a peduncle cell, pierced by a ciliated canal, and two bilobed ciliated cells attached to the peduncle. (2). *Receptaculum excretorium.* A vesicle which is in open communication with the funnel and in osmotic communication with the following parts of the nephridium. It is similar in both genera, and filled with disintegrating material. (3). *Portio afferentia.* The part of the gland, consisting of a single row of round cells, pierced by a sometimes bifurcated canal, which gives off branched canals. Similar in both genera. (4). *Portio glandulosa.* Row of cells, pierced by a smooth canal without side branches or bifurcation. This part is the largest part of the whole organ. Similar in both genera. (5). *Vesicula terminalis.* In *Nephelis* a vesicle, lined by a ciliated epithelium, in *Clepsine* a simple pouch of the *epidermis*, without cilia. (6). *Canalis terminalis.* The short canal by which the terminal vesicle communicates with the exterior. Present in *Nephelis.* In *Clepsine* it is equivalent to the terminal vesicle.

The cells formerly called *Chloragogencells* should now be called *Excretophores.* A preliminary account of these cells has been sent to the 'Zoologischer Anzeizer.' The investigation has been carried out mainly on living tissues, and every source of error has been eliminated.

BASHFORD DEAN, *Rec. Sec'y.*

NATIONAL GEOGRAPHICAL SOCIETY.

CALENDAR, 1895.

Feb. 8.—*Topographic Forms:* MAJ. GILBERT THOMPSON, MR. HENRY GANNETT, MR. G. W. LITTLEHALES.

Feb. 15.—*Shakespeare's England:* REV. G. ARBUTHNOT.

Feb. 22. *Practical Results of the Bering Sea Arbitration:* MR. J. STANLEY-BROWN.

Mar. 1.—*Recent Discoveries in Assyria and Babylonia:* REV. DR. FRANCIS BROWN.

Mar. 8. *Mexican Boundary:* MR. A. T. MOSMAN, MR. STEHMAN FORNEY, CAPT. E. A. MEARNS, U. S. A.

Mar. 15.—*Turkey:* REV. DR. HENRY H. JESSUP.

Mar. 18.—*Washington to Pittsburg and to Niagara Falls; Across the Appalachians:* DR. DAVID T. DAY.

Side Trip to Niagara Falls: MR. G. K. GILBERT.

March 20. Reception at the Arlington Hotel, Washington, D. C., 9 to 11 P. M.

Mar. 22. *Pittsburg to Yellowstone National Park; Pittsburg to St. Paul, through the oil and gas regions:* PROFESSOR EDWARD ORTON.

St. Paul to Yellowstone National Park: MR. WALTER H. WEED.

Mar. 22.—*The Alaskan Boundary:* MR. J. E. McGRATH, MR. J. F. PRATT, MR. H. P. RITTER.

Mar. 25. *Yellowstone National Park to Sacramento; Yellowstone Park; down the Columbia; visit to Mt. Rainier and Portland:* MR. BAILEY WILLIS.

Portland to Crater Lake; Mount Shasta and Sacramento: MR. J. S. DILLER.

Mar. 29.—*Sacramento to northern Arizona; Sacramento; the Golden Gate; Yosemite; Los Angeles; San Bernardino:* MR. W. D. JOHNSON.

From San Bernardino across the deserts; to San Francisco Mt., Arizona: MAJ. J. W. POWELL.

Mar. 29.—*Oregon:* HON. J. H. MITCHELL.

April 1.—*Grand Cañon and Sonora, Mexico; Salt Lake City to the Grand Cañon; a winter in the depth of the Cañon:* MR. CHARLES D. WALCOTT.

Prescott, Phœnix and Tucson, to Sonora, Mexico; visit to the so-called cannibals: MR. W J McGEE.

April 5.—*Across the Rocky Mountains to Denver; Northern Arizona, the Rio Grande, and across the mountains to Denver:* PROF. A. H. THOMPSON.

The Home of the Pueblo Indians: MR. FRANK HAMILTON CUSHING.

April 5.—*Physical Geography of the Great Lakes:* PROF. MARK W. HARRINGTON.

April 8.—*Denver to Washington; Denver to Pueblo, down the Arkansas river, and across the plains to St. Louis:* MR. F. H. NEWELL.

St. Louis to Washington, with visits to the great caves of Ky. and Va.: MAJOR JED. HOTCHKISS.

April 12.—*Argentina,*Columbian University, 8:30 to 9:30 P. M.: Dr. D. ESTANISLAO S. ZEBALLOS.

April 19.—*The Geography and Geology of Nicaragua:* Mr. ROBERT T. HILL.

April 26.—*Antiquities and Aborigines of Peru:* Mr. S. MATHEWSON SCOTT, MR. F. H. CUSHING.

May 3.—*Fredericksburg:* Mr. W J McGEE, MAJ. GILBERT THOMPSON, GEN. JOHN GIBBON, U. S. A.

May 4.—Excursion and Field-Meeting, Fredericksburg, Va., 9 A. M. to 6 P. M.

May 10.—*President's Annual Address:* HON. GARDINER G. HUBBARD.

May 17.—Annual Meeting for the Election of Officers.

PHILOSOPHICAL SOCIETY OF WASHINGTON, MARCH 2.

On the Discovery of Marine Fossils in the Pampean Formation, by Dr. H. Von Ihering: MR. WM. H. DALL.

Classification of Clouds; Illustrated by lantern slides: MR. ALEXANDER McADIE.

The Army Magazine Rifle, Cal. 30: Mr. Rogers Birnie.

Additional Note on Gravity Determinations: Mr. G. K. Gilbert.

William C. Winlock, *Secretary.*

BOSTON SOCIETY OF NATURAL HISTORY, MARCH 6.

The Geographical History of the Lower Mississippi: Mr. L. S. Griswold.

Some Features of the Coastal Plain in the Mississippi Embayment: Mr. C. F. Marbut.

Note on cusped Sand-bars of the Carolina Coast: Mr. Cleveland Abbe, Jr.

Samuel Henshaw, *Secretary.*

SCIENTIFIC JOURNALS.

THE AMERICAN JOURNAL OF SCIENCE, MARCH.

The Appalachian Type of Folding in the White Mountain Range of Inyo County, Cal.: C. D. Walcott.

Notes on the Southern Ice Limit in Eastern Pennsylvania: E. H. Williams.

The Succession of Fossil Faunas at Springfield, Missouri: S. Weller.

Distribution of the Echinoderms of Northeastern America: A. E. Verrill.

Drift Bowlders Between the Mohawk and Susquehanna Rivers: A. P. Brigham.

Scientific Intelligence; Chemistry and Physics; Geology and Mineralogy; Botany; Miscellaneous; Obituary.

AMERICAN CHEMICAL JOURNAL, MARCH.

On the Cupriammonium Double Salts: Theodore William Richards and Andrew Henderson Whitridge.

The Composition of Athenian Pottery: Theodore William Richards.

A Redetermination of the Atomic Weight of Yttrium: Harry C. Jones.

Separation of Nickel and Iron: E. D. Campbell and W. H. Andrews.

Researches on the Complex Inorganic Acids: Wolcott Gibbs.

Cupric Hydride: Edwin J. Bartlett and Walter H. Merrill.

Action of Light on Lead Bromide: R. S. Norris.

The Action of Ammonia upon Dextrose: W. E. Stone.

The Carbohydrates of the Gum of Acacia Decurrens: W. E. Stone.

Reviews and Reports; Notes.

BULLETIN OF THE TORREY BOTANICAL CLUB, FEB.

New Species of Ustilagineæ and Uredineæ: F. B. Ellis and B. M. Everhart.

Contributions to American Bryology—IX: Elizabeth G. Britton.

Japanese Characeæ—II: T. F. Allen.

Tradescantia Virginica var. *villosa* Watson: E. F. Hill.

Some new hybrid Oaks from the Southern States: John K. Small.

Family Nomenclature: V. Havard.

Reviews.

Proceedings of the Club.

Index to Recent Literature Relating to American Botany.

NEW BOOKS.

Antisepsis and Antiseptics. Charles Milton Buchanan. Newark, N. J., The Terhune Co. 1895. xvi+352.

A Laboratory Guide in General Chemistry. George Willard Benton. Boston, D. C. Heath & Co. 1894. Pp. 163.

A Laboratory Manual in Organic Chemistry. W. R. Orndorff. Boston, D. C. Heath & Co. 1894. 82 experiments.

First Lessons in Chemistry. G. P. Phenix. Boston, D. C. Heath & Co. 1894. Pp. 41.

The World of Matter a Guide to the Study of Chemistry and Mineralogy. Harlan Hogue Ballard. Boston, D. C. Heath & Co. 1894. Pp. 264.

Physical Laboratory Manual. H. N. Chute. Boston, D. C. Heath & Co. 1894. Pp. vi + 213.

Practical Methods in Microscopy. Charles H. Clark. Boston, D. C. Heath & Co. 1894. xiv + 219.

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Subscriptions and advertisements should be sent to SCIENCE, 41 N. Queen St., Lancaster, Pa., or 41 East 49th St., New York.

ARGON.

THE plain facts concerning argon are these: For some time past Lord Rayleigh has been engaged on refined work involving the weighing of various gases. Last year he found that the nitrogen obtained from the air is a little heavier than that made from definite chemical compounds. This led him to further experiments and, at the same time, Professor W. Ramsay, of University College, London, also undertook experiments with the object of explaining, if possible, the discrepancy. The general method of work consisted in passing air, first through substances that have the power to remove those constituents that are present in small quantities, such as water vapor, carbonic-acid gas, etc., then through a heated tube containing copper. The oxygen of the air unites with the heated copper, and what has hitherto been regarded as nitrogen remains uncombined. This 'atmospheric nitrogen' was subsequently treated in three different ways for the purpose of removing the nitrogen from it.

(1) It was drawn through clay pipes in the hope that, if the gas is a mixture, one of the constituents would pass through the porous material more easily than the other, and at least a partial separation be thus effected. While something was accomplished in this way, the experiment was on the whole unsatisfactory.

(2) The 'atmospheric nitrogen' was mixed with oxygen in a vessel containing caustic alkali, and electric sparks were passed through the mixture. Under these circumstances the oxygen united with nitrogen and formed a compound which is soluble in alkali. After no further absorption of nitrogen could be effected by sparking, any unchanged oxygen present was removed, and there was then found a residue

of gas which was certainly not oxygen nor nitrogen. This proved to be the substance about which the world is now talking.

In this connection it is of great interest to note that Cavendish, in 1785, probably had this same substance before him free from nitrogen. He performed the experiment above described, and noticed the residue, and says in regard to it: "We may safely conclude that it is not more than $\frac{1}{120}$ of the whole." This is very nearly the truth as regards the relative amount of argon in the air.

(3) The most satisfactory method for obtaining the gas on the large scale consists in passing 'atmosphere nitrogen' over highly-heated magnesium, which has the power of uniting with nitrogen, while the newly-discovered gas has not this power. But, even by this method, the preparation is very slow, and, up to the present, the gas cannot easily be obtained in large quantity.

The new substance is heavier than nitrogen. The density of hydrogen being taken as unity, that of nitrogen is 14, of oxygen 16, and of argon 19.7.

Perhaps the most remarkable property of argon is its inertness. It has not been possible thus far to get it to combine with any other substance, so that anything more than a general comparison with known substances is out of the question. It owes its name to its inertness, argon being derived from two Greek words signifying 'no work.'

A determination of the ratio of the specific heat of argon at constant pressure to that at constant volume was determined by means of observations on the velocity of sound in the gas, and the ratio was found to be 1.66. This is of much importance as showing that the particles of which the gas is made up act as individuals. If this conclusion is correct, it follows further that argon must be either a single element or a mixture of elements, and that, if it is a single element, its atomic weight must be nearly 40, as its density is 19.7 and its atom is identical with its molecule.

Professor Crookes has studied the spectra of argon and, in an article giving his results in detail, he says: "I have found no other spectrum-giving gas or vapour yield spectra at all like those of argon." * * * "As far, therefore, as spectrum work can decide, the verdict must, I think, be that Lord Rayleigh and Professor Ramsay have added one, if not two members to the family of elementary bodies."

Finally, Professor Olszewski, of Cracow, the well-known authority on the liquefaction of gases has succeeded in both liquifying and solidifying argon. It was found to boil at 186.9° C., and to solidify at 189.6° C., forming a mass resembling ice.

To quote from Professor Ramsay's article read before the Royal Society: "There is evidence both for and against the hypothesis that argon is a mixture: *For*, owing to Mr. Crookes' observations of the dual character of its spectrum; *against*, because of Professor Olszewski's statement that it has a definite melting point, a definite boiling point, and a definite critical temperature and pressure; and because, on compressing the gas in presence of its liquid, pressure remains sensibly constant until all gas has condensed to liquid."

The above is a brief account of all that is known about argon, and it would evidently be premature to indulge in speculation regarding its position in the system. It may as well be said at once that, if it is an element or a mixture of elements, it will apparently be difficult to find a place for it on Mendeléeff's table. It will be well to await developments before worrying on this account. If the time should ever come when Mendeléeff's table has to be given up, something better will take its place.

The suggestion has been made repeatedly that argon is perhaps an allotropic form of nitrogen. The strongest argument against this view is the established fact that the gas conducts itself as if made up of individual particles, while any allotropic form of nitrogen, which is heavier than this, must, according to all that we know of such matters, consist of more complex molecules than nitrogen itself.

IRA REMSEN.
JOHNS HOPKINS UNIVERSITY.

*THE FUNDAMENTAL DIFFERENCE BETWEEN PLANTS AND ANIMALS**

To the advanced student, as to the investigator, the question of a definite and accurate distinction by which all true plants can be distinguished from all true animals, is a question of minor interest. To the beginning student the question, on the contrary, is a pressing one for which the answer is urgently claimed. Thus I am led to believe that the definition given below, though it cannot add anything essential to the conceptions of investigators, will nevertheless prove valuable to teachers of biology.

The usual method of drawing a contrast between the animal and vegetable kingdoms, for the purpose of establishing some sort of definition of the two in students' minds, is to leave out of consideration the lower forms, and to take into consideration only the higher forms, on the one side plants with chlorophyll, on the other the multicellular animals or so-called Metazoa. It is then easy to establish a difference in the physiological nutritive processes, emphasizing the synthetic processes, particularly the power of bringing free nitrogen into combinations on the part of plants and the absence of the synthetic process among animals. It is much to be regretted that this method of defining animals and plants has been and still is very widely used, for it leads to inevitable perplexity, because the next thing almost which the student must learn is that the distinction does not hold true. On the one hand, he learns that among plants there are many forms without chlorophyll and that these cannot bring nitrogen into combination and must secure proteid food. On the other hand, he learns that among animals numerous synthetic processes occur, and if he takes up the study of medical physiology he learns many instances of synthetic chemical work on the part of the mammalian body. Dr. F. Pfaff has kindly indicated to me two striking instances of synthesis in the mammalian body, first, the formation of glycuronic acid after the administration of camphor or turpentine, and second, the formation of hippuric acid after the administration of benzoine.

Another distinction often drawn between animals and plants is that of the presence or absence respectively of internal digestive organs. But this again soon leaves the student in the lurch, for the first amœbea he examines knocks that distinction out of the ring.

We may, however, I think, rightly define the two primary divisions of the living world thus:

Animals are organisms which take part of their food in the form of concrete particles, which are lodged in the cell protoplasm by the activity of the protoplasm itself.

Plants are organisms which obtain all their food in either the liquid or gaseous form by osmosis (diffusion).

There are certain facts which appear to invalidate these definitions. The most important of such facts, so far as known to me, is afforded by the Myxomycetes, which, as well known, while in the plasmodium stage of their life-cycle, take solid particles of food very much after Amœba-fashion. Through the kindness of Professors W. G. Farlow and G. L. Goodale, I have learned that there are no other plants which at the present time are known to take solid food

* Read before the American Society of Morphologists at Baltimore, December, 1894.

at any stage. I understand also that botanists are by no means agreed to accept the Myxomycetes as veritable plants. One cannot but ask, Have we not here organisms which connect the two kingdoms? Certainly, in using the above definitions in teaching, it will always be easy to specify the one exception offered by the Myxomycetes and still leave a clear and available conception in the student's mind.

Other facts, which stand in the way of strictly upholding the two definitions, are encountered among animal parasites. For example, a tape-worm in the intestine does not apparently take up any solid food, but is nourished by absorption through the surface of its body of food material in solution. But in these cases we have evidently secondary modifications due to the parasitic life, and in the near relatives of the tape-worms, the trematods and planarians, solid food is taken up. It is to be remarked, too, that it is possible, though perhaps not probable, that even tape-worms will be found on more careful study to take up solid food.

The extent to which it has now been demonstrated that animals take up food in the form of discrete solid particles is not realized generally. The process has been observed with varying degrees of accuracy in the entodermal cells of the digestive tract of hydroids, ctenophores, planarians, trematods, annelids, crustacea, insects, amphibia and mammals, and probably in other forms, which have not come to my notice in this regard. There is here offered a rare opportunity for a valuable research, by making a comparative study of the absorption of solid food. That the protozoa take up particles by means of their pseudopodia is certainly one of the most familiar and most be-taught facts of elementary biology.

I believe that we can also safely teach that the absorption of solid particles of food is to be considered one of the most essential factors in determining the evolution of the animal kingdom. The plant receives its food passively by absorption, and the evolution of the plant world has been dominated by the tendency to increase the external surfaces—to make leaves and roots. The animal, on the contrary, has to obtain at least the solid part of its food by its own active exertions, and to the effects—through natural selection—of the active struggle to secure food we may, I think, safely attribute a large part of the evolution of locomotor nervous and sensory systems of animals. That it has been the only factor cannot be asserted of course for a moment, but it is presumably not going too far in speculative conclusions to look upon it as the most important single factor. An equally important rôle must be attributed to the taking of solid food in connection with the evolution of digestive organs, which are cavities which hold food material until it is absorbed by the cellular walls of the cavities. Indeed, we may expect to find that the entodermal cavity had originally no digestive function whatsoever, but was merely a receptacle to retain the food while the surrounding entodermal cells swallowed it at leisure.

With these speculations I will close, adding only that the speculations have in themselves little value, their only value being to suggest lines of research, which appear promising. The sober naturalist avoids the infernal dipsomania for sheer speculation, and in this article I have already yielded sufficiently to the temptation.

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THE BEST ORDER OF TOPICS IN A TWO-YEARS' COURSE OF ANATOMY IN A MEDICAL SCHOOL.*

Teachers of anatomy differ so widely in their views as to the most useful arrangement of the various branches of the subject

*A paper read at the annual meeting of the *Association of American Anatomists*, in New York, 28th December, 1894.

that it is desirable to clear the field as far as possible at the very beginning of our discussion by the elimination of those points upon which there is substantial agreement. I assume that there is no diversity of opinion on the places which should be occupied by histology and topography. It is to my mind perfectly manifest that the student cannot profitably or comfortably receive instruction in gross anatomy until he has learned the elements of histology: has become familiar with the characters of the various textures which make up the parts and organs of the body, and to which, of necessity, references are constantly made in macroscopic anatomy. By identically the same method of reasoning the conclusion is reached that topography should be taken up latest of all; for it cannot be in a high degree useful to the pupil to work at the relations in space which different organs sustain to each other, until he has acquainted himself with the facts of their shape, size and consistency. To attempt descriptive anatomy without histological knowledge is comparable to studying architectural structures in perfect ignorance of the qualities of building materials, such as stone, brick, wood, iron and mortar; and to undertake regional, before being well grounded in systematic, anatomy is about as possible as reading sentences before acquiring words, or studying the relations of any other things without knowing something about the things themselves. Besides, there is a marked advantage in the incidental, but searching, review of every preceding portion of gross anatomy involved in the pursuit of typographical; and all teachers recognize the vast importance of such repetitions for the student, even if they do not admit that they themselves retain their familiarity with this science of innumerable details only by virtue of incessant review in one way or another.

There is certainly room for difference of opinion concerning the most advantageous marshaling of the remainder of the topics with which we have to deal; but our decision should probably be in largest measure determined by the circumstances in which it is necessary to pursue the study. If the pupil is to devote himself to anatomy only, no great objection is to be raised to the order of subjects adopted in the text-books in most common use—the order which, I think, the majority of teachers employ—beginning with osteology, and following in regular succession with arthrology, myology, angiology, neurology and splanchnology. Much can be said in support of this arrangement. The knowledge of vessels, their origins and terminations, can be of little avail, if there is not a precedent acquaintance with the muscles and other structures which they flush with nourishing blood, or drain of unneeded and effete material; and so, before undertaking angiology, we need especially to study muscles, which constitute so large a part of the human bulk outside of the great cavities, and in which are found so considerable a proportion of the tubes of supply and waste with which we have to deal in the practice of medicine and surgery. The nerves, too, cannot be studied to advantage without antecedent familiarity with the muscles, which are the objective point of their motorial function. In their turn, also, the active organs of locomotion are never learned unless there is a well-laid foundation of skeletal knowledge, upon which to build them; for, in absence of this basis, they are but impotent, flabby, almost shapeless masses of flesh, but little amenable to description, and quite elusive of comprehension. Arthrology is plainly out of the question without osteology, which should immediately precede it. The study of the viscera and organs of special sense concludes the series.

This arrangement is not altogether free from objections. For instance, even after one knows the skeleton and the muscles clothing it, he finds in his study of the arteries much that he cannot fully comprehend from lack of acquaintance with the viscera. But no method can be absolutely perfect: one needs to know all of his anatomy—the whole of everything—in order to understand any one organ perfectly. The problem, therefore, for us, as teachers, is to discover that plan which reduces to the minimum this necessity of knowing a good deal of every department of our great science before entering upon the study of any one of them; and particularly the scheme which makes this need least conspicuous in the earlier portion of the course, when everything is new; for, since the growth of one's anatomical knowledge makes further acquisition in the same line progressively easier day by day, because he is all the time getting nearer to the goal of knowledge of the whole, the last part of the course is naturally that in which there is the least occasion for such help as can be derived from a wise order of topics. After all, however, the arrangement in question is useful, perhaps as good as any other, provided that there is an observance of the condition which I have attached to my commendation of it; but without this provision it seems to me to be clumsy, obstructive, wasteful and irrational.

The condition is that the student is attempting nothing else than anatomy. Practically this is a state of affairs which never obtains in the schools, and is not in the least likely to occur; always physiology is studied synchronously, and usually, also, general chemistry—the latter a branch with no more claim to be regarded as a legitimate topic of medical study than have botany and zoölogy, and, in all fairness to student, school and community, should be required as a preliminary to the medical course. We may confidently count upon finding the first-year student occupied equally with physiology and anatomy. Now, it is so obvious as to require no argument that the action of an organ can never be studied with complete satisfaction until its structure is well understood. Consequently, the anatomy of each part should be learned before its function is presented, in order that the pupil may work intelligently and be spared much difficult and unproductive effort. If the professor of anatomy does not aid him in this matter, the physiologist is driven to perform the task, although it is outside of the proper sphere of his work, and involves the expenditure of much time which he needs for affairs in his own peculiar field. The physiology which we most require is a knowledge of the offices of the viscera, and the teachers of this branch necessarily devote the greater part of their instruction to the consideration of the action of these organs, which, according to the conventional order of topics in the anatomical course, are not touched until all other portions of systematic anatomy have been disposed of. As a result of this, in the early part of the course the anatomist is teaching a vast number of things which are of the smallest possible help to the student of physiology; and, in almost the last part, he goes over ground which has been traversed long before by a suffering colleague, who has been forced into this unwilling usurpation by the unhappy arrangement of the anatomical schedule. In other words, a large and important (to my thinking, the most important) section of anatomy is not taught by the professor of this branch at a period when it is most urgently required by the student, and is presented by him long after it has been already learned.

Surely this state of affairs is, to say the best of it, deplorable, and should not be permitted to continue, if it can be abolished without injustice to the interests of the science to which we dedicate so much of our lives. Each one of us should bear constantly in mind that he is not merely an instructor in a special branch, but is, besides this, a member of a faculty, the purpose of which is to give to medical students the most complete, well-rounded, professional education possible with the available means. On the old lines, which schools have followed far too long, and which are not yet abandoned by all institutions, every professor discoursed to the entire class—a higgledy-piggledy arrangement (perhaps derangement would be the more appropriate designation for so lunatic a scheme) which would not be tolerated for a week in a common school of the lowest grade. Gradually faculties are becoming converted to the idea that a grading of the course is essential to the best results; and those branches which are natural stepping stones to others are completed before advanced studies are undertaken. But much still remains to be done before the most useful system is formulated, and the part of this work which most concerns us is the proper adjustment of our topics to the needs of our colleagues who teach physiology. The plan which I am about to propose is designed especially to attain this end, and will be seen, I trust, to be the most advantageous in other respects, also. It is devised in the spirit which should actuate every individual in a body which is formed to accomplish a given purpose; each one is bound not simply to do these things which will make his department a success, but to do them in such a way as to promote the interests of every other chair. There should be perfect coördination in teaching—the faculty should work always as a 'team,' if a popular expression may be used. In no other way can the highest results be achieved.

In the first place I would have the anatomist ascertain the exact order of topics in the course of his physiological colleague. Let us suppose that the latter purposes, after a little time spent in necessary preliminary considerations, to conduct his class into the realm of the circulation. The anatomist will precede him by a day or two with the study of the organs by means of which circulation is performed. The structure of the heart will be presented with as much of detail as is requisite for the ready comprehension of its action, and this will be followed by the physiological anatomy of the blood vessels: the materials of which they are composed, the arrangement of these, and the variations in their proportions in the large, medium, and small vessels respectively; the physical qualities of the walls; the style of division and union: how the great arterial trunks branch and divide until the most diminutive twigs terminate in capillaries, and how the venous radicles begin in the midst of the tissues and by successive and innumerable conjunctions form larger tubes until the great tap-roots of the system are reached; in short, all those points which aid in the understanding of the function of these organs. He makes no attempt at this stage of the course to present the systematic anatomy of the arteries and veins; perhaps not a single vessel of the great multitude is called by name, except those which, being attached to the heart, must be specifically designated in order to make the description of that organ intelligible. He does not undertake to describe the relations in space which the heart and principal vessels sustain to the parts by which they are surrounded; for he knows that these relations might be very different without essential modification of their action, and that therefore they need not be introduced at this period of the curriculum. Thus, the students are well equipped to receive instruction on the circulation from the professor of physiology, and the latter is free to devote his energies entirely to the work which alone he should be expected to undertake.

This example is no more striking than any other; but it serves well to illustrate on the physiological side the benefits coming from the adoption of the order which I advocate. In this manner the course proceeds; and no portion of the field is entered upon by the physiologist which has not been explored and surveyed as far as structure is concerned by the anatomist in company with the same set of pupils. After the study of the viscera, including the cerebro-spinal centres and the organs of special sense, comes the consideration of the remaining branches of systematic anatomy, beginning with the skeleton and proceeding in the conventional order.

That much advantage accrues to the class in physiology by the execution of this plan seems to me to be perfectly clear. That any anatomical sacrifice is made by it I do not believe. On the contrary, a distinct benefit is gained even in anatomy; for the learning of the function of an organ immediately after the study of its structure serves to emphasize and deepen the impression made by the earlier lesson, and quickens with a living interest what otherwise might remain in the mind only as dry and arbitrary fact, if, indeed, it did not lapse altogether from memory because of its lack of significance.

Incidentally, too, there results great profit of a practical kind, which is lost in following the common order. Students usually know less about visceral anatomy than about any other section of the science. This comparative ignorance depends upon three causes. The first is the fact that the ordinary text-books are far less accurate in the description of the viscera than in that of other parts—a statement which it is unnecessary to substantiate in this learned presence. Second, the study of the viscera is much more difficult than that of other parts. In their best estate they present appearances which are liable to be misleading even to the most careful and experienced observers, as witness the conspicuous errors which for generations passed muster regarding the form of the liver and the position of the stomach—points still misstated in some of the text-books of the day. But another obstacle is often more serious than this. If the organs are fresh, much that is valuable can be learned from them; but when they are the seat of advanced putrefactive changes, as often happens when the muscles and associated parts are still useful for somewhat prolonged examination, they must be removed speedily, without affording the slightest opportunity for careful observation. Third, as the subject of the viscera is usually placed last in the study of systematic anatomy, it is more likely than anything else to be slighted. We all doubtless know from observation, and some of us probably from personal experience, that the enthusiasm of a novice in a study rarely is sustained to the end. In fact, it may be said without incurring the imputation of exaggeration that a large majority of students in any class flag very noticeably towards the close of the term, however eagerly they may have started out. Unquestionably most medical men, young or old, know more about osteology than about any other branch of anatomy. The reasons of this are not far to seek. The skeleton is less perishable than the soft parts and hence the opportunities for the study of it are vastly greater; and, what seems to me to be of quite as deep significance, it is generally the first branch of our science which the student attacks. It is his memorable, first step inside the mighty and mysterious domain of medicine, and, consequently, every detail makes a powerful impression on his plastic mind. Although he sees that his book contains much besides osteology, this is the first and, by inference, the most important of its contents. The common people sometimes speak of a skeleton as an anatomy; and the young student almost deludes himself with the notion that he knows the bulk of anatomy, when he has acquired a very general conception of the bones. Of course, his ideas are silly and childish, and have to be corrected; but we must take human nature as we find it, and, if possible, turn its very weaknesses into useful channels. Now, without having the smallest disposition to belittle the advantage of an accurate knowledge of the skeleton, it has long been a conviction with me that visceral anatomy should be ranked first in the list of topics, considered from the purely utilitarian point of view: that the subject of which our students generally know least is precisely that of which they ought to know most. They come to us in order to be equipped as practitioners. Whatever may be their callow aspirations, however much they may be dazzled and charmed by the brilliant performances of surgery, we and all of our colleagues know that the enormous majority of them must be general practitioners, doing almost no surgical operations, except the strictly minor; having a great many obstetric cases; seeing a multitude of sick infants, a good many ailing women, and not a few acutely ill adults of both sexes. What is the greatest anatomical need of such men? Is it not undeniable that, for one case demanding in them a knowledge of bones, muscles, blood vessels or nerves, they have at least a score in which they must know something definitely about the structure of lungs, heart, stomach, bowels, liver, kidneys, uterus or brain? If, then, visceral anatomy far surpasses all other portions of the field in importance to the enormous majority of practitioners of the healing art, it should be placed first chronologically in the course of systematic anatomy, so that it shall be taught at the time when the learner's mind is most eagerly receptive and most faithfully retentive—provided, of course, that this assignment of position does not conflict with the rights of other things. Unless my argument has utterly miscarried, it is established that the proposed order not only does not sacrifice anything on the physiological side, but is even of conspicuous advantage to it; and I have been unable to discover any way in which it can affect unfavorably the welfare of any department whatsoever. There is no occasion for anxiety lest the postponement of osteology will result in its being ignored or slighted. The facilities for its study are so comparatively abundant, the conventional conception of its importance is so deeply rooted, and the natural and inevitable attraction which it exercises on the student is so strong as to insure the bestowal upon it of a sufficient share of his attention.*

With me the order advocated is not merely a theory: it is a long accomplished fact. For about fifteen years I have had the plan in practical operation, and have not yet observed a single thing which has caused me to regret the change from the ancient system. It appears to me now, as in the beginning, to be the most rational, economical, facile, attractive and useful succession of topics. During this prolonged trial of the order I have had as fellow-members of the Bowdoin faculty in the chair of physiology three gentlemen, of whom two, Drs. B. G. Wilder and C. D. Smith, are members of this Association, and can testify as to the usefulness of the plan.

*It would be foolish to disparage the cultivation of any portion of the field of human anatomy; the more thoroughly the physician knows every part of it, the better equipped will he be as a practitioner. Vastly more blunders than are ever recognized depend upon ignorance of easily known facts of structure. But the tremendous insistence upon the supreme value of osteology, which characterizes the method of some teachers of anatomy, seems to me to demonstrate a lack of sense of proportion, which, while easily enough accounted for by the student of medical history, who appreciates also the dominating (sometimes almost domineering) influence of habit and suggestion upon the mind, is none the less peculiarly unfortunate in its effect.

The third, Dr. Henry Hastings Hunt, has within a month ceased from his labors, and been borne to his honored grave; but I feel justified in giving his testimony emphatically in its favor.

My plan, slightly detailed, is as follows: Beginning with some explanations of a general character, and the definition of certain terms which are so technical that the novice cannot be expected to know them, I give the names, both English and Latin, and the limits of extension of all of the superficial parts; for I have learned that it is not safe to count on anybody's knowing what an anatomist or surgeon means by various terms applied to parts which are visible without dissection, and have vernacular appellations. Histology is then presented in an elementary way, and the student is taught the essential truths about the simple tissues. The different kinds of membranes are discussed, and the structure of glands in general is naturally given the next place. The student is now fairly equipped for the study of the viscera, and these are taken up in whatever order the physiologist of the institution prefers. In one important particular my course at this period differs from visceral anatomy as presented in most of our books; the brain and spinal cord, the noblest and most interesting of all entrails, are included in the company of the viscera, and not, as ordinarily in the text-books, with the nerves. After this come in regular, conventional style the bones, ligaments, muscles, arteries, veins, lymphatics and nerves; and, last of all, topographical, or, as I prefer to call it, relational anatomy.

In this scheme no separate place is assigned to embryology, a subject usually treated in obstetrical and physiological works, as well as in anatomical. By agreement with my colleague in physiology, its systematic presentation is made by him; but all through my course the facts of development are introduced, not only to inform the student upon points of practical moment, but also to illustrate and enforce many features of adult structure.

At the end of his first year in the school the student is required to pass a satisfactory examination in histology, splanchnology, and osteology, and he is not permitted to enter upon second-year studies until he has so passed. At the end of his second year he is examined on the remainder of systematic and all of relational anatomy, failure excluding him from his third year.

It will be observed that I have confined my remarks strictly to the subject announced, and have refrained from discussing the relative merits of various methods of imparting instruction, as by lectures, recitations, demonstrations, and so forth. I wish it to be understood, however, that, if any expression of mine has seemed to imply that the old-time method of teaching by lectures holds the first place in my esteem, I have unwittingly done an injustice to a cherished conviction; for the lecture system, as an exclusive, or even principal, method of instruction, has long seemed to me to be the worst which has been devised.

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CURRENT NOTES ON PHYSIOGRAPHY (IV).

MERRIAM ON THE DISTRIBUTION OF ANIMALS AND PLANTS.

A STUDY that is admirable, alike in its quality and its results, has been presented by Dr. C. Hart Merriam in a vice-presidential address to the National Geographical Society of Washington, under the title, 'Laws of temperature control of the geographical distribution of terrestrial animals and plants' (Nat. Geogr. Mag., VI., 1894, 228–238). The life zones of the United States, as mapped two years ago (Ann. Rep. Sec'y Agriculture, 1893), are now shown to be limited northward by the total quantity of heat during the season of growth and reproduction; and southward by the mean temperature of the hottest part of the year. The 'total quantity of heat' is measured by the sum of the excesses of mean daily temperature over 43°; this temperature being taken as marking 'the inception of physiological activity in plants and reproductive activity in animals.' The 'hottest part of the year' was arbitrarily limited to the six hottest consecutive weeks of summer. The life zones, the northward control, and the southward control are shown on three maps; and the accordances between the controls and the zones are truly surprising. The peculiar over-lapping of boreal and austral types along the Pacific coast, hitherto not clearly understood, is thus shown to obey the same controls as those which elsewhere keeps these types apart; the western coast being exceptional in having a great total quantity of heat, but a very mild summer. The dependence of these temperature controls on general geographical features offers a beautiful illustration of the general principles of climatology.

HARRINGTON'S RAINFALL CHARTS OF THE UNITED STATES.

A QUARTO paper of text and tables and a large atlas of charts, entitled 'Rainfall and Snow of the United States, Compiled to the end of 1891,' by Mark W. Harrington, chief of the Weather Bureau, has lately been issued as Bulletin C, of that office. It is based on all available records, of very different periods and values, but constituting the best body of material now in hand for the study of precipitation in this country. The charts exhibit the monthly, seasonal and annual rainfall, monthly maxima and minima, and many other details. The text calls attention to the chief features in the distribution of precipitation, both in place and season. The unusually heavy rainfall in the southern Appalachians, averaging over sixty inches, and exceeding ninety inches in 1892 at one station, is a new feature. It may be doubted whether the rainfall of the more mountainous belts is in general sufficiently represented. For example, Pike's peak is the only mountain meteorological station in Colorado, and its rainfall (30″) is greater than that of any other station. It might therefore be taken as indicating the rainfall on the mountains of Colorado in general; but, although there are many other lofty peaks, the isohyetal line of 30 inches does not include them. One might, to be sure, in the absence of direct observations, feel some hesitancy in asserting that these other summits actually have a 30-inch rainfall; yet one might feel equal hesitancy in asserting, as the charts do so emphatically, that the high peaks in general have not a 30-inch rainfall. It is stated that "in general the rainfall decreases also with the elevation above sea level;" and the decreased precipitation in passing westward across the Great Plains is taken as an illustration of this generalization. It is questionable whether the illustration is pertinent; for other controls, such as distance inland and relation to mountain ranges, are here presumably of much greater importance than increasing elevation. It is to be regretted that, in the interests of a consistent terminology, Florida should be cited as a region of 'subtropical' rainfall. Florida is a region of summer rains; while regions of subtropical rainfall always have their maximum in winter, as in the region originally so named by Dove, around the Mediterranean, and again with equal distinctness in California, Chili, South Africa and South Australia. The southeastern coast of Asia has a summer rainy season, like Florida; and Florida might therefore with some justice be likened to the regions of monsoon rainfall, but this would hardly do justice to its other relations. As a matter of fact, no technical name has yet been suggested for seasonal rainfall of the Florida kind.

BAROGRAPH RECORD DURING A TORNADO.

The general fall of pressure during the passage of cyclonic storms is an old observation. The short-lived rise of pressure during the onset of a thunderstorm is of more recent detection. The inferred very low pressure in the funnel of a tornado has never been tested by direct observation, unless the tracing of a barograph at Little Rock, Arkansas, on October 2, 1894, may show an effect of this kind. The tornado passed over the Weather Bureau station at 8:28 p. m. of that day, and although the upper story of an adjacent building was blown upon the station, the instruments on its roof generally destroyed, the windows blown in and the furniture drenched with rain, the barograph bravely continued its record; and its interesting curve is reproduced in the Monthly Weather Review for the month in question. As the tornado passed there was a momentary fall and rise of 0.38 inch. Shortly afterwards the storm passed over the gas works, and all the lights in the city went out as if by relief of pressure from the gasometer. As soon as the cloud passed, the tank settled again, the pressure was resumed, and the gas jets could be lighted. Professor Abbe, editor of the Weather Review, points out that the sudden change of pressure recorded on the barograph curve may have been merely a local effect of decrease of pressure by wind suction up the chimney, followed by restored pressure when the windows were broken in; so the inferred low pressure of the tornado funnel still eludes unquestionable record.

NEW YORK STATE WEATHER SERVICE.

The fifth annual report of the New York State meteorological bureau and weather service, of which Professor E. A. Fuertes, of Cornell University, is director, is perhaps the most elaborate and valuable of any of the State service reports yet issued. Besides the summaries of monthly reports for all stations for 1893, with good charts of temperature and rainfall from records at about one hundred stations, there is a comprehensive chapter on the climate of the State, by E. T. Turner, meteorologist to the State service, with a number of interesting plates and charts. For example, the curves of daily mean temperatures and pressures exhibit to a nicety the greater fluctuations of these elements in the winter, when cyclonic action is increased, than in summer, when it is diminished. A neatly tinted map, shaded for elevation, gives a clear idea of the general relief of the State. The few elevated stations in the Adirondacks have a higher mean winter temperature than those in the St. Lawrence valley, more than a thousand feet lower; a notable example of this inversion having occurred under an anticyclone on December 8, 1890, which is illustrated by a special chart. Nocturnal winds, flowing northward past Ithaca to Cayuga Lake, are described as characteristic of the valleys of the southern plateau; they occur on clear nights, both winter and summer, beginning one or two hours after sunset and reaching a velocity of about eight miles an hour before morning. The thickness of this current, as determined by balloons, is only from fifty to a hundred feet. Apart from the immediate value of so well managed a service as this, in the way of displaying weather signals and distributing crop reports, it deserves hearty support from the State in its long task of collecting and discussing authentic climatic data. The number of reporting stations should, however, be largely increased, and for this purpose the service cannot do better than foster the adequate teaching of meteorology in the public schools, both by the publication of special articles serviceable to teachers, and by making these articles known at teachers' and farmers' institutes.

ARGENTINE METEOROLOGICAL REPORTS.

Among the most elaborate discussions of meteorological observations published in America are those of the Argentine Meteorological Office, under the direction of Walter G. Davis, whose headquarters are at Cordova, in the middle of the pampas. The latest volume issued, number IX., is in two parts; the first giving the original observations at Cordova since 1872, the second giving the mean values determined from this important series of records. A notable climatic feature is the occurrence of a wet summer, October to March. and a dry winter, April to September, The summer rains are chiefly supplied by thunderstorms, yet curiously enough the rains exhibit both in quantity and in number of occurrences a distinct afternoon minimum and an early morning maximum; but the scale of cloudiness has its maximum toward midday, and in January in mid-afternoon. High barometric pressure confirms the continental quality of the winter dry season. Westerly winds are rare; northeast and southeast are common, the latter flowing feebly through the night, the former actively through the afternoon; and thus indicating the left-handed or austral deflection that might be expected with increased velocity in the southern hemisphere. The strong diurnal winds last from ten to five o'clock in late summer, but only from noon to three in midwinter; while the duration of the quiet winds of night plainly varies with the period from sunset to sunrise. Although the text and tables are most elaborate, the treatment of the subject is local, numerical and climatic, rather than general, descriptive and meteorological.

THE SPECIOUS TERM, 'REFORESTATION.'

The hard times lately reported as afflicting some of the Western States in the debatable belt, where agriculture is an uncertain occupation, recall by contrast the over-confident opinions, so freely uttered by 'experts' before Congressional committees, concerning the improved climatic conditions that might be expected over the Great Plains as settlement advances. Governmental science will, we fear, suffer severely when the inaccuracies of this quasi scientific testimony are understood. Hardly less misleading than the loose phrases concerning 'the underflow,' from which an inexhaustible water supply has been looked for, is the term 'reforestation,' used with the implication that the barren plains of to-day have been forested in the past. One official has testified: "By the destruction of the forest which originally covered this region, the very condition of its existence and of its natural recuperation was destroyed; and thus, in a reverse manner, reforestation of parts by artificial means may make natural reforestation over the whole area possible by and by. . . . Reforestation on the plains and forest preservation on the mountains is of greater national concern than the location of irrigation reservoirs." There is no shadow of evidence that the Plains have ever been forested since their geographical surroundings were like those of to-day. It is a most gratuitous assumption to use the term 'reforestation' in writing of the Plains. It does harm to those who are tempted to settle there by these and other over-favorable views concerning the climate of the sub-arid region; and it discredits governmental science by exposing it to so easy contradiction.

W. M. Davis.

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ANNUAL RECEPTION OF THE NEW YORK ACADEMY.

The New York Academy of Sciences last year instituted a series of annual receptions, suggested by the famous *conversazione* of the Royal Society of London. The first Reception was held in the Library of Columbia College. The second, held upon March 14th, in the Galleries of the American Fine Arts Society, was much larger and more successful than the first, including 331 separate exhibits, grouped under sixteen branches of Pure and Applied Science. In the South Gallery were placed Physics, Electricity, Astronomy, Mechanics and Chemistry; in the Middle Gallery, Photography, Psychology and Mineralogy; and in the Vanderbilt Gallery, Zoölogy, Palæontology, Human and Comparative Anatomy, Botany and Geology. Each branch was under a Chairman who had entire control of the the general arrangements, and while the exhibits were largely from the educational institutions and museums in and around New York a number of most interesting objects were sent from considerable distances, such as the photographs from the Allegheny and Lick Observatories. Among the very large number of excellent exhibits it is only possible to mention a few of the most novel.

Mr. Charles A. Post, of the Strandhome Observatory, had charge of the astronomy, in which he displayed photographs of star spectra between F. and D. from the Allegheny Observatory, glass positives of comets and the Milky-Way from the Lick Observatory, and a number of new spectroscopic and other astronomical instruments. Professor Mayer, of Stevens' Institute, had charge of the physical section, in which were shown his series of Chladni figures preserved in sand, illustrating the errors of older figures and the accuracy of Lord Rayleigh's theoretical deductions. A number of new physical instruments for spectroscopic and sound measurement were exhibited in operation by Professor Hallock from the Columbia Physical Laboratory. Professor Crocker had charge of electricity, in which were shown Professor Pupin's machines for producing alternating currents for multiplex telegraphy and other purposes, also E. H. Dickerson's acetylene illuminating gas produced from calcium carbide made in an electric furnace. The mechanical exhibit was in charge of Professor R. S. Woodward, and included models of the international prototype metres and kilogrammes, and several pieces of new mechanical apparatus. In the mineralogical exhibit, arranged by Dr. L. P. Gratacap, of the American Museum, was a series of Babylonian and Assyrian cylinders, illustrating the different minerals employed between 4000 and 300 B. C.; also an extensive display of new types of American minerals. The photographic exhibit, in charge of Dr. Edward F. Leaming, besides new apparatus from Zeiss of Jena, included all the recent applications of photography in color printing, and the combination of colors in lantern projection shown by the inventor, Frederic E. Ives, of Philadelphia. Dr. Leaming's micro-photographs of nervous and cellular tissues and of bacteria formed an important feature of this exhibit. The exhibit in experimental psychology was contributed by the department of experimental psychology of Columbia College. The apparatus has been recently made for the college, and with the exception of the harmonium was designed by members of the department. The harmonium was designed by von Helmholtz and Ellis to give pure intervals in place of the equal or tempered intervals used in musical instruments with fixed keys. The other apparatus shown was: (1) an instrument which measures the duration, intensity and area of lights, now being used for the investigation of after-images; (2) an instrument which measures the time (to 0.0001 sec.) objects are exposed to view, now being used to study the legibility of letters and types, and in an altered form to measure the perception, memory and attention of school children; and (3) a new chronograph of very high speed with fixed drum and movable carriage. Physiology was represented by a number of special exhibits made by the Chairman, Prof. J. G. Curtis, and by Professor Thompson, of the New York University. The botanical exhibit, arranged by Dr. Carlton C. Curtis, included an extensive display of new plants from North and South America, Dr. Schneider's studies of lichens, and the morphological and embryological studies carried on under the direction of Dr. Curtis, by the students of Dr. Curtis and of Professor Gregory of Barnard College.

The American Museum contributed two extensive exhibits in Zoölogy and Palæontology, arranged by Professor Allen and Professor Osborn. The Zoölogical exhibit illustrated the rapid improvement in the modern methods of taxidermy by a series of comparisons of specimens of work just completed and that of ten years ago, the most notable being the preparation of the chimpanzee 'Chico' by Mr. Rowley. The results of the current field explorations of the Museum and the natural methods of group mountings were also shown by extensive exhibits. In vertebrate Palæontology the chief feature was three panels showing the stages in the evolution of the horse; first, of the modern skeleton in comparison with that of *Hyracotherium venticolum*, from the Cope Collection recently acquired by the Museum; second, a complete series of feet, and third, a complete series of skulls. Two newly discovered ancestral forms of Titanotheres from the Eocene were also shown, displaying the first rudiments of the great horns which characterize the latest surviving members of this group. The most noteworthy feature in invertebrate Palæontology was the collection shown by Messrs. Van Ingen and Matthew, of what appears to be a sub-Olenellus fauna from the lower Cambrian, in other words, the oldest fauna thus far discovered. Under Geology, as arranged by Professor J. J. Stevenson, was shown an extensive series of eruptive rock from the pre-Cambrian volcanoes along the Atlantic coast, besides many results of Prof. Kemp's field work. The Columbia biological laboratory contributed to the zoölogical exhibit a full series illustrating the Golgi silver nitrate nerve-cell preparations, together with the results obtained by the 'lithium-bicromate' and 'formalin' modification introduced by Mr. Strong, exhibitor. Professor E. B. Wilson, displayed his new series of fertilization stages of the Sea-Uchin, proving that the archoplasm is entirely derived from the spermatozoon. All of these cytological exhibits were accompanied by micro-photographs taken by Dr. Leaming. Dr. T. H. Cheeseman had charge of the bacterial exhibit, including principally a display of preparations by the new formalin method, and an illustration of the stages in the preparation of the anti-toxine treatment of diphtheria. In Anatomy, Professor Huntington displayed a unique series of 194 preparations, showing the comparative anatomy of the caecum and vermiform appendix throughout the vertebrata.

The Exhibit was open throughout the afternoon to students, and throughout the evening to guests of the Academy. The admirable arrangements were largely due to Professor Hallock, Chairman; Dr. Dean, Secretary, and Professor Lee, Chairman of the Reception Committee. The event fully justified the large amount of time and care which was given to its preparation, and in the opinion of all those who were present will prove a great stimulus to the various branches of research now in progress in New York. It has been informally decided to renew these receptions from year to year, and to attempt to give them a more national character by inviting exhibits from other parts of the country. The galleries of the Fine Arts Society, with unlimited wall space for the exhibition of charts and diagrams, with admirable means for electrical illumination for microscopic and other purposes, and with very extensive floor space for tables, is exceptionally adapted to the needs of an extensive exhibition of the annual progress of science. Henry F. Osborn.

CORRESPONDENCE.

AN INTERNATIONAL SCIENTIFIC CATALOGUE AND CONGRESS.

Editor of Science: *Dear Sir*:—In considering your very courteous invitation to contribute something of present interest to your valuable journal, it has occurred to me that I could not perhaps do better than to follow the example set in your issue of Feb. 15th, by the distinguished representatives of my *alma mater*, Prof. Bowditch and his committee, in their report to the Harvard University Council on the circular of the Royal Society, respecting the proposed International Catalogue. My letter of reply to this circular does not, as you will see, in any way conflict or interfere with the recommendations made in that excellent report. It deals almost entirely with other points in the circular which are not directly noticed in the report.

Should the suggestions which I have ventured to make, especially in regard to the meetings of an International Congress of Science in connection with the proposed Catalogue, be finally approved and carried into effect, they may lead to practical results of great importance. Such meetings, held from time to time—perhaps in various cities of the two continents—may not only bring together from all parts of the globe the most eminent votaries and friends of science in fraternal conference, but may help not a little, with other influences which are now constantly at work, in converting Tennyson's 'parliament of man' and 'federation of the world' from a poetical vision into a beneficent reality.

Yours faithfully,

Horatio Hale.

Clinton, Ontario, Canada,
May 30, 1894.

Gentlemen: As you have honored me by addressing to me a copy of your important circular letter, in which you solicit from the recipient the expression of his views respecting the establishment of a 'Central Office or Bureau,' by 'international coöperation,' for the purpose of preparing and publishing, at brief intervals, a catalogue of all scientific publications of every description (whether appearing in periodicals or independently), I cannot, in due courtesy, decline to offer in response such considerations as occur to me, however inadequate they may seem in comparison with others which will reach you from better qualified correspondents.

That the proposed scheme is both highly desirable and abundantly feasible cannot reasonably be doubted by any one who is aware of the immense increase in the number of scientific publications of late years, and the equally rapid increase of scientific associations, public libraries and high institutions of learning, for most of which such a catalogue will be found of very great advantage and ultimately a necessity. The most convenient 'method of inaugurating the scheme' would seem to be by first ascertaining the probable annual cost, which can readily be judged through the experience already gained by the Royal Society in the publication of its annual 'Catalogue of Scientific Papers,' and then by appointing in each (presumed) contributing country, under some appropriate title, an 'Aid Bureau,' which should be an existing institution of high standing, and one that either is already, or can easily be placed, in touch with the chief scientific associations, colleges and public libraries of the country, and can ascertain the amount of contributions which could be obtained from them. In the United States, for example, such a suitable Aid Bureau at once presents itself in the Smithsonian Institution. In Canada and in each of the other British colonies which possesses a Royal Society, this Society will naturally assume the office. In every other country some institution of similar position and character will readily be found.

As to the place of the Central Bureau, and the directing authority under which it should be inaugurated, one would suppose that there can hardly be two opinions. That this place should be London, and this authority the Royal Society of England, would seem to be necessary conclusions from the existing circumstances, at least at the outset. Both place and directory might, of course, be changed hereafter, if this should be found desirable.

It would seem specially advisable, for the purpose of arousing and maintaining an interest in the object in view, and of ensuring the cordial coöperation of all concerned in the work, that general meetings should be held—either annually, or biennially, or triennially, as might be found most convenient—of representatives of all the contributing bodies, or at least of all that contribute a certain defined amount to the fund. Such meetings might be held either at the place of the central office or at other places, as might be decided, from time to time, by the assembled representatives. Such an assemblage would constitute an International Congress of Science, possessing much of the character of those congresses of geologists, of anthropologists, of Orientalists, of Americanists and the like, which have of late years been found so popular and useful, but differing from them in possessing to some extent a representative character, and with it a defined purpose and authority. Its purpose would be that of maintaining a connection among the students of all the sciences throughout the globe, not only by personal acquaintance or correspondence, but also and especially through the medium of the Central Bureau and the Catalogue, which would be directly under the authority of the Congress. In general it may be said that this Congress would speedily become for the whole civilized world what the modern Association for the Advancement of Science is for its own country; with the important difference, however, that the Congress, besides the personal influence of its meetings and the interest that would attach to the volume recording the proceedings of each meeting, would have the much greater influence and usefulness resulting from the permanent activity of its Central Office and the frequent issue of its catalogue of scientific publications.

As regards the 'character of the work to be carried on in the central office,' there seems little to be added to the suggestions of the circular. The final paragraph, in which it is suggested that "arrangements might be made by which, in addition to preparing the catalogue, scientific data might be tabulated as they come to hand in the papers supplied," could perhaps be enlarged, with much advantage, into the creation of a special 'Bureau of Scientific Correspondence,' to which any member of a contributing body might apply for information on questions of fact. As is well known, it constantly happens that through the unavoidable ignorance in which, to a large extent, students of science have heretofore remained of one another's actions, supposed new discoveries are announced and resulting theories suggested, which have been already made known elsewhere. Every such student will appreciate the advantage of being able to refer to a bureau of specialists for information on doubtful points of this description.

On the question of 'the language or languages in which the catalogue should be published,' there would seem to be little difficulty in deciding. If English and French should be jointly selected for this purpose, there would probably be no objection from any quarter. There are very few students of science who are not familiar with one or other of these idioms. And the choice will be made generally acceptable by the fact that they very fairly represent the two great Indo-European branches of language, the Teutonic and the Romanic, in which at least nineteen-twentieths of all scientific publications are likely to appear for many years to come. If the time should arrive when the addition of another language may seem advisable, it can readily be made by the proposed congress or any other authority then governing the Central Bureau.

It would, of course, be understood that the deliberations of the congress and of its sections, and the papers read before them, would not necessarily be restricted to the two idioms of the catalogue, but might be in any language which the congress or any section should at the time decide to admit. This decision, it may be assumed, will always be considerate and liberal to the largest possible degree.

I am your obedient servant,
HORATIO HALE.

The Secretaries of the Royal Society,
BURLINGTON HOUSE, LONDON.

SCIENTIFIC LITERATURE.

A Primer of Mayan Hieroglyphics: By DANIEL G. BRINTON. Ginn & Co., Boston. 1895. 8°, pp. 152.

The public mind is becoming more and more interested in the archæology of Mexico and Central America. At once symptomatic of and a cause of increasing this interest are the numerous explorations of recent years, the exhibition from this region collected for the Exposition, and the notable works published in Mexico, Spain and Germany in connection with the Quadri-centennial celebration of America's discovery.

Nevertheless, students in our own country are somewhat at a disadvantage in this matter. The literature of the subject is not only scattered, but is in various languages, —Spanish, French and German—and it is not easy to keep track of progress. This little volume, by one who has devoted years to the study of 'the American Race,' and who is a specialist in the languages, literature and life of Isthmian people, will therefore be particularly welcome. It not only summarizes the work done, but is a guide to the original publications wherein discussions have been published.

The Mayan hieroglyphic system was in wide-spread use, being represented on monuments of Yucatan, Tabasco, Chiapas, Guatemala and Western Honduras. Though so often compared with that of the Aztecs, it is certainly more fully developed. On the whole, it can not be said to comprise a very great number of simple elements; these, however, are variously combined and united, and the composite *glyphs* are many. The material for study varies. There are books —Codices—written on long strips of paper, which were folded screen-wise. Four such codices are known, called the Codex Troano, C. Cortesianus, C. Peresianus and C. Dresdenis; they are in libraries at Madrid, Paris and Dresden. There are also mural inscriptions cut in stone; elaborate series of calculiform characters chiseled on altars and monoliths; pretty cartouches engraved on amulets or ornaments; symbols or characters painted on pottery; glyphs on hard, firm grained boards of wood like those from Tikal.

Are these characters ideograms or phonetic? There are those who believe they are entirely the former; there are others who claim that many are phonetic. Some admit that both occur. Brinton himself invented, years since, the word *ikonomatic*. He believes that there are some true ideograms in the Mayan texts; very many of the characters, however, he believes are in the nature of rebuses. They still betray their origin as pictures, but are not to be considered as pictures but as characters representing sounds—either the name of the object pictured, used as a phonetic element, or a sound suggested by that. Looking at the whole field he recognizes three groups of elements:

1°. Arithmetical signs, numerals, numerical computations—Mathematical elements.

2°. Pictures of men, animals, fantastic beings, ceremonies, objects, etc.—Pictorial elements.

3°. Graphic elements, proper.

To each of these our author devoted a division of his work.

Numbers, day signs, month signs, are so common in the Codices as to suggest that these are mainly time-counts. The Mayas counted by twenties, and had distinct terms for higher orders of numerals up to at least the sixth power of twenty. They were able to write numbers, even the highest; dots were units, lines were fives, and there were special characters for the score and for higher orders. Förstemann appears to have found that they had a zero sign, and that numbers were written upward, a higher order of units being indicated by position. Maya time divisions are complicated, and a variety of numbers are used in their tables. Thus the numbers 4, 5, 13, 20, 24, 52, 65, 104, 115, 260 and others occur in grouping days and months into years, cycles, &c. The Maya idea of a complete number seems to have been the multiple which should contain all these numbers used in reckoning days. Förstemann claims to find the number 1,366,560 days (= 3744 years) in the Dresden Codex. The eminent German believes the Codices were largely astronomical treatises, and in this opinion Brinton agrees. This is, as he says himself, world-wide distant from the theories of Seler or Thomas. Aside from theories, however, Brinton presents the necessary information, which is gained so far, regarding numbers as they occur in the Codices; he also presents briefly and simply a sketch of his own and Förstemann's astronomical views, and calls attention to the fact that other views exist.

The bulk of the pictorial elements have to do apparently with religious ideas and represent deities, ceremonies or religious objects. Schellhas' paper upon the representations of the gods in the Maya writings will ever remain the foundation for such study. In some cases Brinton agrees with Schellhas; in others he reaches a different identification. A considerable number of the gods are satisfactorily made out; that is certain. Influenced as he is by Förstemann's strongly astronomic views, Dr. Brinton feels that among these representations of deities there should be some of the planet Venus. In all parts of the Codex Troano there are many curious representations of a bee; this he connects with Venus as the *evening star* and merges the latter into the old woman, so often represented with Cuculcan, as the earth goddess. In all the Codices, Brinton counts 825 representations of male deities and 125 of females; he believes that 638 of these have been made out. He says: "This is a satisfactory result and shows that, as far as these pictographs go, the contents of these once mysterious volumes are scarcely an unsolved enigma."

The graphic elements are and long must be the most difficult. The signs of the days and months have long been known; those of the cardinal points have recently been pretty surely identified; the 'monograms' of the gods are fairly agreed upon. In studying the graphic elements the composite glyphs must be analyzed. They consist usually of one main element, with infixed, prefixed, superfixed, postfixed or sub-fixed secondary elements. Then one must, if possible, find the things which these simple elements originally represented. The ideogrammatic force may be gone, but the name of the thing pictured may suggest the phonetic value. The work is not easy. Brinton takes up one after another such as have been most studied, or for which he has a meaning to suggest. That we are still far from final conclusions is shown by the variation in interpretations of different authors. A group of signs which Seler considers are derived from 'man' and signifying 'person,' others distribute among crescent, ear, a serpent's mouth, eye and eye-lash, comb, claw, feather, part of a plant, etc. One of the commonest of glyphs, believed by Brinton to be derived from a picture of a feather ornament, and with the phonetic value of *yax*, and meaning (by metaphor) green, new, young, strong, fresh, virile, etc., is by others variously identified as representing a gourd, a tree, a *zapote* fruit, the phallus, etc. Such diversity of opinion is not discouraging; it only shows that much remains to do.

Our author does not slavishly follow authority. The bee-god sign and the *yax* character already mentioned show independence. His recognition of the *pax* (drum) sign is ingenious and probably strong. He introduces much new argument in identifying the deities. His suggestions in reference to day and month signs are thoughtful.

In so new a field we must have conflict of ideas. Dr. Brinton fairly aims to present all sides. The Primer shows the real position of knowledge on the question as resulting from the labors of Seler, Thomas, Schellhas, Förstemann and a host of other students. It is a good summary of present knowledge with a considerable addition of new and thoughtful material. It points the way, gives suggestion and help. The beginner must have the book, and every worker must recognize that Dr. Brinton by its publication puts all under genuine obligation, whether they agree with all his arguments or not.

FREDERICK STARR.

UNIVERSITY OF CHICAGO, Feb. 16, 1895.

Steam and the Marine Steam Engine. By JOHN YEO. London and New York, Macmillan & Co. Illustrated. 105 Engravings. Pp. xiv, 196. 8vo. $2.50.

This is a book written by a Fleet Engineer of the British Navy, for use at the Royal Naval College and elsewhere, embodying lectures prepared by Mr. Yeo for a course addressed to Executive Officers. It is thought that it may prove also useful for officers of the merchant service, and for students in engineering. It is a very compact presentation of the subject, and, as might be expected, coming from an officer of long service, abounds especially in well-made illustrations exhibiting the construction of the marine engine in its various usual forms and all its details. Of these engravings we can hardly speak too highly. They are largely reproductions of the diagrams and drawings employed in the lecture-room, and reductions of working drawings made especially for the book. The introduction gives an abridged account of the history of the marine engine, from the time of Watt to the present, and indicates, in a general way, the methods of improvement which have brought about the enormous gain, meantime, in economy and power of steamships.

The structure of engines and boilers, and of all their minor parts and accessories, including the slide-valve and its gearing, indicator-diagrams and their interpretation, and the condenser, the screw, and the powering of ships, are subjects treated of with evident knowledge and with brevity and accuracy. Little space is given to theoretical discussions of the thermodynamics or of other principles, mathematical or physical, illustrated by the action of the steam engine, and the special value of the book lies in its presentation of the forms of parts and its descriptive account of the machine. It is well made; paper, type, style and binding all being excellent; and the publishers are to be congratulated on their good work in this respect. R. H. THURSTON.

The Life and Correspondence of William Buckland, D. D., F. R. S. Some time Dean of Westminster, twice President of the Geological Society, and First President of the British Association. By his daughter, MRS. GORDON. With portraits and illustrations. New York, D. Appleton & Co. 1894. Post 8°. Pp. 288. $3.50.

To those who were 'brought up,' geologically speaking, on perhaps the most weighty and yet brilliant of the Bridgewater Treatises, 'Geology and Mineralogy considered with reference to Natural Theology,' and are familiar with the prolonged struggle for existence undergone by the 'noble subterranean science' in the first half of our century, this life of the English participant in the contest will show what a force he must have been in the intellectual and scientific life of his time.

Dean Buckland was one of the creators of the science. Himself inspired by the teachings, though at second-hand, of William Smith, 'the father of English Geology,' he became the teacher of Lyell, of Murchison, of Etheridge, Daubeny, Egerton and Lord Enniskillen. As early as 1809, when a Fellow at Oxford, he had by his energy in collecting, his contagious enthusiasm, and his bold and effective advocacy of the infant science, produced a sort of panic in the minds of those who would have gladly strangled this newly born science.

The philosophic calm and classical serenity of the Oxford dons was sorely vexed and disturbed by the young savant. "Some dreaded lest his example should drive the *amœnitates academicæ* out of fashion." When his shorter journeys on British soil finally led to a longer excursion to the Alps and to Italy, one of the elders is said to have exclaimed: "Well, Buckland is gone to Italy; so, thank God, we shall hear no more of this geology." But young Buckland's zeal, energy, overflowing humor and eloquence, led to his appointment in 1813 to the Readership of Mineralogy, and in 1819 a Professorship of Geology was created for him.

He went on triumphantly in his career of advancing and popularizing his favorite science, overcoming objections and theological narrowness either by a joke, a hearty laugh, a strain of lofty eloquence, or by earnestly insisting that the study of geology, so far from being irreligious or atheistic in its consequences, had a tendency to confirm the evidences of Natural Religion, and that there could be no opposition between the works and the word of God.

His humor, quick wit and overflowing jollity or playful fancy in the lecture room were contagious. His field lectures were largely attended, and many are the stories told of his apt illustrations on these occasions, as well as of some of his adventures on his geological excursions. They are illustrated by rhymes and by comic pictures from the pen and pencil of his fellow geologists. As an example of his graphic mode of explaining the earth as understood in his day, it is said "He compared the world to an apple-dumpling, the fiery froth of which fills the interior, and we have just a crust to stand upon; the hot stuff in the centre often generates gas, and its necessary explosions are called on earth, volcanoes." When riding towards London with a friend on a very dark night, they lost their way. "Buckland therefore dismounted, and taking up a handful of earth, smelt it. 'Uxbridge,' he exclaimed, his geological nose telling him the precise locality." Mr. Etheridge tells the story of Buckland when travelling in Scotland, "in order not to shock the feelings of the Scotchmen on Sunday, carrying his hammer up his sleeve." Ruskin, who was an undergraduate of Christ Church when Buckland was not only the Professor of Geology, but also a Canon of the Cathedral, writes in his 'Præterita:' "Dr. Buckland was extremely like Sydney Smith in his staple of character; no rival with him in wit, but like him in humor, common sense, and benevolently cheerful doctrine of Divinity . . . Geology was only the pleasant occupation of his own merry life."

With these characteristics of head and heart, a sane mind in a sound body, it may be imagined what an immense impetus Buckland gave to the growth and development of the young science. He was the first president of the Royal Geological Society, and the first president of the British Association for the Advancement of Science, which he invited to meet at Oxford. His papers and memoirs were not numerous, though upwards of fifty, besides three general works; perhaps his volume on Caves, 'Reliquiæ Diluvianæ,' was of most lasting value. He was, though at first rejecting Agassiz's theory, one of the first to recognize the fact of the former existence of glaciers in Great Britain.

Buckland was born in 1784 and died in 1856. His last scientific paper appeared in 1849. In 1845 he was appointed by Sir Robert Peel to the deanery of Westminster, and one of the first things he did was to introduce a system of pipe-drainage in Westminster Abbey, the first of its kind ever laid down in London, and which led to the disuse of cesspools and brick sewers throughout that city. He was, then, not only dean and a doorkeeper in that palatial house of the Lord, but he applied his scientific knowledge to the thorough cleansing of its foundations. Cleanliness with the good dean was evidently a synonym of godliness. His sermon delivered in 1848 on the words, 'Wash and be clean,' was almost the first contribution to sanitary science, a subject in which he was far ahead of his time. His interest in medical science, in general charity and philanthropy, in building churches and schools, was informed and enlightened by his early geological training and advanced ideas. When, in 1846, the famine crept over Ireland, and even into England, he met the difficulty while living in his summer house at Islip, and among other wise and kindly acts he supplied the village shops with sacks of hominy and Indian corn. Here also he built a recreation room for the village lads, the forerunner of our boys' clubs and kindred associations.

The story of Buckland's brilliant and useful life is in most respects well told; the illustrations are amusing and often instructive, and we warmly recommend the book as most entertaining reading for geologists, young and old, and indeed for all lovers of nature. A. S. Packard.

GEOLOGY.

Report on the Iron Mountain Sheet, by Arthur Winslow, E. Haworth, Frank L. Nason and others. Arthur Winslow, State Geologist, Mo. Geol. Surv. 1894.

This is the third number of the same series of reports as the Bevier sheet and covers an area of about 250 square miles in portions of Iron, St. Francois and Madison counties. As in the others, the principal feature is the map showing the topography and the geology. This was constructed by Messrs. Haworth, E. H. Lonsdale and C. F. Marbut and is similar in scale and contour interval to the one described above. It is also accompanied by a sheet of columnar and cross sections, showing the structure of Iron mountain and Pilot knob. In the text the peculiar topography of the region, as well as the other physiographic features, are described by Mr. Winslow. Mr. Haworth contributes the portion on the geology of the crystalline rocks and Mr. Winslow that on the geology of the Paleozoic rocks. The economic geology of the iron ores is treated of by Mr. Frank L. Nason, the author of the report on Iron Ores, published by the Missouri Survey in 1892. The report on the building stones is by Mr. G. E. Ladd.

The first of this series, viz., the Higginsville sheet, was issued in folio form, the text being printed on large sheets of the same size as the maps, somewhat similar to the sheet reports issued by the United States Geological Survey, except that the former was stitched. In these later reports the text is printed in octavo form, while the map with the sheet of sections and a sheet of brief explanatory matter is issued in a folio cover separately. A portion of the edition, however, has the map and sheet of sections printed on thin paper, folded and inserted at the end of the pamphlet. Thus this series of reports have been issued in three forms, which may serve to assist in deciding the best form for publication of future reports for different purposes.

J. D. R.

Preliminary Report on the Rainy Lake Gold Region. By H. V. WINCHELL and U. S. GRANT. Geol. and Nat. Hist. Survey of Minn., 23rd Ann. Rept., pp. 36–105. Jan., 1895.

Considerable excitement has been caused during the last year by the reported discoveries of rich gold-bearing veins at Rainy Lake, on the northern border of Minnesota, and accordingly an examination of this region was made by the Geological Survey oj the State. The veins occur in more or less crystalline rocks of Pre-Cambrian age, and can be classed as: (a) fissure veins, (b) segregated viens and (c) fahlbands. The most promising part of the district is in what is known as the Seine River country, in Canadian territory, where there are true fissure veins which furnish a good quality of free-milling ore. Actual mining was conducted during the last summer in but one place—at the Little American mine, in Itasca county, Minn.; but prospecting and exploitation have been carried on in a number of other places. As yet the development is insufficient to warrant the positive assertion that profitable gold mining can be conducted in the Rainy Lake district, but in several localities the prospects are full of encouragement and promise. The report is accompanied by a geological map of the region.

NOTES AND NEWS.

BIOLOGICAL.

THE January number of the *Geological Magazine* contains a note by Professor H. G. Seeley, on the skeleton of *Pareiasaurus baini*. This remarkable animal is one of the Anomodontia which Professor Seeley has been making known to science from the Karoo or Upper Triassic beds of South Africa. He observes that while there are superficial characters which parallel the labyrinthodont amphibia, there is no doubt the animal finds its place among true reptilia. It is remarkable for the number of sharp recurved teeth upon the palate, together with the teeth in sockets on the alveolar margins of the jaw. Notwithstanding the extremely heavy build of the animal, there is much that recalls the lowest mammalia in the shoulder girdle and the fore and hind limbs. It is the shoulder chiefly which indicates this affinity with the Monotremata. The new knowledge which this animal supplies gives a meaning to the ordinal term by showing the resemblances in the teeth to various groups of animals which would not have been suspected from the reptilian structure of the skull, or the mammalian structure of the extremities. The skeleton is figured, as it now appears mounted in the British Museum, of a total length of seven feet, nine inches. It would be difficult to imagine a more grotesque quadruped. Those who have had experience in mounting stone skeletons realize what an extremely difficult undertaking it is, and will judge of this particular mount with leniency; at the same time, an examination of the figure, or still more of the original specimen in the Museum, shows that the limbs have been placed in an unnecessarily awkward and impossible position. There was no necessity for placing the hind limbs so far in front of the center of gravity of the posterior half of the body, or for turning the fore feet so far inward that locomotion in a forward direction would be rendered impossible.

The latest Bulletin from the Museum of Comparative Zoölogy is Professor Agassiz's '*Reconnoissance of the Bahamas and of the elevated coral reef of Cuba in the steam yacht Wild Duck, January to April, 1893*,' covering 200 pages, 47 plates, and a large number of illustrations in the text. It contains a complete survey of this remarkable coralline region, and is not only full of original observations and notes of great value, but brings the region far more easily within the reach of future biological and geological exploration. As the survey in the Wild Duck continued over only four months, it has rather the reconnoissance character of that made by Professor Agassiz in the 'Albatross,' on the west coast of South America, than the thoroughness of the author's work upon the Blake. The Wild Duck was placed at Mr. Agassiz's disposal by Mr. John M. Forbes, and while not fitted like the Government vessels for deep sea work, proved to be admirably adapted for cruising on the Bahama banks, her light draft enabling her to go to every point of interest and to cross and recross the banks where a larger vessel could not follow. The greater part of the Bulletin is descriptive. A number of important problems are discussed, the author closing with an expression of his own views upon the formation of coral reefs, as confirmed by this exploration of the Bahamas: "Substitute subsidence for rising land and remembering that reef coral will not grow at a greater depth than twenty fathoms, we eliminate subsidence as a factor unless we are prepared to accept or imagine a synchronism between the growth of corals and subsidence in a great number of the districts in which they flourish, of which we have no proof."

WELDING OF IRON.

At the last meeting of the Royal Society, according to the *London Times*, a paper on *Iron and Steel at Welding Temperatures* by Mr. T. Wrightson, M. P., was read. The object of the paper was to demonstrate that the phenomenon of welding in iron is identical with that of regelation in ice. The author recapitulated some experiments which were made by him in 1879–80 upon cast iron, and proved the fact that this form of iron possesses the property of expanding while passing from the liquid to the plastic state during a small range of temperature, and then contracts to the solid state, and that the expansion amounts to about 6 per cent. in volume. This property of iron resembles the similar property of water in freezing, which, within a range of about 4° C., expands about 9 per cent. of its liquid volume, and then contracts as the cooling proceeds. Subsequent investigations at the Mint appeared to prove that wrought iron at a welding temperature possesses the same property of cooling under pressure which was proved by Lord Kelvin to exist in freezing water, and on which demonstration the generally received theory of regelation depends. The author distinguished the process of melting together of metals from that of welding. Either process forms a junction, but the latter takes place at a temperature considerably below the melting point. The well-known and useful property of welding in iron appeared, therefore, to depend, as in the case of regelation in ice, upon this critical condition, which exists over a limited range of temperature between the molten and the plastic state. An interesting discussion followed, in which Lord Kelvin, Professor Roberts-Austen, Professor Silvanus Thompson and others joined.

THE JOINT COMMISSION OF THE SCIENTIFIC SOCIETIES OF WASHINGTON.

AT a meeting of the Joint Commission of the Scientific Societies of Washington, on January 25th, recommendations were made which have since been adopted by the Societies represented on the Commission, which are: The Anthropological, the Biological, the Chemical, the Entomological, the Geological, the National Geographic, and the Philosophical Societies.

The resolutions adopted are as follows:

The Joint Commission of the Scientific Societies of Washington, believing that fuller coöperation of the Societies is desirable, and that it can advantageously be provided for by enlarging the powers of the Joint Commission, recommend to the Socities the adoption of the following:

The Joint Commission shall be composed of the officers and administrative boards of the several component Societies

The Commission shall have power:

a. To provide for joint meetings of the Societies; b. To conduct courses of popular lectures; c. To prepare a joint directory of the members of the Societies; d. To distribute to all members of the Societies periodic advance notices of the meetings of the several Societies; e. And to act in the interest of the component Societies at the instance of any of them.

The following officers have been elected: President, Gardiner G. Hubbard; Vice-President, G. Brown Goode; Secretary, J. S. Diller; Treasurer, P. B. Pierce; Members at Large of the Executive Committee, J. W. Powell, William H. Ashmead, George M. Sternberg, G. K. Gilbert, W. H. Dall, Charles E. Munroe and C. D. Walcott.

GENERAL.

THE *Educational Review* for March should be read by all who are interested in elementary and secondary education. The number consists of the report of 'The Committee of Fifteen' appointed by the Department of Superintendence of the National Educational Association and submitted at Cleveland, February 19–21. The three sub-committees report respectively, 'On the training of teachers,' 'On the correlation of studies in elementary education,' and 'On the organization of city school systems.

MR. T. C. MARTIN contributes to the March number of *The Century Magazine* an article on Hermann von Helmholtz well calculated to impress the general reader with the magnitude of Helmholtz' genius. The article is accompanied by a portrait of Helmholtz, as he appeared during his visit to America in 1893, which should be preserved by all men of science.

THE American Book Company has just published a fourth edition of Dana's *Manual of Geology*, the work being enlarged by 150 pages. The entire manuscript, extending to 1000 pages of printed matter, is in Professor Dana's own hand-writing, which is remarkable in the case of an author in his eighty-third year.

A TELESCOPE is being constructed for the Berlin Industrial Exposition, to be held next year, in which the lenses, made by Steinheil of Munich, will be 110 cm. in diameter.

HELMHOLTZ' library has been bought by the German Government for the Physico-Technical Institute.

THE annual appropriation for the University of North Carolina has been made by the Legislature. It had been feared that this might not be done. The recent Legislature has reorganized the Board of Regents of the West Virginia University and has reduced it from thirteen to nine, requiring all the members to be appointed from the two leading political parties, as nearly equally divided between them as practicable; its members are appointed for a period of six years, one-third changing every two years. Owing to former dissensions in the faculty of the University, the time of all the professors expires on June 15th; and the Board, at its meeting in June, will elect an entire new faculty, including president and professors. This applies only to the professors of the University, not to members of the Agricultural Station Staff. Dr. Rudolph J. J. de Roode, Chemist of the Station, resigned the first of February, to accept a more lucrative position in New York. His position has been filled by the appointment of B. H. Hite as chemist and G. Wm. Gray as assistant chemist, both of Johns Hopkins University.

SOCIETIES AND ACADEMIES.

THE BIOLOGICAL SOCIETY OF WASHINGTON.

At the meeting held March 9, 1895, the papers were presented, of which abstracts are here given.

Dr. C. W. Styles spoke of *A double-pored Cestode with occasional single pores.** Great stress has been laid upon the arrangement of the genital pores in the classification of the Cestoda, but this character alone is not of generic value. Stiles has already shown that although *Thysanosoma Giardi* generally possesses alternate genital pores, it occasionally possesses double pores in its segments. In American rabbits, the speaker finds two species of tapeworms, one of which possesses irregularly alternate genital pores and a peculiar arrangement of the eggs in capsules—such as is found in the genus *Darainea;* this makes it possible that this species is the adult stage of the armed cysticercois described from the intestine of rabbits in his Note 31; if this be so, the parasite would be classified with the genus *Darainea*, although, according to Railliet's present classification, based upon the arrangements of the pores, it is an *Andrya.* The second tapeworm possesses *double genital pores.* If classified on its pores alone, it is a *Ctenotœnia* Rail. It differs from the type of the genus (*Ct. marmotæ*) in possessing a double uterus instead of a single uterus. One strobila of this rabbit tapeworm (*Ctenotœnia sp.?*) was found in which most of the segments possessed double pores, but thirteen segments were found with irregularly alternate pores. This anomaly is extremely important, both from a morphological and a systematic standpoint, and the speaker expressed the opinion that a thorough study of a large series of Cestoda in any group would result in greatly modifying the present classification and in suppressing a large number of species.

*To be published as 'Notes on Parasites, 36: A double-pored Cestode with occasional single pores' in Centralblatt für Bact. u. Parasitenkunde, 1895.

Dr. Theo. Holm discussed *Œdema of Violet Leaves.* Leaves of a cultivated garden variety of *Viola odorata* affected with this disease were studied, and their anatomical structure showed several points of interest. The diseased parts of the leaf showed brownish, wart-like swellings on both faces of the blade, above and between the nerves. The following changes were observed in the tissues: The epidermis became very thick-walled, and the stomata modified into narrow, irregular openings. The palisade tissue showed numerous (three or even four) tangential divisions, and swelled up very considerably, pushing out through the epidermis. The pneumatic tissue, which seemed to be the most affected, had increased in size, the cells having divided themselves very considerably so as to form a loose, open tissue of large, roundish cells. The petiole showed similar symptoms of the disease, especially along the keel and the wings. The collenchymatic tissue underneath the epidermis, the bark parenchyma, and the endodermis showed numerous divisions, so that similar swellings were produced like those observed on the leaf blade.

Dr. Geo. M. Sternberg read a paper entitled *Explanation of Acquired Immunity from Infectious Diseases*, an account of which will be printed in the next issue of SCIENCE.

M. B. WAITE,
Recording Secretary.

SCIENTIFIC JOURNALS.

THE JOURNAL OF MORPHOLOGY.

THE latest number of the *Journal of Morphology* is of exceptional importance. Mr. Frank Lillie's article upon the *Embryology of the Unionidiæ* contains a most careful investigation of the relations of the earliest cells in the embryonic cleavage to the adult organs of the body. This is followed by Oliver S. Strong's memoir upon the *Cranial Nerves of the Amphibia*, which opens up a new and thoroughly philosophical interpretation of the cranial nerves, based not upon their numerical relations, but upon their physiological components. This is the result of an investigation of a very difficult character which has been under way for the past five years. The third paper, by Pierre A. Fish, upon the *Adult Nervous System of the Salamander*, is followed by a brief but interesting paper from Professor W. K. Brooks upon the *Sensory Clubs of Certain Cœlenterates.*

The most important feature of this number, however, is contained in three short preliminary papers at the end of the *Journal*, occupying only a few pages, but apparently establishing a new law in the field of fertilization phenomena. The discovery has been made independently by Dr. Wheeler and by Dr. A. D. Mead, of the University of Chicago, and by Professor E. B. Wilson and Mr. A. T. Matthews, of Columbia College. In course of correspondence the authors of these papers learned that they had independently reached the same unexpected conclusion, and it was arranged by the editor that their three communications should appear together. While they mark an important step forward in our knowledge of fertilization, at first sight the results obtained by Dr. Wheeler and Professor Wilson are directly contradictory. Dr. Wheeler proves conclusively that in the fertilization of *Myzostoma* (a parasitic form of Annelid) there are no traces of the archoplasm or dynamic substance in the spermatozoon, and that this element is entirely resident in the ovum. Professor Wilson, on the other hand, independently working on the eggs of the echinoderm *Toxopneustes*, proves that there is no trace of the archoplasm in the ovum, but that it is entirely resident in the spermatozoon. It is too soon to make a general induction from these observations, but at present they appear to wholly set aside the brilliant announcement of Fol in 1891, which has been supported by Guignard and Conklin, that both the ovum and spermatozoon contain archoplasm, and that one feature of segmentation is a 'quadrille of the four centers' derived from these male and female archoplasmic masses. These observations do prove, however, that the archoplasm may be derived exclusively either from one sex or the other, and they show that Fol's law was based upon defective preparations. They tend also to show that the archoplasm is not a bearer of the hereditary qualities, but necessarily a purely neutral dynamic agent.

THE PSYCHOLOGICAL REVIEW, MARCH.

THE current number is largely taken up with the Princeton meeting of the *American Psychological Association*, already reported in SCIENCE (January 11). Authors' abstracts are given of sixteen papers presented, and the address of the President, Prof. William James, is given in full. Mrs. Franklin's paper on *Normal Defect of Vision in the Fovea* was also read before the Association. The only remaining paper consists of Contributions from the Psychological Laboratory of Columbia College. Dr. Grifling describes experiments on the relations between dermal stimuli and sensations, and Mr. Franz gives an account of measurements of the light which is just sufficient to produce an after-image. In addition to discussion and notes, there is an extended survey of recent psychological literature prepared by Profs. Sully, Ormond, Fullerton, Dewey, Baldwin, Donaldson, Cattell, Angell, Gardiner and Duncan, and Drs. Binet, Kirschmann, Tracy and Noyes.

THE POPULAR SCIENCE MONTHLY, MARCH.

THE number opens with an interesting account of *The Birth of a Sicilian Volcano* by Prof. Packard, describing an ascent of Monte Gernellaro, a crater on Mount Etna formed in 1886. In the second paper Dr. Bela Hubbard dwells on the importance of the forests and the need of legislation to prevent destruction by fire. An article by Dr. S. Millington Miller discusses the education of the blind and of the deaf and dumb and their careers. The number includes articles on engraving and bookbinding, two articles on scientific education, and accounts of Tyndall's work and of Thomas Nuttall (with a portrait).

THE ASTROPHYSICAL JOURNAL, MARCH.

Notes on the Atmospheric Bands in the Spectrum of Mars: WILLIAM HUGGINS.

Recent Researches on the Spectra of the Planets: H. C. VOGEL.

Solar Observations made at the Royal Observatory of the Roman College in 1894: P. TACCHINI.

On a very large Protuberance Observed December 24, 1894: J. FÉNYI.

On the Distribution of the Stars and the Distance of the Milky-Way in Aquila and Cygnus: C. EASTON.

Preliminary Table of Solar Spectrum Wave-Lengths. III.: H. A. ROWLAND.

The Modern Spectroscope: F. L. O. WADSWORTH.

Minor Contributions and Notes; Reviews; Recent Publications.

BULLETIN OF THE AMERICAN MATHEMATICAL SOCIETY, FEB.

On a Certain Class of Canonical Forms: RALPH A. ROBERTS.

Hayward's Vector Algebra: MAXIME BÔCHER.

Apolar Triangles on a Conic: F. MORLEY.

An Instance Where a Well-known Test to Prove the Simplicity of a Simple Group is Insufficient: GEORGE S. MILLER.

Briefer Notices; Notes; New Publications.

THE AMERICAN GEOLOGIST, MARCH.

Development of the Corallum in Favosites forbesi, var. occidentalis: GEORGE H. GIRTY.

Early Protozoa: G. F. MATTHEW.

The Stratigraphic Base of the Taconic or Lower Cambrian: N. H. WINCHELL.

The Second Lake Algonquin: F. B. TAYLOR.

Editorial Comment; Review of Recent Geological Literature; Correspondence; Personal and Scientific News.

NEW BOOKS.

Guide to the Study of Common Plants. VOLNEY M. SPALDING. Boston, D. C. Heath & Co. 1895. vii + 294.

Government of the Colony of South Carolina. EDSON L. WHITNEY. Baltimore, The Johns Hopkins University Press. 1895. Pp. 121. 75 cents.

Theoretical Chemistry. W. NERNST. Translated by Charles Steele Palmer. London and New York, Macmillan & Co. 1895. Pp. xxv + 697. $5.

Mechanics. Dynamics. R. S. GLAZEBROOK. Cambridge University Press. New York, Macmillian & Co. 1895. Pp. xii + 251. $1.25.

Diary of a journey through Mongolia and Tibet. WILLIAM WOODVILLE ROCKHILL. Washington, Smithsonian Institution. 1894. Pp. xx + 413.

Noxious and Beneficial Insects of the State of Illinois. S. A. FORBES. Springfield, Ill. 1894. Pp. xi + 165 + xii.

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Subscriptions and advertisements should be sent to SCIENCE, 41 N. Queen St., Lancaster, Pa., or 41 East 49th St., New York.

THE MESOZOIC FLORA OF PORTUGAL COMPARED WITH THAT OF THE UNITED STATES.

HISTORICAL NOTICE.

THE earliest studies in the Mesozoic deposits of Portugal seem to have been made by Mr. Daniel Sharpe, who read a paper before the Geological Society of London on April 11, 1832, describing certain beds in the vicinity of Lisbon and Oporto; in the former of which were included strata referred by him to the Oolite. On the 9th and 23d of January, 1839, he presented a second paper describing more fully the secondary formations in the vicinity of Lisbon.* On November 21, 1849, Mr. Sharpe read still a third paper before the same society† of a much more extended nature and devoted entirely to the secondary formation. In this paper is a full list of all the fossils known down to that date carefully determined by Mr. John Morris. Included in these was a single fossil plant regarded by Mr. Morris as a variety of a species of the Yorkshire Oolite called by Phillips *Cycadites gramineus.* It was found at Cape Mondego, and from this circumstance was given the varietal name *Mundæ.* As Mr. Morris referred Phillips' plant to the genus Zamites, the Portuguese plant was made to bear the name *Zamites gramineus* var. *Mundæ.*

In 1858 Sr. Charles Ribeiro published a series of elaborate papers on the Geology of Portugal,‡ treating chiefly of the Carboniferous; but in two of these§ he considers the Lias and Oolite, mentioning the plant above referred to from Cape Mondego and

* *Geol. Soc. Lond., Proc.*, Vol. I., p. 395; Vol. II., p. 31; Trans., 2d Ser., Vol. VI., p. 115ff.

† *Quart. Journ. Geol. Soc. Lond.*, Vol. VI., pp. 135–201.

‡ *Mem. Acad. Real. Sci. de Lisboa*, New Ser., Vol. II.

§ *Mina de Carvão de Pedra do Cabo Mondego, do districto de Leiria;* op. cit., Pt. II., Third and Fourth Memoirs (these memoirs are separately paged).

four other species from this and other localities.

Meantime other collections were being made, and in 1880 M. Paul Choffat published a somewhat elaborate report on the geology of the Jurassic of Portugal* in which the fossil plants were considered as far as available. The collections were sent by Choffat to Professor Oswald Heer, and a preliminary report upon them was received in time to be inserted as an Addendum. Heer's full report appeared a year later† and constitutes the first important contribution to the Mesozoic flora of Portugal. It also includes a large number of Tertiary plants. The horizons are here regarded as embracing: first, the Rhetic; second, the Jurassic, subdivided into Lias, Oolite or Dogger, and Upper Jurassic or Malm; and third, the Cretaceous, which was largely compared with the Wealden of other parts of Europe. Heer found in these collections 5 Rhetic, 18 Jurassic, and 23 Cretaceous forms. The Cretaceous plants consisted chiefly of ferns, cycads and conifers, but two of them were referred to the monocotyledons. No traces of dicotyledons were discovered.

M. Choffat continued his investigations and after Heer's death sent the plant-impressions to the Marquis Saporta at Aix; the latter was greatly interested in them and published three preliminary reports.‡ What specially attracted him was the presence of certain peculiar forms from this Lower Cretaceous horizon that he regarded as prototypes of the existing dicotyledonous flora. No dicotyledons had thus far been reported from any Lower Cretaceous deposit in Europe, and it had long been supposed that the Cenomanian was the earliest horizon at which this type existed. The several instalments embraced in these papers were from horizons in the Cretaceous, some of which were the same as those containing the plants described by Heer, while others were considerably higher. They contained a number of very remarkable forms, and the Marquis could not doubt that they represented ancestral dicotyledons. The full report upon these interesting collections has been waited for with great impatience, especially by American geologists familiar with our Potomac formation, in which the case is so nearly paralleled. In fact the present writer, having learned through correspondence with the Marquis that large collections were in his hands, and not knowing how soon his report would appear, was so desirous of learning more in regard to them that while in Europe during the past summer, by previous arrangement with him and at his urgent request, he paid a visit to the veteran paleobotanist at Aix, in the South of France, and through his extreme courtesy was not only permitted to examine these collections, but enjoyed the great favor of discussing with him a large number of the most interesting questions to which they give rise. It was then that he learned that the final report was already in press and would soon appear, and proof sheets of the text and plates were then in the possession of the author, so that it was possible to examine the work in immediate connection with the specimens. This work has now appeared* and copies of it are in the hands of American geologists; but it may

* *Étude stratigraphique et Paléontologique des terrains Jurassiques du Portugal.* Première livraison. Le Lias et le Dogger au Nord du Tage. Section des travaux géologiques du Portugal, Lisbonne, 1880.

† *Contributions à la flore fossile du Portugal par le* Dr. Oswald Heer. Section des travaux géologiques du Portugal, Lisbonne, 1881.

‡ *Comptes Rendus Acad. Sci. de Paris*, Vol. CVI., May 28, 1888, pp. 1500–1504; CXI., December 1, 1890, pp. 812–815; CXIII., August 3, 1891, pp. 249–253.

* *Flore fossile du Portugal.* Nouvelles contributions à la flore Mésozoique par le Marquis Saporta. Accompagnées d' une notice stratigraphique par Paul Choffat. (Avec 40 planches.) Direction des travaux géologiques du Portugal, Lisbonne, 1894.

as well be stated here that although a large and voluminous report containing 280 quarto pages and 39 plates, it still comes far short of covering the material that is now in the author's hands. The collections were sent to him in instalments almost every year and are still arriving, but it was necessary to fix some limit to the publication, which was closed at a certain date and the work sent to press, since which time other collections have been received, which were also carefully examined on that occasion at the Chateau of Fonscolombe, the country residence of the Marquis, 16 kilometers north of Aix, and upon which he was at the time actively engaged. These will be reported upon in a subsequent memoir. The remarkable parallelism between the plant bearing deposits of the west coast of Portugal and those of the eastern part of the United States, and especially between the Lower Cretaceous of Portugal and our Potomac formation, gives an especial interest to this memoir.

THE JURASSIC FLORA.

IN America there is a decided time hiatus between the lowest Potomac beds and the next plant bearing horizon below, which is now regarded as belonging to the extreme Upper Triassic and as about the equivalent of the Keuper deposits of Lunz, in Austria.* In Portugal, on the contrary, there appear to be no plant bearing horizons in the Trias proper, but in the Jurassic, which is absent in this country, a considerable number of such deposits have been found. M. Choffat, who prepared the geological part of this memoir, follows as closely as possible the nomenclature of the French geologists, and it is found that plant bearing horizons occur in the Infralias, part of which may be as low as the Rhetic, and some of which is referred to the Sinemurian; in the Lias; in several of the properly Oolitic beds (Toarcian, Bajocian, Callovian, etc.); in several members of the Corallian; in the Kimmeridgian, and in the Portlandian. The Jurassic deposits of Portugal consist of sandstones and limestones, the former predominating below; and while all of them may not be of marine origin, so large a part is fossiliferous that by the aid of the careful stratigraphical investigations of the Portuguese geologist it is possible to fix the position of the plant beds with relation to those holding animal remains, a fact which is of the utmost importance in determining the validity of the evidence of fossil plants in such countries as America, where, for the most part, no such guide exists.

*See *Bull. Geol. Soc. Am.*, Vol. III, 1891, p. 31.

The Jurassic flora of Portugal, as embraced in the present memoir and in that of Heer already mentioned, consists of 122 species, of which 22 are Infralias, 1 Lias, 8 Oolite, 8 Corallian and 88 Kimmeridgian. It is subdivided into 6 Algæ, 6 Equiseta, 70 ferns, 7 Cycads, 24 Conifers and 9 Monocotyledons. Of the ferns, which so largely predominate, 27 species belong to the genus Sphenopteris, 8 to Cladophlebis, 8 to Scleropteris, and 4 each to Pecopteris and Hymenophyllites. Of the conifers, which come next in importance, 5 belong to Pagiophyllum, 4 to Brachyphyllum, and 3 to Thuyites. The cycads belong to the two genera Podozamites and Otozamites. Seven of the Monocotyledons consist of small blades and culms of grasses, grouped under the genus Poacites.

A comparison of this Jurassic flora with that of the American Trias reveals the fact that while only 3 species, *Cheirolepis Münsteri*, *Pagiophyllum peregrinum* and *Palissya Brownii*, are common to the two, there are 14 genera that occur in both. In the number of species the two floras as now known are almost equal, that of the American Trias numbering 119, while that of the Portuguese Jurassic numbers 122. It is therefore important to note in what proportions these 14 genera occur in the two floras:

GENERA COMMON TO AMERICAN TRIAS AND JURASSIC OF PORTUGAL.

Genera.	Number of Species. American Trias.	Number of Species. Jurassic of Portugal.
Baiera	3	1
Brachyphyllum	1 ?	5
Cheirolepis	2	1
Chondrites	3	1
Cladophlebis	7	8
Clathropteris	2	1
Equisetum	6	5
Otozamites	4	3
Pagiophyllum	6	5
Palissya	3	2
Pecopteris	1	4
Podozamites	2	3
Schizoneura	5	1
Voltzia	1	2

When we consider that the two horizons do not at all overlap and that more than three-fourths of the Portuguese plants come from the uppermost members of the Jurassic, it is not to be expected that the correspondence will be very close; and accordingly we not only miss in the Portuguese flora some of the largest American genera, such as Acrostichites, Ctenophyllum, and Pterophyllum, but also some of the most striking and abundant forms, such as Macrotæniopteris, while on the other hand no monocotyledons occur in the American Trias so far as known, and the two largest genera of ferns in the Portuguese Jurassic, Sphenopteris and Scleropteris, are entirely wanting in the American Trias.

THE CRETACEOUS FLORA.

The Cretaceous flora of Portugal has much greater interest for the student of American paleobotany than the Jurassic flora, which has just been considered. First, because, as now known, it is considerably larger, numbering 199 species, but chiefly because we have in America a large number of plant bearing deposits that correspond so closely with those of Portugal that a comparison may be legitimately made that furnishes valuable results. It is true that our American Lower Cretaceous flora has now been so extensively worked that it has assumed relatively large proportions, numbering, so far as known, over 800 species. The Potomac formation alone furnishes no less than 737. The interest is still further heightened by the fact that in the Lower Cretaceous of both Portugal and America, the plant bearing beds occur at a number of distinct horizons, which may not without profit be directly compared in the two countries. For example, the Potomac formation now furnishes at least five distinct horizons from which fossil plants have been obtained, the lowest being that of the James River, which may extend as low as the top of the Jurassic. The next higher is that so well known at Fredericksburg, Virginia, and other points on the Rappahannock and Potomac Rivers. The third is the Mount Vernon clays which directly overlie the last named and have furnished a distinct flora. The fourth is well developed in the vicinity of Aquia Creek, the plant bearing beds near Brooke, Virginia. The fifth is undoubtedly much higher, and there appears to be a considerable thickness of non-fossiliferous deposits intervening between the last named and those plant bearing beds that have been discovered on the eastern side of the District of Columbia and at other points near Washington, on the Severn River, and on the Eastern Shore of the Chesapeake Bay, which have furnished a flora substantially identical with that of the Amboy clays on the Raritan River and of Staten Island, Long Island and Marthas Vineyard, as well as of the Tuscaloosa formation of Alabama.

The Lower Cretaceous of Portugal is subdivided into a very similar series of plant bearing deposits. One locality, Valle-de-Brouco, is referred by Choffat to the Infravalanginian, which is at the very base of the Neocomian and corresponds well with our James River series. An important plant bearing locality between Matta and Valle-de-Lobos is regarded as Valanginian or Neocomian. It may be compared with the Fredericksburg beds of the Potomac formation. The beds of Almargem, which have furnished many species, overlie the recognized Urgonian and probably belong to the upper portion of that subdivision, or possibly to the base of the next one called by the French geologists the Aptian. It corresponds quite closely with the Kome beds of Greenland and may be compared with the Mount Vernon clays of the Potomac formation, though it is probably higher. Then there is a series of beds in the vicinity of Torres-Vedras, viz., at S. Sebastião, Quinta-da-Fonte-Nova, Forca, Quinta-do-Chafariz, Portella-da-Villa, etc., and another series in the vicinity of Cercal and Zambujeiro, which are classed as Aptian, between which and the last named there is a considerable interval, including marine deposits belonging to the Urgonian. Certain other beds, as at Caixaria and Caranguejeira, are less definitely fixed geologically, but probably belong to about the same horizon. The Aptian of the French geologists lies between the Urgonian below and the Albian above, and corresponds in the main with the lower Greensand of England. It may be compared with those deposits of the Potomac formation near Aquia Creek called the Brooke beds by Professor Fontaine, which have yielded a large number of fossil plants, including such well-marked dicotyledons as Celastrophyllum and Sapindophyllum.

Above these beds there is an abundant plant locality at Buarcos, which is classed as Albian, and still higher others at Nazareth, Alcanede and Monsanto, also regarded as Albian, but as belonging to that uppermost member called Vraconnian. The Albian corresponds in a general way with the Gault and is the uppermost section of the Lower Cretaceous, the overlying beds being Cenomanian, which is the lowest subdivision of the Upper Cretaceous. These Albian plant bearing beds may be roughly compared with what has been called in America the Amboy clays, but which has recently been more correctly named by Professor William B. Clark the Raritan formation. In America, as in Portugal, this deposit may also be divided into two parts, a lower and an upper, the former consisting of the beds along the Raritan, which themselves have a considerable thickness and show marked changes in the flora, while to the latter belong the deposits on Staten Island, Long Island and Martha's Vineyard, which have yielded large collections chiefly from indurated nodules formed in red clay.

Finally, in the Valley of Alcantara, at Padro, Pombal and Villa-Verde-de-Tentugal, there are plant bearing beds belonging to the Cenomanian. It is possible that these latter may not be higher than those of Long Island and Gay Head.

The floras of the several horizons in the Lower Cretaceous of Portugal differ less in their abundance than those of the Jurassic; the largest is that of the Valanginian, amounting to 86 species or over 43 per cent.; the Urgonian has yielded only 25 species or 12 per cent., the Aptian 42 species or a little more than 21 per cent., the Lower Albian 58 species or over 25 per cent., and the Upper Albian or Vraconnian 28 species or 14 per cent. The striking coincidence of the parallelism between these horizons and those of the Potomac formation in America is still further heightened by the circumstance, accidental perhaps, that the numerical proportion existing between the species now known at the corresponding horizons in America is very nearly the same. The Basal Potomac, corresponding to the Vraconnian, has yielded 329 species or a little over 44 per cent.; the Mount Vernon clays, which were compared with the Urgonian, 42 species or somewhat less than 6 per cent.; the Aquia Creek beds, corresponding to the Aptian, 137 species or rather more than 18 per cent.; the Raritan beds and their equivalents, compared to the lower Albian, 264 species or nearly 36 per cent.; and the uppermost beds of Marthas Vineyard, Long Island and Staten Island, which may be called the Island Series and compared to the Vraconnian, 133 species or 18 per cent. These results may be put in the following tabular form :

LOWER CRETACEOUS OF PORTUGAL.		POTOMAC FORMATION OF THE UNITED STATES.	
HORIZONS.	PER CENT.	HORIZONS.	PER CENT.
Vraconnian . . .	14	Island series . . .	18
Lower Albian . .	29	Amboy Clays, etc. .	36
Aptian	21	Aquia Creek (Brooke) Series .	18
Urgonian	12	Mt. Vernon Clays .	6
Neocomian	43	James and Rappahannock Series .	44

It will be remembered that the Mount Vernon clays have been very little developed as yet, and when this florula is thoroughly known it will probably fully equal that of the Almargem beds of Portugal, relatively to the total Potomac flora.

Taking the Cretaceous flora of Portugal as a whole, exclusive of the Cenomanian, it is found to consist of 4 algæ, 1 species of Isoetes, 3 of Lycopodites, 1 of Equisetum, 80 of ferns, 15 of cycads, 26 of conifers, 4 of anomalous types, classed by the author under the head of Proangiosperms, 18 of monocotyledons, 41 of dicotyledons, and 6 of forms of uncertain affinity.

It will be seen that as in the Jurassic, so in the Cretaceous the ferns predominate; and of these, 32 species belong to the genus Sphenopteris and 10 to Cladophlebis; 7 of the cycads belong to the genus Podozamites, and 3 to Glossozamites. The conifers are much more evenly distributed, there being 4 species of Brachyphyllum, and 3 each of Sphenolepidium and Thuyites, while a large number of genera have only one or two species ; among these are Abietites, Baiera, Cheirolepis, Frenelopsis, Pagiophyllum, Palæocyparis, Palæolepis, Sequoia and Widdringtonites. The genera referred to the Proangiosperms are Changarniera, Eolirion, Yuccites, Delgadopsis and Protorhipis, some of which will require special mention further on. Half of the monocotyledons consist of grass-like objects referred to poacites, some of which he classes under the Proangiosperms, and others as true monocotyledons. The dicotyledonous flora is here well developed, but most of the forms occur in the Albian. Seven species are referred to a new genus, Proteophyllum, a name too near Protophyllum of Lesquereux, and Proteæphyllum of Fontaine, but the forms are different from both these ; 4 to the new genus Dicotylophyllum, and 3 each to Eucalyptus and Salix.

In comparing the Cretaceous flora of Portugal with that of America it is true that we only find a few species that are common to the two countries, really only five, as follows :

Pecopteris Brauniana Dunk.

Sphenolepidium Kurrianum (Dunk.) Heer.

Sphenolepidium Sternbergianum (Dunk.) Heer.

Sphenopteris Mantelli Brongn.

Sphenopteris valdensis Heer,

the last of which only occurs doubtfully in the Trinity of Texas.

Add to these *Sequoir subulata*, of which a very near variety *lusitanica*, has been found in the Portuguese beds.

We should not, of course, expect the species to be common to any great extent, and the comparison is practically limited to the genera. Looked at from this point of view, we see that the resemblance is indeed close, a great number of the important genera occurring in both floras. There are no less than 46 of these common to the two, though in some cases the author's individuality is probably alone responsible for slight differences of termination in the names. For example, forms referred to Baiera by one would be referred to Baieriopsis by the other, and so with Ctenis and Ctenidium, Myrsine and Myrsinophyllum, Oleandra and Oleandridium, Salix and Saliciphyllum, Thuya and Thuyites, etc.

Many of these genera, when we consider the difference in the size of the two floras, occur in both countries in nearly the same proportion. For example, of Aralia we have in Portugal 2 species, in America 11; of Brachyphyllum, in Portugal 4, in America 9; of Cladophlebis, in Portugal 10, in America 25; of Frenelopsis, in Portugal 2, in America 6; of Laurus, in Portugal 2, in America 8; of Myrica, in Portugal 2, in America 11; of Podozamites, in Portugal 7, in America 15; of Sphenolepidium, in Portugal 3, in America 9, etc. There are, of course, some cases in which the proportion is not the same. Thus, only one species of Magnolia occurs in the Portuguese beds, while in America we have 12, and on the other hand the largest Portuguese genus, Sphenopteris, represented there by 32 species, counts in America only 8 species. But here it may be supposed that the true representative in America of the Sphenopteris type of Portugal is really that exceedingly abundant genus Thyrsopteris, which numbers 40 species in the American beds. This would restore the relative proportions. On the whole, then, it may be considered that the Lower Cretaceous flora of Portugal is botanically speaking a very close repetition of that of America; and in view of the fact that in both countries a number of distinct horizons showing the progressive change in the flora throughout that period have yielded fossil plants in such a way that each of these florules may also be compared, the interest in the subject is almost fascinating.

ARCHETYPAL ANGIOSPERMS.

SPACE will only permit the consideration of one other important aspect, viz., a comparison of the dicotyledonous forms in the two countries, together with those ancestral types which the Marquis Saporta regards as prophetic of that great group of plants. This last question may be considered first. He finds among the specimens certain forms which he refers to the genus Protorhipis of Andræ. This genus was founded in 1855 upon some remarkable forms from the Lias of Steierdorf in Banat, Hungary,* which Andræ regarded as a fern and placed under the Pecopterideæ. He compares it with Jeanpaulia, which has since been proved identical with Baiera and correctly referred to the Coniferæ; also to Cyclopteris, Comptopteris, Diplodictyum, and Thaumatopteris, among fossils, and to Platycerium, among living ferns.

When I first saw the figure of his *Protorhipis Buchii*, I had grave doubts of its being a fern and fully believed that it represented some higher type of vegetation. I am, therefore, not surprised that the Marquis Saporta has arrived at the same conclusion, and am highly gratified that he has had the courage to give it publicity, notwithstanding the fact that Schimper, Schenk, Heer and Nathorst have all been content to regard it as a fern of the type of Drynaria, Platycerium, Allosorus, Clathropteris and the other living and fossil forms already mentioned.

In 1865 Zigno discovered another species, which, however, differs in a marked manner from the original of Andræ, having the margin entire. It is a small, deeply kidney-shaped leaf resembling that of some species of Asarum and was named *P. asarifolia*. This comes from the Oolite of Italy.†

*Lias-Flora von Steierdorf im Banate, by C. J. Andræ, Abhandl. geol. Reichsanst., Vol. II., Abth. 3, No. 4, 1855, pp. 35–36, pl. viii., fig. 1.

†Fl. Foss. Form. Oolithicæ, Vol. I, 1865, p. 180, pl. ix., fig. 2, 2a.

The forms described by Nathorst in 1878,* though much smaller are otherwise similar to *P. Buchii*, and Nathorst at first proposed to refer one of them to that species, but later concluded that it was distinct and made two species, *P. integrifolia* and *P. crenata*.

In 1880 Heer described another small cordate form from the Oolite of Siberia. It is similar to Zigno's species and was named *P. reniformis*.† Two years later, however, he found another similar form in the Kome beds, Urgonian, which is rather cordate than reniform and which he called *P. cordata*.‡ Both these forms have the margin entire.

Saporta in this work has revised all these forms and comes to the conclusion that they cannot be ferns, and although the original *P. Buchii* and both of Nathorst's species so closely resemble dicotyledonous leaves and are somewhat comparable in nervation to Credneria and some fossil Viburnums, as well as to such living genera as Glechoma and Chrysosplenium, still he hesitates to class them in that group. He has carefully refigured both of Nathorst's specimens, and also one that Nathorst figured without naming but regarded as probably a monocotyledon, but which Saporata considers to belong to the same type and calls *P. Nathorstii*. And these he carefully compares with the Portuguese form which he names *P. Choffati*, and classes the whole in special group which he long ago created and denominated the Proangiosperms, as representing the forerunners of both the monocotyledons and dicotyledons. The Portuguese species comes from Cercal, which Choffat places in the Aptian; it is therefore probably somewhat higher than the Kome beds of Greenland from which Heer derives one of his species; all the others, of course, are of far more ancient origin, viz., Jurassic, and it is not to be wondered at that no one should have ventured to refer them to any modern type.

Of the other four genera referred to this group, viz., Changarniera, Yuccites, Delgadopsis and Eolirion, the first two come from the Valanginian (Neocomian) of S. Sebatião, the third from the Aptian of Cercal, and the last from the Albian of Buarcos. They all seem to be ancestral monocotyledons. Delgadopsis occurs in two forms: first, as a sort of culm or broad striate stem; and secondly, in the form of a jointed rhizome, the swollen joints emitting innumerable rootlets, which, when absent, leave peculiar scars.

Choffatia Francheti, regarded by the author as a dicotyledon, is also a very remarkable plant, and has been aptly compared by him to certain euphorbiaceous forms, such as Phyllanthus. It also resembles some species of Euphorbia. It seems to be a floating aquatic, and specimens with the fibrous roots occur in the collection. In some of these descending fibers occupy one side of the stem or rachis, while the floating or aerial leaves occupy the other.

Upon the whole, it cannot be said that any of these higher types, found below the Albian, and corresponding in age to our middle and older Potomac, very closely resemble the plants of the same general class from the American beds of that age, and yet there are certain Potomac forms referred by Professor Fontaine to Menispermites, Hederæphyllum, Proteæphyllum and Populophyllum, whose areolate nervation somewhat resembles that of *Protorhipis Choffati*. The new genus Dicotylophyllum, of which he finds four species in the Aptian of Cercal, and which he very properly regards as a true dicotyledon, somewhat resembles the Protorhipis, but lacks the peculiar areolate

*Fl. Bjuf. Heft 1, p. 42; Heft 2, p. 57, pl. ix., figs. 2, 4.

† Fl. Foss. Arct., Vol. VI., Abth. 1, Pt. 1, p. 8, pl. 1, fig. 4a.

‡ Ibid., Abth. 2, p. 11, pl. iii, fig. 11.

nervation. These leaves are all quite small, but show a somewhat distinct midrib, and usually 2–4 lateral primaries. In form they recall some species of Vitis or Cissities, and *D. cerciforme,* while not resembling Cercis, as the specific name would imply, has many of the characteristics of Hedera. It may be roughly compared with Professor Fontaine's Vitiphyllum from the Potomac of Baltimore, and except in size *D. hederaceum* and *D. corrugatum* are fairly comparable with *Populophyllum reniforme* (cf. Fl. Pot., pl. clvi., f. 3).

In the Albian beds of Buarcos, and especially in the Vraconnian of Nazareth, we begin to find some of the higher types. But the genus Proteophyllum has still a very ancient appearance with a more or less areolate nervation. It is a narrowly lobed leaf, remotely recalling in its general form some species of Dewalquea. It may be possible to trace this form into his *Aralia calomorpha* from the same beds. His *Adoxa prœatavia* is a very peculiar plant, which also reminds one of Vitiphyllum Font., although none of the species of the latter genus which show the branching character have yet been figured. His *Braseniopsis venulosa* has some of the characteristics of Protophyllum of Lesquereux, but is usually smaller and always entire; the nervation is also different, except at the base of the leaf, which has a large expansion below the summit of the petiole, as in Protophyllum. *Myrsinophyllum revisendum* will doubtles have to be revised. It is much like Potomac forms that have been referred to Myrica (e. g., *M. brookensis*) and Celastrophyllum. It is entirely different from the *Myrsine borealis* of Heer, which, with two other species, occur in the Amboy clays and Tuscaloosa formation. His *Geranium lucidum* is an exceedingly definite and handsome form, but it is hard to separate it generically from his *Cissites sinuosus,* and all of these seem to be analogous to our Vitiphyllum. His *Menispermites cercidifolius,* though much smaller, is not unlike Professor Fontaine's *M. Virginiensis,* especially the smaller forms which I have found in the Mt. Vernon clays. His *Aralia proxima* can scarcely be distinguished from *M. Wellingtoniana* of the Dakota group, more common in the Newer Potomac.

It is only in the Nazareth beds (Vraconnian) that we find the typical Amboy Clay flora. Here we have the Eucalyptus, Laurus (Laurophyllum), Salix, Myrsinophyllum, Sapindophyllum, etc., some of which are probably specifically identical with forms described by Newberry, and it is altogether probable that if the posthumous work of Dr. Newberry, now in press, had been in the hands of the present author a large number of the species would have been identified with American forms.

I will only notice one other significant fact. In the Cenomanian beds which overlie these last, as it would seem unconformably, but which may not be so widely separated from them as has been supposed, there occurs a large elongated leaf which the Marquis has called *Chondrophyton laceratum.* It agrees only in its finer nervation with *C. dissectum* Sap. and Mar., the only other species.* It has a very delicate nervation with small polygonal meshes, and an entire paryphodrome margin, but the remarkable fact is that it seems to have a deeply retuse summit. It is evident that from the specimen the author was unable to make this latter out with certainty; but he has drawn the marginal lines so as distinctly to indicate it. So desirous was he that this leaf should be correctly represented that he has given us two interpretations from drawings made at different times, figs. 4, 5 of pl. xxxviii. He states that he considers figure 5 to represent the form better than figure 4; and it is in this

*L. Évolution du Règne Végétal. Par Saporta et Marion. Les Phanérogames, Vol. II., Paris, 1885, p. 120, fig. 126.

that the terminal lobation is most clearly shown. A comparison of this figure with the numerous specimens of *Liriodendropsis simplex* of Newberry leaves no doubt whatever that the Portuguese plant is at least a congener of the American plant, and it is just possible that it may belong to the same species. As this form has been three times published* it is a little surprising that Saporta did not think to compare it with the Portuguese plant. There are differences in the finer nervation, but this is also perceptible between his two drawings of the same specimen; these also differ in different specimens of the American plant, and one or two other species remain to be published. When all the material is illustrated most of these differences will disappear. If any remain it can be ascribed to difference of age and geographical position.

LESTER F. WARD.

WASHINGTON.

EXPLANATION OF ACQUIRED IMMUNITY FROM INFECTIOUS DISEASES.†

It has long been known that, in a considerable number of infectious diseases, a single attack, however mild, affords protection against subsequent attacks of the same disease; that in some cases this protection appears to be permanent, lasting during the life of the individual; that in others it is more or less temporary, as shown by the occurrence of a subsequent attack.

The protection afforded by a single attack not only differs in different diseases, but in the same disease varies greatly in different individuals. Thus certain individuals have been known to suffer several attacks of small-pox or of scarlet fever, although, as a rule, a single attack is protective. Exceptional susceptibility or insusceptibility may be not only an individual but a family characteristic, or it may belong to a particular race.

In those diseases in which second attacks are not infrequent, as, for example, in pneumonia, in influenza or in Asiatic cholera, it is difficult to judge from clinical experience whether a first attack exerts any protective influence. But from experiments upon the lower animals, we are led to believe that a certain degree of immunity, lasting for a longer or shorter time, is afforded by an attack of pneumonia or of cholera, and probably of all infections due to bacterial parasites. In the malarial fevers, which are due to a parasite of a different class, one attack affords no protection, but rather predisposes to a subsequent attack.

In those diseases in which a single attack is generally recognized as being protective, exceptional cases occur in which subsequent attacks are developed as a result of unusual susceptibility or exposure under circumstances especially favorable to infection. Maiselis has recently (1894) gone through the literature accessible to him for the purpose of determining the frequency with which second attacks occur in the various diseases below mentioned. The result is as follows:

	Second Attacks.	Third Attacks.	Fourth Attacks.	Total.
Small-pox . .	505	9	0	514
Scarlet fever .	29	4	0	33
Measles . . .	36	1	0	37
Typhoid fever .	202	5	1	208
Cholera . . .	29	3	2	34

Recent researches indicate that the principal factor in the production of acquired immunity is the presence, in the blood of the immune animal, of some substance capable of neutralizing the toxic products of the particular pathogenic microörganism

* Bull. Torr. Bot. Club, Vol. XIV., New York, Jan. 1887, p. 6, pl. lxii, figs. 2, 3, 4; Am. Journ. Sci., Vol. XXXIX., New Haven, February, 1890, p. 98, pl. ii., figs. 6, 7; Trans. N. Y. Acad, Sci., Vol. XI., 1892, p. 102, pl. ii., figs. 2–7, 9.

† Abstract of a paper read before the Biological Society of Washington, March 9, 1895.

against which immunity exists, or of destroying the germ itself.

The substances which destroy the toxic products of pathogenic bacteria are called antitoxins. As pointed out by Buchner in a recent paper, the antitoxins differ essentially from the so-called alexins, to which natural immunity is ascribed. The alexins are characterized by their germicidal and globulicidal action—they destroy both the red corpuscles and the leucocytes of animals belonging to a different species from that from which they have been obtained, and by their coagulability and instability — destroyed by sunlight and by a temperature of 50° to 55° C. On the other hand, the antitoxins best known (diphtheria and tetanus) have no germicidal or globulicidal action; they resist the action of sunlight and require a temperature of 70° to 80° C. for their destruction.

Our knowledge of the antitoxins dates from the experiments made in the Hygienic Institute of Tokio, by Ogata and Jasuhara, in 1890. These bacteriologists discovered the important fact that the blood of an animal immune against anthrax contains some substance which neutralizes the toxic products of the anthrax bacillus.

In the same year (1890) Behring and Kitasato discovered that the blood of an animal which has an acquired immunity against tetanus or diphtheria, when added to a virulent culture of one or the other of these bacilli, neutralizes the pathogenic power of such cultures, as shown by inoculation into susceptible animals. And also that cultures from which the bacilli have been removed by filtration, and which kill susceptible animals in very small amounts, have their toxic potency destroyed by adding to them the blood of an immune animal, which is thus directly proved to contain an antitoxin which comparative experiments show not to be present in the blood of non-immune animals.

During the past two or three years numerous additional experiments have been reported which confirm the results already referred to, and show that immunity may be produced in a similar manner against the toxic products of various other pathogenic bacteria—the typhoid bacillus, the 'colon bacillus,' streptococcus pyogenes, staphylococcus pyogenes aureus and albus, etc.

The Italian investigators, Tizzoni and Centanni, in 1892, published a preliminary communication in which they gave the results of experiments which appear to show that in guinea-pigs treated with tuberculin, by Koch's method, a substance is developed which neutralizes the pathogenic potency of the tubercle bacillus. Professor Tizzoni and his associate, Dr. Schwarz, have also (1892) obtained evidence that there is an antitoxin of rabies. Blood-serum taken from a rabbit having an artificial immunity against this disease was found to neutralize, *in vitro*, the virulence of the spinal marrow of a rabid animal after a contact of five hours.

Professor Ehrlich, of Berlin, in 1891, published the results of some researches which have an important bearing upon the explanation of acquired immunity, and which show that susceptible animals may be made immune against the action of certain toxic proteids of vegetable origin, other than those produced by bacteria; also that this immunity depends upon the presence of an antitoxin in the blood-serum of the immune animals.

The experiments of Ehrlich were made with two very potent toxalbumins—one ricin, from the castor-oil bean; the other, abrin, from the jequirity bean. The toxic potency of ricin is somewhat greater than that of abrin, and it is estimated by Ehrlich that 1 gm. of this substance would suffice to kill one and a half million of guinea-pigs. When injected beneath the skin in dilute solution it produces intense local inflammation, resulting in necrosis. Mice are less susceptible than guinea-pigs, and are more easily made immune. This is most readily accomplished by giving them small and gradually increasing doses with their food. As a result of this treatment the animal resists subcutaneous injections of 200 to 300 times the fatal dose for animals not having this artificial immunity.

Ehrlich gives the following explanation of the remarkable degree of immunity established in his experiments by the method mentioned:

"All of these phenomena depend, as may easily be shown, upon the fact that the blood contains a body—antiabrin—which completely neutralizes the action of the abrin, probably by destroying this body."

In a later paper (1892) Ehrlich has given an account of subsequent experiments which show that the young of mice which have an acquired immunity for these vegetable toxalbumins may acquire immunity from the ingestion of their mother's milk; and also that immunity from tetanus may be acquired in a brief time by young mice through their mother's milk.

A most interesting question presents itself in connection with the discovery of the antitoxins. Does the animal which is immune from the toxic action of any particular toxalbumin also have an immunity for other toxic proteids of the same class? The experimental evidence on record indicates that it does not. In Ehrlich's experiments with ricin and abrin he ascertained that an animal which had been made immune against one of these substances was quite as susceptible to the toxic action of the other as if it did not possess this immunity, *i. e.*, the anti-toxin of ricin does not destroy abrin, and *vice versa*.

We have also experimental evidence that animals may acquire a certain degree of immunity from the toxic action of the venom of the rattlesnake. This was first demonstrated by Sewall (1887), and has been recently confirmed by Calmette (1894). In his paper detailing the results of his experiments the author last named says:

"Animals may be immunized against the venom of serpents either by means of repeated injections of doses at first feeble and progressively stronger, or by means of successive injections of venom mixed with certain chemical substances, among which I mention especially chloride of gold and the hypochlorites of lime or soda.

"The serum of animals thus treated is at the same time preventive, antitoxic and therapeutic, exactly as is that of animals immunized against diphtheria or tetanus.

"If we inoculate a certain number of rabbits, under the skin of the thigh, with the same dose, 1 millgr. of cobra venom, for example, and, if we treat all of these animals, with the exception of some for control, by subcutaneous or intraperitoneal injections of the serum of rabbits immunized against four millgrs. of the same venom, all of the control animals not treated will die within three or four hours, while all of the animals will recover which receive 5 c. c. of the therapeutic serum within an hour after receiving the venom."

As a rule the antitoxins have no bactericidal action; but it has been shown, by the experiments of Gamaleïa, Pfeiffer and others, that in animals which have an acquired immunity against the spirillum of Asiatic cholera and against spirillum Metchnikovi there is a decided increase in the bactericidal power of the blood-serum, and that immunity probably depends upon this fact.

Certain important questions present themselves in connection with the production of antitoxins and germicidal substances in the blood of immune animals, one of which is: Is the production of the antitoxin continuous while immunity lasts, or does it occur only during the modified attack which results from inoculation with an attenuated virus, or of filtered cultures, the antitoxin being subsequently retained in the circulating blood? The latter supposition does not appear very plausible, but it must be remembered that these antitoxins do not dialyze—*i. e.*, they do not pass through animal membranes—and consequently would not readily escape from the blood-vessels, notwithstanding the fact that they are held in solution in the circulating fluid. On the other hand, the passage of the tetanus antitoxin into the mother's milk would indicate a continuous supply, otherwise the immunity of the mother would soon be lost. Further experiments are required to settle this question in a definite manner, and also to determine the exact source of the antitoxins in the animal body and the *modus operandi* of their production.

GEO. M. STERNBERG.

WASHINGTON.

REMARKING THE MEXICAN BOUNDARY.

MR. A. T. MOSMAN, assistant in the U. S. Coast and Geodetic Survey, one of the commissioners on the part of the United States, presented an interesting summary of the work at a meeting of the National Geographic Society in Washington on the 8th inst.

At the initial meeting of the commissioners for the two countries, it was agreed that any of the old monuments recovered should be taken as defining the line; that new monuments should be interpolated between them, so that no two monuments should be more than 8000 metres apart, as required by the new treaty. The line had been marked under the treaty of 1853, by 52 monuments; the commissioners found 38 of these standing in 1891. On the parallels the new monuments mark the curve of the parallel, but on the oblique lines the monuments recovered were not accurately located on the line joining their extremities, and the boundary on these lines as now marked is, therefore, a broken line. Old monuments were recovered at all important points on the boundary, including all points where the line changed direction, but the distances between them were unequal, and in one instance exceeded 100 miles. The line from El Paso on the Rio Grande to San Diego on the Pacific, 700 miles, is now defined by 258 monuments.

The field work required the redetermination of the geographic positions of the old monuments recovered, and presents some interesting comparisons showing the facility and certainty of modern methods. The longitudes of the old monuments were determined by Emory from transits of the moon and moon culminating stars. In the relocation the longitudes were determined by the telegraphic method, connected with the geodetic work of the Coast Survey by coast survey parties working in conjunction with the commissioners. The greatest difference developed from Emory's positions was 4′ 34″.3 with other differences of 34″ and 54″ and still smaller quantities showing the old work to have been remarkably good for the method. The latitude stations in the new work were about 20 miles apart over the whole line, and at each station an azimuth was observed on Polaris near elongation to start the direction for the new tangent for the parallel and check the tangent ending at the station. The latitude observations were made with the zenith telescope formerly used on the N. W. boundary, but improved with new micrometer and levels. The telescope has a focal length of 826 mm., and the objective a clear diameter of 67 mm. A new departure was made in mounting the instrument on a wooden pier constructed in a simple form, readily transported. Its stability proved as great as a brick or stone cemented pier, as it was not uncommon to secure a whole night's work without relevelling, and the instrument invariably remained for several hours with level correction less than one div.$=1''.28$. The probable errors of the latitude determinations from the U. S. observers $= \pm\ 0''.03$ to $0''.4$. The Mexican observations have not yet been received. The plan of operations agreed upon required independent determinations by the representatives of both governments. This was not practicable in the longitude determinations, but in the latitudes, running the parallels and locations of the numerous monuments, it was strictly carried out. The mean difference in the location of the 258 monuments, was less than three-tenths of a metre ; the maximum difference was only 1.8 m., which occurred in locating a point about midway between two old monuments 100 miles apart, and over a very rough mountainous country, where the distances between water holes was over 60 miles. The angular variations of the lines run by the two parties at this point was a little more than three seconds.

The final results from the astronomical observations were required for immediate use on the ground; to permit the computations the mean declinations for the stars for latitude had been furnished by Professor T. H. Safford, of Amherst. In this way the latitude and azimuth were always available within three or four days after the observations were completed, a feature of such work that, it is believed, has not heretofore been attempted. Mr. Mosman promises that a list of the stars furnished by Professor Safford, some 600, will be published in the report of the commission, to be available for future work in the same latitude.

In locating the intermediate monuments the commission made use of the stadia, with gratifying results. On the parallel of 31° 47′ for a distance of 100 miles both chain and stadia were used for the purpose of comparison. It was found that the stadia was much more reliable than the chain, even on the desert, and in a rough country was much superior. The whole line was measured by both the American and Mexican engineers independently ; when the two results for any distance differed more than one part in 500, remeasurements were made by steel tape or triangulation to discover the error. Many lines determined by triangulation were compared with the lengths determined by stadia, and the results showed that the stadia measurement could be relied on within one part in 1000. One line of 45 miles measured over rolling sand hills differed by one part in 1800 only.

In addition to the astronomical work, a strip of topography was surveyed on the American side $2\frac{1}{2}$ miles wide, and a line of levels was run with the wye level from the Rio Grande to San Diego, giving the elevation of each monument above mean tide of the Pacific Ocean. The levels were checked at Yuma with R. R. levels from San Francisco, showing the infinitismal discrepancy of two hundredths of a metre, probably an accident. At the Rio Grande there is a discrepancy of about two metres, but the datum plane for the R. R. levels at this place is not known.

O.

THE NATURE OF SCIENCE AND ITS RELATION TO PHILOSOPHY.

If any one should ask me, ‘What is physics?’ I would tell him to study in the physical laboratory for ten years and then what he had learned by the time he was through would be the nearest he could get to an answer to the question. So to the question, ‘What is science?’ I can give no other general answer than that to anyone it is just what he knows about it. I can, however, give as a particular answer what I have in my own experience found science to be.

Science consists of weighing evidence and stamping each statement with an index of its reliability. That the sun moves around the earth is, according to the evidence at present produced, a statement with a reliability of 0. That the earth moves around the sun, we at the present day stamp as certain. That Mars contains living beings is to-day stamped as quite improbable. On the scale of probability where 0 means not at all probable, and 1 means secure, $\frac{1}{2}$ means indifferent, we might say that such a statement regarding Mars would have a probability perhaps of $\frac{1}{25}$.

The difference between the unscientific and the scientific mind lies in the extent of evidence. The woman who lately left a fund for a prize to the one who shall establish communication with Mars had gathered enough evidence to give, in her mind, a high degree of probability to the supposition of the possibility of such an undertaking. And yet the members of the French Academy who accepted the money in the sense that it should go to the one making the best contribution to our knowledge of Mars were evidently in possession of enough further evidence to attach a very small degree of probability to the supposition.

This is the actual work of all the sciences. We cannot and dare not make statements except just so far as warranted by the facts. If you say that the act of discrimination increases the time of thought, the psychologist must answer yes, with a high degree of probability, because carefully collected experimental evidence points that way. If you say that consciousness is continuous during sleep, the psychologist must answer that reliable evidence is lacking, and that he is entitled to no opinion either way.

We often hear, from philosophers of the old school, the statement that the facts of the universe are divided into classes, each of which is given over to a science for investigation regarding details, while the general conclusions are reserved for the philosophers.

I must object to the limitation of science to the investigation of individual facts. Many of the problems with which a scientist is most directly concerned are the most general of all. The subject of time is one to which the psychologist and the astronomer devote their special attention. There can hardly be anything more general than the great independent variable, as it is called. Likewise space forms a problem for geometry, physics and psychology.

As every scientist knows, an investigator in one science is forced to learn a dozen other sciences; the more he specializes, the more remotely must he go for his information. For example, the specialist in experimental psychology is obliged to be more or less familiar with the science of measurement, with the astronomical determination of time, with portions of meteorology, with physics, with portions of organic chemistry and physical chemistry, with statistics, ethics, anthropology, etc., etc. The mediæval philosopher likes to bottle things up and label them, but the modern sciences are too lively specimens for that process.

This brings me to the question of the relation of science to philosophy. According to Wundt the work of philosophy is to take up and discuss the most general questions, time, space, number, etc., which cannot be handled by the particular sciences.

But let us consider a moment. Suppose the U. S. Government wishes a report on Lake Tahoe. It would go to the geographer to learn where it is, to the U. S. Survey to learn its measurements, to the chemist to know its composition, to the meteorologist to inquire about its weather, to the land owners for the price of land, to the boatman to learn the sailing qualities, etc., etc. It would print the reports all side by side for each reader to assimilate as he would or could. What it would not do would be to send out a special agent who should look into these matters himself and make his own report. We very well know that such agents filter through more of themselves than of the facts; they see what they bring eyes to see, and no one can be master of a dozen sciences or trades.

Suppose, however, it is desired to have a treatment of the subject of 'time.' Wundt would propose that a special agent, called a philosopher, should gather up all he can from everybody and should present it as he thinks best. So with all the other fundamental questions. The result is that we have as many systems of philosophy as we have writers. Would it not be better to get the astronomer to present his experience with time, then the physicist to present his, then the psychologist, and so on? The reader can then assimilate what he is able, instead of accepting it as previously assimilated by the philosopher, as a kind of 'predigested' food.

A somewhat similar thought was spoken by Paulsen some years ago. I do not know if he has stated it in print. He considered that the day of philosophical systems was past; there could be text-books of philosophy as well as text-books of all sorts of things, but philosophy itself would consist of monographs by specialists.

Of course, on such conditions as these, we should be obliged to conclude that philosophy has no relation to the sciences and that, having the astronomer, the mathematician, the physicist, the geologist, the psychologist, the economist and all the others, we can entirely dispense with the philosopher.

E. W. Scripture.
Yale University.

'SCIENCE.'

[The following article, contributed by one of the original supporters of Science, will prove of interest to those who are not acquainted with the earlier history of the journal. All men of science are under very great obligations to Mr. Bell and Mr. Hubbard for establishing a weekly journal of science in America at a time when the conditions were less favorable than at present; to Mr. Scudder for the high standard maintained during his editorship, and to Mr. Hodges for his faithful and untiring efforts on behalf of the journal.

J. McK. C.]

In 1882 Mr. A. Graham Bell conceived the idea of establishing a scientific journal, which should do for America what 'Nature' does for England. For this purpose, he was willing to contribute, with the coöperation of Mr. Gardiner G. Hubbard, the sum of twenty-five thousand dollars, which, in the estimation of good judges, would be sufficient to start a weekly paper and put it on a paying basis. Mr. Bell furnished the larger proportion of this sum. Mr. Samuel H. Scudder, of Cambridge, Mass., became the editor. President Gilman, of Johns Hopkins; Major Powell, of the Geological Survey; Professor Newcomb, of the Nautical Almanac; Professor O. C. Marsh, of New Haven; and Professor Trowbridge, of Columbia College, agreed to give their advice, and to act with Messrs. Bell, Hubbard and Scudder as a Board of Directors. This board, representing different interests and localities, possessed great weight with the entire community, and was believed to be generally acceptable to scientists.

The first number of 'Science' appeared February 9, 1883, some six or eight months subsequent to the conception of the idea. Mr. Moses King, the first publisher, retired the succeeding September. Shortly after, Mr. C. L. Condit, formerly with the 'Nation,' took charge of the publishing department and continued until the spring of 1886. Mr. Scudder retired from the editorship in 1885 and was succeeded by Mr. N. D. C. Hodges, when the office was removed from Cambridge to New York. It was soon found that twenty-five thousand dollars was not sufficient, and Messrs. Bell and Hubbard continued to advance further sums until, in 1886, they had expended about seventy-five thousand dollars, without having made the paper self-supporting.

An arrangement was then made with Mr.

Hodges to assume the entire charge of SCIENCE for a fixed annual sum. For three years M. Hodges had charge of the paper, under the advice of the Board of Directors. Mr. Hodges made large reduction in expenses of publication, but unfortunately made a larger reduction in the subscription price, from five dollars to three dollars and fifty cents a year.

It was never the intention of Messrs. Bell and Hubbard to make a profit from the publication of SCIENCE, but they did expect its establishment to make a contribution to science.

The circulation of the journal, under the management of Mr. Hodges, largely increased, and the changes made by him and his associate editors, Messrs. D. G. Brinton, of Philadelphia, and Charles Platt, of Baltimore, whose services were given gratuitously were of great value. It was originally supposed that advertisements would contribute largely to its support, but they were not obtained, partly on account of the limited circulation, and more largely because advertisers preferred to publish in special journals rather than in one intended to meet the wants of the scientific public.

The publication of SCIENCE was stopped for a time a year ago, although its circulation was then larger than it ever had been, the stringency of the times preventing many from paying their subscriptions.

At the meeting of the American Association for the Advancement of Science, at Brooklyn in 1894, the renewal of the publication of SCIENCE was brought before the Association. A large committee was chosen to consider its usefulness, and the propriety of contributing towards its support. Mr. Hodges appeared and stated fully his views and plans; the Association then voted that a contribution of fifteen hundred dollars should be made for the purpose of enabling Mr. Hodges to continue its publication. Immediately after Mr. Hodges decided that he could not continue the publication, and therefore this arrangement fell through.

Subsequently the reorganization of SCIENCE was undertaken by Professor Cattell, of Columbia College, who will, we trust, make it a success.

It would not be proper to close this article without an acknowledgment of the great ability, untiring zeal and never flagging interest shown by Mr. Hodges in his connection with SCIENCE.

CORRESPONDENCE.

A CATALOGUE OF SCIENTIFIC LITERATURE.

EDITOR OF SCIENCE:—The admirable plan for a card catalogue of scientific literature recommended to the Royal Society by the Harvard University Council (reprinted in the current volume of SCIENCE, pages 184–186). strongly commends itself to users of scientific literature, and has already been adopted with minor modification by at least one national scientific society. A slight extension of the plan in one respect would seem, however, to be advantageous.

The body of scientific literature is vast and constantly increasing, and scientific authorship and publication are rapidly extending from country to country and from point to point in each country throughout the world. Population is increasing, and with it writing and printing increase; civilization is spreading, and with it literature is expanding in an increasing ratio; science is becoming increasingly important as a directing and controlling force in civilization, and so the growth of scientific writing outstrips that of non-scientific scripture; the domain of science is widening rapidly as research concerning every conceivable subject pushes into and illumines the penumbra of half-knowledge, and thus the subject-matter of scientific literature is differentiated. Moreover, the fashion of scientific publication is changing; few recent investigators spend years on a book, the masterpiece of a decade or a lifetime; most keep pace with the rapid progress of the times by issuing their chapters or sections as completed from time to time in the form of articles or brochures; and thus the average number of titles to be credited to individual authors is increasing. So the augmentation in scientific literature is many-branched and cumulative, and its rate is constantly augmenting. With the multiplication of scientific literature the need for comprehensive cataloguing is multiplied; yet with the multiplication the difficulty of measuring the teeming flood from the scientific press is increased in still larger measure. The task before the Royal Society is one of great magnitude.

It would seem that the success of the scheme for cataloguing scientific literature will depend largely on the intimacy of the relations to be established between the Royal Society, on the one hand, and (1) trade publishers, (2) non-commercial publishers, and (3) individual authors, on the other hand. Now, the basis for the relations between the central organization and trade publishers, and through them with the authors, is the simple one of financial interest; it is set forth in a satisfactory manner in the report of the University Council, who point out that it would be to the interest of the writers, as it would be also to that of the publishers, to prepare summaries suitable for carding by the central organization. In the case of this class of publishers, perhaps the leading interest would be that of the publishers themselves, who might accordingly be trusted to induce negligent authors to prepare the requisite summaries.

The non-commercial publishers include those issuing (a) periodicals put forth without hope of profit and often at individual sacrifice, which it would be useless to advertise in the ordinary way by reason of the limited number of possible subscribers; (b) proceedings, transactions and related serials published in limited editions by many scientific societies; (c) reports of official bureaus, like the U. S. Geological Survey and various State institutions, to whom increased distribution means no profit, but some loss in time, if not money; and (d) privately printed and irregularly published brochures, booklets and leaflets, commonly issued by the authors themselves. All of these classes of publications are important in this and several other countries; collectively, in this country at the present time, at least, they probably contain the major part of the material which should be catalogued by the Royal Society. To bring their contents within reach of a central organization would involve a wide-reaching and constant co-operation, which manifestly cannot be brought about through the ordinary financial stimulus, since the publication is not made on a commercial basis; it can be brought about, if at all, only through the inspiration of creative genius and authorial ambition. There are few scientific writers who would not be willing, indeed glad, to prepare summaries of their writings for the sake of securing wider publicity and more permanent record of their discoveries and ideas; for it is the laudable ambition for publicity and permanent record, for the good of men, that inspires the original writing, if not indeed the research itself. Many of the non-commercial publishers themselves are actuated by similar motives, and would be willing to incur the small tax of periodically sending summaries to the central organization, while others would doubtless be stimulated thereto by the authors themselves; yet, it is probable that so far as the non-commercial publications are concerned, the stronger bond of connection would be that between the central organization and the authors; and since the more natural relation is the hierarchic one, first from central body to the less numerous class and from this in turn to the more numerous, any device that would strengthen the relation between the central body and the publishers would be useful. Thus, it might be well for the Royal Society to furnish sets of cards pertaining to the specialty represented by the non-commercial publication, either in exchange simply for the periodical transmission of summaries or in return for such summaries and for printing in the advertising pages or elsewhere a standing notice of the Royal Society catalogue. The coöperation of the publishers in securing, and indeed in editing, the summaries would be highly desirable, partly because with most writers summaries or abstracts need editorial scrutiny more sadly than their ordinary writing. It may be noted also that in these days of the making of many bibliographies there is a special need for abstracts and summaries for a wide variety of purposes, and the recognition of this need will make easier the way of the Royal Society in putting its plans into execution. Partly for this reason there would seem to be a certain desirability in printing the brief summaries, perhaps in a distinctive type, in conjunction with scientific articles.

The Geological Society of America recently concurred in a report to the Royal Society conforming to that of the Harvard University Council, with a brief addition designed to facilitate obtaining summaries of articles from non-commercial publishers of scientific literature, this addition having been suggested by the writer as one of the committee on the subject.

W J McGee.

TEACHING BOTANY ONE TOPIC AT A TIME, ILLUSTRATED BY SUITABLE MATERIALS AT ANY SEASON OF THE YEAR.

Editor of Science—*Sir:* The recent papers in Science concerning the management of classes in botany prompt the following. In these times, of course, every true teacher of botany insists that his pupils shall study the objects before receiving much, if any, instruction from books or persons. I take it for granted that any teacher of a class beginning subjects that are treated in *Gray's Lessons* would prefer to take them up in about the sequence there given, but he will find it impossible to procure at any season of the year enough suitable material that is fresh to fully illustrate many of the sections of the book. For example, he cannot procure at any one time suitable materials to illustrate the section on stamens. The varieties there illustrated appear at different dates some weeks apart. So of the forms of pistils, the torus, fruits, etc. My plan has been to collect quantities of stamens of the barberry, sassafras, lobelia, cypripedium, mallow, locust, dandelion, lily, tulip tree, blueberry, sage, milkweed, and in most cases preserve each kind by itself in twenty-five per cent. alcohol, or in formalin one hundred of water to one of formalin. These are ready when we want to study stamens. A specimen or more of each kind of the preserved objects for illustrating any section of this subject can be placed in a small dish before each pupil in case fresh specimens cannot be procured. In many instances, when not allowed to dry, these can be gathered up and used for several successive classes.

In like manner, it is very satisfactory to be able, when fruits are to be studied, to have a good many kinds to illustrate the various sorts, such as half grown plums or cherries, the mandrake, bloodroot, violet, mulberry, winter-green, etc. Lessons in morphology can, in this way, be made more impressive than when some of the illustrations are used in one day and others in a week or a month.

W. J. Beal.

Agricultural College, Mich.

SCIENTIFIC LITERATURE.

Nicolái Ivánovich Lobachévsky.—Address pronounced at the commemorative meeting of the Imperial University of Kasán, October 22, 1893, by Professor A. VASILIEV, President of the Physico-Mathematical Society of Kasan.—Translated from the Russian, with a preface, by DR. GEORGE BRUCE HALSTED, President of the Texas Academy of Science.—Volume one of the neomonic series.—Published at The Neomon, 2407 Guadalupe Street, Austin, Texas, U. S. A. 1894. Sm. 8vo, pp. 8+40+17.

Within the last thirty years the name of Lobachevsky has become widely known as that of one of the earliest discoverers in the field of non-Euclidean geometry, a subject which has not only revolutionized geometrical science, but has attracted the attention of physicists, psychologists and philosophers.

Professor Vasiliev's life of Lobachevsky, which we welcome here in an English translation, is without question the best and most authentic source of information on this original mathematical thinker who spent his whole life in a remote Russian town, almost on the confines of civilization, and whose work began to be appreciated by the scientific world only after his death (1856). What lends a peculiar interest to the story of this uneventful life is its intimate association with the growth of the University of Kazàn. Lobachevsky entered this university as a student soon after its foundation, became, immediately after graduation, an instructor, and then a professor in it, was its president for nineteen years during its formative period, and contributed largely to its rise and progress through his administrative ability and untiring energy. This man, who is known abroad as an original investigator in one of the most abstruse branches of mathematics, endeared himself, moreover, to his townsmen in many respects as a progressive and public-spirited citizen, delivering popular lectures on scientific subjects, conducting evening classes in elementary science for workingmen, taking a most active part in the work of the Kazàn Economic and Agricultural Society, and so on.

It is due to these facts that the centennial celebration held by the Physico-Mathematical Society of the University of Kazàn, in 1893, in commemoration of his birth, was participated in not only by professional mathematicians, but also by the whole university and the citizens of Kazàn. It is for this occasion that Professor Vasiliev prepared his biography.

The celebration began with religious services in the University chapel, on Lobachevsky's one hundredth birthday, November 3 (or, according to the old calendar still used in Russia, October 22); at noon the University Senate assembled in solemn session, the foreign delegates were greeted by the president of the university, letters and telegrams of congratulation were read, and several addresses were made commemorating the life and work of the great Russian geometer. On the next day the Physico-Mathematical Society held a public session for the reading of various papers on subjects connected with non-Euclidean geometry. On the 5th of November the Municipal Council of the city of Kazàn dedicated with appropriate ceremonies a memorial tablet, inserted in the front wall of the house in which Lobachevsky had lived. Another meeting of the Physico-Mathematical Society brought the celebration to a close. A sum of several thousand rubles had been collected in the course of the year for the purpose of founding a Lobachevsky medal or prize to be awarded annually, and of erecting a bust of Lobachevsky at Kazàn, in the public square that bears his name.

It is well that this late justice should be done to the memory of a man who during his lifetime never received any public recognition for his scientific work. At the present time no competent mathematician doubts the value of Lobachevsky's investigations in non-Euclidean geometry. For those not familiar with modern mathematical thought it is, however, difficult, if not impossible, to fully appreciate the true value of this subject; they are inclined to attribute undue importance to its possible bearings on non-mathematical questions and to neglect and underrate what is most valuable.

The starting point for Lobachevsky's researches, as for those of all the earlier writers on non-Euclidean geometry (Saccheri, Lambert, the two Bolyais), is given by the theory of parallels in elementary plane geometry which is based by Euclid on his fifth postulate (usually called his "eleventh axiom"). This postulate refers to two lines cut by a transversal, and states that if the sum of the interior angles on one side of the transversal be less than two right angles the lines will meet on this side if sufficiently produced. The numerous attempts that have been made to make a theorem of this proposition, and to *prove* it, have always remained as futile as the attempts to square the circle. They have only shown that it can be replaced by other postulates, such as that only one parallel can be drawn to a given line through a given point, or that the sum of the angles of a triangle is equal to two right angles, etc.

Does it follow that these postulates express an absolute necessary truth? Certainly not. For it can be shown—and this is just what Lobachevsky did—that a perfectly consistent system of geometry can be constructed by rejecting Euclid's postulate and its equivalents, and assuming, say, that more than one parallel can be drawn to a given line through a given point, or that the sum of the angles of a triangle is less than two right angles.

The question of the character of the so-called geometrical axioms thus assumes an aspect very different from the one it had at the beginning of the present century, when they were commonly regarded as necessary logical truths. It is, however, not for the mathematician to decide whether ultimately these axioms express facts of observation unconsciously acquired and made familiar through the constant perception of an actually existing space. For him they represent mere assumptions selected for the purpose of defining his space or his methods of measuring this space.

It would, of course, be very important to know which of the different spaces that the mathematician can thus define corresponds most closely to the facts of observation. But this question is difficult to decide; for while the ordinary Euclidean space appears in this respect to satisfy all demands, the non-Euclidean spaces do the same, at least, approximately within certain limits; and all our observations give only approximate results and are confined within a narrow range of space.

What the mathematician has gained through the generalization of non-Euclidean geometry is a broader horizon and a vastly extended field of research. The multifarious relations by which this new science is connected with the various banches of geometry are admirably set forth by Professor F. Klein, of Göttingen, in his *Vorlesungen über nicht-Euklidische Geometrie* (1889–90). These lectures also trace the historical development of the subject since the times of Gauss. A few more recent investigations were discussed by him in the *Evanston Colloquium* (New York, Macmillan, 1894), in the 6th and 11th lectures.

What Professor Vasiliev tells us about Bartels, who in his earlier years had intimately associated with Gauss, and later, as the first professor of mathematics at the University of Kazàn, became the teacher and protecting friend of Lobachevsky, confirms the supposition that the first impulse to these studies came to him, at least indirectly, from Gauss. To the same source of inspiration must be traced the almost simultaneous, but independent, researches of the Hungarian Wolfgang Bolyai and his son Johann. Gauss himself never published anything on the subject of non-Euclidean geometry; but we know from his letters to Schumacher that he had spent much thought on these questions, which had occupied him from his earliest youth, and had arrived at practically the same results as Lobachevsky and the Bolyais.

In the later developmeut of non-Euclidean geometry and the closely related theory of n-dimensional spaces or manifoldnesses we find among others the names of Grassmann, Riemann, Helmholtz, Cayley, Klein, Lie; and in these the uninitiated may find a sufficient guarantee for the value of the subject.

In conclusion, a few words must be said of the present English translation. The original has been followed so faithfully that anybody possessed of an adequate knowledge of the Russian language will understand the translation very readily. The reading of such unidiomatic English is, however, exceedingly painful. Were it not for the direct statement on the title-page, we should never have ascribed this translation to Professor Halsted, whose vigorous command of the English language is well known. It seems almost incredible that a person whose native language is English should have written, or even passed in the proof, such sentences as these: (p. 3) "So in celebrating this day to Lobachevsky, we must remember with gratitude his teachers." (ib.) "His destiny was to be the teacher and protector not only of Lobachevsky, but of the scientist of our century most influential on the development of mathematics, Gauss. (ib.) "The mathematical ability of the boy-genius awakened the attention of the science-hungry Bartels." (p. 4.) " . . . he received the grade of 'Magister' July 10, 1811, for extraordinary advance in mathematics and physics." (ib.) " . . . the question of the lowering of the grade of a two-termed equation . . ."

The transliteration of Russian names is faulty and inconsistent; thus we find Pouchkin for Pushkin, Demidef for Demidov, Karamzen for Karamzin, Simenov for Simônov, etc. It is inconceivable why the name of the well-known astronomer Littrow should be persistently misspelled Lettrov. On p. 1, for 'November 9, 1807' read 'January 9, 1807.' The statement in the preface, p. vii., that "in 1500 Copernicus was enjoying the friendship of Regiomontanus and fulfilling with distinction the duties of a chair of mathematics" is singularly incorrect. Regiomontanus died in 1476, when Copernicus was three years of age; and, although Rhaeticus, in speaking of the residence at Rome in 1500, refers to Copernicus as 'professor mathematum,' it is now, in the absence of any direct evidence, generally accepted that the author of the *De revolutionibus* was never connected as teacher with any scientific institution.

ALEXANDER ZIWET.
UNIVERSITY OF MICHIGAN.

Laboratory Exercises in Botany, designed for the use of colleges and other schools in which Botany is taught by laboratory methods, by EDSON S. BASTIN, Am. Professor of Materia Medica and Botany and Director of the Microscopical Laboratory in the Philadelphia College of Pharmacy. Philadelphia. 1895. $2.50.

In a review of this volume it should be considered for whom it was written and from that standpoint an estimate should be made whether the purpose has been really accomplished. Being designed for students who are beginners, it leads them from the simple to the complex, and does it, we think, in a very satisfactory manner. As a laboratory guide the work is perhaps a little too voluminous, 540 pages. It is divided into two portions, the first requiring work with the simple microscope, and consists of a series of lessons inductively arranged, which leads the student from a study of the root through the types of the largest families to a study of the seed and embryo. They are designed to give to the student a familiarity with the various forms, without burdening him with the technical descriptive terms, which are, however, summed up in tabulated plates for reference. The full-page illustrations of the first portion are numerous, very simple, excellently drawn and well printed.

The second portion of the volume, 270 pages, on vegetable histology, opens with a chapter on the compound microscope and the use of micro-chemical reagents, and is accompanied by excellent and practical tables of reagents and stains. The purpose of this volume limits its scope. It makes a good working guide to put into the hands of students who can give but a limited time to the study, but further than that, as a work upon vegetable histology, it is meagre.

The arrangement of this portion of the work is less commendable than the first. Its numerous illustrations can be classed as most good, few bad and a number indifferent, in general the simple elements of tissues being good, whereas those showing the tissues themselves, especially the more complex ones, are less to be approved.

The work is one which is admirably adapted for the use of students in pharmacy, for which it was probably first intended, and in the hands of a guide whose methods were similar to those of the writer, we conceive it to be excellent. In general its scope is limited; it gives facts but fails, we think, to point out those logical sequences of growth and development that lead the student to a rounded conception of the science of botany; it nevertheless is by far the best laboratory guide we have seen for directors of laboratories who wish to give their students a practical elementary knowledge of botany.

S. E. Jelliffe.

Principles and Practice of Agricultural Analysis.—By Harvey W. Wiley, Chemist of the U. S. Dept. of Agriculture.—Easton, Chemical Publishing Co., 1894. Vol. I.

We have already called attention to the first part of this admirable work, now being published in monthly installments by the Chemical Publishing Company, and need not again speak of its general excellence of plan. If any fault is to be found with the work it is with its limited title, which is rather apt to mislead some into a supposition that the book will be of service only to the analyst, and as a laboratory manual alone. The twelve parts which have now appeared, nearly 600 pages in all, indicate a work of much broader scope, one which no scientific library can afford to omit from its catalogue. Of the first of the series we have already spoken. In No. 2 the subject of soils and soil formation is continued, the action of earth-worms, bacteria, air, etc., the qualities of the various soils and the discussion of certain peculiar soil types. An interesting chapter on sampling follows, and here is discussed in principle and practice all of the accepted methods now in use in various countries and among the leading workers in agricultural science. The study of the physical properties of soils and the description of methods of mechanical and microscopical analysis, etc., occupies some 200 pages, while the methods of chemical analysis, begun in No. 7 of the series, extends to the present issue. We know of no other work approaching the present in completeness and scientific value. The exhaustive treatment of the subject leaves nothing to be desired, and it would be difficult indeed to criticise any of its features. At the end of each part is a Bibliography of works cited, and an inspection of these lists at once indicates the labor entered upon by the author, as well as that saved to those who have now the benefit of his research.

PHILADELPHIA. CHARLES PLATT.

Nitrogen and Water, or the Water Atoms and Their Relations. Part—The Earth's Atmosphere, by WILLIAM COUTIE.

The author of this polygraph of 31 pages is good enough to assure us that some things remain undiscovered, or at any rate we infer this to be his meaning. To discover the real meaning of many of his sentences would require the application of the calculus, since his thoughts soar off into space in what are apparently curved lines. It is probable that minds of the earth, earthy, like that driving this pen, are incapable of fully grasping the mighty thoughts here set forth. They are certainly startling and go to the root of all things.

It appears that we have all been mistaken in our conception of the design of Creation, at least those who have ventured to form any such conception have been mistaken. The real reason is thus set forth:

"It is evident that it is the law of change that gives the Creator some work to do and something that is new in all time. It is thus to Him the most important of all, for it is to Him preëminently omnipresent, universal and in all things forever new, and without it time would be a monotony and a burden, almost everything would be old and He would have nothing to do."

The following whack at our biological brethren is commended to their attention; their disgraceful Darwinian tendencies make it deserved, if somewhat severe:

"If we now turn to the results in time we find that, first, horse in our knowledge was of the size of a fox and walked on his heels. Now all horses of every kind walk on the point of their longest toe, and they are all many times the weight of a fox. Now, why did all horses get on their toes at the same time, or how did they get on the tips of their toes at all? Darwinism is to me a compound of utility and economy. But by what process of economy or utility did horses get on the point of their toes? To me, it is evidently the exclusive result of their Maker's will, and that the creation and government of the universe is an absolute despotism in all things."

This facer ought to settle the Darwinians; lest it should not, we subjoin another extract of like tenor:

"I found that a butterfly is an insect ornamented by scales, and that they are divided into day flies and night flies, and again divided into six thousand day or butterflies and sixty thousand night or moth flies, and that butterflies are purely and exclusively (so far as they are butterflies) things made for beauty by an agent or Maker who sees beauty of colors in the night, for there are sixty thousand kinds of night flies and only six thousand day flies. This led me to the undoubted belief that Darwinism applied to butterflies is worse than an error, for it leaves out the most important and essential part of the whole, which is, that the origin of species is the direct exclusive result of an intelligent design."

To the initiated the following will perhaps explain how some of Mr. Coutie's results were obtained:

"As the ways of this argument are so far from the ordinary beaten paths, my intent when writing it was to print in full along with it Newton's four rules of reasoning, pages 384 and 385, *Principia*, to show that this is in full and exact accord with them."

"This design led to a full, careful review of the men, their method and their particular results, that I found that these rules are wholly insufficient for my purpose. They are perfect for his purpose, but insufficient when applied to this paper."

This, so far as we are able to understand it, looks black for Newton.

Among other gems of style and statement, we have the following:

"The history of origin leads us far back into the distant past."

"What this subject learns from this observation of the heavens is that the same rules that govern the atoms."

"The density of the air is the result of its own weight."

The author has also discovered a few less important matters of detail. Among other things two new—what shall we call them; not elements for they are, according to our present notions, compound. The first of these new somethings is kirs. This is no common mangy kirs, but a new kind of kirs altogether. He or it—for the author says enough about the relations of the atoms to make one careful—is introduced to our notice as follows:

"The most resultant discovery of all is that kirs is a hydrate of nitrogen, having the atomic form N_3HN_3."

The second something new is Stuart, which is N_3H, it seems. According to the author this, as well as kirs, is unobserved. We understood that Curtius not many years since discovered a compound having the symbol of Stuart, but this is perhaps a mistake. Carbon has been found to be AN., ice is Aq. and made up of Stuart, Cyanogen and more Stuart. Coke equals kars and A. We are nowhere informed what is meant by A, nor is it easy to see what difference there is between 'combining constituents' and 'constituents' except with the eye of faith. The author explains, however, that "The grand difficulty of the calculation is that the revelations at the end constantly contradict the premises at the beginning."

Everything about this wonderful pamphlet is new, even the spelling is sui generis. For example: Flourine, Glucium, Rubedium, Phosphorous, Telerium, Tantalium, Lanthanium, Paladium.

We hope that E. H. Lisk, printer, Troy, N. Y., turned off a large edition of these pamphlets. They will all be needed, and when obtained ought to be carefully preserved as an illustration of the magnificent reach sometimes attained by the American intellect.

EDWARD HART.

NOTES AND NEWS.

BIOLOGY.

THE Tenth Annual Fish Commissioners' Report from Michigan is entirely in the field of fresh-water biology. It is important to mark the rapid development of biological work in the central universities of this country, and to note that the work carried on by the State is so largely by the coöperation of the biologists of the University. Thus two of the papers of this report are by Professor Jacob Reighard, the first being a study of the development of the wall-eyed Pike, the second a valuable résumé of the whole subject of artificial fertilization. The Bulletin, No. 4, of the Commission, which we receive at the same time, contains a preliminary account of the biological examination of Lake St. Clair during the summer of 1893. This was suggested by the continued decrease in the number of Whitefish, but very wisely the work extended over a broader field. The objects of this examination are stated as follows: "(1) To study carefully and in the broadest possible way the life in the lake. After examining the physical characteristics of the lake, such as the color, transparency and chemistry of the water, a study of this sort should include a determination of the kinds of animals and plants in the lake. Every species should be sought out, carefully described and figured, and a specimen of it preserved. Then the habits of each species should be known, its habitat, its food, its enemies and its parasites. The numbers of animals and plants of each species in a given volume of water should be determined and the variations in these numbers in different parts of the lake and at different seasons of the year. Such a collection of data would form a complete picture of the biology of the lake." The work was under the direction of Professor Reighard, assisted by Dr. Ward, of the University of Nebraska, by Mr. Frank Smith, of the University of Illinois, and by several assistants from the University of Michigan. The materials collected were widely distributed for determination, and the reports are by Dr. Blanchard, of Paris, Dr. E. A. Birge, of the University of Wisconsin, and others. The survey seems to have been carried on with all the thoroughness both in the collection of littoral, pelagic and deep-lake types, which characterizes the best marine work, and the final results promise to be of the greatest interest and importance.

Mr. Arthur Bibbins, who has been engaged during the past year in investigating the fauna of the Potomac Formation, in the interest of the Woman's College of Baltimore, has made a considerable collection of reptilian remains, mostly from the vicinity of Muirkirk, Md. The specimens represent the four species of Dinosaurs described by Professor Marsh under the names of *Allosaurus, Pleurocœlus* and *Priconodon*. A tibia, probably that of *Allosaurus*, measures 10 inches in width and 32 inches in length, although the ends are lacking. A single tooth seems to be referable to *Astrodon Johnsoni*, Leidy, which was based on a tooth found at Bladensburg, Md. The conditions are very unfavorable for collecting, as the specimens occur in a tough clay, often at a considerable depth, and are much scattered.

Dr. S. W. Williston, of Lawrence, Kansas, has in press a work, entirely rewritten, on the classification and structure of North American Diptera. It will contain tables of all the North American genera, including those from Central America and the West Indies, together with descriptions of larvæ, habits, anatomy, etc. It will appear next autumn. In its preparation he has had the assistance of Messrs. Aldrich, Townsend, Snow and Johnson, who have kindly prepared or revised the tables of the families with which they are best acquainted.

At the second open meeting of the Royal Society, on February 28th, Prof. W. F. R. Weldon opened a discussion on variation in animals and plants, his remarks being based on the report of a committee, consisting of Mr. Francis Galton, Mr. F. Darwin, Professor Macalister, Professor Meldola, Professor Poulton and Professor Weldon himself, its object being to conduct statistical inquiries into the measurable characteristics of plants and animals. The first part of the report which was presented was described as 'an attempt to measure the death rate due to the selective destruction of *Carcinus mœnas* (the shore crab) with respect to a particular dimension.' Another paper bearing on the subject under consideration was presented by Mr. H. M. Vernon, on 'The Effect of Environment on the Development of Echinoderm Larvæ: An Experimental Inquiry into the Causes of Variation.' An interesting discussion followed, in which Mr. Thiselton Dyer, Professor Ray Lankester, Professor A. Agassiz, Mr. Bateson, Sir H. Howorth and the chairman took part. There seemed to be a prevailing doubt as to the suitability of mathematical methods in biological research.

Prof. H. W. Conn contributes to the March number of the *American Naturalist* an account of the Cold Spring Harbor Biological Laboratory, of which he is the director. The article is illustrated by four plates, showing the buildings and location. The laboratory was organized by Prof. F. W. Hooper as a branch of the Brooklyn Institute of Arts and Sciences, and held its first session in July and August, 1890, under the direction of Dr. Bashford Dean, now of Columbia College. The Cold Spring Laboratory does not rival the Wood's Holl Laboratory in the amount of research work accomplished, but offers exceptional facilities for students requiring instruction.

APPROPRIATIONS FOR THE U. S. GEOLOGICAL SURVEY.

The appropriations for the U. S. Geological Survey for the fiscal year 1895–96, as made by Congress at its last session, will enable the bureau to continue its work under favorable circumstances. The appropriations for topography, geology, paleontology and chemistry are the same as those for the present year, except that in the case of geology there is an additional appropriation of $5,000 for the specific object of the investigation of the gold and coal resources of Alaska. For the rest, there is an appropriation for the preparation of the report on the mineral resources of the United States of $18,000, an increase of $3,000; and further was inserted in connection with this work, under the head of Public Printing and Binding, a clause providing for the printing of advance copies of papers on economic resources, and for this work an appropriation of $2,000 was made. Under the head of engraving and printing the geological maps of the United States, authority was granted the Director to sell copies of topographic maps, with a descriptive text, at cost, with ten per centum added. The object of this item is to provide for the preparation of a series of ten or more maps, with text, to illustrate the typical topographic features of the United States, for use principally in teaching. It is anticipated that the maps and text will be prepared during the summer. To the appropriation for 'gauging the streams and determining the water supply of the United States, including the investigation of under-ground currents and artesian wells in arid and semi-arid regions,' $7,500 was added, making the appropriation for this work $20,000.

The total appropriation for the Survey, including all field and office expenses and salaries, is $515,000.

An appropriation of $200,000 was made for a survey of the lands of the Indian Territory, with the provision that the "Secretary of the Interior may in his discretion direct that the surveys in the Indian Territory, herein authorized, or any part of them, be made under the supervision of the Director of the Geological Survey." This work will result in the making simultaneously of a land subdivision survey and a topographic map.

GENERAL.

THE German Anthropological Society is publishing an extensive description of the anthropological collections of Germany. Sixteen parts (costing from 2–15 M.), prepared by competent authorities, have already been issued.

THE *Technologisches Wörterbuch*, edited by Gustav Eger and published by Vieweg, Brunswick, is a full English-German and German-English dictionary of scientific and technical words, which should have as large a sale in America as in Germany.

THE first volume of the memoirs from the Department of Botany of Columbia College, a monograph of the *North American Species of the Genus Polygonum*, by John K. Small, is now in press.

DR. ERNST MACH, Professor of Physics in the University of Prague, has accepted a Professorship of Philosophy in the University of Vienna, and will direct a Laboratory of Experimental Psychology.

PROFESSOR E. W. HOPKINS, of Bryn Mawr College, succeeds Professor Whitney in the chair of Sanskrit and Comparative Philology, and Professor E. G. Bourne, of Western Reserve College, has been elected Professor of History, at Yale University.

PROF. WEIERSTRASS, of Berlin, has been elected Foreign Associate of the Paris Academy of Sciences; he received forty-three votes, one being given to Prof. Frankland and one to Prof. Huxley.

PROF. E. DORN succeeds Prof. Knoblauch as Director of the Physical Laboratory of the University of Halle.

PROF. M. K. RÖNTGEN, of Würtzburg, has been called to the chair of Physics in the University of Freiberg, vacated by Prof. E. Warburg.

DR. R. BRAUNS has been made Professor of Mineralogy in the University of Tübingen.

Dr. A. Kossel has been made Professor of Physiology in the University of Marburg.

Dr. K. Boedeker, Professor of Chemistry in the University of Göttingen, died on February 22d, aged seventy-nine years.

Sir William Savory, an eminent surgeon, and at one time Professor of Comparative Anatomy and Physiology at the College of Surgeons, died on March 4th, at London, in his sixty-ninth year.

Dr. Georg von Gizycki, Associate Professor of Philosophy in the University of Berlin, died early in the present month.

Dr. Darwin G. Eaton, formerly Professor of Natural History in Packer Institute, died on March 17th, at the age of seventy-two years.

Prof. Peter H. Vander Weyde, editor of *Manufacturer and Builder*, and formerly Professor in Girard College and at the Cooper Institute, died at New York, on March 18th, at the age of eighty-two years.

Dr. Henry Coppée, Acting President of Lehigh University, Professor of English Literature in the University of Pennsylvania, 1855 to 1866, and President of Lehigh University, 1866 to 1875, died at Bethlehem on March 21st, at the age of seventy-five years.

SCIENTIFIC JOURNALS.

THE PHYSICAL REVIEW, MARCH–APRIL.

On the Attractions of Crystalline and Isotropic Masses at Small Distances: A. Stanley Mackenzie.

The Influence of Temperature upon the Transparency of Solutions: Edward L. Nichols and Mary C. Spencer.

Determination of the Electric Conductivity of Certain Salt Solutions: Albert C. MacGregory.

The Apparent Forces between Fine Solid Particles Totally Immersed in Liquids, II: W. J. A. Bliss.

Minor Contributions; New Books.

THE AMERICAN NATURALIST, MARCH.

In the Region of the New Fossil, Dæmonelix: Frederick C. Kenyon.

The Cold Spring Harbor Biological Laboratory: H. W. Conn.

Minor Time Divisions of the Ice Age: Warren Upham.

The Skunk as a Source of Rabies: W. Wade.

The Classification of the Lepidoptera: Vernon L. Kellog.

Recent Literature; Recent Books and Pamphlets. General Notes:—Geography and Travels; Mineralogy: Geology and Palæontology; Botany; Zoölogy; Embryology; Psychology; Archæology and Ethnology.

THE BOTANICAL GAZETTE, MARCH.

Apparatus for Physiological Botany (With plates IX.–XII.): W. C. Stevens.

On the 'List of Pteridophyta and Spermatophyta of Northeastern America: B. L. Robinson.

Flowers and Insects, XIII.: Charles Robertson.

Noteworthy Anatomical and Physiological Researches.

Briefer Articles; Editorial; Current Literature; Notes and News; Supplement.

NEW BOOKS.

Louisiana Folk-Tales. Collected and edited by Alcée Fortiér. Boston and New York, published for the American Folk-Lore Society, Houghton, Mifflin & Co. 1895. Pp. xi+122. $2.

The Free Trade Struggle in England. M. M. Trumbull. 2d Edition. Chicago, The Open Court Publishing Co. 1895. Pp. 288. 35 cts.

Beiträge zur Kentniss des Wesens der Säcular-Variations des Erdmagnetismus. Louis A. Bauer. Berlin, Mayer & Müller. 1895. Pp. 54. M. 3.

Field, Forest and Garden Botany. Asa Gray. Revised and extended by L. H. Bailey. New York, American Book Co. 1895. Pp. 519.

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Subscriptions and advertisements should be sent to SCIENCE, 41 N. Queen St., Lancaster, Pa., or 41 East 49th St., New York.

*THE ANIMAL AS A MACHINE AND PRIME MOVER.**

THE writer of these papers has been greatly interested in the study of the vital machine in its relations to the special work of the engineer and to the methods illustrated by it in transformation of potential energies into the mechanical form for useful purposes in the industries.

The value of this form of prime motor to the engineer is enormous, though rarely appreciated or realized. Until the introduction of the steam-engine into mills and factories through the inventions and enterprise of Watt and his partner, at the beginning of the century, horse-power and manual labor only were available for any work for which water-power could not be obtained, and hundreds of horses had even been employed, in earlier times, in draining of single mines. But, even at the present time, the horse is the prime motor for an enormous section of the industries; and all transportation on short routes or available lines, all agricultural work nearly, and work of whatever kind on the highway and in the by-ways must rely on this vital machine for its performance.

The theory of the machine and study of its methods of operation, of energy-conversion, and of economical application of power, is one of the most important subjects practically presented to either the engineer or the man of science, and this for two quite different reasons. In the first place, the vital machine has a higher efficiency than any steam-engine and involves methods of transformation, storage and application of energy which are as yet a mystery, and which, could they be discovered and simulated in engineering practice, might possibly prove

*Abstracted from The Animal as a Prime Motor; N. Y., J. Wiley & Sons, 1894. Journal of the Franklin Institute, Jan.–March, 1895.

more enormously valuable as improvements upon current methods than was the invention of the modern steam-engine and the displacement of the old machines of Worcester and Savery. It is also possible that nature's ways of producing light and electricity, as well as power, may be ultimately found immensely more economical than those of man. They certainly are quite different, and are inconceivably more efficient in themselves, as single transformations, than any processes yet discovered by science. In the second place, the laws of operation of the vital machine being fully revealed, it is possible that we may find ways of promoting the improvement of the machine in such a manner as to make the animal mechanism a more efficient and a better apparatus for the use of man, and even, perhaps, find ways of improving the instrument employed by the mind in its special operations, as well as the mechanism of the frame in which it is given a home and a vehicle.

The outcome of the investigations made up to the present time may be stated perhaps in the briefest and most intelligible way in the form of a series of theorems, thus:

(1.) *The Vital Machine is not a thermodynamic engine, a heat-motor.*

Many writers have taken for granted the now obviously incorrect hypothesis that, since the machine is evidently a source of heat, and its energy is derived from combustible materials, it must, therefore, be a heat-engine and its operations necessarily thermodynamic. This is easily disproved. In any thermodynamic machine, of whatever class, among the heat-motors, the proportion of heat converted into work, the 'efficiency' of the machine is measured by the range of temperature, from the highest to the lowest in the cycle operated in by the thermodynamic mechanism, divided by the maximum *absolute* temperature in the cycle. For the animal machine this would ordinarily be the widest range of temperature attainable in thermodynamic conversion divided by about 300° C. But the machine is, in this case, a mass of circulating fluids of fair conductivity, mainly, and can have no sensible range of temperature, so far as can be seen; and, in fact, it is known that differences of but one or two degrees, in different parts of the body, the only actual differences of temperature, are produced by a slight warming of the venous blood by chemical action, or by proximity to or distance from the epidermis. As a thermodynamic engine, even were it possible, therefore, the machine should have an exceedingly low efficiency. The fact is that its efficiency exceeds that of any heat-engine known to man, under the most favorable possible practical working conditions.

The vital engine is certainly not thermodynamic; its heat is a 'by-product.'

(2.) *The machine is probably not electrodynamic.*

Scoresby and Joule, and Sir William Thomson 'Lord Kelvin' and others among later writers, have suggested that the machine may be, as some have said, an electrodynamic machine, others an electro-magnetic engine. In support of this view it is pointed out that, in some cases, as in the gymnotus, the torpedo and some fifty other creatures, powerful electric batteries, accumulators, are found in the animal system; that all animals seem to have conductors, the nerves, and that electricity *leakage* is always to be detected in the living creature —currents passing in various directions through the body and leaking outward to the surface in all parts. The nerves terminate in 'plates' having close relation in form and structure to the more highly developed cells of the storage batteries of the eel and similar animal producers of electricity.

A great variety of facts and considerations based upon research in this field conspire to indicate, if not to fully prove, that the passage of the electric current along the nerve is the initial act in the motion and energy-production of the muscle. On the other hand, however, it may probably be stated, as conclusively ascertained, that there is no representative of the mechanism of our electro-dynamic machines, either of generator or motor, in the muscle, where, unquestionably, the applied energy is set free and utilized. There is no equivalent of magnet, of solenoid, of field or of armature. On the other hand, it is indicated by numerous and varied investigations and observations that the electric current has for its office, in the vital machine, the promotion of the chemical actions which accompany all motion and development of force and power. The familiar effects of currents having their origin outside the body afford illustrations of the fact and the method of action of these currents. The electric currents, so far as existing in the system, have light work to perform; and where, as in the gymnotus, they are given more formidable tasks, they require for their production and application very large special organs, and occupy an exorbitantly large proportion of the body.

The vital machine is probably not an electro-dynamic motor.

(3.) *The animal prime mover is very probably an example of an exceedingly highly organized and efficient chemico-dynamic motor.*

There are but three known forms of energy available in conversion of the stored potential energy of the foods into dynamic form. Two of these have been seen to be, the one certainly, the other probably, unutilized in the energy-conversion of the vital machine. The third, until some as yet undiscovered process and energy is found to be available, must be assumed to be the source of all dynamic phenomena in the animal system. The machine is probably a chemico-dynamic prime mover, in which the developments of energy in active form, their magnitude and their applications, are directed by the supreme authority of the system through a very perfect arrangement of electric apparatus, by means of which the necessary orders are telegraphed to the various points at which energy is to be liberated and applied, and by the currents traversing which apparatus the chemical reactions needed in transformation of the potential energy of the fats and glycose, and of the products of broken-down tissue, into active and useful form are inaugurated. Electricity, or some related energy, serves as the directing and stimulating power, and the resolution of fats and other substances into glycosic compounds and their oxidation, at the point at which power is to be developed, into carbon-dioxide and water, by chemical changes resulting in the transformation of potential into actual energies, supplies the working power of the system. The presence of electricity is always observable in the vital machine, and the chemist-physiologists have traced the processes of supply and transportation of potential energy and of the liberation of active energies down to the very last, though still mysterious, act of utilization.

These authorities are now apparently substantially unanimous in declaring it well settled that the action of muscle, for example, is due to what is termed an 'explosive' chemical action in the mass of the organ, the outcome of which is mechanical energy and the liberation of carbon-dioxide. The physicist-physiologists are equally united in testifying that the provocation of this explosive action, at will and in proper quantity, is effected by a nerve-impulse which is more nearly like the electric current than any other known form of physical energy; and the process of doing work by muscular action is likened to the firing of a charge of explosive in the mine by a current sent over a wire, in this case along a nerve, and the provocation, by its action, of instantaneous oxidation of carbon into carbon-dioxide with change of the physical state from the solid or liquid form into gas liberated in a small space under high compression, and thus in a condition to perform maximum work by its expansion.

(4.) *In this chemico-dynamic machine, the energy displayed in its dynamic operations, as in its muscular work, is generated and applied locally.*

It has been supposed by some writers that the power of the muscular system was derived by transmission from some central or remote source to the point of application, by the nervous system, there to be utilized in the act of muscular stress. It is now well ascertained that not only is there no provision for such transmission of energy, but that the liberation of energy occurs within the mass of the muscle itself, and within its tissue-cells. That the action is local is easily seen in the fact that the excised heart, an excised bit of intestinal muscle, the corpuscles of the blood itself, and the amœba-form protoplasm of which the flesh is composed, in its minutest elements, possess this attribute of energy-development. The heart beats, often for hours in some cases, after removal from the body; the excised muscular tissue exhibits its rythmic pulsations visibly after isolation; the white blood corpuscle, even, propels itself independently into the locality in which it is to join its energies and activities with those of the already built-up living substance; the elemental protoplasm everywhere exhibits these characteristics of what we call 'living' matter.

Thus complete elemental vital systems are found distributed, in many forms, in all parts of the machine, with their directing and initiative forces as well as their energy-transforming apparatus.

Further: It is now well settled and easily shown that the potential energy supplied is tendered to the working system in the form of glucosic matter, sugars, produced from fats and starches, and sent through the arterial pipe lines to the capillaries and thence into the very cells of the organs in which work is done. There they are resolved into carbon-dioxide and water; the location and to some extent the nature of the energy-transformation being thus fully revealed. It is a local transformation of chemical into mechanical energy, directly or indirectly, at the very point and in the very cell, apparently, where the work of that elementary portion of potential energy is performed. The question remaining to be solved is whether this transformation is direct or indirect, a single step or a series of energy-changes, not whether it is effected locally or generally or within some special organ appropriated to that duty. Each cell appears to be an elementary prime motor, an elemental vital machine; and the muscular mechanism is a combination of innumerable elements of similar composition and method of action, in each of which a similar process of energy-transformation is conducted.

This process is not thermodynamic, is probably not electro-dynamic, is presumably chemico-dynamic, by which is meant that the energy of chemical action is probably directly transmuted into mechanical energy, not, as in thermodynamic machines, first into heat and then into work. A thermodynamic link in the chain would mean the loss of a large fraction of the whole supply; but it still remains to be ascertained how direct chemico-dynamic conversion of energy can give the remarkable efficiency observed in the vital machine.

(5) *The Nerve-Impulse, the physical energy relied upon for communicating the voluntary and the automatic stimuli which determine the time and intensity of the action of the muscular motor-system, is probably a form of electric energy or some closely related physical action.*

This is a system of telegraphy from nerve-ganglia, spine and brain which does not, as had been formerly supposed by some writers, transmit energy, but simply indicates where and when locally available stored energy is to be liberated and applied to definite purposes by appropriate muscles. It demands energy only in the manner and in the degree in which the electric current firing a mine expends energy in the initiation of the chemical action resulting in the tremendous effects observed. The work is done by the more or less complete transformation of the potential energy available in chemical combinations into mechanical energy, once the electric spark fires the charge.

The passage of the electric current through the fresh muscle produces the same effects as the nerve-impulse, and these effects may be reproduced again and again, until the muscle loses its store of glycose or until its structure changes. At every effort, the flexed muscle consumes glycose and liberates carbon-dioxide, precisely as in its natural operation under the stimulus of the nerve-impulse. This parallelism of action and effect may be taken as, perhaps, good circumstantial evidence. In every animal system, and in every mass of muscle within it, electricity-leakage, or other movements of electricity, may always be detected by the familiar methods of the electrician, and this everywhere distributed energy unquestionably originates in the system itself, and has place and purpose in its economy. In special cases, as in the gymnotus, Nature has magnified its work and given it larger place in the working of the machine than ordinarily, and thus has given us an opportunity to observe, on this magnified scale of working, both the form of the special constructions for the production of this form of energy, and the method of its transmission and application. We find the electric system of the gymnotus to be simply a development of the nerves and terminal plates found in all animals. That they have a common office, though very different in relative magnitude and importance in the two cases, is undoubted. That the origin, however, of this form of energy, simply as required for telegraphy, is chemical is very certain, also, since it must find its source in the common store of potential energy supplied the whole system. That this chemical process may be somewhat different from that producing chemico-dynamic effects is not improbable; especially as the presence of combustible fats of peculiar composition seem always an essential to nerve action. But all chemical action is accompanied by electric phenomena, and Nature here seems to make the fact subservient to her plans. But she adopts singular methods, and possibly a peculiar form of this energy; and the minute quantity detected by investigators, and the slow rate of progress along the nerve fibers, are elements of as yet unrevealed mystery. The familiar exhausting effect of continued nervous expenditure may be either due to large energy expenditure or to restricted supply of the special form of potential from which it is derived.

(6.) *The nature, source and methods of development and transformation of brain and nerve power do not appear to have been yet discovered, or even surmised.*

The fact that such energy is subject to exhaustion and renewal by precisely the same processes, so far as can be observed, certainly under the same conditions as produce fatigue or favor recuperation of muscular power would seem to justify the inference that the potential energy of the food and the processes of nutrition and of development of active physical energies in the brain, spine and nerves are so modified in these glands as to give a special product in the form of vital energy, and perhaps of brain-power, and of those initiative forces of the whole nervous system which inaugurate and direct, automatically or intelligently, the currents of nerve-impulse and set in operation and sustain the whole complicated life-system. But how the mind seizes upon these forces and compels these energies to work its will, or how the spine, and automatic mechanism, generally, is set in communication with the mind on the one side, and the organism of the machine on the other, remains a mystery challenging every resource of talent and the highest genius in the investigator. So far as a judgment or even a surmise is permitted, it may probably be assumed that, like all other energies of the vital machine, those of brain and spine and nervous system have a definite, quantivalent relation with the familiar physical energies, and fall within the province of modern scientific research. They demand, beyond a doubt, their proportion of the potential energy supplied in the daily ration.

(7.) *Observed phenomena and statistical data upon which these deductions are founded may be summarized as follows:*

Taking the human vital engine, in illustration, the amount of potential energy supplied the average individual may be taken at 2,500 or 3,000 calories when doing no external muscular work, 4,000 calories when performing a full day's work as a laborer.* This corresponds to 10,000 to 16,000 British thermal units, to from 8,000,000 to 12,500,000, nearly, British dynamic units, foot-pounds per individual per day, of which supply a part is wasted by defective digestion and assimilation and a portion by various defects of the machine itself. Taking the energy-supply of the vital machine as 8,000,000 foot-pounds for the man of sedentary habits and performing brain-work and 10,000,000 for a steady and hard-working laboring man, who does much muscular labor and little thinking, we have the basis of estimates which, though probably not very precise, may yet answer present purposes in giving general conclusions.

Of this eight or ten millions of foot-pounds of energy supplied the machine in potential form, in the foods, not less than fifteen per cent. must be reckoned for deficiency of digestion and transformation into available form in the chyme and chyle, the solutions from which the system draws it for its various special purposes. This seems the minimum usual loss, and an excess is commonly observed, which is furnished by larger food-supply than the assumed figures as here given. A good 'digestion coefficient' is 85 per cent.*

Of the 8,000,000 foot-pounds of energy furnished in the food of the brain-worker, or 10,000,000 supplied the day laborerer, something like 7,000,000 in the one case, and 8,500,000 in the other, pass into the reservoirs of potential energy of the vital machine, and circulate in the blood through all its organs; giving up to each that peculiar form of nutriment needed for its work or for its own maintenance. The muscles draw upon it for energy to be converted into the work of external labor or of internal operations essential to life; the various glands elaborate from it those special compositions required for their purposes; the brain and nervous system absorb from it the material for consumption in the operations directed by the mind or automatically conducted by the vital powers of the animal system. Of the 8,500,000 foot-pounds of energy thus furnished the mechanism of the laboring man, in the best cases of application, under most favorable conditions, about 2,000,000 are applied to the performance of

*Pavy on Foods; Mott's Manual; Thurston's Animal as a Prime Mover; Year Book of the New York State Reformatory, 1894; Reports of the Conn. Agricultural Station.

*Flint's Muscular Power; Woods's Digestibility of Feeding Stuffs, Awater's Studies of Dietaries, Report of Conn. Ag. Experimental Station, 1893.

the day's work; which is equivalent to saying that the efficiency of this vital machine, considered simply as prime mover, is 23½ per cent. If efficiency of conversion of potential into dynamic energy of muscular work, internal as well as external, is considered, it is very possible that this figure may be doubled, and the efficiency to be taken in comparison with that of heat-engines may be somewhere between forty and fifty per cent. If the internal work of thought and of brain and nerve power is considered useful work, and the total compared with the energy supply, the efficiency will be a still higher figure, perhaps fifty or even sixty per cent.*

But the highest total efficiency of the best steam-engine yet constructed is but about twenty per cent., with its thermodynamic range of about 200° F. (111° C.) degrees, Fahr., and that of the best gas-engine is but about the same, with a range of ten times that extent. If the vital machine be a thermodynamic engine, therefore, its known efficiency, with no recognized temperature, range of heat 'let down,' is not less than twenty-five per cent. higher than, and may be twice as high as, the best heat-engines constructed by man. This is recognized by engineer and thermodynamist alike, as a *reductio ad absurdum*, and the vital engine is certainly not a heat-engine.

The facts regarding the distribution of potential energy to the various organs of the body; the development by each organ of its special form of product in new compositions or in a special energy; the localization of energy-transformations in the cells of the muscle, or other energy-producer; the accompanying liberation of carbon-dioxide from consumption of glycosic material; the utilization of a telegraphic, or rather a semaphoric, system communication

* Weisbach's Mechanics of Engineering; Rankine's Prime Movers; Thurston's Animal as a Prime Motor; Reynolds' Memoir of Joule.

between the mind or the interior automaton of the spine and cerebellum and the point of useful application of energy all: these are familiar to all physiologists.* Beyond these known phenomena lie the mysteries which the engineers, if possible more than the physiologists themselves, most desire to see completely solved. When they are thoroughly investigated and the operations of the vital machine become fully known, in all their details of energy-transformation, it may be possible to secure new prime movers of similarly high efficiency and thus to double the life of the race by prolonging the period marking the endurance of our supplies of potential energy in the coal-fields of the world. Should it prove that only by preliminary manufacture of fuel, in the form of sugars, can this result be attained, it may seem unlikely that, even when these operations are no longer mysteries, commercial applications of nature's methods can be expected tō prove successful; yet when it is considered that the sugars are simply carbon and water, it will not be denied by either engineer, chemist or physiologist that a possibility still remains of effecting so enormously important an advance in the prime motors. If, further, nature's economies in light-production can be paralleled, the engineer may ultimately furnish heat, light and power, the three great products of his special labors of most value to the race, with insignificant wastes and approximately perfect efficiency and maximum cheapness. Given perfect efficiency of power-production and the main problem is solved.

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* Foster's Physiology; Encyclopædia Britannica, Art. Physiology; Chauveau's Le Travail Musculaire.

HARSHBERGER ON THE ORIGIN OF OUR VERNAL FLORA.

By way of a review of a paper by Mr. Henry L. Clarke, in the *American Natural-*

ist for September, 1893, XXVII., 769–781, entitled 'The Philosophy of Flower Seasons,' I have just contributed an article to the same journal, Feb., 1895, XXIX., 97–117, giving the results of local observations on the same subject. After my paper was in type, I found a short article in *Nature*, XXVII., 7, by J. E. Taylor, entitled 'The Origin of our Vernal Flora,' which suggested some reflections bearing upon the problem. These, with other thoughts relating to the subject, were too late to be incorporated in an article which was already of considerable length. A consideration of these items, however, may not be out of place in connection with an examination of the article by Mr. Harshberger, in Science, Jan. 25, 1895; New Series, I., 92–98.

Commenting upon the fact that it is usual to assign an Arctic origin to our mountain flora, but without giving references, Mr. Taylor says: "Seeing that temperature is so largely influential in explaining the distribution of flowering plants, it occurs to me that not only may height above the sea-level answer to northern distribution, but *seasonal occurrence* as well." Briefly, this covers Mr. Harshberger's propositions numbered 1, 2 and 3 on page 95.

Mr. Taylor observes that the early flowering plants blossom two or three months earlier in Great Britain than within the polar circle. For example, *Chrysosplenium oppositifolium* and *C. alternifolium* bloom 'in March or April; within the Arctic circle not until June and July, and even so late as August.' This suggests a general retardation of flower seasons as we go northward, and I have used this assumption as in part explaining the late blooming of some of the luxuriant, highly specialized groups, which MacMillan* calls 'north-bound.' In many of these, flowering is preceded by a long vegetative period. In the northward movement, if the vegetative period remains of the same length, it seems probable that the flowering would be later in consequence of this period beginning later.

*Higher Seed-Plants of the Minnesota Valley, 1892.

Mr. Clarke's paper is an elaboration of the idea of the preponderance of the less-specialized flowers in the early part of the season and of the more highly specialized flowers in summer and autumn, and I have criticised this theory from the standpoint of the local flora of my neighborhood, and have undertaken to account for flower seasons as a result of the competition of flowers among themselves and in correlation with the flight of the anthophilous insect fauna. The reader is referred to these papers for a more extended discussion of the relations of flower seasons and the specializations of floral structure.

Mr. Harshberger's observations upon the lull or break in the continuity of the floral procession, which he says at times occurs, is quite interesting. He says: "Such a break seems to occur in the neighborhood of Philadelphia between the twenty-fifth day of May and the tenth or fifteenth day of June, when the first true summer plants appear. Curiously enough, this period corresponds with the time of the ice saints in the United States, when there is a possibility of frost over a large portion of our continental area."

There is a lull, however, which, at least as regards the entomophilous flora, takes place, not 'at times,' but regularly. The frost may, indeed, in many cases have a very definite effect in preventing plants from advancing into the spring months, probably indirectly, however, through its influence upon the vegetative state which precedes flowering. The time of the ice saints, according to Harrington,* is from May 19th to 24th, while the floral depression is later.

In the neighborhood of Carlinville, Ill., the entomophilous flora shows a slight de-

**Harper's Monthly*, LXXXVIII., 878. 1894. Article cited by Harshberger.

cline in June, and many of the groups show well marked June depressions, as will be seen from my curves (in article cited). The dominant families show maxima before June or after, but not one of them shows a June maximum. The depression sometimes occurs in very homogeneous groups, as the Scrophulariaceæ, there being no particular distinctions between the early and late ones. The gap sometimes separates species of the same genus. As a rule, the vernal flowers belong to plants of low habit which bloom in the woodlands, which are now warm and sunny, or upon the open grounds. About June the former become overshadowed by the leaves which have appeared on the trees, and the latter by the later more luxuriant vegetation. Thus the species of *Viola* and *Lithospermum* produce attractive flowers until about this time, when they either stop blooming altogether, or resort to the production of cleistogamic flowers.

One fact, which was not mentioned in my paper, but is shown in my curves, is that the groups of anthophilous insects show the same tendency to form early or late maxima, which emphasizes the importance of the correlations of the two sets of more or less mutually dependent organisms. The Syrphidæ, Empidæ and Andrenidæ show early maxima, while all of the other families show late ones. In the case of the dominant genera of bees, *Anthophora*, *Synhalonia* and *Osmia* reach their maxima early, but the other genera predominate late. *Nomada* breaks into a large early group and a small late one, just like *Andrena*, upon which it is parasitic.

In the case of our trees, I suspect that the flowers were always produced before the appearance of the leaves, an arrangement which would be most favorable to their anemophilous pollination. Mr. Harshberger certainly seems very wide of the mark in explaining the retention by trees of their adaptation for wind aid in transferring their pollen. In the first place, their height exposes them to the wind in such a way as to make wind pollination quite favorable, while the wind may also interfere with insect visits. The fact that the most highly specialized flower visiting insects are not so abundant in spring will not do, for they are not the insects which are most likely to favor incipient stages of entomophily. The less specialized bees (Andrenidæ) and the flower flies (Syrphidæ) are most abundant in spring, and they would be the most favorable guests in the less specialized states of insect-adaptation. Moreover, flower-loving insects are very abundant in the woodlands in the spring before the leaves appear, and that is the very time that the wind pollinated trees are in bloom. By resorting to entomophily, the trees would only come in competition with the terrestrial flora, which is more favorably situated for insect visits and is very attractive to the early insect fauna.

The author states that "Trees of abnormal habit frequently show atavism, flowering in the late autumn, if exceptionally warm." Such cases as *Hamamelis* are examples. I am inclined to Foerste's* view that the autumnal blooming is a case of precocious development of a spring flower. According to him, *Hamamelis* has distinct hibernacula and in cold autumns holds over until next spring.

If the generally accepted flower theory is true, one would expect to find the highest specialized flowers at that part of the season when the most highly specialized flower visiting insects are most abundant. But it is hard to understand how Mr. Harshberger could attribute this modification to the Lepidoptera. As far as adaptation for flower-pollination is concerned, the bees are beyond question the most highly specialized. Müller† says: "Bees, as the most

*Bot. Gazette, XVII., 3, 1892.

†Fertilization of Flowers, 595, 1883.

skilful and diligent visitors, have played the chief part in the evolution of flowers; we owe to them the most numerous, most varied and most specialized forms." The Lepidoptera have given rise to some highly specialized flowers, but I think it would be hard in a single case to show a probability that the incipient stages of irregularity were induced by their visits.

That the less specialized flowers are spring flowers is only true in a general way. From my present data it appears that the maximum of the entomophilous Choripetalæ is in August, though further observations may show a greater number in spring. Including the anemophilous species, the Choripetalæ will certainly show an early maximum, and that is the extent of the justification of their being called spring plants. The same is true of the entomophilous Monocotyledons. If the blooming seasons of all of the Monocotyledons of a given neighborhood be worked out, I doubt if they will show a vernal maximum, though the position of *Carex* may accomplish this result. The Gamopetalæ have a late maximum, but none of them are free from the competition of the Monocotyledons or the Choripetalæ.

It seems to me that Mr. Harshberger has contributed an important point in reference to the general positions of the flower groups by indicating the influence of the retreat of glacial winter. Making use of this suggestion we may suppose that, as the warm seasons became longer, a large proportion of the Monocotyledons and Choripetalæ moved northward, climbed the mountains or opened their flowers early. While the more highly specialized groups were by no means thus relieved from the competition of the less specialized, there can be little doubt but that in the later months they found a time when that competition was less severe. This may aid us in explaining what has struck me as a fact in the phænological habits of the flora of my neighborhood. I have indicated that the introduced plants, the aquatics and the degraded entomophilous flowers tend to prolong their blooming seasons, and have supposed that this results from their being more relieved from the competition which besets the other flowers. Although the data have not been arranged to test the point thoroughly, it has occurred to me that the later plants in general bloom longer than the early ones. (In investigating this proposition, it may be proper to eliminate some of the very late ones, whose seasons are not cut short by competition, but by way of preparation for the approaching winter.) The later species thus appear to have entered a position where competition was less severe. It may be, however, that they show the effects of competition less, merely on account of their superiority.

Mr. Harshberger attributes floral modifications to the 'irritating action of insects on vegetal protoplasm.' This suggests Henslow's* theory. As far as I know, that theory has not been accepted by any one who has made a serious investigation of the relations of flowers and insects, and for that reason it has not seemed justifiable to discuss it at length. It seems safe to say that it has not been shown that direct insect contact will induce floral modifications, or that the theory will account for the most ordinary facts of floral structure.

Finally, with regard to the literature, I notice that Mr. Harshberger quotes MacMillan (l. c.) without giving references. On consulting this author, I find that the general proposition of the early blooming of the less specialized plants and the late blooming of the more highly specialized is at least strongly suggested, and that too evidently on the authority of persons cited in a bibliographical list. The autumn-flowering of the Compositæ is distinctly stated. From his observations in Flanders,

* The Origin of Floral Structures, 1888.

MacLeod* concludes that the less specialized flowers, as well as insects, prefer the springtime, while the more highly specialized prefer the later months. This anticipates my statement of the same general result.

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BRISSON'S GENERA OF MAMMALS, 1762.

IN 1756 Brisson published, in Paris, the mammal volume of his '*Regnum Animale in Classes IX Distributum.*' It is a quarto, with the descriptive matter in French and Latin, in parallel columns, and contains a folding table or key on which the generic names are given in proper Latin form. But since the work antedates by two years the 10th edition of Linnæus' *Systema Naturæ*, which by common consent is accepted as the starting point in Zoölogical nomenclature, the names cannot be used. Six years later, however, a second edition of Brisson appeared. It is a rare octavo, wholly in Latin, and was printed at Leyden in 1762 †. It is of special importance because it falls between the two editions of Linnæus that are available in Zoölogical nomenclature (10th Ed., 1758; 12th Ed., 1766), and hence may be considered, so far as the genera of mammals are concerned, as a part of the foundation of the nomenclature. The specific names are not exclusively binomial and cannot be used, but the generic names given in the keys (pp. 12–13 and 218) are in due Latin form, and are entitled to recognition.

Although the work was not printed until four years after the 10th edition of Linnæus, the 6th edition (1748) is the only one quoted. Still 25 of the 46 genera given are the same as those published by Linnæus in the 10th Ed. (1758). Of the remaining 21, ten are strictly synonymous with and antedated by Linnæan genera, and consequently cannot be used either in a generic or sub-generic sense. These are:

Brisson, 1762.		Linnæus, 1758.
Pholidotus	=	Manis
Tardigradus	=	Bradypus
Cataphractus	=	Dasypus
Hircus	=	Capra
Aries	=	Ovis
Musaraneus	=	Sorex
Prosimia	=	Lemur
Philander	=	Didelphis
Cetus	=	Physeter
Ceratodon	=	Monodon

The remaining eleven are introduced by Brisson for the first time and are entitled to recognition. They are:

Odobenus	Glis
Giraffa	Pteropus
Tragulus	Hyæna
Hydrochœrus	Meles
Tapirus	Lutra
Cuniculus	

Most of these are now in current use, but are attributed to later writers, and in several cases wrong species are taken as types. Carrying the date back to 1762 not only gives them greater stability, but also establishes the types in a satisfactory manner. All but one of the genera take Linnæan species for types, as follows:

The type of *Odobenus* is *O. odobenus* Brisson = *Phoca rosmarus* Linn., which becomes *Odobenus rosmarus* (Linn.) 1758. It thus seems as if the Walrus, after oscillating for a century and a half between *Odobenus* and *Trichechus*, might fairly claim a permanent abiding place.

The type of *Giraffa* is *G. giraffa* Brisson = *Cervus camelopardalis* Linn., which becomes *Giraffa camelopardalis* (Linn.) 1758.

The type of *Tragulus* is *T. indicus* Brisson = *Capra pygmea* Linn., which becomes *Tragulus pygmeus* (Linn.) 1758.

* Over de bevruchting der bloemen in het Kempisch gedeelte van Vlaanderen. Bot. Jaarboek, VI., 1894.

† Regnum Animale in Classes IX. Distributum . . . Quadrupedum & Cetaceorum . . . A. D. Brisson . . . Editio altera auctior . . . Lugduni Batavorum . . . 1762.

The type of *Hydrochœris* is *H. hydrochœrus* Brisson=*Sus hydrochœris* Linn. (12th Ed.), which becomes *Hydrochœrus hydrochœris* (Linn.) 1766.

The type of *Tapirus* is *T. tapirus* Brisson=*Hippopotamus terrestris* Linn., which becomes *Tapirus terrestris* (Linn.) 1758.

The type of *Cuniculus* may be fixed on *C. cauda longissima* Brisson, which becomes *Cuniculus alactaga* (Olivier) 1800.* *Cuniculus* is one of the few genera in which Brisson did not indicate the type by repeating the generic name for the first species. It was made up of a heterogeneous assemblage comprising no less than six modern genera and five families of Rodents as follows:

Cavia Pallas	1766	(*Caviidæ*)
Lemmus Link	1795	(*Muridæ*)
Cœlogenus Cuv.	1807	(*Dasyproctidæ*)
Dasyprocta Ill.	1811	
Anisonyx Raf.	1817	(*Sciuridæ*)
Allactaga Cuv.	1836	(*Dipodidæ*)

According to the A. O. U. Code, therefore, *Allactaga*, having been left in *Cuniculus* until all the others had been taken out, must stand as the type of *Cuniculus*.

The type of *Glis* is *Glis glis* Brisson=*Sciurus glis* Linn. (12th Ed.), 1766, which becomes *Glis glis* (Linn.) 1766.

The type of *Pteropus* is *P. pteropus* Brisson =*Vespertilio vampyrus* Linn., which becomes *Pteropus vampyrus* (Linn.) 1758, replacing *Pteropus edulis* Auct.†

The type of *Hyæna* is *H. hyæna* Brisson=*Canis hynæa* Linn., which becomes *Hynæa hynæa* (Linn.) 1758.

The type of *Meles* is *M. meles* Brisson=*Ursus meles* Linn., which becomes *Meles meles* (Linn.) 1758.

The type of *Lutra* is *L. lutra* Brisson=*Mustela lutra* Linn., which becomes *Lutra lutra* (Linn.) 1758.

C. Hart Merriam.

* *Dipus alactaga* Olivier, Bull. Soc. Philomatique, II., No. 40, 1800, p. 121; also Tilloch's Philosophical Mag., Oct., 1800, p. 90.

† See *Gray*, List of Specimens of Mammals, British Museum, 1843, p. 37; and particularly *Thomas*, Proc. Zool. Soc., London, 1892, p. 316, foot note.

NOTES ON AGRICULTURE (I.)

ELECTRO-HORTICULTURE.

The latest results drawn from experiments with electric light upon vegetation are by Professor Rane in Bulletin No. 37 of the West Virginia Experiment Station. Investigations along this general line began in 1861, when Herve-Mango demonstrated that electric light can cause the formation of green material (chlorophyll) in plants and produce other phenomena, as turning toward the light (heliotropism). Prilleaeux, in 1869, showed that assimilation in plants goes on in the presence of artificial light. Dr. Siemens experimented largely with arc lights, both within and at other times outside of and above the plant houses. Professor Bailey, who at Cornell University has tested electric lighting extensively during the past few years, in reviewing Dr. Siemens' work, writes: "He used the term electro-horticulture to designate this new application of electric energy. He anticipated that in the future the horticulturist will have the means of making himself particularly independent of solar light for producing a high quality of fruit at all seasons of the year . . . . whatever may be the value of electric light to horticulture, the practical value of Siemens' experiments is still great." After years of trial Professor Bailey stated in one of his reports: "I am convinced that the electric light can be used to advantage in the forcing of some plants."

In the fall of 1892 Professor Rane introduced the use of the incandescent light in place of the arc lamp, and his recent report with its illustrations from photographs of plants, etc., has features of interest to all who are interested in science, as well as the market gardener. He finds that "the incandescent electric light has a marked effect upon greenhouse plants," it being "beneficial to some plants grown for foliage, such as lettuce. Flowering plants blossomed earlier and continued in bloom longer under the light" than elsewhere. Plants like spinach and endive "quickly ran to seed, which is objectionable in forcing these plants for sale. Most plants tended toward a taller growth under the light." The fact of plants responding promptly to electric light is widely demonstrated, but that it will be an economical method of growing crops is not so clearly shown.

SOIL TREATMENT OF ORCHARDS FOR DROUGHT.

In many parts of our country crop growing is very uncertain, due to a lack of sufficient rainfall. This fact has led the Nebraska Experiment Station to make a study of methods of mitigating the ill effects of dry weather. Professor Card* reports results upon an old orchard, a third of which was mowed, a third pastured and the remaining third cultivated every two weeks. The trees in cultivated ground suffered much less from the drought and hot winds than those in sod, the foliage being more vigorous and without the wilting during the hot windy days common to the trees in the sod ground.

The fruit was larger and better upon the cultivated trees than elsewhere. An examination of the soil showed that for every 100 barrels of water in the first twenty inches of sod ground there were 140 barrels in the cultivated ground. The soil in all regions when drought is experienced needs a covering of mulch. It is not practicable to add a mulch of straw or other material, but the upper few inches of the soil when kept light and mellow serves as a mulch for all below. Therefore a key to the solution of the problem is to plow deep; even subsoiling will pay for some crops, and then mulch by means of a mellow layer upon the top produced by frequent cultivation.

*'Some Obstacles to Successful Fruit Growing,' Bulletin 39 Neb. Experimentation Station.

THE RUSSIAN THISTLE.

No other species of plant has received so large amount of attention as has been given during the past two years to the Russian Thistle (*Salsola Kali Tragus* (L) Moq.). Not only the botanists have been interested, but law makers in legislative halls have paused in their party strife to listen to the demands of their constituents for enactments against this newly arrived and miserable plant pest.

Many of the Experiment Stations have published bulletins of greater or less size with full-page engravings of the thistle in its various parts or conditions of growth. Recently a large emergency poster has been issued by a Central-Western Station to be displayed in public places as a means of information and warning to all whom it may concern. The National Government has shared in this work by issuing a bulletin from the Department of Agriculture, while Congress was asked to appropriate vast sums to put down this rapidly spreading, prickly weed.

As the name indicates, this enemy to American agriculture came to our country from Russia, where it is called by a name having the meaning of 'Leap-the-field.' In German it is 'Wind witch,' and with us the same idea is embodied in the name of 'Tumble weed,' namely its capacity for traveling with the wind. When it matures in autumn the stem decays at the surface of the ground, and the large bushy, prickly plant is easily blown for long distances by the wind, and when twenty or so of these plants become entangled and formed into a giant ball the structure is quite formidable.

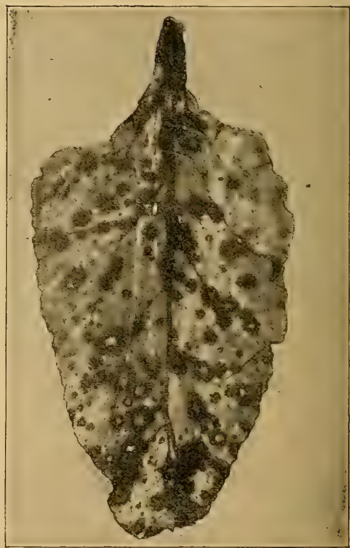
The new conditions of the far-prairie States, where a rich soil and open country prevail, the spread of this pest has been phenomenal. From a single center in South Dakota, where it was brought in flax seed from Russia a few years ago, it has been disseminated in all directions, so that to-day it may be expected in almost any State in the Union. Its spread is not confined to to its natural methods, for with our lines of railway running in all directions the seeds are carried rapidly and for long distances.

As an outcome of the advent of the Russian thistle, there has been a wide and thorough awakening upon the subject of weeds which will result in a better understanding of these foes, their ways of migration to and throughout our country, and the best methods of subduing them.

THE BEET-LEAF SPOT AND ITS REMEDIES.

The last Bulletin (No. 107) of the New Jersey Experiment Station describes a fungous trouble of beets in the United States, the *Cercospora beticola*, Sacc., which causes a conspicuous spotting of the foliage. There seems to be no respect shown for any varieties of beets, for the writer has made special visits to the trial grounds of large seedgrowers, and all sorts of beets, from the oldest to the newest kinds, were found with their foliage about equally injured.

The common name of 'Leaf Spot' well describes the general appearance of the beet leaves infested with this Cercospora, for they are at first more or less covered with small light or ashy spots, which later often become holes by the disappearance of the tissue previously killed by the fungus. Figure 1 is an engraving made from a sun print of a beet leaf, natural size, that was badly infested with the Cercospora. Full-sized leaves often become mutilated, and sometimes scarcely more than the framework remains. The fungus itself is quite similar in structure and habits of growth to those causing leaf spots and blights in other crops. The so-called 'rust' of celery is due to a Cercospora (*Cercospora Apii* Fr.), as likewise is the violet leaf spot (*Cercospora Violæ* Sacc.). These fungi consist of slender threads which run through the substance of the leaf, and, coming to the surface in groups, pass through the openings (stomata) in the skin, and in clusters bear long, slender spores in considerable numbers. These spores, when mature, fall from their points of attachment and soon germinate, thus spreading the fungus and causing other spots.



During the past season, under the special charge of Mr. J. A. Kelsey, spraying experiments have been carried out to check the Cercospora of the beet. A field of Mangolds, kindly provided by Supt. E. A. Jones, at the College Farm, was experimented upon with Bordeaux mixture.

As the season progressed the Bordeaux

mixture made so striking a difference in the plants that it could have been observed by anyone passing along the side of the field. The untreated rows had the foliage smaller, more upright and badly spotted with the fungus, while the sprayed plants showed a rank growth of foliage, nearly green throughout, more inclined to lop and much less spotted than the untreated plants.

The difference between the roots in the treated and untreated rows shown below in pounds was not so great as that seen in the foliage.

	Sprayed.	Unsprayed.
Roots, ...	416¼ lbs.	331 lbs.
Leaves, ...	63¾ lbs.	49 lbs.
Total, ...	480 lbs.	380 lbs.

This is an increase of nearly twenty-six per cent., or one-quarter in round numbers. Therefore, the conclusion is that whatever the crop may have been per acre in this case, spraying with Bordeaux mixture would have increased it one-fourth, or, for example, from nine tons to twelve tons.

BYRON D. HALSTED.

SCIENCE IN CANADA.

A NEW volume of the transactions of the Royal Society of Canada (Volume XII.) will shortly be issued. It will be the largest of the series and will contain a bibliography of the work of the Society, collectively and individually. This Society was founded in 1882 by the Marquis of Lorne, at that time Governor-General of Canada, and was organized, to some extent, on the basis of the *Institut de France*. It consists of four sections, of which two are scientific, one being devoted to the physical and chemical, the other to the biological and geological sciences. The system of *éloges*, introduced originally by the French Section (I.), has of late been adopted by the other sections also. Carefully conducted, this feature cannot fail to be of value to the future inquirer. An accurate catalogue of deceased members' works, with their dates of publication, etc., and an impartial estimate, ought to accompany the biography.

The scientific members of the Royal Society of Canada comprise several scientific workers and writers of continental, a few of European, fame. Except one year (1891) it has always met at Ottawa, a rendezvous which, though inconvenient for members living at a great distance, has some important advantages, such as access to the National Library, the Archives Bureau, the offices, museum and library of the Geological Survey and the Central Farm, with its laboratories, etc. All these departments are represented in the membership.

Not the least of the services that the Royal Society has rendered to Canada is that which arises from the affiliation of the principal local societies throughout the Dominion. Some of these are important bodies, which publish transactions of their own, and have done a fair share of original work. Among these may be mentioned the Natural History Society of Montreal, founded in 1827; the Canadian Institute (1851), the Hamilton Association (1856), the Nova Scotia Institute of Natural Science (1862), the Entomological Society of Ontario (1863), the Murchison Society, Belleville (1873), the Ottawa Field Naturalists' Club (1879), the Canadian Society of Civil Engineers (1888), the Natural History Society of British Columbia (1889) and the Literary and Scientific Society of Winnepeg (1879). It will be seen that this list practically covers the Dominion from Atlantic to Pacific, and when it is added that every one of these bodies is represented at the May meeting by a delegate, who reads a statement of the year's work, published in the ensuing volume, it will be admitted that the plan is not unfruitful. Some of these allied societies have organized their work into departments, and their reports in the proceedings of the Royal Society form a valuable record of scientific development. The yearly volume of the R. S. C. is thus both a stimulant and a testimony to scientific progress.

To even outline the character of the work done by the local societies just enumerated would occupy a good deal of space. In some cases the name indicates the general trend of inquiry, but for the most part this can only be learned by consulting reports. The Entomological Society of Canada has long had a reputation for steady and painstaking work, and the commendations that it won at the Centennial Exposition (1876) were not undeserved. The Natural History Society of Montreal has two courses of lectures every winter; the regular monthly meetings yielding papers that are strictly scientific, while the Somerville lectures (founded by a Presbyterian minister more than half a century ago) are of a more popular character.

The two latest of these Somerville lectures were delivered by Prof. Saunders, Superintendent of the Central Farm, Ottawa, and Dr. Robert Bell, F. G. S., of the Geological Survey, their subjects being 'The Resources of the Soil,' and 'The Mammals of Canada,' respectively. Dr. Bell's lecture, which was delivered on the 15th ult., covered an immense habitat or succession of habitats, and was the result of personal observation from the international frontier to the extreme north. The members of the Survey have traversed the vast region between Hudson Bay and the Rocky Mountains, some of them having spent seasons in the Yukon country, others in the Barren Lands. Dr. Bell went on two expeditions to Hudson Bay. In his lecture he spoke of the moose, the red deer, the reindeer, the Rocky Mountain sheep, the antelope, the arctic bear, the seal, the walrus, the whale, the porpoise, the beaver, the cat family, the fox, in his varieties and the smaller mammals, especially the fur-bearing species. He mentioned the domestication of wild animals by the aborigines, and suggested the following of their example. The lecture was perhaps rather economic than scientific; though, as largely the result of personal observation, it had a greater value than most popular lectures.

A Montreal society that has been doing good work in an unostentatious way is the Society for the Study of Comparative Psychology, of which Professor T. W. Mills, M. D., author of a work on 'The Dog,' may be said to have been the founder. Most of the papers read at the Society's meetings are based on observations of the habits of animals, several of the members being, like the president, Dr. Mills, connected with the Veterinary College, affiliated to McGill University. At the last meeting (on the 8th ult.), Mr. A. Dell read a paper on the Evolution of Language, Mr. C. A. Bantelle another on Habit. In both observations of animals were used (in part) for illustration. Mr. B. K. Baldwin read a paper on the relation between the intellectual status of the horse and his owner, in which he showed that by sympathy and kindness lower races attained greater control over their horses than higher races without those qualities.

Another society that has been doing some quasi-scientific work is the Folk-Lore, or rather the Montreal Branch of the American Folk-Lore Society. It meets at the houses of members monthly, when papers are read and discussions take place. At the last meeting, Dr. D. S. Kellogg, of Plattsburg, N. Y., gave an interesting paper on the Folk-Lore of the Lake Champlain Valley, the importance of which was increased by the fact that every belief, usage, saying and tradition mentioned had been collected by the essayist in the course of an extensive practice. In almost every case, the source of the story or incident was mentioned. Dr. Kellogg's paper admirably exemplified how profitably a busy professional man, of scientific habit of mind, may utilize his spare *quarts d' heures* and odd moments. J. T. C.

CORRESPONDENCE.

THE CLASSIFICATION OF SKULLS.

Editor of Science: In 'Varieties of the Human Species, Principles and Method of Classification' (Le Varieta Umane. Principi e methodo di classificazione. Di Giuseppe Sergi. Torino, 1893), which constitutes one of the Smithsonian Miscellaneous Collections, 1894, the skulls are grouped as follows:

NORMA VERTICALIS.

1. Ellipsoid (ellipsoides).
2. Pentagonoid (pentagonoides).
3. Rhomboid (rhomboides).
4. Ovoid (ovoides).
5. Sphenoid (sphenoides).
6. Spheroid (sphæroides).
7. Byrsoid (byrsoides).
8. Parallelepipedoid (parallelepipedoides.)
9. Cylindroid (cylindroides).
10. Cuboid (cuboides).
11. Trapezoid (trapezoides).
12. Acmonoid (acmonoides).
13. Lophocephalic (lophocephalus).
14. Chomatocephalus (chomatocephalus).
15. Platycephalic (platycephalus).
16. Skopeloid (skopeloides).

In 'Observations upon the Cranial Forms of the American Aborigines based upon Specimens contained in the Collection of the Academy of Natural Sciences of Philadelphia,' by J. Aitken Meigs, Proceedings of the Academy of Natural Sciences of Philadelphia, 1866, 232, occurs the following classification of skulls:

A—Pyramidal or Pyramidocephalic Form.
B—Oval or Oidocephalic Form.
 I Cymbecephalic Form.
 II Narrow Oval Form (Stenocephalic).
 III Broad Oval Form (Eurycephalic).
 IV Barrel-shaped or Cylindrical Form (Cylindricephalic).
 V Angular Oblong Form.
C—Arched or Hypsecephalic Form.
 I Archecephali.
 II Phoxocephali.
D—Wedge-shaped or Sphenocephalic Form.
E—Flat or Platycephalic Form (Subglobular).
F—Globular or Sphærocephalic Form.
G—Square, Cuboidal or Cubicephalic Form.

The two classifications are sufficiently alike to suggest comparisons. Confining my remarks to the forms in Meig's table, which are best illustrated in the *norma verticalis*, I note that:

Oidocephalic = Ovoides.
Cymbecephali = Ellipsoides & Pentagonoides.
Cylindricephali = Cylindroides.
Angularly Oblong Form = Rhomboides.
Archecephali = Trapezoides & Acmonoides.
Phoxocephalic = Lophocephalus.
Sphenocephalic = Sphenoides.
Platycephalic = Platycephalic.
Sphærocephalic = Sphæroides.
Cubicephalic = Cuboides.

Thus six out of sixteen names of Sergi's classification are included in Meig's classification. I conclude from comparison of Méig's types with Sergi's figures that the forms are identical.

Ellipsoides and Pentagonoides are included in Cymbecephali; Rhomboides is the same as the skulls included under 'Angularly Oblong Form;' Lophocephalus is a synonym of Phoxocephalic; Parallelepipedoides appears to be a variety of Cylindricephali; Trapezoides and Acmonoides are included in Archecephali.

So long as Sergi endeavors to establish a classification which he desires to be tested by the methods of zoölogy and botany (p. 60), the names he proposes must be judged by the law of priority of publication.

Harrison Allen.

Philadelphia, March 16, 1895.

NOTES ON THE BIOLOGY OF THE LOBSTER; A CORRECTION.

In an article entitled 'Notes on the Biology of the Lobster' (Science N. S. Vol. I., No. 10, p. 263.) the following sentence occurs: "After hatching a brood in May, the female usually molts and afterwards extrudes a new batch of eggs." This should be corrected to read thus: *After hatching a brood in May, the female usually molts, but does not extrude a new batch of eggs until the following year.*

These notes were culled from a fuller paper, and this slip in the context crept in unobserved. It is, however, corrected in the latter part of the article.

Francis H. Herrick.

SCIENTIFIC LITERATURE.

THE TYRANNY OF THE MONISTIC CREED, A REVIEW.

Der Monismus als Band zwischen Religion und Wissenschaft. Glaubensbekenntniss eines Naturforschers. Ernst Haeckel. Bonn, Emil Strauss. 1893 (Vierte Auflage).

Monism. The Confession of Faith of a Man of Science. Ernst Haeckel. Translated from the German by J. Gilchrist. London, Adam and Charles Black. 1894.

The influence of a 'creed' on the progress of science is a proper subject for discussion by men of science, and it is to this, and not to the value of the basis for Haeckel's 'faith,' that we will direct attention.

As he defines it, Monism "is the conviction that there lives one spirit in all things and that the whole cognizable world is constituted, and has been developed, in accordance with one fundamental law."

This positive creed is very different from a modest confession of ignorance, which leaves us free to follow wherever future discoveries may lead, for the monistic creed is based on the assumption that what we know is a proper measure of what we do not know, as if we could have any measure of the unknown.

An enthusiastic admirer of Haeckel's scientific researches may be pardoned a word of comment on this published statement of his creed.

He tells us all eminent and unprejudiced men of science who have the courage of their opinions think as he does. No one likes to be called a bigot or a coward, or to be accused of ignorance, but those who do not agree with Haeckel must fortify their souls by the thought that this argument is no new thing in history.

Science is justified by works and not by faith, and when Haeckel says 'Credo' and not 'Scio' we need not discuss the value of his belief, although its influence on the progress of science is a more practical matter.

The struggle for intellectual freedom is often called a conflict between religion and science, but while the men of science have burst through those Pillars of Hercules which, according to Bacon, are 'fixed by fate,' they have had no wish to demolish these ancient landmarks, but only to force a passage on to the great ocean of natural knowledge. Least of all do they desire to set up new bounds.

So far a creed involves, or seems to its holders to involve, preconceptions on matters which fall within the province of research or discovery, it is an obstacle to the progress of knowledge and a proper subject for scientific examination.

I shall try to show that the monistic 'confession of faith' has led to the discounting of the possibilities of future discovery, and that it has thus obstructed progress.

One of its results is intolerance of doubt on the problems of life. In this field the monist holds that those who are not with him are against him, and he admits no middle ground. More freedom is permitted in other fields of thought.

We may say that, since we know nothing about it, we neither believe that the planet Mars is nor that it is not inhabited, but no such philosophic doubt is permitted in biology.

If a teacher of natural science were to say he does not believe life *is* the outcome of the physical and chemical properties of protoplasm he would most surely be reported as believing it *is not* the result of these properties, and he would straightway be branded a dangerous scientific heretic or a weak brother of the faith, and his confession of ignorance would be put on record as positive belief.

This antipathy to philosophic doubt on the problems of life is clearly due to the dogmatism of the monistic creed, which cannot admit the presence of any unjoined links in our knowledge of nature.

We might be indifferent to this intolerance if it did not cause the most essential characteristics of life to be ignored or pushed into the background.

It is as true now as it was in Bacon's day that: "Whoever, unable to doubt, and eager to affirm, shall establish principles proved, as he believes, . . and according to the unmoved truth of these, shall reject or receive others, . . he shall exchange things for words, reason for insanity, the world for a fable, *and shall be unable to interpret.*"

The essential characteristic of life is fitness.

A living organism is a being which *uses* the world around it for its own good.

I, for one, am unable to find, in inorganic matter, any germ of this wonderful attribute.

It is possible that after chemistry has given us artificial protoplasm this may be shaped, by selection or some other agency, into persistent adjustment to the shifting world around it, and that it may thus become alive.

Everything is possible in the unknown, but why should we believe anything on the subject until we have evidence?

Of one thing we may be sure. The artificial production of protoplasm would not be a solution of the problem of life. The nature of the problem must be grasped in all its length and breadth, with all its difficulties, before we can hope to solve it.

Many biologists have sought to solve it by transforming Huxley's carefully guarded statement that protoplasm is the physical basis of life into the dogma that life is the sum of the physical properties of protoplasm.

Life cannot go on without food, and we may say with propriety that bread is the *staff* of life, but the agency which shapes the food into the specific structure of an organism exquisitely adapted to the conditions of the world around it is to be sought somewhere else than in the properties of bread.

One of the distinctive characteristics of this organizing agency is that it may exist in a germ without any visible organization. Another is that, so far as we know, it has been handed down, in an unbroken line, from the oldest living things, generation after generation, to the modern forms of life, and that it has leavened the whole hump of living matter.

While we know nothing of its nature or origin, and must guard against any unproved assumption, there seem, from our present standpoint, to be insuperable objections to the view that this agency is either matter or energy. While we know it only in union with protoplasm, it would seem that, if it is matter, it must, long ago, have reached the *minimum divisibile*. If it is energy, or wave motion, or perigenesis of plastidules, it is hard to understand why it has not been dissipated and exhausted. We know that it exists, and this is in itself a fact of the utmost moment.

We are told that the belief that it has, at some time, arisen from the properties of inorganic matter is a logical necessity, but the only logical necessity is that when our knowledge ends we should confess ignorance.

Young men who have been trained in the routine of the laboratory tell us all their interest in biology would be gone if they did not believe all its problems are, in the long run, to be resolved into physics and chemistry.

The only answer we can give them is that noble work has been done in natural science by men like Wallace, who believe that life is fundamentally different from matter, and also by men like Haeckel, who believe the opposite.

They also serve science who only stand and wait, and among them I would wish to be numbered.

While nothing is gained by giving a name to the unknown agency which is the essence of life, it is better to call it a 'vital principle' than to deny or ignore its existence. It is better to be called a 'vitalist,' or any other hard name by zealous monists, than to be convicted of teaching, as proved, what we know is not proven.

The word *vitality* is as innocent as *electricity* or *gravity;* in fact, Newton's use of this word led Leibnitz to charge him with infidelity to the spirit of science, although no one need fear to follow where Newton leads.

The older vitalists may have looked on a mere word as an explanation, but the reason the word has fallen into disrepute is the antagonism of the monists to the view that the problem of life presents any peculiar difficulties.

Many thoughtful men of science have held that the 'faith' of men like Haeckel ignores many of the data which are furnished by our scientific knowledge of the world around us.

Huxley, in his essay on the Physical Basis of Life (1868), says it is necessary for a wise life to be fully possessed of two beliefs: "The first, that the order of nature is ascertainable by our faculties to an extent which is practically unlimited; the second, that our volition *counts for something as a condition of the course of events.* Each of these beliefs can be verified experimentally as often as we like to try."

Again, twenty-five years later (1893), he says (Evolution and Ethics) that, fragile reed as man may be, "there lies within him a fund of energy, operating intelligently, and *so far akin to that which pervades the universe that it is competent to influence and modify the cosmic process.*"

Clearly this man of science has no overwhelming dread of the charge of anthropomorphism or animism, or of any charge except lack of caution.

I think that he would also admit that every living thing contains some small part of this influence which 'counts for something as a condition of the course of events,' and that it must be reckoned with in our attempts at a philosophy of the universe.

W. K. Brooks.

Johns Hopkins University.

The Life and Writings of Constantine Samuel Rafinesque. (Filson Club Publications No. 10.) Prepared for the Filson Club and read at its meeting, Monday, April 2, 1894. By Richard Ellsworth Call, M. A., M. Sc., M. D. Louisville, Ky., John P. Morton & Co. 1895. 4to. pp. xiii + 227. Portraits, etc. Paper. Price $2.50, net.

This sumptuous volume is published by a Historical Club in Louisville, Kentucky, as a memorial to one of the pioneer naturalists and explorers of the Ohio valley, a man whose brilliant intellect, eccentric character and unhappy fate will always cause his career to be looked upon with interest, and whose nervous and appalling industry has been the cause of a myriad of perplexities to students of the nomenclature of plants and animals in Europe as well as in America.

Born in Constantinople in 1783, his father a French merchant from Marseilles, his mother a Greek woman of Saxon parentage, Constantine Rafinesque early entered upon the career of a wanderer. The roving habit of mind which soon became a part of his nature led him into a mental vagabondage that influenced his career even more than the lack of a permanent place of abode. His youth was passed in Turkey, Leghorn, Marseilles, Pisa and Genoa. He had good opportunities for study and reading, and before he was twelve had, as he himself records, read the great Universal History and one thousand volumes of books on many pleasing and interesting subjects. He was ravenous for facts, which he gathered, classified and wrote down in his notebooks. He began to collect fishes and birds, shells and crabs, plants and minerals, found or made names for them, copied maps from rare works, and made new ones from his own surveys. His precocious mind, unguided and undisciplined, wandered at will over the entire field of books and nature, and by the time he reached the age of nineteen he had formed his own character and equipped himself for the career which lay before him. He became a man of catalogues, of categories, of classifications. He possessed much native critical acumen, and it is possible, though scarcely probable, that as his present biographer suggests, had he during the formative period been firmly guided by some master hand, he might have become one of the world's greatest naturalists. Lacking such guidance, however, he was by no means fitted to enter upon a scientific career in a country like the United States, so when, at the age of twenty, he crossed the Atlantic he brought with him the germs of failure and bitter disappointment.

From 1802 to 1805 he lived in Philadelphia. From 1806 to 1815 he was in Sicily, where he did some of his best work in his 'Index to Sicilian Ichthyology,' and in his often quoted 'Caratteri.' Here he established his monthly journal, the 'Mirror of the Sciences' (Specchio delle Scienze, etc.), which endured throughout the twelve months of 1814, but ended with its second volume. Rafinesque was not only the editor, but almost the sole contributor to this journal, in which he printed no less than sixty-eight articles upon a great variety of subjects—upon animals, plants, minerals, meteorology, physics, chemistry, political economy, archæology, history and literature, besides many critical reviews. His fatal tendency to 'scatter' was already apparent, and in the work which he did for the 'Specchio' all the weaknesses of his subsequent career were foreshadowed. While in Sicily, for political reasons, he assumed the surname, Schmaltz, that of his mother's family.

In 1815 he returned to America, and was shipwrecked on the coast of Connecticut, losing all his books, manuscripts and collections. For the next three years he lived in New York, and during this period he contributed to the 'American Monthly Magazine' a number of really brilliant and learned articles. So masterly, indeed, were these that it seemed as if he were likely to become one of the leaders in American scientific thought. It seems probable that he was at this time steadied and guided by his friend and patron, Dr. Samuel Latham Mitchill, whom he greatly respected and admired; at all events, when he left New York, signs of deterioration appeared in his methods. In 1818 he crossed the Alleghanies, and in the following year became a professor in the Transylvania University, at Lexington, Ky.

There he remained for seven years, sadly ill at ease among the old-school college professors who composed the faculty, yet, from the showing even of his own complaints, treated with singular indulgence by them, and allowed to devote the most of his time to his excursions and to his writing. While here he printed nearly one hundred papers, chiefly descriptions of new plants and animals. From 1825 to 1840 his life was so irregular and his wanderings so extensive that his biographer has made no attempt to follow its course. Philadelphia was his home, when he had one, but he was a soured and disappointed man. His health was bad, and he could not get any one to print his voluminous writings. He established his 'Atlantic Journal,' which soon failed. He published various works by subscription, and also added to his income by the sale of 'Pulmel,' a medicine for the cure of consumption, concerning which he wrote a book. In his later years he established in Philadelphia his 'Divitial Institution and Six Per Cent. Savings Bank,' which seems to have had some degree of success. He died in 1840, in poverty and almost friendless, and is buried in an unmarked grave.

His career is described well and in sympathetic mood by Professor Call, who sums up the story of his last years in these words: "The experiences through which he had passed, which involved some of the saddest that come to men, had so broken him that there is little question that he was not of sound mind during these latest years. He was not, however, the irresponsible madman some would have us believe; rather, his was monomania and took the direction of descriptions of new forms of plant and animal life. But more than this, his defect was that peculiar form of monomania which believed only in himself; which gave his own work a value which does not always attach to it; which made him neglect the work of others, or, if it were noticed, impelled him to caustic and unwise criticism."

This judicious estimate, which is intended by Professor Call to apply only to his later years, I should be disposed with some slight reserves, to accept as a fair summary of his entire life-work, for all of the faults of his latest works were, as I have already suggested, foreshadowed in his Sicilian writings of 1814. The sympathy which I once felt for Rafinesque has almost vanished with the reading of the whole story of his life, for the man, as shown by his own private papers, appears to have been singularly unsympathetic and unlovable, enveloped in a mantle of self-esteem and interested in natural objects solely because he found in them something to name and to classify. In all his writings there appears scarcely a gleam of love or enthusiasm for nature, and he speaks of his fellow-men only in words of criticism or malediction. It would doubtless have been much better if he had never touched pen to paper. The fact that he had a keen eye and a remarkable power of diagnosis, and that he had learned the methods of systematic description, made his activity all the more pernicious, since regard for painstaking accuracy was as foreign to him as love of nature.

The canons of nomenclature which now prevail among American naturalists force them to take cognizance of all his descriptions and to use his names, whenever by any possibility his meaning can be determined. In many instances I have known him to be given the benefit of a doubt. So the unwelcome name of Rafinesque is constantly obtruding itself in almost every branch of zoölogy and botany, and it is likely to remain for a long time an obstacle in the way of securing the recognition of American nomenclature in Europe. He stands nevertheless as an important figure in early American biological literature, and whether we like him or not he cannot be ignored. It is fortunate, then, that all relating to his work has at last been brought together in so convenient a form.

The minute and scholarly bibliography,

which includes in all 420 titles, is most valuable. Professor Call's estimate of the value of these writings is a very kindly one. Bad as it was, Rafinesque's work unquestionably entitles him to recognition as the pioneer student of the ichthyology and conchology of the Mississippi valley, and he was also among the earliest to study its botany and its prehistoric archæology.

All the existing portraits of Rafinesque are reproduced, as well as a specimen of his handwriting, and in the appendix is reprinted his will, which affords a better insight into his character than all else he ever wrote.

The book is exhaustively complete, well written and beautifully printed, and in its publication the author and the Filson Club have accomplished admirably the task which they had undertaken. They have reared a noble monument to him who was 'the first Professor of Natural Science west of the Alleghanies.'

G. Brown Goode.

The Royal Natural History. Edited by Richard Lydekker. Illustrated by 72 colored plates and 1600 engravings. Frederick Warne & Co., London and New York. Royal 8°. 1894–95. Issued in monthly parts.

The second full volume of this important work is now out and, like the first, is devoted entirely to the Mammalia. The first comprised the Apes, Monkeys, Bats, Insectivores and part of the Carnivores; the second completes the Carnivores and includes also the Ungulates, Manatees and Dugongs. The well-known reputation of the editor and principal author, Mr. Lydekker, gives special value to these parts.

In general scope and plan of treatment the work resembles Brehm's *Thierleben*, of which several editions have appeared in Germany, and the *Standard Natural History*, published in this country. The illustrations are in the main borrowed from Brehm; they were pirated by the *Standard Natural History* ten years ago, and here appear for the third time. Of course this is not the fault of the author; but it is a pity original works cannot have original illustrations. Good plates are as much a part of a book as the text itself, and should be allowed to stand unmolested as monuments to the author. It is not intended to deprecate the exchange of technical figures or the judicious bringing together of scattered cuts illustrating special subjects—a very different thing from the wholesale reproduction of a previous author's pictures.

The original cuts are not of high merit. Those of the hooded seal and skull of the cave bear are gross caricatures, and nearly all the skulls and teeth are far inferior to modern standards for such work; and it is not too much to say that Mr. Lydekker himself, in previous publications, has done much toward fixing these higher standards. The colored plates are cheap chromos, in striking contrast to the excellent and artistic plates borrowed from Brehm.

In quoting American writers on 'big game' the most authentic and best informed writers are not always chosen. The one book that is beyond all comparison the best yet written on our larger mammals—I refer of course to Roosevelt's *Wilderness Hunter*—is apparently unknown to the editor. As a natural result some surprising statements are made, as, for instance, when Oregon antelope hunters are told that the pronghorn has 'almost or quite disappeared' from their State.

Some confusion arises from different usages of the common names of animals. The statement that in North America "the range of the *elk* appears to have extended originally from about the 43d to the 70th parallel of latitude, its northern limit being marked by the southern limit of the so-called barren grounds," will take the breath away from most Americans who read it for the first time, but a careful perusal of the context shows that our *moose* is the animal meant.

The hooded seal is said to be 'nowhere met with in large numbers,' a statement that will bear qualification in view of the fact that many thousands are sometimes taken by single vessels at the Newfoundland and Labrador seal fisheries. More than 15,000 were killed on the ice and brought to Newfoundland in March, 1883, by a sealer—the Proteus—which I accompanied as surgeon-naturalist, and similar catches are not rare.

In the matter of genera, the comprehensive groups of the past are commonly used instead of the smaller groups of to-day. The same conservatism characterizes the treatment of species—perhaps a good fault in a popular work, though one that can be carried too far—as when a dozen skunks are lumped under a single name, and the most specialized of our true foxes is left out.

The author seems to be constitutionally averse to the recognition of American species as distinct from their European representatives. This is shown by his treatment of our wolf, red fox, lynx, wolverine, marten and weasels. Even in the case of the mink the opinion is expressed that the American and European animals are 'mere local varieties of a single species.' The only explanation of such statements, from a man of Lydekker's experience in studying fossil mammals, is that he has not personally compared the skulls and teeth of the American and European forms. The number of American species is reduced out of all proportion to the sharpness of their characters or the size of the areas they inhabit. Thus, while three martens are accorded specific rank for Eurasia, only one is allowed for America, and it is given as doubtfully distinct. It should be stated, however, that no European collection of mammals contains more than a fraction of our species; hence it is not so surprising that a foreign author should fail to appreciate their characters.

The common skunk of New England is said to range from Hudson Bay to Guatemala, but it does not reach even the Southern United States. Again, skunks are said to be good climbers, but neither *Mephitis* nor *Conepatus* can climb trees—the ability to do this being limited to the agile weasel-like members of the genus *Spilogale*.

The article on the fur seal is full of misstatements and savors too strongly of a political argument from the British side of the case. The number of fur-seals killed at the Pribilof Islands each year is said to be 'limited to 100,000,' and it is implied that the number actually killed is still larger. As a matter of fact, 100,000 have not been killed since 1889, while the number killed at the islands since 1890 is as follows: 1890, 25,701; 1891, 14,406; 1892, 7,509; 1893, 7,390; 1894, 15,033.

We are told that the seals taken at sea (by pelagic sealers) 'appear to be exclusively young males or barren females.' In reality the great majority of these seals are breeding females. The author's ideas of humanity are simply past comprehension. He says: "Of the two methods of sealing, the shooting in the open sea is decidedly to be preferred on humanitarian grounds, more especially if it be true, as asserted, that on the Pribiloffs a considerable number of breeding female seals are killed before their cubs are old enough to shift for themselves." No female seals are ever killed on the islands except by accident—possibly one in many thousands—while in the open sea, as already stated, the great majority are females. Of these females, those killed on their way to the islands in spring are heavy with young, and those killed in Bering Sea in summer are nursing; so two lives are sacrificed for every one taken. Ever since pelagic sealing has been carried on in Bering Sea, thousands of motherless 'pups' have died on the islands each year of starvation.

It is lamentable that the author has been so grossly deceived in these matters, and still more unfortunate that a scientific work should be tainted with partisan odor.

It is stated that no islands in Bering Sea besides St. Paul and St. George are inhabited by fur-seals. This must be a slip of the pen, for of course Mr. Lydekker knows that the Commander Islands are the breeding grounds of the *west* Bering Sea herd, just as the Pribilof Islands are the home of the *east* Bering sea herd.

In the matter of nomenclature the author seems to be on the fence. In some cases the law of priority is rigidly enforced; in others a name in common use is retained rather than the earlier name. Preoccupied generic names are as a rule discarded, but *Bassaris*, though preoccupied, is given instead of *Bassariscus*—doubtless by oversight.

The author's attitude as to genera is shown by the remark that in a certain group only one genus can be admitted 'on account of intermediate forms.' Is this not a surprising position for one of the most distinguished of living paleontologists? Are not all mammals connected by intermediate forms, living or extinct, even if all are not yet discovered? And would not Lydekker's system, if logically enforced, result sooner or later in the destruction of most of our generic groups? Is it not more rational to found genera on the weight of characters as presented in extremes of differentiation rather than on the accident of the survival or extinction of annectant species?

As a general criticism of the Royal Natural History, so far as now issued, it may be said that the parts on American mammals are weak. On the other hand, the foreign species—foreign from our standpoint—are treated with a fullness and reliability not to be found in any other work. The magnitude of the undertaking and the haste in which the parts had to be prepared (to appear monthly) inevitably led to occasional inaccuracies; but the defects are far outweighed by the merits, and the work will prove helpful to naturalists and amateurs alike for many years to come. It is, indeed, a great satisfaction to be able to turn to a single publication in which the principal facts respecting the mammals of the world are brought down to date and stated with clearness and authority.

C. Hart Merriam.

The Book of Antelopes. By P. L. Sclater and Oldfield Thomas. Illustrated by Joseph Wolf and J. Smith. 4°, London, R. H. Porter, 1894–95.

The second part of this handsome and useful work, dated January, 1895, has come to hand. The distinguished authors make no attempt to offer a complete scientific treatise on the antelopes, but furnish "descriptive letter-press [with full synonymy] for the beautiful series of lithographic plates drawn some twenty years ago under the supervision of the late Sir Victor Brooke, making thereto such necessary modifications and additions as the progress of science demands."

The work comprises the diverse members of the Bovidæ commonly called antelopes, hartbeests, gnus, duikers, water-boks and gazelles, and also the gemsbok, saiga, oryx, eland and many others. The geographic range of each species is given, together with an interesting account of its habits and peculiarities. Besides the full page colored plates, there are many excellent cuts in the text, mostly of horns and skulls. The book therefore is helpful alike to the naturalist and the sportsman, and is a handsome addition to any library.

The animals treated in the first two parts are the hartbeests and gnus (genera *Bubalis*, *Damaliscus* and *Connochœtes*), all belonging to the subfamily Bubalidinæ, and residents of Africa and Arabia. Twelve colored plates have been issued, and seven are promised with the next number, which will be devoted to the duikers (*Cephalophus*).

C. H. M.

NOTES AND NEWS.

Prof. S. Calvin, State Geologist of Iowa, announces that reprints of the photographs accumulated by the survey may be had for $12\frac{1}{2}$ cents each. A descriptive list of views may be had on application to the State Geologist at Des Moines; all orders to be made by the numbers of this list. If this practice were generally adopted by our State Surveys, it would be greatly to the advantage of many students and teachers.

Prof. W. R. Newbold, of the University of Pennsylvania, has become one of the associate editors of the *American Naturalist.* In the current number he gives an account of 'The Present State of Psychology.'

Dr. Wirtinger has been made Professor of Mathematics in the University of Innsbruck.

Dr. Wilder D. Bancroft, now Instructor in Harvard University, has been appointed Assistant Professor of Physical Chemistry in Cornell University.

Prof. Francis Gotch, now of University College, Liverpool, has been elected to the Waynflete Chair of Physiology at the University of Oxford, vacant by the transference of Prof. Burdon Saunderson to the Regius Professorship of Medicine.

James E. Oliver, Professor of Mathematics in Cornell University, died at Ithaca, on March 28th.

The Duke d'Orléans has presented the Imperial Institute of London with his extensive collection of specimens of natural history, costumes and curiosities.

Mr. Lester F. Ward writes that he has just received from the family confirmation of the reported death of the Marquis Saporta. He died at Aix on January 25th.

Dr. Nikola Tesla suffered a serious loss in the destruction of his laboratory by fire on March 13th.

General de Masouty, Founder and Director of the Pic du Midi Observatory, died recently at the age of eighty years.

Dr. Harrison Allen has been elected a member of the Council of the Philadelphia Academy of Natural Sciences, to fill the vacancy caused by the death of John H. Redfield.

The Joint Commission of the Scientific Societies of Washington has begun to publish a monthly programme, giving the dates of meeting of the various scientific societies of Washington for the ensuing month and a full list of papers to be presented.

At a meeting of the New York Alumni of the Johns Hopkins University, on March 29th, President Gilman made an address on 'Impending Problems in Education.' An address was also made by Professor Butler, of Columbia College.

A National Exhibition of Industry and Fine Arts in the City of Mexico will be opened on April 2d, and an International Exposition is proposed for Montreal, to be opened on May 4th.

At the last meeting of the Academy of Natural Sciences at Philadelphia, Miss Emma Walter read a paper entitled *Does the Delaware Water Gap Consist of Two River Gorges?* She adduced evidence to show that the river once flowed through the Gap from the south towards the north; that this north-flowing river was pre-glacial, and that much the greater part of the erosion is the work of this old river, the remainder being due to the action of the present south-flowing stream.

SOCIETIES AND ACADEMIES.

NEW YORK ACADEMY OF SCIENCES.

The Section of Geology and Mineralogy met March 18, and after electing as officers for the ensuing year, Prof. J. J. Stevenson, chairman, and Prof. J. F. Kemp, secretary, listened to a lecture by Prof. J. J. Stevenson on 'The Origin of the Pennsylvania Anthracite,' of which the following is an abstract:

Long ago H. D. Rogers showed that the coal regions of Pennsylvania are divided into longitudinal basins or troughs. The first series embraces the area between the Great Valley and the Alleghany Mountains and contains the several anthracite fields as well as the semi-bituminous fields of Broad Top and the Potomac River. Beyond the Alleghanies are six well marked basins containing bituminous coal.

Along a line from central Ohio, eastward to the Potomac coal field, one finds noteworthy variations in dip, the amount being insignificant in Ohio, but very great in the first series of basins. The increase is not regular, there being no change practically from the coke basins of eastern Pennsylvania until within three or four miles of the Potomac field, where the dip becomes very abrupt. This line shows the extremes of variations, for further northward there is in all of the basins a diminution of disturbance, even in the anthracite areas, while southward there is a similar decrease, except in the last.

Analysis of coal samples from the Pittsburg bed, in the several basins, show a progressive decrease in proportion of volatile matter toward the east or southeast. H. D. Rogers regarded this decrease as due to influence of steam or other gas escaping from crevices made during the folding of the rocks, for he asserted that the volatile increased as the flexures diminished in strength. Stevenson in 1877 showed that no such relation exists. Lesley in 1879 thought that earth-heat might have caused the change, as coals in the anthracite region were buried under a very deep covering of rocks; but there is no evidence that the coal measures were thicker at the east than in western Pennsylvania, while there is every reason for supposing that the coal measures were thinner there than at the southwest. There is therefore no good ground for supposing that the earth-heat would be effective, for in Virginia, where the thickness is very great, the coals at the bottom of the column are very rich in volatile matter.

Professor Lesley has suggested that the change in the coal might have been due to oxidation. The rocks of the anthracite region are consolidated gravels with little of argillaceous matters, whereas those of the bituminous area are largely argillaceous, which, being undisturbed, lute down the coals, preventing percolation of water and the escape of gases. But in fact the bituminous fields afford all types of coal from highly bituminous to hard anthracite, and sections in many portions of the anthracite fields show more clay beds than do those in S. W. Virginia where the coal is highly bituminous.

It is not necessary to regard metamorphism as the sole cause of anthracite. It is not called in to explain a variation of ten per cent. in the same beds within short distances, and it cannot explain the occurrence of bituminous in one bench and of anthracite in another in the same opening in Sullivan County, Pa., or equally of semi-bituminous and dry anthracite in different benches of the Mammoth. It does seem as though the conversion of the coal must have been practically complete before entombment; otherwise the variations of coal of the same age in different areas would seem to be inexplicable.

In Pennsylvania the decrease in volatile bears no relation to the extent of plication, but it bears close relation to the thickening of the coal. The decrease in all of the areas is toward the old shore line at the north and northeast. In the anthracite area it is very gradual until one passes the prongs in the southern field, where the thickness of coal increases abruptly. With that abrupt increase in thickness is an equally abrupt change in the amount of volatile. It seems probable that the anthracite of Pennsylvania is due to the long continuance of coal-making periods during which the chemical change was unchecked, leading eventually to complete loss of the hydrogen and oxygen.

At the conclusion of the paper, discussion followed, but failed to shake the speaker's main points. A paper by J. E. Wortman, on 'The Geology of the Bad Lands,' was postponed until the next meeting.

J. F. Kemp,
Secretary.

SCIENTIFIC JOURNALS.

BULLETIN OF THE AMERICAN MATHEMATICAL SOCIETY, MARCH.

Arthur Cayley: Professor Charlotte Angas Scott.

The Theory of Functions: Professor W. F. Osgood.

On the Introduction of the Notion of Hyperbolic Functions: Professor M. W. Haskell.

Notes; New Publications.

THE JOURNAL OF THE AMERICAN CHEMICAL SOCIETY, APRIL.

The Superiority of Barium Hydroxide Solution as an Absorbent in Carbon Determinations in Steel: James O. Handy.

The Contributions of Chemistry to the Methods of Preventing and Extinguishing Conflagration: Thomas H. Norton.

Note on the Estimation of Iron and Alumina in Phosphates: K. P. McElroy.

Some Practical Points in the Manufacture of Nitroglycerol: J. E. Blomén.

Methods for the Examination of Glycerol for use in the Nytroglycerol Manufacture: G. E. Barton.

Estimation of Tellurium in Copper Bullion: Cabell Whitehead.

The Use of Sulphurous Acid ($HNaSO_3$) in Manufacture of Glocose Syrup and Grape-Sugar: Horace E. Horton.

The Furfurol-Yielding Constituents of Plants: C. F. Cross, E. J. Bevan and C. Beadle.

The Separation of Solid and Liquid Fatty Acids: E. Twitchell.

Improved Methods of Water Analysis: Irving A. Bachman.

A Cheap Form of Self-Regulating Gas Generator: W. W. Andrews.

Some of the Properties of Calcium Carbide: F. P. Venable and Thomas Clarke.

Note on the Determination of Zinc: P. W. Shimer.

On the Determination of Cane-Sugar in the Presence of Commercial Glucose: H. A. Weber and William McPherson.

On the Action of Acetic and Hydrochloric Acids on Sucrose: H. A. Weber and William McPherson.

Method of Determining Chromium in Chrome Ore: Edmund Clark.

New Books; Notes.

NEW BOOKS.

Manual of Geology. James D. Dana. Fourth Edition. New York, American Book Co. 1895. Pp. 1087.

A Course of Elementary Practical Bacteriology. A. A. Kanthack and J. H. Drysdale. London and New York, Macmillan & Co. 1895. Pp. xxii+181. $1.10.

Elementary Biology. Emanuel R. Boyer. Boston, D. C. Heath & Co. Pp. xxi + 235.

The Geological and Natural History Survey of Minnesota. N. H. Winchell. Minneapolis, Harrison & Smith. 1895. Pp. 254.

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Subscriptions and advertisements should be sent to SCIENCE, 41 N. Queen St., Lancaster, Pa., or 41 East 49th St., New York.

*THE EDUCATIONAL AND INDUSTRIAL VALUE OF SCIENCE.**

ON the occasion of the formal dedication of a building devoted to the teaching of science, it is fitting that something should be said respecting the claims of science to such generous recognition and such ample provision for its cultivation in a young university, established by a Commonwealth itself still 'in its teens.' In the Atlantic States the stagecoach is almost obsolete. It has given way to the railway, and it is an open question whether transportation by steam will not ultimately yield to the agile trolley wheel. So the old-time college, devoted to the ancient languages, mathematics, and a little leaven of moral philosophy, with its slow-going ways, its simple outfit of benches, a teacher's desk and a chapel, has been superseded by the modern university, with its complex organization, its multiplicity of courses and subjects of study, its laboratories and equipment, and its corps of trained, eager, alert instructors, who are not expected to teach a book only, but to add to the sum of human knowledge, and to awaken in kindred spirits at least an enthusiasm for study, a delight in investigation, which has proved the most efficient stimulus to high intellectual attainments. The erection of the Hale Scientific Building indicates that the University of Colorado aims to pursue its way untrammeled by ancient traditions, with the spirit of modern ideas in education, and in touch with the most progressive institutions of learning.

Shall we pause a moment to inquire what has wrought this change in the aims and methods of higher education in the United States? What new conditions make it possible for a young university like that at Chicago to forge toward the front in two or three short years? Universities have always been considered as institutions of slow growth. They represent the accretions of

*Read at Boulder, Col., March 9, 1895.

years and centuries even, if we broaden our view sufficiently to include those of Europe. Such indeed are the customs, the traditions and the general policy of a great university with decades or centuries of history behind it. Every ancient seat of learning has a character peculiarly its own. There is an indescribable charm attaching to crumbling, ivy-cumbered walls; to time-stained libraries, that point with motionless fingers back toward their more silent authors; a subtle influence in the steady gaze of the famous sons of the college, as they look down on the younger generation from the deepening canvas in the memorial portrait hall. Who that has a fibre of his soul tuned to vibrate in unison with melodies of the past can fail to feel an energetic thrill as he stands among the distinguished sons of the Harvard of former years ranged around the walls of 'Memorial Hall,' or as he walks softly through the portrait gallery of Christ Church College in Oxford? These influences are not to be despised. They are an inheritance from the long past and are still potent. Addison still walks under the arching trees by the quiet stream at the back of Magdalen College; Wolsey and Wesley and Gladstone still linger in the noble hall of Christ Church; and Newton's rooms remain near the imposing gateway of Trinity College in Cambridge. I love to step within the charmed circle of such subtle influences, to yield to the magic spell, and to count myself a part of all this glorious past.

But the modern spirit prevades the oldest institutions, and great seats of learning are rising on new foundations. In both old and new the most marked characteristic of the teaching of the present is the scientific method. It has pervaded every department and has proved the leaven that, being taken and hid in the ancient curriculum, as inert as the three measures of meal, has leavened the whole. Till the introduction of serious scientific study with laboratory facilities, the educational methods which had prevailed for centuries were still current. As late as twenty-five years ago in a respectable New England college it was not possible for a student to learn his science by means of laboratory study. All this has now changed, and no less important a change has taken place in the teaching of language and literature. It is significant that this advance in pedagogical practice, the introduction of the method by investigation as compared with mere memoriter acquisition, has been coincident with the introduction of the serious study of science into our American colleges and universities. Twenty-five years ago the Massachusetts Institute of Technology led the way by introducing the physical laboratory into the study of physics. Some progress had already been made in the teaching of chemistry by direct contact with chemical reactions at the work table. It is only fifty years since Liebig inaugurated the system of studying chemistry by the laboratory method, and it is highly probable that the physical laboratory established by William B. Rogers in Boston marked the introduction into the regular curriculum of instruction in physics by experiment.* I venture to say that no greater success has followed any new departure in education. The physical laboratory is now a necessary part of every institution devoted to higher learning; its growth has been phenomenal. Enormous sums of money have been expended for physical laboratories and their equipment. The example set by this oldest branch of science has had a most beneficent influence in several directions. It has improved the quality of the work in the secondary schools. The physical laboratory is now a necessary part of every first-class high school equipment. It has also stimulated and advanced original work. Every

*Professor Mendenhall in *The Quarterly Calendar* of the University of Chicago, August, 1894.

instructor competent to fill a professor's chair in physics is now expected to add something to the stock of knowledge by his independent investigations. It has thus made graduate instruction possible in American universities, a movement having the most hopeful outlook and of the most profound educational import.

A third and most complete leavening influence is that the method by experiment and original investigation adopted by science has compelled other departments of learning to become its imitators, so that now the laboratory method prevails in nearly every department of learning. This result is too patent to be questioned even. Psychology, language and history have yielded to the powerful example set by physics and chemistry. Archæology has its work-room, its laboratory; language its photographs, its projections, its casts and reproduction of ancient life and times; while psychology has appropriated not only the methods, but the apparatus of the physicist.

Now a movement which has been such a powerful operator in solving the problem of education in every branch of learning has a significant value in the intellectual training of American youth. In fact, the value of science in any system of liberal education is so generally admitted that it is an almost needless expenditure of energy to enter into a discussion relative to its merits. It is no new comer for whom room is benevolently or patronizingly made in order that it may display its powers and demonstrate its worth. It acknowledges other claimants as peers, but admits no superiors. It came long ago to stay.

I should like to point out two or three aspects of the study and pursuit of science not often alluded to or recognized, but on which I lay much stress. The first relates to the cultivation and chastening of the faculty of imagination. Sir Benjamin Brodie said in a presidential address to the Royal Society many years ago: "Physical investigation, more than anything besides, helps to teach us the actual value and right use of the imagination—of that wondrous faculty which, left to ramble uncontrolled, leads us astray into a wilderness of perplexities and errors, a land of mists and shadows; but which, properly controlled by experience and reflection, becomes the noblest attribute of man, the source of poetic genius, the instrument of discovery in science, without the aid of which Newton would never have invented fluxions, nor Davy have decomposed the earths and alkalies, nor would Columbus have found another continent." It would be a grievous mistake to suppose that the cultivation of science contributes only to accuracy and exactness; to the development of the habit and power of observation, and to the education of the reasoning faculty as applied to the concrete—to the objects and phenomena of nature. All of these constitute a valuable training and are demonstrable results of an honest effort to understand and coördinate the phenomena of nature. But as soon as the student of science passes beyond the mere elements he must train himself to the habit of conceiving things which "eye hath not seen, nor ear heard, nor have entered into the heart of man." He must emancipate himself as much as possible from the domination of his sensations, and must learn that sense-perceptions should not be projected into the outer world of nature, but that they are only symbols of objective phenomena presented to consciousness, which the imagination, aided by reason and reflection, must interpret. Not only is the imagination called into activity by the common occurrences of the natural world lying along the level and the horizon of man's experience, but it is powerfully stimulated by the more remote phenomena above him and below him. Man contemplates the starry firmament on high, the spangled heavens, flecked with barely discernible patches of light; he puts together these trembling nebulæ, as the dismembered parts of a puzzle panorama of the heavens; and out of them all, triumphant over time and space, he constructs a nebular theory of the visible universe. He thus concludes that the various bodies of the solar system "once formed parts of the same undislocated mass; that matter in a nebulous form preceded matter in a dense form; that as the ages rolled away, heat was wasted, condensation followed, planets were detached, and that finally the chief portion of the fiery cloud reached, by self-compression, the magnitude and density of our sun" (Tyndall).

On the one hand, the telescope and spectroscope are aids to the imagination in penetrating the almost inscrutable mystery of the skies; on the other, the microscope enables it to descend somewhat into the no less limitless underworld, and to sink the exploring plummet to depths as far removed from the field of the microscope as the celestial boundaries are beyond the vision attained by the telescope.

How wonderful, also, is the ethereal medium which man's imagination has constructed, the vehicle of the energy wafted to us from sun and stars! To the mental vision this medium fills all space and quivers with radiant energy—that winged Mercury, bearing messages to man from all the worlds on high. Even electrical and magnetic phenomena are utterly inexplicable without it. The imagination of Faraday, of Maxwell, and of Hertz, has woven out of it a texture of lines of electric and magnetic force, which are as real to the electrician as the machines and conductors which he mantles with them. Every conductor conveying a current, every permanent or electromagnet, is surrounded with its system of lines of force in the ether. And when an alternating current traverses a conductor these lines of magnetic force are propagated outward from it in waves which spread with the velocity of light. In fact, they are identical with light objectively, except in point of wave-length. Thus the theory, imagined by Maxwell with the insight of marvelous genius, and confirmed later by the classical experiments of the lamented Hertz, is now accepted doctrine by physicists the world over. The existence of the ether is now seen to be a necessary consequence of Roemer's discovery in 1676 of the finite speed of light. For the transmission of light is the transmission of energy; and a medium of transmission is a necessary postulate as the repository of this energy during the time of transmission. Newton imagined the light-giving body projecting minute particles, or corpuscles, through space and carrying their energy with them as a bullet carries its energy to the mark. These entering the eye excite vision by impact upon the retina. But Newton's corpuscular theory failed because of its final complexity and the crucial test applied to it by the great experimenter, Foucault.

The undulatory theory, on the other hand, requires a continuous medium, and the energy is handed along from particle to particle as an undulation. In this way energy is conveyed by sound and by water-waves across the surface of the sea. According to this theory, a luminous body is the center or source of a disturbance in the ether which is propagated in waves through space. They are electromagnetic in origin, travel with the velocity of light, and entering the eye excite the sense of vision. Thus far have we been helped along by the imagination of genius and the contributory aid of experiment. Mean and unfruitful indeed is the science which has not been enriched, extended and vivified by the scientific imagination. Where dull reason halts and the understanding is confounded by appalling obstacles, imagination overleaps them all and the barriers are dissolved away. The boundaries of scientific inquiry have thus been moved forward and new territory has been added to the cultivated domain.

Again, let me direct your attention to another feature attending the prosecution of scientific research. While it is undoubtedly destructive of credulity, and is perhaps but a weak ally of faith, it is nevertheless a powerful promoter of honesty. The object which the scientific investigator sets before him is to ascertain the truth. He is devoted to it and pursues it with unremitting toil. But this is not all. He not only seeks truth, but he must be true himself. It is difficult to conceive of any circumstances which would induce him to play a dishonest part in scientific research. He has every inducement not only to accuracy but to honesty. He may unwittingly blunder and fall into error, but if he is untrue he is certain to be exposed. No discovery is permitted to go unverified. It must undergo the searching examination of scientific inquiry. The investigator must submit his data and must seek to have his results confirmed. There is, therefore, every inducement for him to be absolutely truthful. This condition imposes upon him also the habit of conservatism and moderation in statement. He is not expected to plead a cause or to make the most of the occasion for himself. In this regard his position is in contrast with those whose profession makes them the allies of faith, but whose moderation is not always known to all men; for their assertions are not brought to the touchstone of revision and justification, and the released word flies over the unguarded wall. The habit of the scientific investigator is to subject every question to the scrutiny of reason and to weigh probabilities. He obeys the injunction, "Prove all things; hold fast that which is good." He respects conscience, but has no use for credulity. He exhibits devotion to principle, but dogmatism, whether in science or religion, has no place in his creed. He looks not only upon the things which are seen, but also upon the things which are unseen. You may suffer me to remind you that the most noted American atheist is not a man of science, while one of the forceful books of modern times, 'The Unseen Universe,' which aims to lay a foundation for belief in a future life without the aid of inspiration, was written by two distinguished physicists. Science examines the foundations of belief. It takes nothing from mere tradition, on authority, nor because it is an inheritance from the past. It admits its own limitations and the somewhat circumscribed boundaries set to the field of its inquiries; but within this province it seeks to ascertain only the truth. It recognizes not only the promise and potency of matter, but the power which makes for righteousness.

Turning now to some more practical matters, it is strongly urged that the study of science should begin early, before the taste for such study has become atrophied by too excessive attention to language and mathematics. It is a fact established by observation that if a student gets his first introduction to science only after he is well along in his college course he comes to it with a mental inaptitude that often produces discouragement and precludes the possibility of much satisfaction in its pursuit. The procedure in scientific study, especially when it includes the method of the laboratory, is so radically different from that involved in the study of language that one trained only in the latter finds himself in a foreign field when he enters the former. The study of language, considered merely as the symbolism of thought, or the instrument for its expression, is most valuable and essential. You shall hear no word from me designed to depreciate the value of linguistic study and training. It is rather to be deprecated that scientific men do not generally pay more attention to the formation of a correct English style, and do not oftener acquire the ability to express the results of their studies in more elegant English diction. On the other hand, an exclusive training in the so-called humanities leaves the student unsymmetrically developed. The elementary study of language is largely a study of the forms and symbols of speech; to the young student, at least, the thought is altogether a secondary consideration. Mathematics furnishes a training in the relations of abstract number, and in the manipulation of symbols invented to facilitate operations expressing the relations between related quantities. It is not only a valuable agency in mental development, but it is a powerful instrument for the investigation of phenomena in those branches of science to which applied mathematics is indispensable. Science has more to do than either language or mathematics with objective phenomena. The student of science soon finds that he has a new set of relationships with which to deal. He may be familiar with mathematical theorems and solutions, but his first difficulty is to see the points of attachment of mathematics to the facts of physical science. He is armed with a weapon of most modern design and exquisite workmanship, and he has possibly obtained some skill in target practice, but he has no eye for game. He may be too short-sighted to see that there is any game even.

Skill in the use of scientific methods of reasoning and acquirement comes only after the mind has been kept for some time in contact with science, so that it has acquired the scientific spirit and aptitude. The preparation for the scientific work of the university should therefore begin in the secondary schools. Continuity in scientific acquisition is as essential as in that of language or mathematics. While six, or even eight, years are given to language in the high school, counting the four years with three studies each as twelve, it is thought by some to be an evidence of great magnanimity if two years out of the twelve are given over to the mere elements of physical and biological science. It is obvious to any careful observer that much improvement has been made in the teaching of science in secondary schools within the last few years. More competent teachers are employed, laboratory facilities have been provided, better manuals have been written, and the tone of the science department has been improved by the fact that preparation in science at last leads to something further in the university. This continuity in the pursuit of scientific studies has already furnished qualified teachers for the lower schools. What wonder if the teaching of science in the schools should not have proved as fruitful as was once hoped! Till recently language and mathematics have had the training of the teachers throughout our whole educational history, and if science secured entrance to a secondary school at all it got there in a secondary place. All that science asks is to be placed on equal footing with other lines of study. It demands no preferences and is strenuous that no ultimate bounties shall be extended to other branches. There should be no favored nations in the world of education. It recognizes no excellences in language or literature to justify superior awards at graduation. There are no sacred vessels in education which science may not touch, no shibboleth which she cannot pronounce, no holy of holies which she should be forbidden to enter. The ideal culture course is not all science, not all language, and not all mathematics, but a judicious combination of these and other branches. It would be no less logical for one to make one's course chiefly science than to make it chiefly language; but when the student has successfully completed his course, making due allowance for personal differences and needs, no reason seems to me valid for not crowning the equivalent work of all with the same degree.

Reference to the other aspect of my subject has, perhaps, been too long delayed. Science has not only educational value of a high order, but industrial applications as well. Discovery and scientific training precede invention. The quality of mind that discovers the laws of nature is of a higher order than that which makes application of them. The genius of Faraday and Henry, who discovered the laws of magnetic induction, must not be dimmed or diminished by reduction to the level of even the greatest living inventors. The contributions of these men to the well-being, comfort and happiness of mankind cannot be over-estimated. They laid the foundation in magnificent discoveries of those splendid applications which have dazzled the world in recent years. So thoroughly intrenched in theory and practice are Faraday's conceptions at the present day that they enter into every design of motor or dynamo. They have been shot through the entire body of practice and are intertwined with every thread of electrical thought.

On the other hand, one must not fail to note that the wonderful applications of science have reacted in a favorable way upon theory and investigation. They have proved an effective stimulus to research and have furnished a multitude of problems for original investigation. Scientific discovery and inventions involving scientific laws are two handmaids of national improvement. They are largér agencies for the advance of modern civilization than any others. Astronomy has made splendid contributions to navigation since Galileo suffered for teaching that the earth revolves daily on its axis and yearly round the sun. It has also made possible modern chronometry by giving us the accurate unit of time. The contributions of modern chemistry are so numerous and so important that it is difficult to particularize. It has taken a useless refuse of the gas retort and converted it into resplendent dyes that rival the gorgeous colors of the rainbow. It has improved and cheapened the processes of manufacturing iron till the cost of the ore and the fuel control the price of the product; and old establishments, far removed from the cheap supply of either, have had to succumb to the march of events.

Bacteriology, the ally of chemistry, working largely by chemical methods, gives the fairest promise of discovering the cause and the prevention of disease. Its beneficent aim now is to devise methods of securing immunity from the most deadly diseases, whose ravages are greater than those of great civil wars. Important discoveries in this direction are impending, and medicine is fast becoming a science instead of a body of empirical rules.

Bacteriology has already isolated and identified a large number of pathogenic or disease-producing germs and hopes in time to corral them. It has demonstrated that disease is not due simply to the presence of the bacillus, but to the specific poison resulting from its growth. It has added consumption and pneumonia to the list of infectious diseases; and the discovery of the cause is a long stride toward the goal of prevention.

The specific direction in which the large body of scientific discovery is turned to practical account is in the several branches of engineering. The civil, mechanical, electrical and mining engineers are the prophets of the new civilization. They have pierced the highest mountains; hung highways over the most dizzy cañons; constructed a rushing steed that feeds on the compressed vegetation of the carboniferous age and wearies not; they have brought the nations together so that the great oceans scarcely separate them; they have bound continents together by wonderful cables embedded in slimy ooze at the bottom of the sea. Eiffel reared his tower a thousand feet to pierce the sky; Baker projected three of his out 1700 feet horizontally without staging to bridge the Firth of Forth; and over them fly four hundred trains daily without slackening speed; each span is longer than the Brooklyn bridge, and there are three spans. The seven wonders of the world have become seventy, and still the modern engineer pauses not. He now soberly contemplates a deep waterway from the great Northwest to the Atlantic coast. He has not even abandoned the problem of aerial navigation, but attacks it on a new principle. Archimedes is said to have declared that if he had a place for a fulcrum he could move the world. Professor Vernon Boys has just weighed the earth and determined its density to the third decimal place by means of two gilded balls suspended by a fiber of quartz, finer and stronger than a spider's web. Not content that the earth yields her yearly increase, and that the sea furnishes abundant food, the engineer burrows into the eternal hills and seeks for hid treasures in the depths of the earth. The gold and the silver he wishes to be his also. He even establishes an electric plant some 1600 feet underground, converts the power of the descending stream of water into electric energy, and sends it back to the surface for further service.

He has contemplated the colossal cataract at Niagara not only as a display of natural grandeur, but as an example of unlimited power running to waste. At last he is nearly ready to recover a small part of this power and to transmit it to distant cities, where it may turn the wheels of industry or be transmuted into light. No grander problems remain for solution than those even now confronting the electrical engineer. The swiftness with which he has already passed from one almost insurmountable task to another has amazed no one more than those most familiar with the means employed. If electrical engineering is still in its infancy it is certainly a giant infant. It has long since outgrown its toys. With the nerve and audacity of vigorous young manhood it quails before no obstacles and acknowledges no impossibilities. Having practically banished the plodding horse from the street railway, it is getting ready to enter the lists against the locomotive. If your city is not seated near a source of power it will undertake to bring the power to you. The mountain can not go to the city, but the city can go to the mountain for its power. Electrical engineering stands at the door of the twentieth century, ready to accept the tasks that it imposes, and eager to enter upon a new period of discovery and application.

A marked feature of educational history in the United States for the past twenty-five years is the rapid increase in engineering schools, partly on independent foundations, and partly as a professional department of universities. Of this latter class the only ones existing a quarter of a century ago, so far as I know, were the Lawrence Scientific School at Harvard, the Sheffield Scientific School at Yale, and the courses in Civil Engineering in the Universities of Pennsylvania and Michigan. The first two, as their name implies, were devoted quite as much to the teaching of pure science as to engineering. They attracted but little attention, and in fact the Lawrence School had but a moribund existence for many years after the establishment of the Institute of Technology in Boston. Recently it has had new vigor infused into it and has profited by the growing interest in engineering education. Cornell and the State Universities have led the way in the establishment of engineering schools, and their example has been followed in a way that demonstrates more completely than anything else could that a popular demand exists for engineering instruction.

Civil engineering came into the University of Michigan in 1853, with the late Dr. Alexander Winchell, as an adjunct of Physics. It had an independent instructor in 1857 in the person of Professor De Volson Wood, who is well known in the profession at the present day. Mining engineering followed in 1875. Mechanical engineering was introduced by a professor detailed from the U. S. Navy Department in 1881. Finally the course in electrical engineering was begun in 1889. The success of this last course has more than justified its introduction, as the roster of students in it already exceeds that of either of the older engineering courses. This growth is attributable to the popular interest in the subject.

The engineering courses are primarily professional as distinguished from the literary curriculum. They lay the foundation in theory and a moderate amount of practice for distinguished careers in a private professional capacity and at the same time in the service of the State. A large portion of the graduates of American technical schools have been very successful in their professional career. The presence of a considerable body of trained engineers, distributed throughout the country, has had a marked influence on the number and character of the public improvements made. If a great commonwealth is justified in maintaining an institution of higher learning because of the public weal, as I fully believe it is, then the maintenance of schools of engineering is approved by considerations of high public interest.

From an educational point of view, the courses in engineering furnish a thorough and by no means narrow intellectual training. The rigid discipline in pure and applied mathematics, the courses in physics and chemistry, the attention given to modern languages, are all additional to the special instruction in engineering studies; and while they serve as a foundation for them their value as a means of intellectual culture are just as great as if they were pursued for this purpose alone. An eminent scholar, Professor Ritter of Germany, has recently testified to the success of technical education in the United States and says that the Americans have outdone Europeans in this regard. The theoretical side of the technical branches Professor Ritter believes to be less solid here than in Germany; but against this defect he sets the "truly grand achievements in engineering and machine construction in the United States." In the normal growth of our engineering courses they will gradually be strengthened on the theoretical side. At the same time we can not guard too carefully against the crowding out of that amount of practice obtainable from a well-equipped engineering laboratory and such tests of actual machinery as may be accessible. The highest justification of the American plan of engineering schools is to be found in the prominent part taken by comparatively recent graduates in the most difficult undertakings of engineering practice.

In the provision for science and engineering, indicated by the dedication of the Hale Scientific Building, the University of Colorado is following the best examples of American education. It has made a noble beginning in the cultivation of science, the augury we may be permitted to hope of a brilliant future. A wide world of discovery yet remains. The remark of an eminent physicist that the future discoveries of physical science are to be looked for in the sixth place of decimals is rendered rather ludicrous by the recent discovery of 'Argon,' a new constituent of the atmosphere, composing about two per cent. of its weight. If the air we breathe can furnish a new and almost unsuspected element, what other surprises may hide in equally common things? The twitching of a dead frog's leg a hundred years ago started a train of discoveries in electricity that have revolutionized the world. But Galvani was not the first anatomist who used the frog as illustrative material. Science knows no ultimate limits beyond which she may not go. The mountains of Colorado are not yet exhausted of their precious metals, nor has nature yet thrown up her hands as a signal that she no longer resists the uncovering of all her treasure.

I bear to you the congratulations of the Mother of State Universities, and the wish that this institution may be an intellectual light attracting the youths of Colorado, and a glory to this great Commonwealth.

Henry S. Carhart.
University of Michigan.

THE GROWTH OF FIRST-BORN CHILDREN.

During the year 1892 I made arrangements for a series of measurements of school children, one of the objects of which was the determination of any existing difference between the growth of first-born and later-born children. The measurements were taken in Toronto, under the direction of Dr. A. F. Chamberlain, and in Oakland, Cal., through the kindness of Professor Earl Barnes. The following table contains the results of the observations taken in Oakland.

The columns named 'Differences' gives the amount to be added to the average stature and weight in order to obtain the statures and weights of first-born and later-born children. The figures printed in parenthesis designate the numbers of individuals measured.

Statures of Boys in Millimeters.

Ages. Years.	Average Stature.	Differences Between Average Stature and Stature of: First Born Children.	Second Born Children.	Third Born Children.	Fourth Born Children.	Later Born Children.
6.5	1137 (145)	+ 7 (30)	+ 7 (39)	—13 (25)	— 2 (16)	— 5 (33)
7.5	1180 (197)	+11 (49)	— 4 (42)	+13 (31)	± 0 (24)	—10 (46)
8.5	1249 (234)	— 3 (57)	— 7 (54)	— 1 (32)	—18 (25)	—21 (61)
9.5	1283 (220)	+ 2 (57)	— 2 (47)	+ 5 (38)	+ 5 (23)	+ 1 (46)
10.5	1334 (243)	± 0 (66)	+33 (49)	—18 (41)	—15 (35)	— 8 (47)
11.5	1379 (208)	— 1 (58)	+ 1 (39)	+16 (32)	—13 (27)	— 1 (45)
12.5	1426 (230)	+20 (66)	— 1 (47)	— 4 (38)	— 5 (36)	—19 (41)
13.5	1482 (184)	+16 (54)	+10 (43)	+16 (28)	—31 (26)	—25 (30)
14.5	1556 (163)	+11 (46)	—19 (40)	+ 4 (27)	± 0 (25)	+ 8 (24)
15.5	1632 (118)	+ 6 (35)	+ 8 (29)	—18 (22)	—14 (15)	+ 4 (17)
16.5	1668 (116)	—19 (29)	+17 (30)	+21 (18)	—20 (13)	± 0 (25)
Average Differences.		+4.5	+4.0	+1.9	—7.9	—6.9

Statures of Girls in Millimeters.

Ages. Years.	Average Stature.	Difference Between Average Stature and Stature of: First Born Children.	Second Born Children.	Third Born Children.	Fourth Born Children.	Later Born Children.
6.5	1125 (113)	+11 (32)	± 0 (28)	— 9 (15)	—16 (10)	— 1 (28)
7.5	1175 (199)	+ 8 (49)	— 1 (40)	+ 3 (44)	— 4 (24)	—11 (42)
8.5	1226 (221)	+14 (52)	—11 (46)	— 9 (43)	+13 (19)	— 4 (61)
9.5	1277 (252)	— 4 (65)	— 3 (57)	+14 (47)	—17 (21)	+ 5 (50)
10.5	1335 (224)	+ 7 (59)	— 2 (46)	+15 (28)	— 6 (26)	—11 (59)
11.5	1389 (226)	+12 (52)	+10 (41)	— 3 (32)	+ 3 (34)	—14 (61)
12.5	1450 (283)	+ 3 (65)	+14 (56)	— 1 (55)	+ 7 (40)	+ 8 (67)
13.5	1516 (222)	— 3 (62)	+ 9 (48)	—19 (38)	+ 6 (29)	+ 9 (45)
14.5	1566 (241)	+ 9 (61)	± 0 (68)	— 8 (38)	—17 (23)	— 1 (49)
15.5	1577 (170)	— 2 (42)	+11 (36)	— 6 (32)	— 1 (19)	— 5 (41)
16.5	1597 (127)	+15 (30)	—38 (28)	— 3 (23)	— 1 (14)	—18 (32)
17.5	1597 (99)	+10 (30)	—21 (19)	— 8 (19)	± 0 (15)	+14 (16)
18 & older	1602 (82)	+12 (27)	— 5 (20)	—25 (10)	—10 (9)	— 1 (16)
Average Differences.		+7.1	—2.8	—4.5	—3.3	—2.3

WEIGHTS OF BOYS IN POUNDS.

Ages. Years.	Average Weight.		Difference Between Average Weight and Weights of First Born Children.		Second Born Children.		Third Born Children.		Fourth Born Children.		Later Born Children	
6.5	47.7	(147)	—0.3	(28)	+0.7	(38)	+0.1	(26)	—0.1	(18)	—0.5	(35)
7.5	51.7	(191)	+1.1	(48)	—0.6	(42)	+0.1	(32)	—1.0	(21)	±0.0	(44)
8.5	57.3	(229)	—0.3	(58)	+0.2	(52)	+0.5	(32)	+0.7	(26)	—0.6	(57)
9.5	62.2	(212)	—0.4	(57)	+0.1	(45)	—0.2	(36)	—0.2	(22)	—0.1	(43)
10.5	69.0	(235)	—1.6	(64)	+5.4	(47)	—2.1	(39)	—1.4	(36)	—0.1	(44)
11.5	74.8	(206)	+1.0	(58)	—0.9	(38)	+1.2	(33)	—0.9	(27)	—0.3	(44)
12.5	81.6	(224)	+2.1	(64)	+1.2	(46)	—0.4	(37)	—2.6	(31)	—1.8	(41)
13.5	89.1	(185)	+2.0	(50)	+2.3	(46)	+4.1	(28)	—8.9	(32)	—2.5	(32)
14.5	105.1	(160)	+1.6	(47)	—0.7	(38)	—0.2	(26)	—1.4	(23)	+0.5	(25)
15.5	119.5	(114)	—3.0	(33)	—1.7	(27)	+0.1	(21)	+0.8	(15)	+1.8	(17)
Average Differences.			+0.82		+0.60		+0.32		—1.58		—0.44	

WEIGHTS OF GIRLS IN POUNDS.

Ages. Years.	Average Weight.		Difference Between Average Weight and Weights of First Born Children.		Second Born Children.		Third Born Children.		Fourth Born Children.		Later Born Children.	
6.5	45.7	(123)	±0.0	(31)	+0.9	(30)	—1.0	(15)	—1.2	(10)	+0.4	(32)
7.5	49.6	(186)	—0.1	(45)	+0.6	(37)	—0.1	(42)	—0.5	(23)	+0.1	(39)
8.5	55.7	(217)	+0.6	(50)	+0.3	(45)	—1.1	(42)	+0.8	(21)	±0.0	(59)
9.5	60.0	(242)	—1.5	(64)	+0.3	(57)	+2.1	(48)	—3.1	(22)	+1.0	(46)
10.5	66.8	(221)	+0.4	(57)	—0.8	(45)	—1.8	(28)	+2.5	(25)	—1.0	(60)
11.5	74.3	(222)	+2.1	(50)	—1.2	(41)	+0.4	(31)	+0.7	(32)	—1.2	(62)
12.5	84.2	(280)	+1.2	(67)	+2.6	(56)	—3.2	(54)	—0.4	(39)	—0.2	(64)
13.5	94.2	(220)	—0.9	(62)	+3.9	(47)	—2.6	(37)	+0.3	(29)	—1.2	(45)
14.5	105.8	(235)	+0.4	(60)	+1.3	(64)	—4.2	(35)	—1.4	(25)	+1.7	(49)
15.5	110.7	(165)	+0.1	(41)	+0.1	(32)	—3.5	(33)	+2.4	(19)	+1.2	(40)
16.5	116.5	(124)	+7.9	(29)	—1.5	(27)	—3.9	(22)	—7.5	(14)	—0.1	(32)
17.5	117.4	(99)	+1.9	(30)	—0.5	(18)	—3.2	(19)	+4.1	(15)	—1.2	(16)
18 & older	118.3	(82)	+2.4	(27)	+0.4	(20)	—0.1	(10)	—6.0	(9)	—1.1	(16)
Average Differences.			+1.12		+0.48		—1.71		—0.72		—0.12	

It appears from these four tables that first-born children exceed later-born children in stature as well as in weight; that this difference prevails from the sixth year until the adult state in females, and from the sixth year to the fifteenth in males. The material is not sufficiently extensive to show if the same is true of the adult males. Although the difference is not large, it occurs with such regularity that there can be no doubt as to the reality of the phenomenon. The available material is not very extensive, and the subdivision into five classes makes each class so small that the existing irregularities are not surprising. A preliminary investigation of the Toronto material is entirely in accord with the results derived from the Oakland material, the difference in favor of the first-born being, if anything, more marked.

We are, therefore, justified in grouping the measurements into two classes: first-born individuals and later-born individuals. This increases the difference of stature of the two groups to 10 mm. in girls, to 7 mm. in boys, and the differences of weight to 1.6 pounds in girls and to 1.2 pounds in boys. The tables seem to indicate that second-born children exceed somewhat later-born children in stature and weight, but the material is not sufficiently extensive to allow us to make a safe deduction on this question.

It would seem likely that the greater

vigor of the mother at the time of birth of the first child and the greater care bestowed upon the first child during its early childhood may be the cause of the phenomenon. The cares of the increasing household tend to weaken the mother and to decrease the amount of motherly attention devoted to later-born children. It is remarkable that the relation of size existing at the time of birth should be reversed in later life; it having been shown that the weight and length of new-born infants increases from the first-born to the later-born children.*

A comparison between the above table and others shows that the children of Oakland exceed those of all other cities of the United States in which measurements have been made, in height as well as in weight.

Franz Boas.

Washington, D. C.

CURRENT NOTES ON ANTHROPOLOGY (V.).

SUBDIVISIONS OF THE STONE AGE.

Those students who make use of Mortillet's excellent manual 'Le Préhistorique Antiquité de l'Homme,' now a little out of date, will be glad to learn the subdivisions of prehistoric time as taught this winter in his courses at the École d' Anthropologie, of Paris.

He divides the Stone Age into three 'periods,' covering six 'epochs.' The oldest is the eolithic, beginning with the 'Thenaysienne,' referring to the rather doubtful flints from the station of Thenay. Above this is the 'Puycournienne,' based on the finds at Puy-Courny. The palæolithic epochs remain the same as given in his manual, to wit: beginning with the oldest, the Chelleenne, the Acheuleenne, the Moustérienne, the Solutréenne and the Magdalenienne. Then follow two epochs which fill in the 'hiatus' which he formerly taught existed between the palæolithic and neolithic periods. They are the Tourassienne and the Compignyenne, referring to stations on the upper Garonne and the lower Seine. These bring us to the Robenhausienne, of Zurich, and so on.

The changes indicated are significant. I have before referred to those of similar character in the scheme of M. Salmon (see Science, p. 254). A leading question has been whether we can trace the oldest historic population of Europe in an uninterrupted culture-development back to the rough stone age (*pace*, Messrs. McGuire & Co.). This would seem now to be the case; and this carries with it the increased probability that the cradle of the Aryan or Indo-Germanic peoples was in western Europe.

THE ORIGIN OF LANGUAGE.

Some years ago the Society of Anthropology of Paris passed a resolution to reject all papers written to show the origin of language; believing that all discusssions of that subject are fruitless and time-wasting. One has but to look over the historical sketch of the hypotheses advanced, written recently by Professor Steinthal under the title 'Die Ursprung der Sprache,' to become convinced how much nonsense has been poured out concerning this theme. Among others, he represents a full analysis of the theories of Ludwig Noiré, showing at once their acuteness and the vicious circle of reasoning, arriving nowhere, in which the author involves himself.

Nevertheless, Noiré has found admirers in this country, and the Open Court Publishing Company of Chicago has printed a pamphlet of 57 pages, 'On the Origin of Language and the Logos Theory, by Ludwig Noiré." It will be found an excellent presentation of his views for those who wish to learn them.

There is but one scientific method of approaching this problem, and that is not the *a priori* style adopted by most writers, but

* H. Fasbender in Ztschr. für Geburtshülfe und Gynäkologie, Vol. III., p. 286. Stuttgart, 1878.

by a patient analysis of the structure (morphology) of the languages of savage tribes. These reveal to us human speech on its lowest terms and it will be found something quite different from what we expected. Noiré's examples, on the contrary, are taken from the highly developed Aryan languages, and from their vocabulary, not from their morphology. Nearly all writers follow the same false trail, and consequently reach no results worth naming.

RECENT STUDIES IN CRANIOLOGY.

The pathological effects of cretinism on the form of the skull have received inadequate attention. For this reason, a brief paper by Dr. Harrison Allen in the *New York Medical Journal*, for February 2, 1895, on the influence exerted by this condition on the shape of the nasal chambers and other cranial elements, is a welcome contribution.

The distinguished Roman craniologist, Professor Giuseppe Sergi, has added another to his many interesting studies of Mediterranean craniology by a paper of sixty pages in the *Bulletin* of the Medical Academy of Rome, 1894–1895, entitled 'Studi di Antropologia Laziale,' in which he discusses a number of skulls derived from cemeteries of ancient Latium. His conclusions are as we might expect, that the *populus romanus* of the Empire was decidedly mixed in blood and cranial types.

The island of Engano adjoins Sumatra, and little has been known about the physical type of its inhabitants, who, moreover, are rapidly dying out. For that reason, additional value is attached to a study of the skulls and bones brought from there by Dr. Modigliani, prepared by Dr. I. Danielli, and published in the 'Archivio per l' Antropologiae l' Etnologia,' Vol. XXIII. They appear to have belonged to a Malaysian people, with a dash of Negrito blood. A mixed population, at any rate, occupied the island, for the precise genealogy of which we must await further researches.

AFRICAN FOLK-LORE AND ETHNOGRAPHY.

Important additions to the ethnography and folk-lore of the Bantu tribes have been recently made by Mr. Heli Chatelain, late U. S. commercial agent at Loanda, West Africa. First to be noticed is a volume of 315 pages, published by the American Folk-Lore Society, entitled 'Folk-Tales of Angola.' These are fifty tales, faithfully recorded from the lips of the native speakers, with the original Kimbundu text, a literal English translation and an instructive introduction and notes. It is an excellent and original study of these prominent tribes from the point of view of the folk-lorist.

An article broader in scope, by Mr. Chatelain, entitled 'African Races' is published in the *Journal of American Folk-Lore* for December last. In it the author undertakes to present the result of his observations and theorizing on African ethnography in general. The main point which he endeavors to prove is that there is no true racial or linguistic difference between the Bantu and the Sudanese negroes. The reasons for this, advanced in the note to page 207, are far from satisfactory. Mr. Chatelain, though a most competent linguist, clearly does not appreciate the value of linguistics in ethnography; and it is slightly preposterous to forbid any ethnologist to have an opinion about the affinities of a tribe unless he has lived with it. At that rate, that class of scientists would find their field limited indeed. There are many reasons, not discussed by Mr. Chatelain, for holding the Sudanese of pure type to be as different from the Bantus as, say, the Sibiric tribes of Asia are different from the Sinitic peoples; and that is all that has been maintained.

DR. EMIL SCHMIDT'S RECENT WORKS.

Dr. Emil Schmidt, of Leipzig, is favorably known to anthropologists by his many practical contributions to their science. His text-book on physical anthropology is the best manual extant. Quite lately I referred to his investigations into the pre-Columbian history of the United States (see Science, p. 256). These were a chapter of his large volume, 'Vorgeschichte Nordamerikas, im Gebiet der Vereinigten Staaten' (pp. 216, Braunschweig, 1894). This is divided into four parts, one on the very oldest relics of man in the area of the United States; the second on the prehistoric copper implements of North America; the third on the prehistoric Indians of North America east of the Rocky Mountains; and the fourth on those in the southwestern portions of the United States. These topics are treated with a thorough knowledge of the best authorities and a calm judgement. The book will, I hope, have a translation into English.

In another work, 'Reise nach Südindien' (pp. 314, Leipzig, 1894), Dr. Schmidt gives the results of his own observations and investigations into the native tribes of southern India. It is written in popular style, abundantly enriched with illustrations of the natives and of the scenery, and replete with valuable information.

THE ANCIENT ETHNOGRAPHY OF WESTERN ASIA.

There is no other portion of the globe of equal area the ancient ethnography of which is so interesting to the history of human culture as western Asia, in the land area included between the four seas, the Black, the Caspian, the Persian Gulf and the Mediterranean. This embraces Palestine, Mesopotamia and the upper Euphrates valley, eastern Asia Minor, Armenia, Mount Ararat and many other wondrous sites of old. Here lay the Garden of Eden, the holy cities and the earliest centers of civilization.

A most valuable contribution to the study of its earliest geography and ethnography, as understood by the ancient Egyptians and preserved in their writings, appeared a little over a year ago from the pen of Professor W. Max Müller, now of Philadelphia (Asien und Europa nach altegyptischen Denkmälern, pp. 403, Leipzig, 1893). It is very abundantly illustrated with copies of the ethnic types found on the Egyptian monuments and with texts in the hieroglyphic script of the Nilotic scribes. As the author is one of the most accomplished Egyptologists living, his translations of the hieroglyphs are peculiarly valuable to the ethnographer, since few students of that specialty have paid attention to ethnic descriptions. A map appended to the volume locates from Egyptian sources those troublesome people, the Hittites, this time, in Cappadocia, as well as the Mitanni, the Kilak, and other little known tribes. The numerous drawings of the faces, costumes, armors, etc., of these former inhabitants, as well as the profound linguistic analysis of texts, render this volume one of exceptional value.

D. G. Brinton.

University of Pennsylvania.

CORRESPONDENCE.

A CARD CATALOGUE OF SCIENTIFIC LITERATURE.

Editor of Science—*Dear Sir:* I presume that there is no doubt of the existence of considerable demand among workers in, and writers upon, various branches of science for an index catalogue of the books and papers relating to the subjects in which they are interested, and that an accurate card catalogue, each card to be promptly furnished as soon as the book or paper is published, will best meet this demand. It is also desired that each card should contain a brief summary of the contents of the article.

A large number of investigators and writers would be glad to have their work done for them by some automatic or mechanical means, as far as possible, up to a point just short of the conclusions or results. These, of course, they prefer to prepare and state themselves. Those who like literary research would be pleased to have coöperative laboratories established in which, for a moderate annual subscription, they could have any experiments made which they might suggest, the results to be reported to them for their use. Others would prefer to do the experimenting themselves, and have some one else tell them everything that other people have done and written about the matter. And if each party is able and willing to pay for the assistance he requires, and can find persons competent to give that assistance and willing to do the work merely for the pay offered, every one will agree that it is a good thing, and will furnish new channels of employment and remuneration for experts, for which channels the need is steadily increasing.

It is, however, not clear that the benefits to science and to humanity, which would result from a complete card index of science up to date and available for every one who would like to consult it, would be so great as to make it the duty of any existing scientific body or institution to incur the great expense of taking charge of the matter or to contribute largely to its support.

Physicians meet with some cases for which it is desirable that the food should be carefully minced and partially digested before it is given, and sometimes it is necessary to push this food far back on the tongue to make sure that it will be swallowed, or even to forcibly inject it, but in most cases this benefits no one but the patient.

There is a very considerable number of men now engaged in preparing abstracts and summaries of what is known in various branches of science, and publishing them as monographs, monthly reviews, year books, etc.; and in medicine, at all events, the supply of this kind of material is quite equal to the paying demand for it.

Moreover, it is not certain that the investigator who wishes to know everything that has been suggested with regard to the subject which he has under consideration will be much happier when he gets his card index up to date, if he has not made it himself. He will find references to articles by Smith, and Schmidt, and Smitovich; but where are the books containing these articles? Very probably, after a week's hunt and correspondence, he finds that there are one or two of them that are not in any library accessible to him, and then he is decidedly worse off than he would be if he did not know that they existed.

It is probable that such complete card catalogues with abstracts would be the means of adding largely to the bulk of scientific literature, as the Index Catalogue of the National Medical Library and the Index Medicus have done to the literature of medicine. The bibliography and the abstracts will be published over and over again in successive papers by different writers.

The expediency of having such card indexes prepared depends upon the cost, and upon whether the money could be used to better advantage in promoting the increase and diffusion of knowledge in other ways. I should suppose that $25,000 a year would be a moderate estimate for providing 25 copies of such a card index for all branches of science, and to bring the cost within this limit would require careful selection.

If each author were to make his own abstract, and every article thus abstracted is to be indexed, probably $50,000 a year would be required. Much might be done for the advancement of science with a fund of $25,000 per annum.

I do not wish to be understood as opposing the preparation and furnishing of an universal card index; the schemes proposed are beautiful in the glow and shimmer of their optimism—reminding one of Chimmie Fadden, "Up t' de limit an' strikin' er great pace t' git on de odder side of it," but they must be looked at from the practical business point of view by those who are to defray the cost, and who have, I feel sure, other important uses for their money and for the skilled brains required for such work, and more definite information is wanted with regard to the number of titles, etc., which must be indexed annually upon such a scheme before a wise decision can be made. For general Biology, Morphology, Physiology, Bacteriology and scientific Pathology, and other subjects of scientific importance connected with medicine, I think that about 10,000 cards a year would be sufficient if all second-hand matter and hash were carefully excluded.

Very truly yours,

J. S. Billings.

Washington.

SCIENTIFIC LITERATURE.

The Great Ice Age and its Relation to the Antiquity of Man. By James Geikie, LL. D., D. C. L., F. R. S., etc. Murchison Professor of Geology and Mineralogy in the University of Edinburgh, formerly of H. M. Geological Survey of Scotland. Third Edition, largely rewritten, with maps and illustrations. New York, D. Appleton & Company. 1895. 8vo., xxviii + 850.

Twenty-two years ago the first edition of this book appeared in England. The author then endeavored to give a systematic account of the Glacial Epoch, with special reference to its changes of climate. In so doing he entered first quite fully into the geological history of glacial and post-glacial Scotland, presenting many elementary matters, and taking more than half the book for this purpose. Afterwards he discussed the glacial phenomena as exhibited in England, Ireland, Scandinavia, Switzerland and North America. A newly acquired view with him related to the age of the paleolithic deposits of southern England—all of which he referred to inter-glacial and pre-glacial times. It was this book that first called the attention of many geologists to the doctrine of several periods of cold in the ice age separated by as many times of milder conditions. Like the early doctrine of Agassiz and Buckland that the drift phenomena were to be explained by the agency of glaciers, so this theory of a series of cold and warm periods has been vigorously contested by geologists, but bids fair to be as generally accepted as the former. In 1877 a second edition of the book appeared. The author remarks in its preface that great additions to our knowlege of the facts had been made, above those first presented, all of which strengthened his argument that the epoch was not one continuous age of ice, but consisted of a series of alternate cold and warm or genial periods; while the ancient cave-deposits cannot be assigned to a later date than the last genial interval of the ice age, and some of them were probably still older. Among the more important alterations he notes a change in the use of the terms *till* and *boulder clay*. Instead of calling one purely glacial and the other partly marine, both are referred more or less directly to the grinding action of glaciers, and are strictly synonymous terms. Likewise he modifies his view of the *kames;* none of them are now regarded as of marine origin. There has been no great submergence of Scotland since the close of the glacial epoch, and thus the Scotch deposits are brought into much closer relationship with those of England. In the interim he made many personal studies of the English phenomena until able to say positively that after the deposition of the ossiferous gravels and *Cyrena* beds, a great ice-sheet stretched south as far as the valley of the Humber, thus proving the existence of a later ice incursion. In the first edition the term *kames* was not differentiated from *esker* and *åsar*, and all of them were believed to have been of marine origin; now he separates the kames from the esker and åsar and adopts Hummel's river theory of the origin of the latter, besides disowning the necessity of any marine agency in the formation of the kames. The accounts of the glacial phenomena in Europe and America are given with greater fullness in the second edition. The second edition attained a bulk of xxx + 624 pages and a larger size of page than the first, which had xxv + 524 pages.

The third and present edition shows a similar increase in size above its predecessor, but not so great a modification in the fundamental principles. About one-fourth of the subject-matter, or that relating chiefly to Alpine, Arctic and Scottish parts has been revised; but the other three-fourths have been entirely rewritten. The glacial and interglacial deposits of the continent are treated with a fullness that was impossible before. Many sections of it have been visited personally and the results of others verified. Aid has been received from a multitude of friendly fellow laborers. Necessarily because of the astonishing increase in the literature of Surface Geology, many important contributions are unnoticed. He does not profess to write the history of the rise and progress of glacial geology, but simply to sketch its present position. Nowhere, he says, has glacial geology been more actively prosecuted in recent years than in America. While he has endeavored to keep abreast of this, he preferred to have a summary of the American evidence prepared by a recognized authority; and hence called upon Professor T. C. Chamberlin, of Chicago, to furnish him with a digest of this material; which is of great service to everyone, since we have been awaiting almost with impatience the announcement of some general statements here first presented to the public. Professor Geikie also expresses his great gratification that his conclusions should essentially agree with those of Professor Penck, of Vienna, in respect to the glacial phenomena of the Alpine lands, the Pyrenees and Auvergne.

The following is a summary of the glacial succession in Europe as determined by Professor Geikie from a consideration of all the facts:

1. *Older Pliocene.*—Before the advent of the cold the sea occupied considerable tracts in the east and south of England, in Belgium, Holland, northern and western France and the coast lands of the Mediterranean, and boreal forms are just beginning to make their appearance.

2. *Newer Pliocene—First Glacial Epoch.*—The Weybourn crag and Chillesford clay of England with their pronounced arctic fauna represent a part of the evidence for this time of cold; also the bottom moraine near the Baltic sea, in southern Sweden, where the movement was from the southeast to the northwest. Arctic animal remains have also been detected in East Prussia at a similar horizon. Hence it is suggested that a gigantic glacier occupied the basin of the Baltic sea, and the mountainous parts of Scandinavia and the British Isles were snow clad. In the Alps the snow line was depressed for 4,000 feet or so below its present level, and all the great mountain valleys were filled with glaciers which left behind terminal moraines at the foot of the chain. In central France very considerable glaciers descended from the great volcanic cones of Auvergne and Cantal.

3. *First Interglacial Epoch. Latest Pliocene. Forest Bed of Cromer.*—The arctic fauna retreated from the North Sea, and dry land occupied the southern part of this sea up to the latitude of Norfolk. The river

Rhine flowed across this land. A temperate flora, much like that now existing in England, prevailed; and among the land animals were elephants, hippopotami, rhinoceroses, horses, bison, boar, deer, machærodus, hyæna, wolves, glutton, bear, beaver, etc. In other parts of Europe similar genial conditions prevailed. A luxuriant deciduous flora occupied the valleys of the Alps, attaining heights greater than the present limits of the same vegetation. Elephants existed with the flora in northern Italy. From the amount of river-erosion effected during this epoch it would appear that the stage was one of long duration.

4. *Second or Maximum Glacial Epoch.*—The mountains of Scandinavia seem to have been the center of dispersion of the ice at this time, and the glaciers extended easterly so as to become confluent with the Ural system in western Siberia, southwesterly into the basin of the Volga, southerly into the basin of the Dnieper, Poland, Saxony, Belgium, southwesterly to the British Islands, excepting a small part of southern England, and to the westward 600 feet below the present surface of the Atlantic ocean, from off Ireland to the Arctic sea. Both the Baltic and North seas were covered by ice, and erratics from the Scandinavian hills were strewn more or less over this entire area. They were also transported from lower to higher levels in the British islands, to a height of 3500 feet in Scotland, and the highest peaks may have projected through the ice as *Nunatakker*, like the bare spots thus designated in Greenland. This area is rudely elliptical in shape, 2700 miles long and 1600 miles wide. In Switzerland the Alpine glaciers reached their greatest extension, the snow line extending 4700 feet lower than it is at present, the ice being 4000 feet thick in the low grounds, and immense blocks of stone were carried across to the Jura Mountains to an elevation of 3099 feet above Lake Geneva. In connection with the presence of this ice, Arctic-Alpine plants and animals occupied the low grounds of Europe, extending even to the Mediterranean. This epoch constituted the beginning of the pleistocene or quaternary period.

5. *Second Interglacial Epoch.*—The return of the temperate flora and fauna in north Germany and central Russia is suggestive of a milder and less extreme climate than is now experienced in those regions. Britain must have been connected with the continent and Italy with North Africa. The rivers of this epoch eroded their valleys to great depths.

6. *Third Glacial Epoch.*—An extensive ice-sheet overwhelmed most of the British Islands and much of the continent. The northwestern limits are much the same in the edges of the Atlantic and Arctic oceans, but to the east it extended about a hundred miles beyond St. Petersburg, and just reached Berlin to the south. From the Alps glaciers descended to the low grounds, dropping conspicuous moraines, which extend in curving lines between the highly denuded moraines of the earlier epochs, and the associated extensive fluvio-glacial gravels.

7. *Third Interglacial Epoch.*—The youngest interglacial beds of the Baltic coastlands belong here, with both arctic and temperate marine faunas—as the mammoth, wooly rhinoceros, hare, urus and Irish deer. It is probable that a considerable portion of the old alluvial deposits of Britain and Ireland, hitherto classed as post-glacial, belong here.

8. *Fourth Glacial Epoch.*—The ice-sheets of the British Islands are now local and entirely separate from the Scandinavian mass. In Scotland the snow line did not exceed 1600 feet in elevation above the sea; the land was 100 feet higher than now, and an arctic marine fauna occupied the coasts. The Scandinavian peninsula supported an ice-sheet of more importance,

which discharged icebergs at the mouths of fiords in western Norway. Finland was overwhelmed, and the Baltic basin was occupied by a great ice stream, which invaded north Germany and Denmark. As the ice melted, a wide area in Scandinavia was submerged in a cold sea communicating with the Baltic. In the Alps the snow line was 300 feet lower than now.

9. *Fourth Interglacial Epoch.*—The British Islands were connected with the continent. Deciduous trees spread far north into regions now bereft of them. The Baltic sea became converted into a great lake; Denmark and Sweden were united; the Rhine flowed quite near England and Scotland, over the upraised bed of the North Sea, meeting the main ocean above Bergen; the Seine flowed through the English channel beyond Brest, and there was a large river flowing over the bed of the Irish Sea, having the Severn for a tributary, and meeting the ocean quite near the mouth of the Seine, and there was a land connection between the continent, Great Britain, Iceland and Greenland. When the salt water finally returned, the fauna was more temperate than it is at present. This epoch is not yet recognized in the Alps.

10. *Fifth Glacial Epoch.*—In Scotland the snow line reached an average height of 2,500 feet, the shore line being fifty feet lower than it is now. Occasionally glaciers discharged bergs into the sea on the northwest coast of Scotland. Most of the corrie rock-basins of the British Islands were excavated in this epoch, each one marking the presence of a distinct glacier. In the Alps there were advances of the glaciers giving rise to terminal moraines, the snow line reaching a depression of 1,600 feet below the present limit.

11. *Fifth Interglacial Epoch.*—The upper 'buried forests' of northwest Europe show that this epoch was characterized by drier conditions and a remarkable recrudescence of forest growth. It is uncertain whether Britain was connected with the continent.

12. *Sixth Glacial Epoch.*—This is indicated by the latest raised beaches of Scotland, indicating twenty or thirty feet of depression. The snow line stood at an elevation of 3,500 feet, and thus a few small glaciers could exist in the loftiest highlands. In the western Alps there were some high level moraines.

13. *The Present.*—The sea has retreated to its present level, drier conditions prevail and permanent snow fields have disappeared from most of the regions in northern Europe once so completely submerged by glacial ice. The term post-glacial properly describes only the present epoch.

Professor Geikie devotes three chapters to a discussion of the presence of man in the Pleistocene. His bones and implements are found chiefly in the extra-glacial regions, associated with the remains of both extinct and living mammalia, such as have been mentioned as occurring in several of the interglacial epochs. Man would naturally migrate towards the glaciers as they receded, and retreat southerly as they advanced. The large animals would have done the same; hence a perfectly satisfactory correlation of the several terranes in the glaciated and extra-glacial regions is of difficult attainment. Our author concludes that Paleolithic man existed abundantly in the second interglacial epoch in company with the *elephas antiquus* and hippopotamus. Some of the caves occupied by him appear to have been abandoned before the third glacial epoch reached its climax, because they are sealed up by the moraines of that stage. During this epoch Paleolithic man seems to have retired to southern France, and, if negative evidence is of value, he never revisited northwestern Europe.

American geologists will be more than pleased with the sketch of the glacial phenomena of North America by Prof. Chamberlin. The facts correspond in a general way with those described by Professor Geikie in Europe. The attempt is made to group the stages of glaciation and deglaciation both on a two-fold and a three-fold basis, without deciding which is the more acceptable. The foundation of the grouping is what is called 'imbrication' of the till, or the superposition of the later or more northern sheets upon the earlier or more southern ones. The oldest is the *Kansan*, next the *East Iowan*, and thirdly the *East Wisconsin* stage of glaciation, followed by six, seven or more terminal moraines. Professor Geikie says that these general conclusions harmonize with the results obtained in Europe, and without hesitation he correlates the Kansan stage with his second glacial epoch, the time of maximum glaciation, after which the ice sheets declined in importance.

Granting the correctness of the correspondence of the Kansan stage to the second or maximum glacial epoch of Geikie, American geologists can easily complete the correlation. The Lafayette or Orange sand deposit will correspond to the first or Pliocene phase of the glacial epoch. This reference will be satisfactory to those who believe in elevation as a prime cause of refrigeration, as it is generally conceded that the late Pliocene was a time of continental uplift. It should be satisfactory to the advocates of the unity or continuity of the ice-age, because there was just one period of maximum intensity or culmination of refrigeration—the Kansan phase. It was preceded by the Pliocene-Lafayette flood, and followed by the gradually less intense Iowan, Wisconsin and later phases. It will, however, enlarge our conceptions of the magnitude of the ice age in geological history; for we cannot deny that the remotest centers of dispersion have been active from the beginning of refrigeration. The latest geological epochs are fundamentally glacial for the countries above forty degrees of latitude on both sides of the equator; ice-action characterizes the time. The writer has hitherto been esteemed an advocate of unity; but he has repeatedly insisted that the several margins of glacial accumulation indicate just so many phases of more intense glaciation, and that they are to be our criteria of classification. He is satisfied that they can be interpreted to correspond with the several glacial and interglacial epochs established by Professor Geikie.

It remains only to notice the chapter upon the cause of the climatic and geographical changes of the glacial period. The ratio of precipitation was the same as now prevails. Snow fields gathered most abundantly in those regions which in our day enjoy the largest rainfall. What are now dry regions were formerly regions of limited snowfall. But the amount of precipitation was greater, snow in the north and rain in the south. Arctic currents prevailed near the equatorial in the cold epochs, but the reverse was true in the interglacial phases. The land seems to have been elevated at the commencement of every cold epoch and depressed at its close, submergence having been more characteristic of the glacial than of the interglacial phase. The fiord valleys were mostly excavated before glacial times. The Scandinavian flora migrated to Greenland after the close of the fourth glacial epoch, when the land was continuous between the continents. There are considerations favorable to the view that the accumulations of ice in the several glacial epochs produced depressions, not excluding epeirogenic warpings of the crust. The cause of the remarkable connection between glaciation and depression is still an unsolved problem. All the proposed astronomical causes of refrigeration are rejected as untenable, except that of Dr Croll, supplemented by Ball, who believed the climatic changes of the glacial period resulted from the combined influence of precession of the equinoxes and secular changes in the eccentricity of the earth's orbit. In favor of this view, the mean temperature of the globe was lowered, and the ratio of the precipitation increased; the dominant set of the currents in the Atlantic was from north to south in the colder terms. In the interglacial climates the summers were cooler and the winters warmer, while the Atlantic currents flowed northerly. The maximum glaciation came early, succeeded by cold epochs of diminishing severity. Glacial epochs in the northern hemisphere were necessarily contemporaneous with interglacial conditions in the southern hemisphere. Hence the astronomical theory would appear to offer the best solution of the glacial puzzle; while it is conceded that this answer is not completely satisfactory. C. H. Hitchcock.

Biological Lectures and Addresses, by Arthur Milnes Marshall. Macmillan & Co., New York. Price $2.25.

Lectures on the Darwinian Theory, by Arthur Milnes Marshall. Macmillan & Co., New York. Price $2.25.

It was a curious coincidence by which accidents in mountain climbing deprived English science of two of its prominent biologists, and two who were at the same time personal friends. Prof. F. M. Balfour, as every one remembers, lost his life in a journey in the Alps, and Prof. Arthur Milnes Marshall, upon the last day of 1893, in a somewhat similar manner, met his death in mountain climbing. Prof. Balfour and Prof. Marshall were personal friends and naturally worked upon kindred subjects, although their work was very unlike. Prof. Marshall was still a young man, only about forty years of age. Early in life he entered upon studies looking toward the profession of medicine, but in 1879 gladly accepted the chair of Zoölogy in Owens College, and continued to occupy the chair until his death.

His additions to the literature of science have been of two general types. There are first a series of papers embodying the results of original research. These, because of his intimate association with Balfour, were at first of an embryological nature, while some of the later ones were more distinctly anatomical. His chief contributions to science of this sort were upon the *Segmental value of Cranial Nerves*, the *Pennatulida of the Porcupine and Triton Expeditions*, and upon *The Nervous System of the Crinoids*. The second class of his papers were more distinctly characteristic of his special powers. They were of a more general character and included a text-book on *The Frog*, on *Practical Zoölogy*, and a more recent work upon *Vertebrate Embryology*. In addition, we have in the recent posthumous volumes a large number of lectures and addresses given in various places before various societies.

Above all things, Professor Marshall was a teacher. It was in this direction that his powers showed at their best. He had the happy way of putting subjects so that they were intelligible to his audiences, and had the somewhat unusual power of putting himself in the position of his audiences, in such a way that he could understand how and what was needed in his teaching to render his subjects clear. His lectures were always abundantly illustrated both by drawings, and especially by homely though terse illustrations. His illustrations for rendering scientific facts intelligible were drawn sometimes from the most surprising sources, and altogether rendered his addresses and his class lectures of the very highest character in the way of scientific teaching. Since his death Macmillan & Co. have published his collected lectures and addresses in the two volumes which are the subject of this notice. The first series consists of miscellaneous addresses given by him at various intervals between 1879 and the time of his death, and before a number of debating societies and scientific organizations, ending with his presidential address before the British Association in 1890. These addresses are all designed for a somewhat popular audience, and treat of different scientific subjects in a clear, entertaining manner. Among the most interesting of them the lectures that will, perhaps, first commend themselves to the reader are those on Fresh Water Animals, on Inheritance, on Shapes and Sizes of Animals, and the one upon the Recapitulate Theory. Professor Marshall possessed in a wonderful degree the power of seizing hold of the salient points of abstract scientific subjects and isolating them from the cumbersome mass of details with which they are associated in ordinary scientific discussions. The result is that in a few pages the reader obtains a clearer conception of the salient points in a subject like embryology by reading the last of the essays in this volume than he might obtain from the careful perusal of many lengthy books upon the subject. Details, of course, are left out, but the salient and interesting points which embryology teaches and attempts to teach are presented with wonderful clearness. The addresses are, in short, popular science of the highest type, and one does not wonder after reading them that Professor Marshall was one of the most popular lecturers in the University Extension courses.

Every teacher is aware how difficult it is to send a young student to literature that will give him a clear, succinct account of evolution. Scientific discussions of one and another phase of the subject are abundant, but usually they are beyond the comprehension of the ordinary reader. Many a student having been recommended to read Darwin's Origin of Species reads the book with an utter failure to comprehend Darwinism. Nor is this the fault of the student. Even the better class of thinking students are so handicapped by the abundance of material in that Darwinian classic that the thread of the argument is lost, and they are just as likely to confuse Darwin's views with those of Lamarck as they are to understand Darwinism. Few students who are beginning the study of modern biology will have any proper appreciation of Darwinism from the study of the Origin of Species, or, indeed, from the study of most of the scientific writings on evolution, unless the essential facts are presented to them in some form of introduction. For this reason the series of lectures on the Darwinian theory by Professor Marshall are especially valuable. These lectures are not encumbered with numerous details, but seize hold of the thread of the Darwinian argument and present it before the reader in such a way that he cannot fail to understand evolution and Darwinism after having finished such a volume. This series of essays will, therefore, be perhaps the best literature to which a student can be sent at the present time to enable him to understand what evolution was before Darwin, what Darwin added, and what have been the subsequent modifications and criticisms of Darwin's theory. Professor Marshall writes as a partisan and thorough believer in Darwin, and presents his facts in such a way that his readers cannot fail to recognize the full force of the Darwinian argument. Indeed, he naturally exaggerates the force of many arguments, frequently begs the very question of the issue, and the essays are by no means calculated to be critical discussions. The lectures cannot be considered as a fair presentation of the Darwinian theory. The innocent reader will conclude that the argument upon Darwinism is all on one side, that every essential feature of it is abundantly demonstrated and all criticisms are refuted. But, in spite of this fault, which comes naturally from one who is attempting to teach a theory in which he so fully believes, the outline of the Darwinian theory is an exceptionally good one. Certain it is that nothing in our literature at the present time will give such a terse, clear presentation of the Darwinian hypothesis with the arguments in its favor, and of the additions which have been made to this hypothesis subsequent to the writings of Darwin himself.

These two books are, then, designed for popular reading. They are perhaps as good an illustration of the especial character of Prof. Marshall's power in teaching as could be found. They are valuable additions to that class of books in which the English language is beginning to abound, viz., popular scientific writings that actually *teach science*. H. W. CONN.

WESLEYAN UNIVERSITY.

Elements of Astronomy.—By GEORGE W. PARKER, of Trinity College, Dublin. Longmans, Green & Co., London and New York. 8vo., 236 pages. $1.75.

The book is designed as a connecting link between the elementary school-astronomies and the higher treatises used as text-books in the universities. It treats the subject almost exclusively from the geometrical point of view, breaking up the matter into propositions, corollaries and problems, arranged in an order which is probably logical enough in its mathematical sequence, but strikes one as rather peculiar. The book will be found useful by teachers who have 'examination papers' to draw up, since it presents a large number of them, as well as numerous 'exercises' and problems well suited to test a student's understanding of the subject-matter.

What the book professes to do is in the main very well done. The statements and definitions are intelligible and correct, and the reasoning is generally clear and logical. The writer's description of the instruments and methods of practical astronomy make it evident, however, that he has had very little actual experience in that sort of work. It reads rather strangely, for instance, to be told that the way to find the value of a micrometer-screw revolution is to 'note how many turns correspond to the sun's diameter.'

Regarded as an elementary presentation of 'Astronomy' taken as a whole, the book must be pronounced extremely one-sided and defective. Astrophysics is most inadequately dealt with; the whole subject of spectroscopy is dismissed with six pages and a single old diagram of the dispersion of light by a prism; and all physical matters relating to sun, planets, comets, stars and nebulæ are treated on the same general scale. C. A. Y.

Qualitative Chemical Analysis of Inorganic Substances—As practiced in Georgetown College, D. C. American Book Co., New York. 1894.

Rev. H. T. B. Tarr, S. J., formerly professor of chemistry in Georgetown College, prepared a series of tables for analytical purposes, which have been wholly recast and incorporared into the work now before us. The present editor, Rev. T. W. Fox, S. J., speaks of the book as being 'useful in a course such as is given at Georgetown and in similar institutions throughout the country.'

The 'grouping of the bases' is that generally adopted by writers on qualitative analysis the world over. We believe, however, that it would have been wiser and better for the student had the author divided his third group, consisting of the metals precipitated by ammonium sulphide from neutral or alkaline solutions, into two groups. But this is merely a matter of opinion.

We observe that the properties of the metals are first studied, after which the author draws up a table for the analysis of a mixture of metals, constituting a particular group, accompanied by explanatory notes. This order is preserved throughout the book, which consists of sixty-one pages. We trust that the author and the reader will pardon us when we declare that we think such tabular schemes, so early in the course of analysis, are apt to make the student a mere machine—precisely what the author, in his introductory remarks, announces that he wishes to avoid, for he writes, "A mere mechanical acquaintance with a working scheme for separating * * * * * is at best but a questionable accomplishment," etc. And, for some unaccountable reason—perhaps from natural, human depravity or perversity—the great majority of students, beginning analysis, do wed themselves to such a table or scheme and cling to it, despite the rough handling they may receive from an earnest and intelligent quiz-master. But we are rambling. On returning to our subject we discover in it no new methods of separation, no new characteristic test or tests for the various elements; the landmarks in these directions remain unchanged. This is pardonable, seeing that "no pretense is made to originality, either in matter or in method."

Part II. considers the 'acid analysis' and commences with excellent advice for the student, who must now, more than ever, apply what knowledge he may have acquired in regard to the metals and their various combinations with acids.

Brief chapters on 'preliminary examinations,' the solution of solid substances, a table of solubilities, and an appendix, dealing with the preparation of the ordinary reagents, conclude the book.

The little volume is well written and nicely printed. Its chief merit seems to be that it presents its author's particular method of instructing students in this most important branch of chemistry, upon which many others have likewise prepared similar brochures. The same kindly welcome given them must be accorded this latest arrival. Each does some good, and together they will doubtless do great good.

Edgar F. Smith.

A Course of Elementary Practical Bacteriology, Including Bacteriological Analyses and Chemistry. By A. A. Kanthack and I. H. Drysdale. XXII. 181 pp. Sm .8°. Macmillan & Co., London and New York. 1895. Price $1.10.

This is a laboratory hand-book which will be interesting to all practical workers in bacteriology, since it gives the details of methods used in the Laboratory of St. Bartholomew's Hospital in London. Some of these methods are not so useful as those now employed in American Laboratories; as, for example, that given for the collection and sterilisation of blood serum, while some are probably more rapid and convenient. As the authors remark, every laboratory has its own ways and means, its 'short cuts' and 'tips,' which are not always published, and it is necessay to work for a little while in the laboratory to become acquainted with them. The descriptions given are simple and straightforward, and well calculated to meet the wants of students. The plan and order of the several lessons will be found interesting by teachers of the subject. The lessons in Bacteriological Chemistry contain good matter not usually found in a manual of this kind.

NOTES AND NEWS.

TYPHOID INFECTION OF OYSTERS.

The *Medical News* of March 23, contains a paper by C. I. Foote, giving the results of experiments with oysters, and with the water in which they grow, to determine the possibilities of their becoming infected with the bacillus of typhoid. He found that this bacillus will live in brackish water, taken from just above oyster beds, for at least eight days, even in very cold weather. In apparently normal and healthy oysters and in their juice he found bacteria of various kinds; the number of which that will grow in gelatin ranging from 240 to 1680 per c.c. The number found in the water over the oysters was 9520 per c.c., indicating that the water is purified by being taken into the shell. He inoculated a number of oysters with typhoid bacilli by injecting a culture of these organisms between the edges of the shells. The results indicate that the bacilli can live in the oyster for from one to two weeks, but it is doubtful whether they multiply there. But the oysters were cleaned before inoculation, and, after the operation, were apparently not placed in water, but simply kept in a cool room. The research would have given much more definite and conclusive results if the oysters had been placed in brackish water, and then the typhoid bacilli added to this water, so that they might have been taken in and disposed of in the natural way.

ARGON.

According to the *London Times*, M. Berthelot has supplied the first information concerning the chemical properties of argon. In experimenting with a small quantity of that substance, furnished by Professor Ramsay, he has found that under the influence of the silent electric discharge it combines with various organic compounds, and notably with benzene. It is decidedly interesting to discover that argon, which is supposed to be totally inert, and has been vainly subjected to all the most potent agencies at the command of the chemist, is all the time capable of forming a variety of combinations under conditions which always exist in the atmosphere. Great interest also attaches to M. Berthelot's communication in connection with the obscurity which hangs over the chemical nature and relationships of the new substance. For he pointed out years ago that nitrogen combines, under the influence of the silent discharge, with hydrocarbons like benzene, with carbohydrates, such as go to build up the tissues of plants, and even with tertiary products, such as ether.

GENERAL.

Dr. William S. W. Ruschenberger, President of the Philadelphia Academy of Science from 1869 to 1881, died on March 24th, at the age of eighty-seven years.

Dr. John A. Ryder, Professor of Embryology in the University of Pennsylvania, died on March 26th.

The Library Building of Harvard University will be altered during the present summer in such a manner that the space for books will be greatly enlarged.

The North Dakota State University must be closed until the next session of the Legislature, in January, 1897, owing to the fact that the appropriation has been reduced from $63,000 to $15,000.

The British Association will meet at Liverpool in 1896. The Council have resolved to nominate Sir Joseph Lister for President.

T. G. Crowell & Co. announce 'Forests and Forestry' by the Hon. B. E. Fernow, of the Department of Agriculture, and 'Marriage and the Family,' by Professor George E. Howard, of Stanford University.

The sixty-third annual meeting of the British Medical Association will be held in London, July 30th to August 2d, 1895.

The next meeting of the American Microscopical Society will be held at Cornell University, Ithaca, New York, on August 21, 22 and 23, 1895.

Dr. K. Schmidt has been made Professor of Physics in the University of Halle.

The two final volumes of the report on the scientific results of the voyage of H. M. S. Challenger, prepared under the direction of Dr. John Murray, have now been published by Eyre & Spottiswoode, London. The completed work fills 50 large quarto volumes containing about 29,500 pages and illustrated by over 3,000 plates. These two concluding volumes are mainly occupied by a general summary of the scientific results of the voyage.

Dr. A. R. Forsyth, of Trinity College, has been elected to the Sadlerian Professorship of Mathematics in the University of Cambridge, succeeding the late Professor Cayley.

According to the *American Geologist*, efforts are being made looking towards a geological survey of the State of Maine.

Dr. John P. Lotsy, now Associate in Botany at Johns Hopkins University, has accepted the Directorship of the Botanical Gardens on the Island of Java.

The Lake Superior Mining Institute made an excursion on March 6th, 7th and 8th, from Duluth to the Mesabi iron range. The mines were visited and in the evenings meetings were held, at which papers were presented by Dr. L. L. Hubbard, Dr. U. S. Grant, Mr. F. W. Denton, Mr. F. F. Sharpless and Mr. E. F. Brown.

The tenth annual meeting of the American Association for the Advancement of Physical Education will be held at the Teachers' College, New York, on April 25th, 26th and 27th.

The Journal of Mental Science gives, in the last number, a retrospect of Normal Psychology, prepared by Mr. Havelock Ellis, and proposes to give regular summaries of the progress of psychology.

The Chemical Society has conferred its Faraday medal upon Lord Rayleigh in recognition of the investigation which has led to the discovery of Argon. Dumas, Canizzaro, Wurtz, Helmholtz, and Mendeléeff have been the previous recipients of the medal.

Rev. Herbert A. James, principal of Cheltenham College, has been elected head master of Rugby, succeeding the Rev. Dr. Percival.

The Woods Holl Biological Lectures for 1894, in the press of Ginn & Co., include : I. *Life from a Physical Standpoint.*—A. E. Dolbear. II. *A Dynamical Hypothesis of Inheritance.*—John A. Ryder. III. *On the Limits of Divisibility of Living Matter.*—Jacques Loeb. IV. *The Differentiation of Species on the Gálapagos Islands and the Origin of the Group.*—G. Baur. V. *Search for the Unknown Factors of Evolution.*—H. F. Osborn. VI. *The Embryological Criterion of Homology.*—E. B. Wilson. VII. *Cell-Division and Development.*—J. P. McMurrich. VIII. *The Problems, Methods and Scope of Developmental Mechanics.*—W. M. Wheeler (Roux's). IX. *The Organization of Botanical Museums for Schools, Colleges and Universities.*—J. M. Macfarlane. X. *The Centrosome.*—S. Watasé. XI. *Evolution and Epigenesis.*—C. O. Whitman. XII. *Bonnet's Theory of Evolution.*—C. O. Whitman. XIII. *Bonnet on Palingenesis and Germs.*—C. O. Whitman.

SOCIETIES AND ACADEMIES.

BIOLOGICAL SOCIETY OF WASHINGTON, MARCH 23.

Mr. Charles T. Simpson read a paper on the 'Respective Values of the Shell and Soft Parts in Naiad Classification.' Mr. Simpson deprecated the fashion of many conchologists of late in basing classification wholly on the soft parts and stated that his studies of the Naiads, or fresh water mussels, go to show that among them, at least, he has found the characters of the soft parts of the animal more variable and less reliable for the purposes of classification than those of the shell. That, while in some cases the soft parts give us the key to true affinities, in others they are worthless, and we must rely on the shell for a knowledge of relationships. Numerous cases were cited showing such variation. In *Unio novi-eboraci* the branchiæ are sometimes free only a short distance on the posterior part of the abdominal sac; in other cases they are united the whole length, and the same is found to be true to a great extent in *U. multiplicatus.* In that species and some others not closely related the embryos are found in all four leaves of the branchiæ, but in all other North American forms they only occupy the outer leaves.

The statement was made that the dissection of a single animal of a widely distributed and variable species will probably not give any more knowledge of all its characters than the examination of a single shell, *Castalia*, *Castalina* and *Glabaris*, South American Naiads, may either have no siphons at all, or have them perfectly developed, and this variation occurs in the same species. The families *Unionidæ* and *Mutilidæ* were founded on the absence or presence of this character. In a new arrangement of the Naiads v. Ihering has based the family *Unionidæ* on the fact that the embryo is a *glochidium*, in which the soft parts are enclosed in a bivalve shell, and the *Mutilidæ* was established on the fact that the embryo is a *lasidium*, divided into three parts, the middle one only being protected by a single shell.

Basing a classification on these characters it will be found that *the genera of the unionidæ have invariably heterodont teeth, or vestiges of them, while in the mutilidæ the arrangement is essentially taxadont.*

It is claimed that similar circumstances of environment may produce like characters of unrelated forms; the *Mycetopus* of South America and *Solenaia* of China are burrowers, and though belonging to different families closely resemble each other in the elongated shell and greatly developed foot, and have both been placed in one genus on this account. *Anodonta angulata* burrows in rapid streams and differs greatly in appearance from *A. dejecta*, which is closely related but lives in stagnant water. The two were shown to have affinities by connecting forms.

Dr. Stiles spoke* 'On the Presence of Adult Cestodes in Hogs.' He called attention to the remarkable fact that no adult tapeworms were described as regular inhabitants of *Sus*, and discussed the cases recently mentioned by Cholochowsky in Russia and two cases which had recently been reported to him from Iowa. One of the Iowa cases was certainly a case of chance parasitism in this host, and although there are no satisfactory data upon which to base an opinion concerning the other cases, he thought helminthologists in general would not admit the forms mentioned to the lists of the parasites of hogs.

Mr. Coville laid before the society a copy of the newly published list of ferns and flowering plants of the northeastern United States, prepared by a committee of the Botanical Club, A. A. A. S., in accordance with the nomenclature rules adopted by the Club, and gave a brief history of the recent nomenclature reform in botany. He pointed out the fact that in a recent criticism of the List by Dr. B. L. Robinson, who represents those still favoring the old system, only a single specific point of vital principle in the new system was really discussed, the other items of criticism referring to details which do not involve the principles themselves. Mr. Coville pointed out that in view of the success of the new system as already tried by several of our leading botanical institutions and as tested for many years past in other branches of biological science, together with the prevailing dissatisfaction regarding the old system among working botanists, the new code gives every

*Notes on Parasites, 34; Centralbl. f. Bakt., u. Par. 1895.

promise of satisfactorily solving the nomenclature problem.

Professor Joseph F. James made some remarks on 'Daimonelix and Allied Fossil.' He gave an account of the large fossil 'cork screws' described by Professor Barbour from the Bad-Lands of northwestern Nebraska, calling attention to their peculiar features. He noted the fact that while they had heretofore been considered as unique and without resemblance to other fossils, that in reality several other similar forms had been described. One of these was figured by Heer in 1865 in 'Die Urwelt der Schweiz,' under the name of 'screw-stones,' which presents all the characters of *Daimonelix* as figured by Barbour. In 1863 Professor James Hall described *Spirophyton* and gave a restoration of *S. typum*. In a view of one of the whorls there is a great correspondence between it and a figure of the same character given by Barbour. In 1883 Professor Newberry described *Spiraxis*, also a genus of screw-like fossils which presents features similar to *Daimonelix*. Heer's fossil occurs in the Miocene of Switzerland, while *Spirophyton* and *Spiraxis* occur in the Chemung of New York and Pennsylvania. The wide distribution of the forms is interesting as showing that *Daimonelix* is not an 'accident' as hinted by some. Whether it is a plant or not must be decided in the future, although there is a strong presumption that such is the case.

FREDERIC A. LUCAS,
Secretary.

SCIENTIFIC JOURNALS.

AMERICAN CHEMICAL JOURNAL, APRIL.

Argon, A New Constituent of the Atmosphere: LORD RAYLEIGH and WILLIAM RAMSAY.

On the Spectra of Argon: WILLIAM CROOKES.

The Liquefaction and Solidification of Argon: K. OLSZEWSKI.

On the Atomic Weight of Oxygen. Synthesis of Weighed Quantities of Water from Weighed Quantities of Hydrogen and of Oxygen: EDWARD W. MORLEY.

On the Chloronitrides of Phosphorus: H. N. STOKES.

On the Saponification of the Ethers of the Sulphonic Acids by Alcohols: J. H. KASTLE and PAUL MURRILL.

Contributions from the Chemical Laboratory of Harvard College. LXXXVI. On the Cupriammonium Double Salts: THEODORE WILLIAM RICHARDS and GEORGE OENSLAGER.

Basswood-oil: F. G. WIECHMANN.

Note.

AMERICAN JOURNAL OF SCIENCE, APRIL.

Niagara and the Great Lakes: F. B. TAYLOR.

Disturbances in the Direction of the Plumb-line in the Hawaiian Islands: E. D. PRESTON.

Glacial Lake St. Lawrence of Professor Warren Upham: R. CHALMERS.

Argon, a New Constituent of the Atmosphere: LORD RAYLEIGH and W. RAMSAY.

Velocity of Electric Waves: J. TROWBRIDGE and W. DUANE.

Epochs and Stages of the Glacial Period: W. UPHAM.

Structure and Appendages of Trinucleus: C. E. BEECHER.

Scientific Intelligence; Chemistry and Physics; Geology and Mineralogy; Botany; Miscellaneous Scientific Intelligence; Obituary.

AMERICAN GEOLOGIST, APRIL.

The Stratigraphy of Northwestern Louisiana: T. WAYLAND VAUGHAN.

The Paleontologic Base of the Taconic or Lower Cambrian: N. H. WINCHELL.

The Missouri Lead and Zinc Deposits: JAMES D. ROBERTSON.

On the Mud and Sand Dikes of the White River Miocene: E. C. CASE.

Editorial Comment; Review of recent Geological Literature; Recent Publications; Personal and Scientific News.

NEW BOOKS.

A travers le Caucase. ÉMILE LEVIER. Neuchâtel, Attinger Frères. Pp. 346.

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Subscriptions and advertisements should be sent to SCIENCE, 41 N. Queen St., Lancaster, Pa., or 41 East 49th St., New York.

ON MARINE MOLLUSKS FROM THE PAMPEAN FORMATION.

IT is known that D'Orbigny considered the pampas as a marine formation, Burmeister as a fluvio-lacustrine deposit. In a paper on the Lagoa dos patos, in 1885, I referred to conditions which I considered important for the study of such formations as the Pampean. All discussions hitherto lay great stress on the absence of marine fossils in the Pampean mud. But this fact itself seems to rest partly on the belief of Burmeister that marine organisms are not to be found in the formation.

Burmeister (Descr. Phys. Rep. Arg. II., 1876, p. 177) having seen fragments of an *Astræa* found at a depth of two meters at San Nicolas, and believing that their presence was due to some disturbance of the beds, said that it is not possible to understand how they could have reached the locality where they were found.

Burmeister's view, above cited, will be essentially modified by the announcement which I am able to make of the following list of marine shells received by me from the distinguished Argentine paleontologist, Dr. Florentino Ameghino. The specimens are from the 'formacion pampeana, piso belgranense', near La Plata.

Purpura hæmastoma L.
Nassa polygona Orb.
Bullia deformis King.
Olivancillaria auricularia Lam.
Voluta brasiliana Sol.
Litorina flava King.
Litoridina australis Orb.
Crepidula fornicata? Lam.
Ostrea cristata Born.
Ostrea puelchana Orb.

Mytilus platensis Orb.

Mytilus exustus L (magellanicus Rve. fide Dall.).

Arca Martensii Recl.

Azara labiata Mat.

Tagelus gibbus Spgl. (platensis Orb.)

Mactra patagonica Orb.

*Mactra Dalli v. Iher. (M. Byronensis fide Dall).

†Mactra riograndensis v. Thes. (M. isabelleana Orb. fide v. Martens).

Cytherea rostrata Koch.

An otolith of a Sciænoid fish, Micropogon undulatus L., very common at Rio Grande do Sul, and probably also in the La Plata estuary.

All these mollusks are common species of the Atlantic coast of Uruguay and Argentina and most of them also from Rio Grande do Sul. Only three of them are of special interest, as not now found living in these latitudes.

Purpura hæmastoma L., still common on the coast at Rio Grande do Sul, is, I believe, not now known from the La Plata region. D'Orbigny, Petit and other authors have suggested that this species has been distributed through the agency of navigation. It is therefore important to note that it occurs fossil in America, as it does in the European Tertiary.

Litorina flava King, common from the West Indies to Santa Caterina, is not known to occur at Rio Grande do Sul.

Nassa polygona Orb. seems to have almost the same distribution as *Litorina flava*. I use D'Orbigny's name in default of the complete synonymy. Prof. von Martens considers it synonymous with *N. polygonata* Lam. Hidalgo, treating it in extenso (Moll. del viage al Pacifico, III., p. 39) regards it as being the same as *N. ciniscelus* Reeve, with *antillarum* Dkr. and *sturnii* Phil. as varieties. So I prefer the name of D'Orbigny, as to the application of which there is no doubt.

These are, therefore, species once reaching to the 35° of south latitude, which now do not occur south of Santa Caterina or Rio Grande do Sul. It is quite possible that other species exist in the actual fauna which are dying out. For example, *Neritina meleagris* Lam., found at Santa Caterina. It occurs also in the bay of Paranagua, but only in one locality, though formerly it was much more common, being not rare in the shell mounds of the Sambaquis. Dunker (Jahrb. d. Deutsche mal. Ges. 1875, p. 245) says that *N. meleagris* is common at Montevideo, but this seems to be an error, as D'Orbigny, myself and others have not found the species in the La Plata region, either recent or fossil.

It was the opinion of Darwin, shared in part by Burmeister, that deep bays entered long distances into the interior during the Pampean formation, which for the most part is due to the action of winds and fresh water. To this I also agree. To such a gulf we owe the existence of the marine shells. The important facts discovered by Ameghino give a new turn to the discussion of the origin of the pampas

As Dall has shown that in Florida some of the Pampean mammals occur in beds covered by marine pliocene limestone, there cannot be any doubt that the pampean formation is in part of Pliocene age. It seemed that with the important study of Santiago Roth the pampas question might be considered as settled, but the facts here considered awaken doubts. It is quite possible that observations here brought together may be increased with time and more and more tend to modify the basis of our knowledge.

* This seems to me different from the Chilian form.

† A very common species on the coast at Rio Grande do Sul, but probably undescribed. Prof. von Martens named it *M. isabelleana* Orb., but this is a species with the beaks more inflated and the valves not so thick. Descriptions will be published elsewhere.

I am not aware of the distribution of *Astræa* and other corals south to Paranagua. It is quite possible that the *Astræa*, like the mollusks above mentioned, was a denizen of warmer water, demonstrating that the temperature of the Atlantic Ocean in this region has diminished since the Tertiary epoch.

Santiago Roth says that marine (Tertiary ?) shells also occur at Buenos Ayres at a considerable depth, and at other localities in the Pampean beds. The question is a difficult one, and only in the future may it be possible to fully appreciate such facts as are here put on record. The Argentine geologists have hitherto paid little attention to the study of the fossil mollusks, and for this reason this first contribution of Ameghino is encouraging and important.

H. VON IHERING.

MUSEO PAULISTA, SAN PAULO, BRAZIL.

USE OF THE INITIAL CAPITAL IN SPECIFIC NAMES OF PLANTS.

THE idea seems to prevail among some naturalists, as may be seen from a recent review in this journal (p. 162), that the retention of the initial capital in certain specific names of plants is a barbarous relic that the botanists themselves cannot honestly defend. As a matter of fact, this is very far from the truth, for it is almost universally adopted in botany, and for good and logical reasons. In the latest authoritative enumeration of American plants, namely, the *List of Pteridophyta and Spermatophyta*, there are four classes of specific names that are written with an initial capital: (1) Species named in honor of persons; (2) species named from places; (3) names of old genera, tribes or sections used as specific names; (4) substantives used as specific names.

The first case is based largely on sentiment. It, to the botanist, does not look well or dignified to write a person's name with a lower case initial. The name was given as an honor or monument to the person, and should be maintained as such. Not *Sedum torreyi*, *Plantago purshii*, but *S. Torreyi* and *P. Purshii*.

The second case is, perhaps, least defensible of all, yet it seems most natural and logical to give the name of a place as nearly as it is usually written, at least in English speaking countries. Thus, *Sambucus Canadensis* and *Campanula Americana*, rather than *S. canadensis* or *C. americana*.

The third case, namely the capitalization of specific names derived from old genera, tribes or sections, is in the highest degree valuable and condusive to accuracy. As names derived from these sources do not necessarily agree in case and number with the generic word, the initial capital calls attention to this, saves much trouble, and reduces the probability of error. *Campanula Medium*, for example, would half the time be changed into *Campanula Media*, but for the initial. So also with *Convolvulus Sepium*, *Achillea Millefolium*, *Delphinium Consolida Vaccinium Oxycoccus*, and hundreds of others that could be mentioned.

The ease with which words of this kind are changed is very well shown by the spelling of the name of the ruffed-grouse in the *Century Dictionary*. The correct name is *Bonasa Umbellus* and it is so printed in most places, but under the vocabulary word Bonasa it is *B. umbella*. This is, of course, quite a different thing, and simply shows that some unguided proof-reader, observing that the termination *us* did not agree with Bonasa, changed it.

The fourth case is much the same as the one just considered. Substantives do not necessarily agree with the generic word, and it is a matter of much convenience and information to write them with an initial capital, *e. g.*, *Ilex Dahoon*, *Gaultheria Shallon*. In this form they stand out in bold relief, while if the lower case was used there would be the constant tendency to make them harmonize in termination with the genus word.

The use or disuse of this capital initial may not be a matter of much importance, but if there were no rule upon it there would be lack of that uniformity which is so much to be desired. If left to personal choice, some writers would use it and others would not. The British Association Revised Code (1865), the code of the French Zoölogical Society and that of the International Zoölogical Congress leave the matter to individual preference. The code of nomenclature of the American Ornithologists' Union (canon viii.) expressly decides against capitals, although agreeing 'that it is a trivial matter.' The International Botanical Congress of 1867 and the committee of the American Association (1894) agree as to its adoption. Therefore, in addition to the above mentioned reasons, botanists write these classes of specific names with an initial capital for the sake of uniformity in botanical writings.

F. H. Knowlton.

DENSITY AND DIAMETER OF TERRESTRIAL PLANETS.

Recent determinations of the mass of Mercury have brought out a relation between the densities and diameters of the terrestrial planets which have not heretofore been thought possible on account of the supposed great density of Mercury.

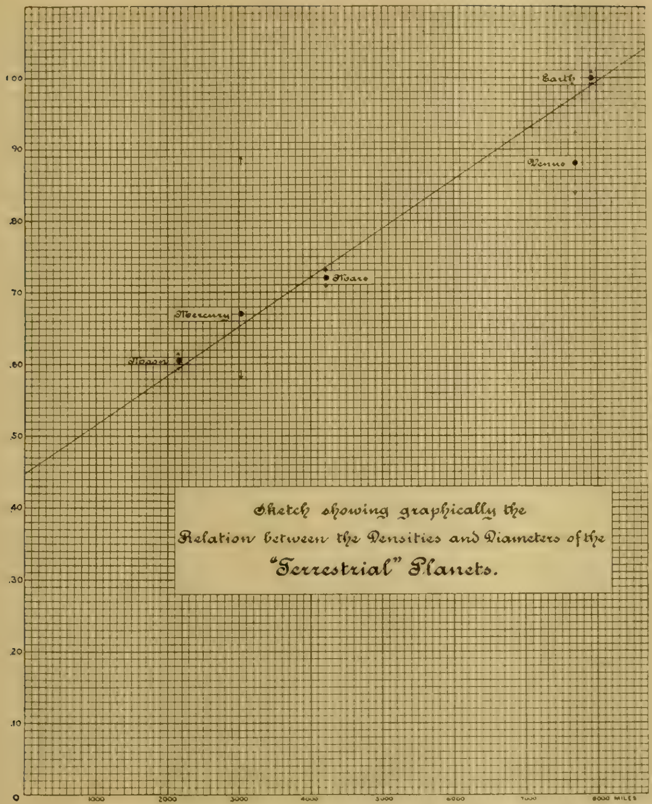
The accompanying sketch shows graphically this relation. The planets have been plotted with their diameters in miles as abscissa and their density, the earth as one, as ordinates. It is seen that these points lie approximately in a straight line. The data has been taken from Harkness' 'Solar Parallax' and Young's Astronomy. The masses from the former and the diameters from the latter, except that the density of Mercury is that lately announced by Backlund from a discussion of Encke's comet.

The probable error of the density has been obtained by combining the probable errors of the mass and diameter, and is shown in the sketch by the arrow-heads above and below the plotted points. It will be seen that the earth, Mars and the moon have much smaller probable errors than Mercury and Venus, since these latter have no known satellites to aid in determining their masses. If the most probable straight line be drawn with respect to the former, it will be as shown in the drawing. This line passes within the limits of the probable errors of all except Venus.

It will be observed that the straight line when prolonged to the left does not pass through the origin of coördinates, but cuts the ordinate at some distance above it. This indicates that a planet with a very small diameter would still have a considerable density. Meteroic stones of small diameter, when they reach the earth, do have a density about the same as that of terrestrial rocks, and this is about the density which is indicated in the drawing.

If this relation should prove to be the true law, then the mass of a terrestrial planet could be determined from its diameter. The mass of Venus so determined would be about one-tenth greater than as given. Venus is the only one of the five that is any more discrepant than might be expected from its probable error. The probable error of this planet as given may be too small. An increase of one-tenth in the mass, or a decrease of one-thirtieth in the diameter, would make Venus accordant. A sufficient increase in her mass would explain the movement in Mercury's perihelion. If the mass of Mercury proves to be as small as now supposed, that is about one-thirtieth that of the earth, it may explain some of his irregularities.

Prof. Young has pointed out that a body 200 miles in diameter near the sun would not be likely to be accidentally discovered, although it might be seen with some of the best instruments during transit across the



Sun's disc. It is, therefore, possible that Mercury may have an undiscovered satellite 200 miles in diameter. If so, and the satellite should be as far from Mercury as the moon is from the earth, it would take 150 days to make one complete revolution around the planet, or nearly twice as long as it takes Mercury to revolve about the sun. Such a satellite would have sufficient mass to cause Mercury to revolve in a secondary orbit 150 miles in diameter, which would be a measurable quantity.

E. S. WHEELER.

SAULT STE. MARIE, MICH.

THE DISTRIBUTION OF THE BLOW-GUN.

THE blow-gun is one of the most remarkable savage devices in which compressed air is used as a motive force. Primarily, the blow-gun is a simple tube of cane, smoothly cleared of the joint septums, through which light darts feathered with a tuft of down, or pieces of pith, are propelled by the breath.

The blow-gun is used for killing birds and small mammals. Frequently the arrows are poisoned, rendering the light dart effective on larger game. The chief merit of the blow-gun is its accuracy and the silence with which it may be employed.

The penetration of the blow-gun dart is greater than would be imagined. At the distance of 50 feet I have driven a blunt dart one-quarter of an inch into a pine plank. It is stated that the range of the blow-gun among some tribes is from 80 to 100 yards.

Apropos to Professor Mason's paper connecting the Eastern Asiatics with the Americans along a great natural migration line, the distribution of the blow-gun may be interesting.

The blow-gun is a tropical or sub-tropical device, and may be looked for in regions where bamboo or cane grows. Nevertheless these tubes are often made of hard wood, single, or of two excavated pieces joined together, and frequently one tube is thrust inside of another to secure rigidity. The examination of many of these blow-guns inspires a great respect for the ingenuity and mechanical skill of the workers.

The curious fact of distribution, however, is that the Malays and American aborigines alone use the blow-gun. The Malay specimens of the blow-gun existing in the National Museum are from the Dyaks of Borneo, the Javanese, the Kyans of Burma and the Johore people from the Malay peninsula. The literature also supplies other Malay localities.

The North American specimens are from the Chetimachas of Louisiana, who frequently combine the tubes in series, forming a compound blow-gun and the Cherokees of the Carolinas. From Central Amercia, the Indians of Honduras and Costa Rica ; from South America, several Amazon tribes from Equador east and from British Guiana employ the blow-gun.

WALTER HOUGH.

PSYCHOLOGY.*

PSYCHOLOGY, as we all know, is the 'science of mind.' But such a definition does little more than raise the question, What is mind? We cannot take mind for granted, for it is the very thing that psychology has to investigate. And yet, although 'mind' is one of those words which it is impossible to define, everyone is able to attach some sort of meaning to it. What do you yourselves mean when you talk of your 'mind?' You mean, probably, some particular group or set of your internal experiences; some tangle or other of feelings, thoughts, desires, resolutions, ideas, wishes, hopes, actions, emotions, impulses, expectations, memories. There are plenty of words, expressing different 'sides' of mind, as they are called. Mind, then, is the sum total of all these experiences—of all these processes. There is no mind beyond them; the term is simply the collective name of all such processes as those which I have enumerated.

I said, however, that when you talk, in an everyday way, of your 'mind,' you probably refer to some special set or group of these experiences. When you say, "I cannot make up my mind whether to do it or not," you mean that you cannot make up your present mind. Now here the psychologist makes a distinction. We use the term 'consciousness' to express the mind of the present moment. Thus if I were to ask you to tell me something of your experiences just now, I should say to you: "Look into your consciousness, and see whether so-and-so is taking place or not." Or, again, if I were to analyze for you your present state of mind—to try and imagine what is going

* A lecture delivered to the Class in General Philosophy (Introductory) in Cornell University, December, 1894.

on inside of you as you listen to me—I should speak technically of analyzing your consciousness. Consciousness is the mind at any moment. Mind, therefore, is the sum-total of consciousnesses experienced in the lifetime of the individual. You have one mind, extending (I hope) over seventy full years; but the mind upon which you experiment at any given moment for psychological purposes—or the mind which you make up at a given moment—is called your consciousness. So that psychology, while it is the science of mind, in the sense that it deals with all the mental experiences of a man, from the time of his birth to the time of his death, deals in any special hour, during any special enquiry, with the phenomena of consciousness.

But consciousness—as the number of words in my catalogue of a moment ago sufficiently indicated—is a very intricate, complex and tangled matter. If we are to examine it at all carefully, we must try, first of all, to get some sort of order into its phenomena. Let us begin the attempt at once of describing our internal experiences, as accurately as possible.

We notice, at the outset, that we are to a large extent at the mercy of our surroundings, of things outside of us. We are not free to see what we like, to hear what we like, to touch what we like; what we see and hear and touch is all determined for us, by the physical nature of the bodies from which impressions come. You can understand, of course, that this is true in the simple instances that I have given; but I want to prove to you that it is true of a very large part, indeed, of our mental experience. Put down in the first place (1) sensations and perceptions. Every time that one of our sense-organs is excited, is put in action, that is done by means of something in the external world. An ether-vibration makes us see; an air-vibration makes us hear or smell, and so on. Those are sensations. And perceptions only differ from sensations in being more complicated. Thus in the sphere of sight, you perceive a house or a tree; in the sphere of hearing you perceive a musical harmony or a musical discord; in the sphere of touch you perceive that a complex of impressions is a piece of wood, or a piece of wire, or what not. The tree and the house are compound impressions, containing many colors and many shapes; the musical chord is a compound of three or four or more simple tones, and so on. All this, very obviously, comes from the outside world. So, too, does (2) memory. You cannot remember what has not happened. If you try to remember a name, you try to recover a lost perception—the perception of the spoken word. If you try to remember a picture, you are attempting to recover a lost visual perception. It is for this reason that the psychologist distinguishes kinds or types of memory—the visual, the auditory and the motor. People who can play chess blindfold have the visual memory very highly developed. They do not, perhaps, see every piece in their mind's eye, but they see the board as a whole, and know where each piece upon it is. Most 'extempore' speakers, too, rely upon their visual memory. There is comparatively little true extempore speaking done. Of course, if a man is thoroughly familiar with his subject, or is speaking under the influence of strong emotion, he may be able to address an audience without preparation. But most of us who speak 'without notes' do so by the aid of our visual memory; we see what we have written, mentally, paragraph by paragraph, and when our eyes are on our hearers, are really reading from a memory manuscript. Instances of good auditory memory, again, are furnished by those fortunate persons who can recall accurately the airs of an opera that they have only once heard. And people who play the piano 'by ear' play by finger-memory; their memories are muscular or motor. All these memories, then, depend upon the external world. So (3) does imagination. Imagination can put perceptions together in new or unusual ways; but it can never make a new perception. Try to imagine a color which is different from all the colors that are known. You cannot do it. You may imagine mixtures of colors, hues and tints obtained from combinations of the known colors, which you have never actually seen; but you cannot imagine a new color. The same fact comes out in works of fiction. When Baron Munchausen takes you to the moon or the dog-star, and shows you their inhabitants; and when Peter Wilkins describes to you the population of the South Pole—these people are simply human beings, with their characters changed and modified in various ways. They can take their eyes out of their heads and pass them round to their neighbors, or they have wings which fold around them and serve as clothing; but there is nothing new in all this. It is only the putting of the perceptions together that is new, not the perceptions themselves. And the same is true of all the constructions of the imagination, as they are called, devils, centaurs, sea-serpents, dragons, hippogriffs, ghosts and the rest of them.

The world outside of us, then, is responsible for a good deal of our mental furniture. We can simplify matters, here, for purposes of classification, by grouping together sensation, perception, memory-image and imaginary representation, as 'ideas.' Sensation is the raw material from which ideas are built up. As for the other usages: if you cannot remember, you say 'I haven't any idea of what that man's name was;' and if you are endeavoring to imagine a circumstance, you say 'I haven't any idea of how that could have happened.'

So much for the first principal category of mental experience. Now, in the second place, we are in some respects not at the mercy of the world outside, but the world is at our mercy. What is the great difference between the animal and the plant? Surely this, that the animal can move at will, while the plant is stationary. That seems to be a very simple matter; but just consider how much it means. If the plant is going to lead a stationary life, it can take advantage of the fact—I speak metaphorically, of course—to be careless of its shape and size; or rather, it must make itself as big and as complicated as it can, in order to secure all the nourishment possible from one settled spot. The result is that the plant carries its lungs and its digestive apparatus all over it, on the outside. You know the functions of leaves and roots. With the animal the reverse is the case. It is going to move about. It can seek food in different places. The best thing for it, therefore, is to have its lungs and digestive organs packed away inside of it; so that it can get about with as light a weight to carry, and as convenient a balance of that weight, as possible. There must be no loose ends left on the outside, injury to which would mean inefficiency or death. Well! You see that, by moving among things at its own will and pleasure the animal has a certain power over the external world. How is this power represented in consciousness? In two principal ways: (1) Whenever we move; or, to put the matter more technically, and more definitely with reference to ourselves as distinct from the lower animals, whenever we act, we have in consciousness the experience of effort, of endeavor. This is an experience quite different from the experience that comes to us as ideas. We can have, naturally, an idea of effort; that would be the idea of some person making the effort, or the idea of some obstacle to be overcome by effort, or what not. But besides the idea of effort, we experience effort itself. That is one of the hardest points in psychology to have made clear to you, or to make clear to yourselves. This instance may help you: You know that we speak of one man as having more 'go' in him than his neighbor, without implying by the phrase that he has more ideas. There are many names for the effort-experience. Some psychologists speak of it as the experience of spontaneity, of one's own initiative; others of an activity in consciousness. 'Effort' is at once the most concrete and, I think, the most intelligible word. (2) Our power over the world outside, again, is manifested in another way —by the phenomena of attention. Not every process among our physical surroundings has us at its mercy in the same degree. We are exposed to all manner of impressions; but they are not all alike powerful to affect our consciousness. Think of your own state of mind now. You have presented to you a certain number of visual impressions—the room, its furniture, the people about you. You are subject to certain temperature sensations; to certain pressures, from your clothing; to certain organic sensations, hunger or satiety. Each of you has a large stock of memories, ready to crowd into consciousness if they are allowed to. Each of you, again, has the day's programme in his mind; he can imagine what will be done between now and bed-time; and this train of ideas of the imagination is ready to sweep across his mind, if free play is given to it. But all this medley of conflicting influences you are able, if you like, to neglect. You can just brush them aside, by attending to the single series of auditory impressions that is affecting you, to the succession of words which I am speaking. When the whole of your surroundings is pressing in upon you through the avenues of the sense-organs, clamoring for notice, you have the power of choosing which shall be let in at the door of consciousness. Only those facts cross the threshold to which you desire to attend.

"But," you may say, "suppose that this is true, what has attention to do with movement? You told us that it was movement that distinguished the animal from the plant, and that along with movement went power over the external world. Now what has movement to do with attention?" That is a perfectly fair question, but one which I cannot here answer for you in detail. To understand the fact of the connection thoroughly—and the connection is a fact—you must have studied psychology. But I can give you a pair of statements which will be better than nothing. The first is this: Whenever we attend, we move. I do not mean that the whole body moves, that there is locomotion: but that there is movement,—movement in the eye, movement in the ear, movement in the scalp, movement somewhere. And the second is this: It is the moving thing that attracts the attention. You cannot attend to one single thing, one really single thing, for more than a few seconds together. Either you go to sleep, or you go into hysterics. On the other hand, one is almost constrained to attend to anything that moves. You can hear the single voice that carries the melody, when there is an orchestra of half-a-hundred instruments thundering on at the same time, because the melody changes, the tones move; while the accompaniment is relatively stationary. So that attention to the melody is easy. If any of you have been out shooting after dark, you will know that one tells the game by its movement. So long as it is still, it is safe. But let it move, and though the eyes have been looking in a quite wrong direction, the attention is drawn upon it by force, as it were; one cannot help seeing it.

Those, then, are two categories of mental experience. There is one more to mention. This self of ours, this 'I,' which is exposed to the physical changes in the world in part, and in part helps to bring about physical changes in the world by moving to and fro in it, is not indifferent to what goes on in either case. It does not just have ideas, on the one hand; and attend to them or move in consequence of them, on the other. It does more; it feels. It feels when impressions come in; it feels when efforts go out. So that alongside of ideas and efforts must come a third category of mental experience—feelings. Feeling is of two kinds, pleasurable and painful. It is quite distinct in consciousness from ideation, and from effort and attention. That is another of the points which arise at the very beginnings of a study of psychology that it is extremely difficult to get clear about—that pleasure and pain, as such, belong to an entirely different order of processes from the processes which we call collectively ideas. But it is a fact, despite the intimate interconnection of the two in our concrete experience. Let me try to drive it home for you by two illustrations. You cannot remember a pleasure or pain. When you try to recall the pain of a flogging that you had at school, what you recall is really only the complex of perceptions, not the pain itself. You remember all the circumstances—your being sentenced, the people standing round you, the room in which the fatal event took place, the master who did the deed. All these are ideas. But so far are you from being able to remember the actual pain of the flogging that the memory of the circumstances to-day may be actually pleasant; you smile as you look back on them. That is the first illustration; the second is this: You cannot attend to a pleasure or pain as such. It is a common saying that if you attend to a toothache, for instance, you 'make it worse.' That is bad psychology. You attend, in reality, to the tooth. That means that you perceive the tooth more clearly than anything else for the time being; your idea of the tooth is the very strongest in consciousness. But by attending to the idea and so making it clearer, the feeling that goes along with the idea is made clearer, too. So the pain 'gets worse,' not because you attended to it, but because you attended to the group of perceptions with which it was connected.

Now, then, we have got our raw material into something like order. Consciousness, instead of being a shapeless tangle and maze of various intertwined and interwoven processes—as it appeared to us to be when we started out on our enquiry—has proved to be capable of arrangement and simplification. You may, it is true, raise the objection that our table of contents is, perhaps, not inclusive of every known mental state. Where, you may ask, is emotion; where is expectation; where are all the rest of the familiar terms for mental experiences? Well, you must take my word for it, that all these other states of mind or mental experiences can be derived from the three simple processes which I have named to you. If you were to work through a psychology, you would find that there was nothing treated of, in any chapter of it, which was not a compound of these three sets of elements—ideas, feelings and efforts—mixed in different proportions. And that being the case, it is these three elements with which psychology begins. She first of all describes them, as minutely and accurately as possible; and then furnishes a theory or an explanation of them, in the sense that she gives the conditions, bodily and mental, of their appearance in consciousness. Under what conditions do we have this and this perception? Under what conditions do we remember and imagine? Under what conditions do we feel so and so, attend to this and that? These are the questions that come up for answer.

Into those questions we cannot here enter. Let it be sufficient for you, in this lecture, to have learned the names and characters of the simplest items of mental experience—of those items which are always and invariably present in our concrete, every-day experiences. Draw for yourselves an outline map of mind. You must make three countries, as it were, within that map. Ideas must go in in one color to the right; efforts in another to the left; and feelings will lie in the middle between the two. And you must suppose that each of these three territories has an independent government; but that their governments are very friendly, and often take joint action—indeed, that they hardly ever think of taking action of themselves. Especially must you conceive that both idea and effort have right of way through any part of the dominion of feeling; and that the communications are so open, and the relations so close, that scarcely anything can affect idea or effort, from the outside or from the inside, that does not also exert an effect upon feeling. The detailed survey of the three territories, and the laying down of roads through them for the student to follow—that is the further business of Psychology.

E. B. Titchener.

LOSS OF PROFESSOR MILNE'S SEISMOLOGICAL APPARATUS, LIBRARY AND COLLECTION.

Every one interested in Seismology knows of the great work done by Professor John Milne, F. R. S., during a residence of nearly a quarter of a century in Japan, which country became, a decade ago, the earthquake laboratory of the world. Through his interest, and that which he kindled in other foreign residents, the Seismological Society of Japan was organized about fifteen years ago. During its active existence its Annual Reports contained the most important contributions to Seismology anywhere published, and it is not too much to say that the work of this Society amounted to a revolution in the methods of observation and research. To its Transactions, Professor Milne was by far the largest contributor. When the rapid decrease of the number of foreign scientific men resident in Japan threatened the life of the Society, he tactfully enlisted the support and cooperation of the Japanese. The issue, by the University, of an extensive and valuable series of scientific memoirs, tended, naturally, to divert much of the active interest which they for a time manifested, and a few years ago the publication of the Transactions of the Seismological Society ceased. Professor Milne was not discouraged however, and at his own risk and expense at once substituted a periodical which he called the 'Seismological Journal,' which he has continued to issue at great pecuniary loss and which contains many valuable and important contributions to the science.

During all of these years, with a tireless and inexhaustible industry and a rare ingenuity of design and wealth of mechanical resource, he had invented, constructed and put into use a variety of earthquake detectors, recorders, measurers, wave and tremor registers and even earthquake 'avoiders' or 'nullifiers,' which, with the numerous devices and inventions of other foreign students of Seismology in Japan, the value of which he was quick to recognize and utilize, constituted a collection the like of which never existed before. Besides these instrumental appliances Professor Milne had accumulated an extensive and valuable library of Seismology, including many early and rare pamphlets and volumes and almost everything published on the subject during the past fifteen years.

His connection with the Japanese Government is shortly to terminate, and he had prepared a complete equipment for an observatory to be set up in England on his return to that country, by means of which he hoped to show that earthquakes travel around the globe, and to be able to study them there.

Those who have been aware of all these facts, and all who are now made aware of them for the first time, will, I am sure, experience a feeling of great regret on learning of the destruction by fire on February 17th of practically all of these valuable accumulations of years of labor, together with personal effects of great interest and value to Professor Milne.

The observatory in which these things were, and which is now gone forever, was also an object of much interest in its relation to the educational development of Japan during the past twenty years. It was erected nearly that many years ago, a little before the close of Dr. Murray's connection with the Department of Education. It contained in the beginning a good but small Equatorial by Alvan Clark and a Transit. One end of it was used as a meteorological observatory under the direction of the writer during several years, being equipped with a good collection of self-registering instruments obtained mostly from London, the results of the use of which were published as Annual Scientific Memoirs by the authorities of the University. The transit wing was utilized by Professor W. S. Chaplin in his courses in Civil Engineering, until the Astronomical part of it was placed in the hands of Professor H. M. Paul, who served the University as Professor of Astronomy for several years, beginning in 1880. When a few years later the Engineering College became an integral part of the University and the whole was located in the Kaga Yashiki, the observatory was turned over to Professor Milne, an addition to it was built and he made a Seismological 'Laboratory and Bazaar' out of it, residing in a part of it. It was a comfortable bungalow sort of a structure, located in the Kaga Yashiki, just in the rear of the row of dwellings where, fifteen years ago, lived, beginning at the entrance to the Compound, Fenollosa, Mendenhall, Braun, Cooper, Morse, Chaplin, Ewing and Atkinson, all Professors in the University and exhibiting a mixture of American, Spanish, German, English and Scotch blood which illustrates the disposition of the young-old nation to get what it wants wherever it thinks it can find it. When it became the home of Professor Milne it became the source of a delightful hospitality which many 'globe trotters' of all lands have enjoyed, and thousands besides his scientific friends will sympathize with him in his great loss.

In a recent letter from Professor Milne he says:

"Just now you and Paul may be breathing all that is left of the old observatory and my belongings."

He sends me a characteristic and graphic account of the occurrence, 'prepared,' he says, 'for maiden aunts and relatives,' from which the following extract will, I am sure, be of interest to all readers:

"As nearly all the transactions of the Seismological Society were packed up to go to Europe, a few that had middle places in the boxes may be saved, but I doubt if even out of 2500 copies I shall get more than two or three hundred. All my old earthquake books, some of which even dated from 1500 to 1600, but which were perhaps more curious than useful, seem to have gone. One function they had was to inspire the globe trotter, or travelling clergyman, with respect for a science that was apparently so ancient. Amongst them there was a poem called 'the earthquake,' A. D. 1750, but I know that by heart. The new books were volumes of bound pamphlets in all sorts of languages which I had slashed out of the publications of all sorts of societies. Perhaps the burning of them was a visitation for my Goth-like behaviour.

Instruments were fused or vaporized. Sixteen specially constructed clocks which would turn drums once a day, once a week, or drive a band of paper for two years, together with seismographs and horizontal pendulums, self-recording thermometers and barometers, microscopes, and a museum of old and new contrivances are now in the scrap heap. Until to-day, I felt I had the observatory I intended to put up in England completely furnished, and I was proud of the furniture.

One very cruel cut was the picking up of an insurance policy dated 1878, which fluttered out of the ruins. One reason that I have not insured for some years past is because day and night I always had for purposes of continuous photography open benzine lamps burning in my house, and I should have had to tell the agent about the little tricks they played when first I used them. It may sound odd, but I do not think a stranger to their ways can light one so that nothing shall happen during the next three days. Against eccentricities like these I insured myself by having above them a bunch of fluffy paper, which, if the lamp blazed up, was burned and burned its suspended string. This was followed by the falling of a lever, when an electric bell in my bedroom and one in the kitchen was set going.

Outside the door of the instrument room stood fire-extinguishers and a heap of rugs. From time to time I had 'fire drill,' going through the operation of turning up a lamp, burning the paper, ringing the bells, alarming everybody, and then putting out the conflagration—in fact, very much like what happens on ship-board, only I had real fire—which was easily extinguished.

But what happened was the unexpected; the fire broke out in the midst of a pile of wood in an out-house, and this, with a nice wind blowing, on a Sunday morning, when there was no one near to help.

And now I have next to nothing—decorations, medals, diplomas, clothes, manuscripts, extending over twenty-five years, and everything else has gone up in smoke; still it is not altogether a misfortune.

I shall not have a sale, nor the worry of selecting amongst my accumulations; there will be no buying boxes and packing up, neither will there be any haggling with custom house officials, or trouble in collecting on an insurance policy. On the other hand, I shall have new clothes, and some time or other, I hope, new clocks and new instruments, whilst what I have got is the knowledge that I have many sincere and kind friends. Their clothes don't fit, but the sympathy that they have expressed and the little things they have sent me tells me that I should never be homeless in Japan. Looked at in the right way; like an earthquake, a fire may, after all, be a blessing in disguise, but, of course, it is sometimes pretty well wrapped up.

Dies iræ, dies illa,
Solvet sæclum in favilla."

Professor Milne asks me to make public the loss of his address book and his desire to send to all to whom it may be due, copies of Vol. IV. of the 'Seismological Journal.' This, he says, is an unusually large number, and he hopes an unusually valuable contribution to Seismology—his 'expiring effort;' and he asks all to whom this volume should be sent to address him, care Japan Mail Office, Yokohama.

Out of the few hundred copies, more or less, of the Transactions of the Seismological Society of Japan, he will be able to make up some sets; and those desiring to obtain them should address him, care Geological Society, Burlington House, London. And finally, he earnestly desires to receive, in exchange or otherwise, copies of any papers on or relating to earthquakes, volcanoes, or earth movements in general.

I am sure that every one who can will respond to this last appeal and cheerfully do whatever is possible to assist Professor Milne to replace, as far as may be, the accumulations of a quarter of a century, converted into sunset-reddening dust in a few short moments. T. C. M.

CORRESPONDENCE.

THE IDEAL INDEX TO SCIENTIFIC LITERATURE.

To the Editor of Science: Since you have been so kind as to ask me to contribute to Science my views as to how the plan of cataloguing scientific literature may best be accomplished, I venture to present the following considerations. It is probable that some of the ideas suggested are impracticable, and indeed that the plan is too extensive and unwieldly to be undertaken as a whole at the present time. The literature of science is so vast and the number of workers so great, the degree of specialization in modern work so intense and the participation in research so wide-spread over the world, that a really adequate and serviceable index must, of necessity, be of great extent, and undertaken upon a scale of considerable magnificence.

It may be that the time has not yet come when the scientific men of all the world can coöperate together in such a task as this, but if coöperation is possible in any field of intellectual activity, surely it is in that of science. Such coöperation is not only essential to thorough work in indexing, but would also have a most important influence in promoting united efforts in other branches of scientific activity.

The considerations suggested are these:

1. The catalogue should be international in name and scope. This is essential in order to secure the unreserved support of all nations engaged in the production of scientific literature. It should, therefore, not bear the imprint of any society or organization, or derive its distinctive character from any one nation. Since the titles will, of necessity, be quoted exactly, it might be well that all annotations and comments should be in the same language as the title. To insist that only English or French should be used would be fatal to its general adoption by other countries. Titles in the Scandinavian, Slavonic and Oriental Languages and dialects and others would, however, need to be translated into French, German or English.

2. It should be exhaustive within its own limits, no latitude being given to the judgment and taste of its editors, in the matter of rejecting titles.

3. It should be printed in annual installments, each installment including every paper or work printed within a single year, and each installment should be published in not more than six (preferably not more than three) months after the close of the year.

4. The publication should be in the form of a bibliographical catalogue, with the titles arranged alphabetically by authors, the papers by each author to be numbered, beginning with number one. This would render it possible to identify any paper, either in an annual or a general index, by simple reference to author, year and number.

In recommending that the catalogue shall be published in book form, I am by no means unmindful of the merits of the card-catalogue system in work of this kind. I use card-catalogues freely in my own work, and in the National Museum there are hundreds of thousands of cards by means of which the vast collections of specimens and papers are kept under control. The card-index has its limitations, however, and these are nowhere more evident than in connection with such a scheme as a universal scientific catalogue.

The very bulk and unwieldiness of the card system is an objection, which may be partly appreciated if one can imagine the contents of the ten volumes of the Royal Society's Catalogue transformed into card form and arranged in drawers.*

In the volumes as they now stand, the eye can sweep rapidly over page after page in search of a given title, and thirty or forty impressions pass to the mind at a glance, instead of one, while the strain upon the attention caused by turning over the pages is much less than where each title card is scrutinized singly.

For finding a book or reference when the name of the author or its title is known, the card system is without rival. It is less useful, however, when, as often happens, one is 'looking up' a subject in a general way. A card-catalogue, after it has attained to great bulk, requires much labor

* Dr. Carrington Bolton prepared the copy for his 'Select Bibliography of Chemistry' on slips of standard sizes, and it filled 7 standard trays or a length of nearly 9 feet. The slips were on thin paper—if they had been of card the lengths would have been nearly 20 feet. When printed the 12,000 titles were presented in a light convenient octavo volume of about 1,200 pages.

in consultation and a vast amount of pains-taking care to insert new cards and keep it in order. Then, too, one of its features which makes it particularly advantageous in the hands of an individual scholar, is that the cards may be continually sorted and rearranged. This would be practically impossible with a great card index intended for the use of many in a public institution. Volumes like those of the Royal Society index may be carried to the desk of the student. A card-catalogue he must consult in its place of deposit, probably in a crowded and noisy library. Then, too, after a period of years the card index will represent the investment of hundreds and soon of thousands of dollars, on the part of each possessor, and the tendency will be to place constantly narrowing restrictions upon its use.

The needs of library workers might be met in part by printing a special edition of the catalogue on one side of the page, so that the titles might be cut and pasted upon cards.* Indeed, if there were a sufficient demand, a special edition of the catalogue might be printed on cards. Whatever may be said of the advantages of the card system, it is certain that it would not be accepted in Europe.

Every one remembers the plan of Jewett, who, in the early days of the Smithsonian Institution, proposed a universal bibliography. His plan was to electrotype each title upon a separate block, and to supply these blocks, either for printing cards, or to be made up into catalogues in any system of arrangement desired. His project almost succeeded fifty years ago, when there was much less demand, much less money, and much more in the way of mechanical obstacles, than at present. The modern type-setting machine, which casts each line of type in a single bar, would lend itself admirably to such co-operative work.

5. A subject-index of the most exhaustive character should be issued in connection with each annual publication, but since this index cannot so conveniently be made until the catalogue itself has been set in type, it might be well not to delay the distribution of the catalogue itself until the index is ready, but cause the latter to follow as soon as practicable.

6. The adoption of this index as a part of the plan would render it practicable to issue the entire record of the year's work in one single alphabetical series, if this were deemed desirable. It might be, however, that it would be more convenient, and less expensive to subscribers interested in special branches of science, if the titles were arranged in more than one series. To divide it into two—one for the physical and one for the natural sciences—would be quite practicable; perhaps philology, history, economics and mechanical science might each have a volume of its own. Whether further subdivision would answer, is a question for careful discussion.

7. The catalogue should embrace within its determined scope all publications in the following categories:

(a). Publications of scientific academies and societies.

(b). Scientific publications of universities, colleges, and technical schools.

(c). Publications of scientific expeditions.

(d). Scientific publications of national, municipal and other governments.

* In order to facilitate this, the name of the author might well be printed in bold-faced type, and *repeated at the beginning of each title.* This increases the cost but little, and adds much to the usefulness of the bibliography, if it is to be cut up and rearranged, either for a catalogue, as I have suggested, or as 'copy' for other bibliographies. The width of the title as printed should not exceed 4½ inches, whether the publication is in octavo form or larger. It will then come within the limits of the standard cards.

(e) Independently published scientific books of reputable character.

(f) All articles in journals and magazines devoted exclusively to the sciences.*

(g) Articles of scientific importance in the general periodical literature of the day, and in the cyclopædias and works of reference, at the discretion of the editorial committees.

(h) All bibliographical publications, relating wholly or in part to scientific literature, including important library catalogues, etc.†

(i) All authors-separates or offprints with independent titles and paging. (Including even scientific addresses and special papers in ephemeral journals, when practicable.)

(k) *Festschriften:* Memorial works and others, coöperative volumes, these to be analyzed and indexed as periodicals.

(l) Scientific biography, the history of science and scientific institutions, etc.

8. The catalogue should embrace the following divisions :

A. General Science.

B. Mathematics.

C. Astronomy.

D. Meteorology.

E. Physics (including Astrophysics).

F. Chemistry.

G. Mineralogy.

H. Geology and Physiography.

I. Biology (including Morphology, Physiology, Systematic Botany and Zöology, Geographical Distribution of Life, Pathology, Psychophysics, etc.).

K. Anthropology (including Prehistoric Archæology, Ethnography, Comparative Technology, Folk-Lore, *Culturgeschichte*, etc.

L. Economic Science and Statistics (under determined limitations).

M. Mechanical Science and Engineering (under determined limitations).

N. Philology.

O. History, at least to the extent of including Archæology and the History of Institutions.

P. Geography (including all serious works of travel and works of reference geographically arranged).

In connection with this annual bibliography, an effort might be made to induce all persons and societies engaged in bibliographical work to adopt the same system, so that every title prepared and printed might be available for use in the universal catalogue of scientific literature, beginning with the birth of science, which, it is hoped, may in time be printed. In this connection there might be committees to advise with bibliographical workers, and whose function it would be in part to discourage duplication of work. A central office or a bulletin might be established, in which should be recorded all manuscript and published bibliographies in existence, and means provided by which persons proposing to do bibliography-work may ascertain whether the field which they intend to work in has already been covered.

No system for organizing this work has been suggested, but it is evident that if all the energy and all the money yearly expended upon the printing of partial bibliographies could be concentrated, there would be no lack of means for accomplishing very much more than has been here proposed. To secure such coöperation the proposed catalogue must meet, as fully as possible, the necessities of librarians, readers in libra-

* Book reviews and important book notices should probably be included, but whether they should be cited under the names of their authors, or parenthetically under the titles of the publications to which they relate, is a question. The latter is probably better, especially if cross references should be made under the name of the author of each review.

† It is suggested that even bibliographical appendices of importance, published in connection with books or articles, should be separately indexed, and that the annotations should indicate with precision their exact scope and character.

ries, investigators and writers, booksellers and book buyers.

It is evident, however, that existing agencies which are now engaged in bibliographical and index work should all be conciliated and enlisted in the work.

The Royal Society, the Smithsonian Institution, the special societies, such as the Zoölogical Society of London, the American Chemical Society, all groups of bibliographers engaged in the preparation of such works as the *Zeitschrift für Orientalische Bibliographie*, and the great individual bibliographers, like Professor Carus, should be brought in.

The sale of the work would undoubtedly cover the expense of printing and publishing, and it is not impossible that a considerable part of the expense of compiling might also thus be covered.

Considerable money subsidies would however be essential if the thing is to be done well.

The editorial work should doubtless be done without regard to geographical considerations, under the direction of specialized societies or institutions which should also be depositories of special information in regard to the bibliography to which they are devoted. It would be well, however, that in every country there should be a central office or depot where all the publications of that country should be systematically gathered.

It would seem also that some suitable plan should be devised for giving individual credit to the persons by whom the work is done, for there is an immense deal of self-sacrificing and conscientious work put into bibliography, and the pride of the bibliographer in having produced a thorough and workmanlike contribution in his chosen field is perhaps scarcely less than that of literary authorship.

G. Brown Goode.

U. S. National Museum.

SCIENTIFIC LITERATURE.

A Handbook of the Birds of Eastern North America. By Frank M. Chapman. New York, D. Appleton & Co. 1895. 12°, pp. 420. Library edition, heavy paper, broad margins. Pocket edition, thin paper, no margins, $3.00.

We live in a period of unusual productiveness in ornithological literature. We have technical works of scientific merit, popular works of literary merit, and local lists almost without end. But ornithologists and amateurs alike have long felt the need of a compact handbook small enough to be carried in the pocket, and full enough to afford means of ready identification. Another desideratum was that it should be written in language not too technical for the beginner. The older ornithologists, while recognizing the demand for such a book, have been too busy with special studies, and it has remained for one of the younger men to bring out.

Mr. Frank M. Chapman, the author of the present *Handbook*, has sought to fill the gap. He has written a book so free from technicalities as to be intelligible to a fourteen-year old boy, and so convenient and full of original information as to be indispensable to the working ornithologist. His plan is unique; his descriptions are from actual specimens (not compiled); they are written in plain English, so that no glossary is necessary, and are accompanied by numerous figures of heads, feet and tails as aids to identification. The description of each species is followed by paragraphs giving the geographic range (and the breeding range is commonly discriminated from the migratory and winter ranges); the time of presence at Washington, Long Id. [water birds], Sing Sing and Cambridge;* descriptions of the nest and eggs, and a brief popular ac-

* The data for these 4 stations are contributed respectively by Chas. W. Richmond, Wm. Dutcher, Dr. A. K. Fisher and William Brewster.

count of the habits. The latter is a special feature of the book. Many of the biographies are contributed by well-known authors and were written expressly for this work—a novel departure. Among the names signed to these articles are those of Mrs. Olive Thorne Miller, Miss Florence A. Merriam, William Brewster, Eugene P. Bicknell, Jonathan Dwight, Jr., Ernest E. Thompson and Bradford Torrey. But it would be unfair to imply that the contributed biographies, excellent as they are, are better than those of the author. Mr. Chapman is not only a naturalist of wide field experience and a close observer; he is in addition a true lover of birds, and his short sketches of the different species contain the essence of their life histories.

Another feature of the book is the keys to species. These keys have been prepared with great care, and, while not always dichotomous, are so complete as to enable the student to identify the females and young as well as the adult birds—a rare merit. A chromolithograph chart comprising 30 colors serves as a key to the terms used in describing plumages—an advantage not possessed by any other American Ornithology. The illustrations also are helpful. The text figures, more than 150 in number, will prove of great assistance. The frontispiece is a colored plate of the Bob-white or Quail in a bramble thicket, by Ernest E. Thompson. The other full-page plates are engraved half-tone reproductions of photographs. One shows the heads of 15 kinds of ducks and will be most useful. The remaining 16 are photographs of mounted birds in natural surroundings and serve to embellish the book. One of the best and most artistic shows a rail on his marsh (from a group in the American Museum).

Fifteen profusely illustrated pages are filled by the keys to the larger groups, and the figures alone should suffice to enable beginners to refer any bird to its proper family.

The systematic part of the book is prefaced by 40 pages of introduction, in which an effort is made to place the study of birds on a higher plane than that of the mere collector and student of technicalities. Mr. Chapman well says: "Birds, because of their beauty, the charm of their songs, and the ease with which they may be observed, are usually the forms of animal life which first attract the young naturalist's attention. . . . . The uninstructed beginner usually expends his energies in making a collection, for he knows no better way of pursuing his study of birds than to kill and stuff them! Collecting specimens is a step in the scientific study of birds, but ornithology would have small claim to our consideration if its possibilities ended here."

The scope of the introduction may be seen from the chapter headings: The study of ornithology; The study of birds out of doors (including bird calendars for the vicinity of New York); Collecting birds, their nests and eggs; Plan of the work (including a bird diagram, feather patterns, and so on).

It is hard to find anything worthy of serious criticism in this excellent and timely book. The use of English inches instead of millimeters is a blemish in a work of scientific value, and is less excusable since the persons who use it will be students and graduates of our schools, who are familiar with the system. We trust that in the next edition the author will not only substitute millimeters for inches and fractions, and make all the keys dichotomous, but that he will enlarge the scope of the work so as to take in the great West as well as the East—giving us a 'Handbook of the birds of America north of Mexico.'

The plan and originality of Chapman's Handbook, its copious illustrations, bountiful keys, succinct accounts of habits, convenient size and low price insure it wide popularity; while as a handbook of the birds of eastern America it is bound to supersede all other works. It is a boon to the amateur, a convenience to the professional, and will prove a help and incentive to the study of birds. Such books are now among the greatest needs in all departments of natural history.

C. HART MERRIAM.

National Geographic Monographs, prepared under the auspices of the National Geographic Society. No. 1, Physiographic Processes; No. 2, Physiographic Features. By J. W. POWELL, late director of the United States Geological Survey. New York, American Book Company, 1895. Twenty cents a number. $1.50 a year (ten numbers).

The first two numbers of the geographic monographs, announced in SCIENCE No. 10, have lately been issued under the above titles. The series is to appear monthly during the school year, the special object of the publication being "to supply to teachers and students of geography fresh and interesting material with which to supplement the regular text-book."

A series of essays like this deserves a warm welcome from those who are interested in raising the standard of geographical teaching, and the two numbers now issued are of particular importance in several ways. They affirm, with an emphasis not hitherto given in this country, that the proper foundation of geographical study is an understanding of physiographical processes; they mark the entrance of various members of our National scientific bureaus into the work of publishing the best selections from their knowledge in essentially elementary form, with the intention of aiding teachers and scholars in our schools; they represent not simply the temporary effort of an individual, but the continued efforts of a body of experts to introduce subjects of better quality and treatment into ordinary geographical study. Such an undertaking, if successfully maintained, cannot fail to impress itself strongly all through our educational system, for, instead of appalling the reader at the outset with a large treatise of heavy cost, it continually tempts him to go further and further by the successive appearance of attractive and interesting but inexpensive pamphlets, month after month and year after year.

The publishers present the monographs in good form, well illustrated, and certainly at a very moderate price.

It is particularly interesting to receive in these two numbers the results of Major Powell's long consideration of physiographic questions. For some years his attention has been so largely given to administrative work in connection with the National Geological Survey that we have had comparatively little from his pen; but now we learn the general views that have been gradually forming during his long experience of the many aspects of geography and geology; here we find tersely presented his matured opinions on the essential elementary conceptions concerning deformation and denudation, about which our teachers are as a body so indifferent, so skeptical or so timid. Mountains are not described as the result of chaotic uplifts, but as the unconsumed remnants of broadly uplifted and deeply eroded masses. The product of long-continued denudation is not illustrated by a canyon or a valley, as so many of the textbooks in current use imply, but by a broad surface of faint relief, close to baselevel. The lesson of our West that volcanic action is not so dependent on neighborhood to the sea as has been generally supposed is given perhaps too much importance; for no association of vulcanism with the ocean is mentioned. Among geologists, these announcements may not be regarded as novel, nor are they so presented; but it is certainly novel to have them addressed to teachers of geography, and to have them emphasized as of fundamental importance to such teachers by placing them in the first two numbers of a series of geographical monographs. Much good must result from this earnest inculcation of modern physiographical principles.

The character of the two monographs may be inferred from the following outlines: The 'processes' open with an account of the three moving envelopes of the earth—air, water and rock. Their mutual interpenetration and characteristic movements are described; the more important headings being rainfall, run-off, floods; kinds of rock, structure of the rock envelope, age of rocks, interchange of land and sea; vulcanism, diastrophism and gradation. The 'features' are classified as plains and plateaus of various kinds, mountains, valleys, hills, cliffs, special forms, stream channels and cataracts, fountains, caverns, lakes, marshes, coast forms, islands. The intelligent teacher cannot fail to be interested and broadened by a careful study of these suggestive pages.

There are, however, a number of considerations which cast a shade of doubt on the plan of beginning this series of monographs with two general essays of comparatively abstract treatment. From the very nature of the case, when so small a space as thirty pages is allowed for subjects so large as 'physiographic processes' and 'physiographic features,' there can be little room saved for the introduction of concrete illustrations. Consequently, instead of inculcating physiographic process by example, it is here inculcated almost entirely by abstract generalities. Our teachers are already educated rather too much in this way; they have not enough knowledge of fact to take the best advantage of so rich a feast of generalization as is here presented. The same comment may be made on the classification of features; the broad scheme of classification here announced is of much value to the expert, who has already in mind a multitude of examples with which to fill each pigeonhole in the scheme; but it is of much less value to the school teacher, whose knowledge of geographical facts is generally very narrow, except in so far as they are concerned with empirical data, such as the position of cities, the length of rivers or the height of mountains. With features as the result of processes, teachers have heretofore had very little to do; and they can hardly now be ready to use an extended classification of land forms, few of which are made real by illustration or example. It may be doubted whether these general monographs would not have met a better appreciation two or three years hence, after other monographs had presented in detail a good number of individual features as the result of particular processes.

There is another way in which the discussion of processess and the classification of features as here given may embarrass the teacher. He may naturally expect, from the leading place of these monographs, that they are authoritative as to plan and terms, and that the latter monographs will follow the beginning thus made. But, as a matter of fact, it is at present too early in the development of the new subject of physiography to expect any one plan of description or any one scheme of terminology to gain general adoption; particularly a plan or scheme not hitherto published, not modified by expert criticism, and not generally assented to by various investigators. As a suggestion to his fellow experts, these plans of treatment from one of so wide a knowledge as Major Powell are of high value; but as formulations of method, according to which later writers of monographs should arrange their own studies, they are of unknown value, because as yet untested by repeated use and public criticism. It is highly probable that each of the later writers of the monographs will depart from the plan here presented and introduce methods and terms of his own: so little advance has yet been made towards a general concensus of opinion in this new subject, the rational study of the forms of the land.

In its fundamental principles the classification of features proposed by Major Powell will endure, for it is based on structure and process, not on external form alone. In some other respects it does not seem acceptable, for there is a certain inconsistency and incompleteness in its terminology that is disturbing. For example, diastrophism having been defined in the first monograph as meaning upheaval or subsidence, with or without faulting or flexure, and gradation having been defined as including all processes of disintegration, transportation and deposition, we read in the second monograph that diastrophic mountains and diastrophic hills result essentially from the action of gradational processes on uplifted masses; but that diastrophic valleys, diastrophic cliffs, diastrophic cataracts and diastrophic islands result from movement alone without degradation; and no place is given to mountains of essentially constructional form, corresponding in origin to the diastrophic valleys and cliffs.

Valleys of gradation, cliffs of gradation and gradational cataracts result from processes of degradation; yet it must of course be understood that the land masses acted on by gradational processes had in these cases, as well as in the case of diastrophic mountains or hills, in some way gained an effective height above baselevel; hence it would be more consistent to call most mountains and hills 'gradational;' and thus reserve the adjective 'diastrophic' for mountains and hills made by diastrophism, like diastrophic valleys and diastrophic cliffs. Gradational islands are deposits of land waste near shore, and gradational hills are heaps of debris left directly or indirectly by glaciers; while sand dunes are given an equivalent value with gradational hills, instead of being placed with glacial hills under a general gradational heading.

Sea plains are plains of ultimate denudation with reference to the sea as the controlling baselevel; the sea plain may be enlarged by sedimentation along its margin, but no mention is made of the numerous plains resulting from the uplift of smooth sea-bottoms. Lake plains are formed with their baselevel depending on the level of lakes; lake-bottom plains, revealed by the deepening of the lake outlet ("the waters of the lake rush through the newly opened channel, and the lake is drained in whole or in part," is an unfortunate suggestion of a sudden change that must be very rare in nature), are included, but without special name, under the same heading with plains produced by denudation of the surrounding land down to lake level; and without any indication that the latter are rare and the former common.

The gradual change of opinion regarding the comparative efficacy of marine and subærial erosion gives some justification of the small share of space devoted to the processes of the seashore: but it is to be regretted that they are so disproportionately condensed. After nearly two pages about inland cliffs of gradation, sea cliffs are dismissed with less than two lines of text: "On sea coasts and lake shores, sapping is carried on by the waves, and cliffs are often produced." Floods are rather fully treated and flood plains are given about two pages, but deltas are dismissed with the briefest mention. Coast-forms in the second essay have less than two pages of the total thirty. The explicit omission of seashore features, or their postponement to a later monograph, would have been preferable to so brief a treatment.

Those who have enjoyed Major Powell's eloquent accounts of his western explorations will be glad to see again here something of the fervor of his style; but in a few cases it has led him too far for the creation of the best impression on readers so literal-minded and so ready to accept and quote authority as teachers are. It is over-eloquent to say: "The tides sweep back and forth across the surface of the sea, and alternately lash the shores with their crested waves," or "The purple cloud is painted with dust, and the sapphire sky is adamant on wings." After all the efforts to drive 'burning mountains' out of school geographies, it is disconcerting to read here about 'floods of fire' from volcanoes. In view of the importance of the gentler processes of nature, it is unfortunate to find in the closing summary of the second essay a very figurative expression regarding the three great physiographic processes: "How fire, earthquake and flood have been involved in fashioning the land and sea." The plain-spoken teacher will have difficulty here in distinguishing between poetry and prose.

There are occasional brief or over-generalized statements that must raise unnecessary questions in the teacher's mind. In mentioning the tides, the apparent diurnal rotation of the moon around the earth is worded: 'As the moon revolves about the earth from east to west.' A little later, it is said: "The seas are heated under the tropics;" but schoolmasters are the very persons who know that the tropics and the torrid zone are not one and the same. The surface currents of the ocean are referred entirely to convectional movement in the ocean itself; no surface currents being ascribed to the winds; and it is said that "all surface currents drift eastward in going towards the poles;" although this is wide open to qualification. It is inconsistent with the teachings of modern physics to speak of the 'flow of . . . heat from the fiery globes of space.'

The corrections of small things is a vexatious matter. It is little less than a nuisance to the author to have to stop for so small a trifle as the choice between 'under the tropics' and 'within the tropics.' This distracts him from the main line of thought along which he is constructing his essay. Minute corrections call for mental characteristics that are petty in comparison with the creative ability that produces the essay itself; and from an author as independent and original as Major Powell self-correction of these relatively trifling verbal matters is hardly to be expected. Yet it will be unfortunate if the editing of the future monographs does not involve such revisions as will reduce their inconsistencies to a minimum; for when teachers discover that they can take exception to certain parts of their text, their confidence in the rest of it is weakened. They have not as a rule much sense of perspective in these matters; and, as with book-keepers, a little error is in their opinion about as dangerous as a great one. They are confirmed in this habit of thought by the character of the contests, of which they are frequent witnesses, that grow out of the rivalry of publishers and the strife of book agents. Knowing this, the best way to prevent the confirmation of the habit is to give it no opportunity for practice. Even though the personality of the author be in a measure lost, it is best to scrutinize very carefully all books intended for school teachers, and exclude from them every statement and phrase that will distract the reader from the essential line of thought and set him to differing from the author on matters of subordinate value. For this purpose an experienced book agent makes a most useful proof-reader; and his services should be secured, if possible, by those who are acting for the National Geographic Society in the supervision of these monographs. His advice will be found very serviceable to authors whose previous practice in writing has been on essays for scientific journals and governmental reports.

HARVARD UNIVERSITY. W. M. DAVIS.

Butterflies and Moths (British). By W. FURNEAUX. London, Longmans. 1894. 12°.

This is by no means a complete treatise on these insects, which would be quite impossible in the 350 pages to which it is limited; but rather a selection has been made of such as the author thinks would prove most desirable. The number of British butterflies, however, is so limited (66 species) that place is found for all of them. A brief description and general account is given of each species mentioned, together with a figure of most of them; a certain amount of attention is paid to the early stages and especially to the caterpillar; but the book is very weak indeed on all points as to classification, the common characters of groups being hardly hinted at; it is therefore intended almost exclusively for the amateur, and not for the serious student. The introduction, which occupies about a third of the book, and is of as much interest to an outsider as to a Briton, is exceptionally good for a work of this class, though here again it is lean as regards all matters of structure or classification. The illustrations in the text, and they are numerous, are with few exceptions unusually good; those on the twelve colored plates not so good. The figure of the egg of *Pieris brassicæ,* on p. 14, is upside down.

S. H. S.

The Pygmies. By A. DE QUARTREFAGES. Translated by FREDERICK STARR. Illustrated. Pp. 255. D. Appleton & Co. 1895.

This volume forms number 2 of the Anthropological Series, edited by Professor Starr, of the University of Chicago. The original appeared in Paris about eight years ago, and the name of the distinguished author, as well as the interest of the subject, insured it considerable attention.

He approaches the topic historically with a chapter on the accounts of the pygmies which are found in classical writings, and an attempt to analyze them in the light of modern research. Turning to later sources, a full history is supplied of what was known ten years ago of the dwarf tribes of Melanesia, of the Mincopies of the Andaman islands, of the Negritos of Indonesia, of the Negrillos of Central Africa, and of the Hottentots and Bushmen of the southern portions of that continent. Special attention is given to the physical peculiarities of the tribes mentioned and to their sociologic condition. A chapter of some length is devoted to the religious beliefs of the Bushmen and Hottentots, successfully controverting the statement often advanced that these humble peoples had no religion at all. The illustrations, thirty-one in number, are fairly well done, though printed rather carelessly. The translator has accomplished his task well, and the text reads pleasantly.

It is to be regretted that the large material accumulated in the last ten years on this subject was not more freely called upon. Mr. Haliburton, Professor Kollman and Dr. Virchow have contributed monographs which should not be overlooked. Emin Bey's anthropometric reports on the Negrillos are the best we have; but these names are not referred to. We should have liked, also, a chapter on the causes which bring about decrease in stature, a physiological study of its etiology. Probably any people would become dwarfs under given conditions, and the trait is therefore not a racial one.

D. G. BRINTON.

An Introduction to Structural Botany (Flowering Plants). By D. H. SCOTT. London and New York, Macmillan & Co. 288 pp. 113 figs. $1.00.

The author intends that this shall be a book for beginners. Three types are chosen to illustrate the structure of the flowering plants, the wall flower (*Cheiranthus Cheiri*

L.); the white lily (*Lilium candidum* L.); and the Spruce fir (*Picea excelsa* Link). He has also introduced a chapter of 32 pages on the 'physiology of nutrition.' The language of the book is exceedingly simple. Some of the original figures are very good. In general it may be stated that the subject-matter is well treated. The author intends at some future time to present in a similar way the cryptogamic types.

The fact that the author begins the study of structural botany with the highest types will be objected to by most modern botanists. Many will also question the advisability of attempting to present structural botany in an elementary way.

ALBERT SCHNEIDER.

NOTES AND NEWS.

ARGON.

M. BERTHELOT has communicated to the Academy of Sciences the fuller details which he promised concerniug his experiments upon argon. Towards the end of February he received from Professor Ramsay 37 cubic centimètres of the gas, with which small quantity he has obtained positive results of the greatest interest. Following the process by which he formerly effected the direct combination of nitrogen with various organic compounds, he finds that argon is equally absorbed by these bodies, though apparently with somewhat less facility. The action of the silent discharge upon a mixture of argon and benzene vapor is accompanied by a feeble violet luminosity visible in the dark. In one of five experiments he found that a fluorescent substance was produced, which developed a magnificent greenish light and a peculiar spectrum. M. Berthelot took 100 volumes of Professor Ramsay's gas, added a drop or two of the hydrocarbon, and exposed the mixture to the silent discharge at moderate tension for about ten hours. The excess of benzene vapor being removed in the usual way, the mixture was found to have been reduced to 89 volumes. More benzene was then added, and the experiment was repeated with higher tension, which in three hours produced a reduction of volume equal to 25 per cent. On again submitting the gaseous residue with benzene to very high tension discharge he found the final result to be 32 volumes. Analysis showed this residue to contain only 17 volumes of argon, the other 15 volumes being hydrogen, free or combined, and benzene vapor. In other words, M. Berthelot has effected the combination of 83 per cent. of the argon under experiment, and was prevented only by the dimensions of his apparatus from carrying the condensation yet further.

The quantity at his disposal was too small to permit of complete examination of its products, but he is able to say that they resemble those produced when nitrogen mixed with benzene is submitted to the silent discharge. That is to say, they consist of a yellow resinous matter condensed on the surface of the glass tubes employed. This matter on being heated decomposes, forming volatile products and a carbonaceous residue. The volatile products restore the color of reddened litmus paper, proving the production of alkali by the decomposition, though the quantity of matter at command was too small to allow of its nature being demonstrated. In any case, M. Berthelot concludes, the conditions in which argon is condensed by hydrocarbons tend to assimilate it yet more closely with nitrogen.

He adds that if it were permitted to assume 42 instead of 40 as the molecular weight of argon—an assumption which the limits of error in the experiments hitherto made do not, in his opinion, exclude—this weight would represent one and a half times that of nitrogen; in other words, argon would stand to nitrogen in the same relation as ozone to oxygen. There is, however, the fundamental difference that argon and nitrogen are not transformable into one another, any more than the isomeric or polymeric metals. Without insisting upon points which are still conjectural, M. Berthelot observes that in any case he has demonstrated that the inactivity of argon disappears in the conditions he describes. When the gas can be obtained in considerable quantities, he says it will be easy by ordinary chemical methods to take these primary combinations, or their analogues obtainable with oxygen, hydrogen, or water, as a point of departure for the preparation of the normal series of more simple compounds.—*London Times*.

AT the anniversary meeting of the Chemical Society, Professor Ramsay stated that he had examined the gas (which according to an observation of Hillebrand's was nitrogen) given off by the mineral clevite when treated with sulphuric acid, and discovered that it contained argon. Spectroscopic examination showed a very bright yellow line nearly coincident with the yellow sodium line. This line was found to be identical in position with the yellow line observed in the spectrum of the sun's chromosphere, and attributed to the hypothetical element helium. Whether helium could be separated from argon remained to be seen. Mr. Crookes gave some additional particulars of the spectrum of the gas from clevite. He found certain coincidences with the band spectrum of nitrogen, particularly in the ultra violet region, but some lines were present which were not found in the nitrogen spectrum, and *vice versa*.

DR. B. BRAUNER, Professor of Chemistry in the Bohemian University, Prague, has written to *Nature*, suggesting that argon possibly exists in nebulæ. He points out that a strong argon line, measured by Mr. Crookes, has practically the same wavelength as the chief nebula line, and thinks that the line at λ 3729.8 in the 'blue' spectrum of the new substance represents the line at λ 3730, found in the spectra of nebulæ and white stars.

PALEONTOLOGY.

PROFESSOR H. J. SEELEY has recently published a paper in the *Philosophical Transactions* upon *The Reputed Mammals from the Karroo Formation of Cape Colony*, in which he reconsiders the evidence as to the mammalian nature of Theriodesmus and of Tritylodon. He established the former genus some years ago upon a fore-arm; the latter was established by Richard Owen in 1884, upon a skull. In his previous papers the author has described both of these types as mammalian, and the skull has invariably been placed with the mesozoic Monotremes, owing to the resemblances which its teeth present, both in the crown and in the multiple fangs, to other mammals of this very ancient and widespread group of multituberculates. Professor Seeley, in his renewed examination of the skull of Tritylodon, believes that he finds evidences of 'post-frontal' and 'pre-frontal,' and possibly of a 'transverse' bone, as in the Theriodont reptiles. This evidence he considers overweighs the distinctively mammalian characters of the teeth. If it is subsequently confirmed by more satisfactory material this will be another example of the independent development of what we have always considered distinctively mammalian characters within the reptilian class. Another remarkable species of an undoubted reptile is the *Diademodon tetragonus*, in which the single-fanged or reptilian molar teeth are capped with crowns which bear a most striking resemblance to a low-crowned quadritubercular mammalian molar. These discoveries in the Karroo Formation promise to yield most interesting and surprising results, although if the position here taken is correct, it is somewhat disappointing to have such a type as Tritylodon taken from the class mammalia. The evidence does not seem to be conclusive.

SIR WILLIAM DAWSON.

At the last regular monthly meeting of the Montreal Natural History Society (26th ult.), Sir J. William Dawson read a paper on the skeleton of a 'white whale' (*Beluga*), recently found in a brickyard off the Papineau Road, Montreal. The specimen, which was imbedded in the Leda clay, belongs to a species once abundant, and still not at all uncommon, in the lower St. Lawrence. Though it is now rarely known to ascend the river to fresh water, a stuffed specimen in the museum of the N. H. Society is said to have been caught near Montreal. The fossil was below the normal length, being about 12 feet.

Since his retirement from the principalship of McGill University, Sir William Dawson has turned his larger leisure to good account. Besides three important works issued from the press during the last two years, he has found time for special courses of lectures and an unfailing succession of papers on a wide range of subjects. Just forty years ago he entered on his task of building up McGill College. The status of the university when his supervision ceased, in 1893, is one of the things on which Canadian science may well congratulate itself.

J. T. C.

GENERAL.

The Niles bill incorporating the New York Zoölogical Society, and providing for the establishment of a zoölogical garden, has been passed by the Senate at Albany.

D. Appleton & Co. announce a *Criminology Series* edited by Mr. Douglas Morrison, the first volume of which, *The Female Offender*, by Professor C. Lombroso, will be issued this month.

The *Academische Revue* is a new journal edited by Dr. Paul Von Salvisberg and published by the International Hochschulwesen in Munich. In addition to original articles on educational interests it proposes to publish academic news, and the editor will be glad to have items of news sent to him.

The building used as a school of manual training by the New York Institution for the Instruction of the Deaf and Dumb, at One Hundred and Sixty-fifth Street and Fort Washington Avenue, was burned on April 8th, causing a loss of $40,000. The building stood about 400 feet from the main buildings of the institution.

At a meeting on March 28th, the Court of St. Andrew's University decided to found two medical chairs, the one of materia medica and the other of anatomy.

Macmillan & Co. have in press a translation, by Dr Charles R. Eastman, of Prof. Karl von Zittel's 'Elements of Paleontology.'

Dr. Thomas M. Drown, now Professor of Chemistry in the Massachusetts Institute of Technology, has been elected President of Lehigh University.

Luigi Ferri, Professor of Philosophy in the University of Rome, died recently at the age of 68.

Dr. G. Glogau, Professor of Philosophy in the University of Kiel, died recently in Greece at the age of 50.

Professors Erman, E. Schmidt and Stumpf, of the University of Berlin, have been elected members of the Prussian Academy of Sciences.

The British Government spent in 1894 £4,802 on the destruction of locusts in Cyprus. The methods used were the collection of eggs during the summer and winter and the purchase of live locusts by weight in spring.

The following lectures will be given before the Royal Institution, of London, after Easter: Professor George Forbes, three lectures on 'Alternating and Interrupted

Electric Currents'; Professor E. Ray Lankester, four lectures on 'Thirty Years' Progress in Biological Science'; Professor Dewar, four lectures on 'The Liquefaction of Gases'; Dr. William Huggins, three lectures on 'The Instruments and Methods of Spectroscopic Astronomy.'

SOCIETIES AND ACADEMIES.

ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA.

AT the meeting on March 26th Dr. M. V. Ball called attention to the microscopic preparation of the germ characteristic of erysipelas, the botanical name of which is *Streptococcus pyogenes.* The culture of the organism had been used with most gratifying success in the treatment of cancer, the cure of some cases having been reported, while others had been manifestly benefited. A subcutaneous injection of the culture raises the temperature to 104° in 20 minutes. This palliative effect of the poison of erysipelas had long been known, the improvement of cancer cases accidentally affected having been noticed years ago in hospitals.

Dr. S. G. Dixon spoke of the morphological resemblance between *Actinomyces*, or the ray fungus, and *Ægerita candida*, a white fungus, found growing on damp decaying wood. The former is believed to produce in cattle and man the disease known as lump jaw, or *Actinomyces.* Should the two fungi prove to be identical, the hitherto unknown cause of lump jaw in cattle would not only be explained, but cattle breeders would be enabled to prevent, to a great extent, the much-dreaded disease.

Mr. Henry C. Pilsbry exhibited fine specimens of the genus *Cerion*, and called special attention to the variations of the teeth or plates on the collumella, some of which extended far into the shell, while in other individuals they are quite superficial, the external characters, however, remaining the same. He believed the use of these folds was to enable the mollusk to keep a more firm grasp of the shell, and thus move it about more freely, as it hangs from twigs and leaves.

The geographical distribution of the species is peculiar. They inhabit Cuba, Hayti, the Bahamas and Florida Keys and reappear in Curacoa, off the northern coast of South America, but are completely absent from Jamaica and the Caribbean chain. There is, therefore, a wide gap between the northern and southern areas inhabited by the genus *Cerion*, although the islands in this space are apparently favorable to the existence of snails. A suite of specimens illustrating species of *Cerion* was exhibited.

EDW. J. NOLAN, *Recording Secretary.*

NEW YORK ACADEMY OF SCIENCES.

AT the meeting of the Section of Astronomy and Physics of the New York Academy of Sciences on April 1st Professor R. S. Woodward was elected chairman and William Hallock secretary for the following year.

President Rees gave a very interesting resumé of the work done in astronomy during 1894. This paper may appear in SCIENCE a little later.

President Rees then showed some of Professor Barnard's wonderful photographs of the Milky-Way, pointing out the evidences of the peculiar geometrical clustering of the stars in certain parts, as well as the 'dark lanes' and 'star streams' discovered by Barnard. He also showed photographs of several comets, especially Brooks', which went through such interesting changes. The photographs brought out most beautifully the unusual structure of the tail, and the sudden changes in shape, especially when it seemed to have encountered a resisting medium and apparently broke the tail near its middle.

The pictures were discussed and admired

by the members. Mr. C. A. Post admitted that his skepticism as to 'star streams' had been conquered, and argued that from the photograph it seemed more probable that Brooks' comet had run its head against the obstacle rather than its tail, as maintained by Professor Barnard.

WM. HALLOCK, *Sec'y of Section.*

THE TEXAS ACADEMY OF SCIENCE, APRIL 5.

Brief announcement of my recent discoveries in the mathematics of engineering: DR. G. B. HALSTED.

The storm-water storage system of irrigation: ROBERT A. THOMPSON.

Cometary Orbits as related to the solar system: CHARLES K. McDONALD.

Microscopic exhibition of slides sent by Dr. A. J. Smith on the organism which causes malarial fever: W. W. NORMAN.

SCIENTIFIC JOURNALS.

AMERICAN JOURNAL OF MATHEMATICS, APRIL.

A Method for Calculating Simultaneously all the Roots of an Equation: EMORY McCLINTOCK.

Sur le logarithme de la fonction gamma: CH. HERMITE.

Sur la pression dans les mileux diélectriques ou magnétiques: P. DUHEM.

On Ternary Substitution-Groups of Finite Order which leave a Triangular unchanged: H. MASCHKE.

PSYCHE, APRIL.

A Comparison of Colias hecla with Colias meadii and Colias elis: THOMAS E. BEAN.

Western Pediciœ, Bittacomorphœ and Trichoceræ: C. R. OSTEN SACKEN.

Failure to emerge of Actias luna: CAROLINE G. SOULE.

Entomological Notes.

JOURNAL OF GEOLOGY, FEB.–MARCH.

Sedimentary Measurement of Cretaceous Time: G. K. GILBERT.

Use of the Aneroid Barometer in Geological Surveying: C. W. ROLFE.

A Petrographical Sketch of Ægina and Methana: Part III. HENRY S. WASHINGTON.

On Clinton Conglomerates and Wave Marks in Ohio and Kentucky (Concluded): AUG. F. FOERSTE.

Glacial Studies in Greenland: T. C. CHAMBERLIN.

Editorials; Publications.

BULLETIN OF THE TORREY BOTANICAL CLUB. APRIL.

Biographical Sketch of Dr. J. Bernard Brinton (with portrait): By a Committee of the Philadelphia Botanical Club.

Food Plants of the North American Indians: V. HARVARD.

The Classification of the Archegoniates: LUCIEN M. UNDERWOOD.

Rules for Citation adopted by the Madison Botanical Congress and Section G., A. A. A. S.

Proceedings of the Club.

Index to Recent Literature Relating to American Botany

NEW BOOKS.

The Story of the Stars. G. F. CHAMBERS. New York, D. Appleton. 1895. Pp. 160.

Evolution and Effort. Edmond Kelly. New York, D. Appleton, & Co. 1885. Pp. vii+297. $1.25.

A Primer of Evolution. Edward Clodd. New York and London, Longmans, Green & Co. 1895. Pp. 186.

Repetitorum der Chemie. CARL ARNOLD. 6th Ed. Hamburg und Leipzig, Leopold Voss. 1894. Pp. x + 613. M. 6.

Anleitung zur Mikrochemischen Analyse. H. BEHRENS. Hamburg und Leipzig, Leopold Voss. 1895. Pp. xi + 224. M. 6.

Bildungselemente und Erziehlicher Wert des Unterrichts in der Chemie. RUDOLPH ARENDO. Hamburg und Leipzig, Leopold Voss. 1895. Pp. 103. M. 2.

Le Petrole. A. JACCARD. Paris, Felix Alcan. 1895. Pp. xii + 292.

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Subscriptions and advertisements should be sent to Science, 41 N. Queen St., Lancaster, Pa., or 41 East 49th St., New York.

NATIONAL ACADEMY OF SCIENCES.

The *National Academy of Sciences* met at Washington on April 16, 17, 18 and 19, Professor O. C. Marsh, president, in the chair. The following members were reported as present:

Professor C. Abbe, Gen. Henry L. Abbot, U. S. A., Professor Alexander Agassiz, Professor George F. Barker, Professor Carl Barus, Dr. John S. Billings, Professor H. P. Bowditch, Mr. Lewis Boss, Professor W. K. Brooks, General Thomas L. Casey, U. S. A., Professor Charles F. Chandler, Professor S. C. Chandler, General Cyrus B. Comstock, Professor E. D. Cope, Professor Russel H. Chittenden, Professor Theodore N. Gill, Professor Wolcott Gibbs, Mr. G. K. Gilbert, Professor G. Brown Goode, Professor Benjamin A. Gould, Professor Arnold Hague, Professor Asaph Hall, Professor Charles S. Hastings, Mr. George W. Hill, Professor O. C. Marsh, Professor T. C. Mendenhall, Dr. S. Weir Mitchell, Professor A. A. Michelson, Mr. Edward S. Morse, Professor Simon Newcomb, U. S. N., Professor Ira Remsen, Professor Henry A. Rowland, Professor Charles A. Schott, Professor John Trowbridge, General Francis A. Walker, Professor Charles A. White.

The papers entered to be read were as follows:

1. *On Some Variations in the Genus Encope:* A. Agassiz and W. McM. Woodworth.
2. *Notes on the Florida Reef:* A. Agassiz.
3. *The Progress of the Publications on the Expedition of 1891 of the U. S. Fish Commission Steamer 'Albatross,' Lieut. Commander Z. L. Tanner, commanding:* A. Agassiz.
4. *On Soil Bacteria:* M. P. Ravenel. (Introduced by J. S. Billings.)

5. *A. Linkage Showing the Laws of the Refraction of Light:* A. M. Mayer.
6. *On the Color Relations of the Atoms, Ions and Molecules:* M. Carey Lea.
7. *Mechanical Interpretations of the Variations of Latitude:* R. S. Woodward. (Introduced by S. C. Chandler.)
8. *On a New Determination of the Nutation-Constant, and some allied topics:* S. C. Chandler.
9. *On the Secular Motion of a Free Magnetic Needle:* L. A. Bauer. (Introduced by C. Abbe.)
10. *On the Composition of Expired Air, and Its Effect Upon Animal Life:* J. S. Billings.
11. *Systematic Catalogue of European Fishes:* Th. Gill.
12. *The Extinct Cetacea of North America:* E. D. Cope.
13. *On the Application of a Percentage Method in the Study of the Distribution of Oceanic Fishes.*
    A. Definition of Eleven Faunas and Two Sub-faunas of Deep Sea Fishes.
    B. The Relationships and Origin of the Carribeo-Mexican and Mediterranean Subfaunas: G. Brown Goode.
14. *On the Two Isomeric Chlorides of Orthosulpho-benzoic Acid:* Ira Remsen.
15. *On Some Compounds Containing two Halogen Atoms in Combination with Nitrogen:* Ira Remsen.
16. *Presentation of the Watson Medal to Mr. Seth C. Chandler, for his Researches on the Variation of Latitudes, on Variable Stars, and for his other works in Astronomy.*
17. *Biographical Memoir of Dr. Lewis M. Rutherfurd:* B. A. Gould.
18. *Relation of Jupiter's Orbit to the Mean Plane of Four Hundred and One Minor Planet Orbits:* H. A. Newton.
19. *Orbit of Miss Mitchell's Comet, 1847, VI:* H. A. Newton.

The officers elected were as follows: President, Prof. Wolcott Gibbs; Vice-president, Gen. F. A. Walker; Home Secretary, Prof. Asaph Hall; Foreign Secretary, Prof. A. Agassiz; Treasurer, Dr. John S. Billings; additional members of the Council, Prof. George J. Brush, Prof. George L. Goodale, Dr. B. A. Gould, Prof. O. C. Marsh, Prof. Simon Newcomb and Prof. Ira Remsen.

The new members elected were Prof. W. L. Elkin, professor of astronomy in Yale Observatory; Prof. C. S. Sargent, professor of botany in Harvard University; Dr. W. H. Welch, professor of pathology in Johns Hopkins University, and Prof. C. O. Whitman, professor of biology in the University of Chicago. The foreign associates elected were Prof. Rudolph Leuckart, professor of zoölogy in the University of Leipsic; Prof. Julius von Sachs, professor of botany in the University of Würtzburg, and Prof. Sophus Lie, professor of mathematics in the University of Leipsic.

The Barnard Medal was awarded to Lord Rayleigh for his discovery of argon, and the Watson Medal to Professor S. C. Chandler for his researches on the variation of latitude and other subjects.

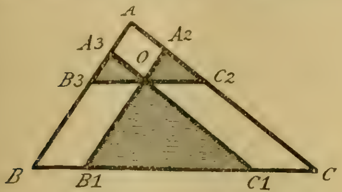
The autumn meeting of the Academy will be held at Philadelphia, beginning October 29.

ARTHUR CAYLEY.

How Professor Cayley touched everything mathematical, and touched nothing which he did not adorn, may be illustrated by the following unpublished letters, which were the first expression of discoveries that have since taken their permanent place in our best text-books. They are both the outcome of the sudden and fruitful interest in *linkage*, dating from Sylvester's interview with Tchébychev, when, leaving behind him the diagram of the now celebrated Peaucellier's Cell, the illustrious Russian gave in parting the characteristic advice: "Take to kinematics; it will repay you; it is more fecund than geometry; it adds a fourth dimension to space."

I will transcribe the letters exactly, not only because the recent death of Tchébychev, followed in less than two months by that of Cayley, gives them now a special pertinence, but because it is of interest to compare one with what is given on 'tram motion' in Kempe's 'How to Draw a Straight Line,' and the other with its reproduction by no less a master than Clifford on pages 149, 150 of his Dynamic, whence I add figure 2.

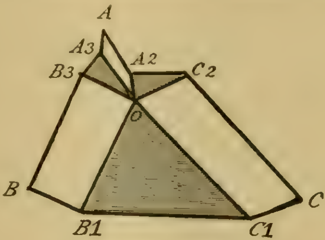
"Robert's theorem of 3-bar motion takes the following elegant form: Take a triangle



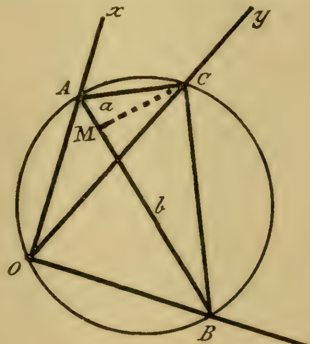
ABC and a point O and through O draw lines parallel to the sides as in the figure, the 3 shaded △'s are of course similar to ABC. Now imagine a linkage composed of the shaded △'s and the bars AA_2, AA_3, BB_3, BB_1, CC_1, CC_2 pivoted together at A, B, C, A_2, A_3, B_3, B_1, C_2, C_1, O; then, however, the figure is moved [of course A_3, B_3 do not continue in the line AB, etc.], the triangle ABC *will remain similar to the shaded triangles;* and if in any position of the figure we fix the points A, B, C, then the point O will be movable in a curve, viz.: we have the *same* curve described by O considered as the vertex of $OA_3\ B_3$, where the two radii are AA_3, BB_3—by O considered as the vertex of $OA_2\ C_2$, etc.—and by O considered as the vertex of $OB_1\ C_1$, etc."

CAMBRIDGE, Feb. 22, 1876.

"The porism is *very* pretty; it was new to me, though I think it ought not to have been so. Look at the theorem thus: Imagine a plane, two points thereof, A, C moving in fixed lines Ox, Oy. Describe the circle OAC, which consider as a circle fixed in the plane and movable with it. Then the theorem is that any point B of this circle moves in a line OB through O. In particular B may be the opposite extremity of the diameter through A, and we have



then the points A,B moving on the lines Ox and OB at right angles to each other, viz.: the general case of a plane moving two points thereof on two fixed lines is reduced to this well-known particular case. And the theorem comes to this, that dividing



the rod AB at pleasure into two parts AM, MB, and drawing MC at right angles, and a mean proportional, the locus of C is a right line through O, which is of course easily proved." Yours very sincerely,

A. CAYLEY.

CAMBRIDGE, May 5.

GEORGE BRUCE HALSTED.

AUSTIN, TEXAS, Feb. 15, 1895.

*THE PROTOLENUS FAUNA.**

The above article will be one of especial interest to students of the early Paleozoic faunas, since it describes one of the oldest known.

From time to time during the last thirty or forty years discoveries of fossils have been made in the Cambrian rocks of eastern Canada. Those of the St. Lawrence valley and northern Newfoundland were by Billings referred to the 'Lower Potsdam,' but at a later date, together with others found in that valley and in southern Newfoundland, they have been more specially correlated with the Olenellus Fauna by C. D. Walcott and others.

Other fossils found in the lower part of the Cambrian rocks in New Brunswick below the Paradoxides bed were naturally at first thought to be also of this fauna, but, as will be seen by considerations advanced further on, it does not now seem possible so to establish the relationship.

The discoveries in New Brunswick have from time to time been reported in articles published by G. F. Matthew in the Transactions of the Royal Society of Canada, but such important additions were disclosed through the collections made by W. D. Matthew in 1892 and 1893, and by him in conjunction with G. van Ingen for Columbia College, New York, in 1894, that a special article on this, the Protolenus fauna, has been written. From this article the following abstract has been made of the character of the fauna, and the conclusions arrived at from its study.

The fauna consists of Foraminifera, Sponges, Molluscs and Crustaceans. All the Foraminifera described are referred to the genera *Orbulina* and *Globigerina;* the sponges include *Protospongia* and others. The molluscs are mostly hyalithoid shells of the genera *Orthotheca*, *Hyolithus* and *Diplotheca*. A remarkable mollusc having a helicoid shell and supposed to be a Heteropod, enables me to establish a new genus. The Crustaceans are chiefly of two groups, Ostracoda and Trilobita, of which the former are remarkable for the large number of genera and species, as compared with the trilobites; two predominant and characteristic genera are *Hipponicharion* and *Beyrichona*. All the trilobites are of genera peculiar to this fauna, except *Ellipsocephalus*, which, although one of the dominating types, also occurs in the Paradoxides beds of Europe. The most characteristic genus or trilobites is *Protolenus*, which is abundantly present in the typical beds.

The following are some of the salient characters of the fauna as at present known. *All the trilobites have continuous eyelobes.* This is a decidedly primitive character, and its value in this respect is shown by the genus *Paradoxides* of the overlying fauna, which began with small species having such eyelobes, and culminated in the large forms of the upper Paradoxides beds in which the eye-lobe was considerably shortened. This shortening of the eyelobe was carried still further in the *Oleni* of the Upper Cambrian, dwarfed forms, with a general similarity to the *Paradoxides*, in which the eyelobe is almost on a line with the front of the glabella.

The important family of Ptychoparidæ is absent. This family did not have continuous eyelobes, for in the young, when this projecting fold first shows itself, it is short and at the lateral margin of the head-shield. No trilobite with such an eyelobe has been found in this fauna. The Ptychoparidæ had about a dozen species in the Olenellus Fauna, and became quite common in that with Paradoxides, and continued to abound throughout the Cambrian period.

The genus Conocoryphe is absent. This is specially a type of the Lower Paradoxides

*Abstract of a paper communicated to the New York Academy of Sciences by G. F. Matthew, of St. John, N. B.

beds and under, the name of *Conocoryphe trilineata* (*Atops trilineatus*), is claimed as a characteristic fossil of the Olenellus Zone.

The genus Microdiscus is absent. This trilobite is especially characteristic of the Olenellus Zone and continued to live with Paradoxides. Here it occurs in the Paradoxides Zone, but is absent from the Protolenus Fauna.

The genus Olenellus is absent. Though carefully looked for, no example of this genus has been found among the trilobites of the Protolenus Fauna, hence, though this fauna apparently holds the place where we might naturally expect to find *Olenellus*, that genus proves to be absent, or at least not at all characteristic; and, as so many of its associate genera also are absent, *we cannot regard this fauna* as the *Fauna of Olenellus*.

Of the genera of trilobites that are present *Micmacca* has affinity with *Zacanthoides*. It differs in the course of the posterior exterior of the dorsal suture. The relation will seem closer if we suppose a movement of the eyelobe during the growth of *Zacanthoides* similar to that which occurred in the Ptychoparidæ, by which the eyelobe was drawn in toward the glabella, while at the same time there was a projection of the posterior extension of the dorsal suture outward toward the general angle. If this change were shown to have occurred in *Zacanthoides, Micmacca* might be looked upon as an ancestral form of that genus.

In this fauna there is a very primitive assemblage of Brachiopods, of forms which it is in many cases difficult to assign to any known genus. Many are small, some are minute, and the larger species belong to the Obolidæ and Siphonotretidæ.

The Gasteropoda have already been alluded to; among these *Pelagiella* (n. gen.) is remarkable for the peculiar aperture which seems to indicate a free swimming Heteropod.

This fauna is distinguished from that of Olenellus by two marked features; it is more *primitive* and also more *pelagic*.

The way in which the trilobites are bound together by the single feature of a continuous eyelobe shows a unity of origin and a close relationship not found in any other fauna. And yet among these trilobites there are forms which in other respects are parallel to the types which developed in the later faunas; thus in *Protolenus* we have have the flat pleura with the diagonal furrow of *Paradoxides* and the deeply grooved, geniculate pleura of *Ptychoparia*, and at the same time the prominent glabella and deep dorsal furrows of *Solenopleura*. *Micmacca*, as has already been said predicated *Zacanthoides* of a later fauna, and *Protagraulos* in its almost obliterated glabella and flat cephalic shield closely resembles *Agraulos* of the Paradoxides Fauna.

It is a more *pelagic* fauna than that of Olenellus, for we notice the absence of many forms differentiated for shore-conditions. Trilobites with fixed outer cheeks, like *Olenellus* and *Microdiscus* are absent; calcareous corals and sponges are rare; thick-shelled brachiopods and the Orthidæ are wanting, or rare; no Lamellibranch is known, but Foraminifera are quite common in some of the beds.

The question of the antiquity of this fauna as compared with that of Olenellus is discussed. The facies of the fauna as above described indicates a greater antiquity, but if the two faunas were contemporaneous, that of Olenellus may have reached these shores first.

VOLCANIC DUST IN TEXAS.

SOMETIME since the writer was given, for examination by the microscope, a sample of a white, fine-grained silicious deposit by Prof. R. T. Hill, of the U. S. Geological Survey, who writes as follows concerning it:

"The material which I gave you was collected by an old Texas friend of mine, Mr. S. P. Ford, in December, 1893, who said that at first he supposed it was chalk, but had since come to the conclusion that it is something else. When I wrote to Mr. Ford that I thought it was volcanic glass, probably derived from some of the now extinct vents along the Rocky Mountain front, he expressed some doubt as to this mode of origin, and said :

"'This specimen was from a solid hill from thirty to forty feet high, composed entirely of this stuff. The point I make is that, on account of its thickness, the crater must have been somewhere very close, and if so, is it not something heretofore unknown in Texas? The exact locality is on Duck creek, in Dickens county, about 50 miles northwest of the Double Mountain.' (Dickens county is in northwestern Texas, in the Brazos River drainage.—Author.)

"This specimen undoubtedly comes from the post-Cretaceous formations constituting the great Llano Estacado. Perhaps you will remember that in 1886 I collected some similar material from near Wray, Colorado, and Hecla, Nebraska, which was described by Prof. Merrill of the National Museum, in the American Journal of Science. This Texas material seems very similar to that of the Colorada-Nebraska locality, both in appearance and in geological position. I wish that more was known of the stratigraphy of the Texas beds. The Colorado specimens occur in what is called the White River Tertiary."

An examination by the microscope shows that the white material is volcanic glass, in the angular and fluted forms figured by Merrill,* as characteristic of volcanic dust from Furnas county, in southern Nebraska. Diller † also describes and figures similar forms of glass particles from Norway, Krakatoa, Truckee River and Breakhart Hill, the latter a hill to the north of Boston, Mass. In the same article he describes volcanic dust from Unalashka, which fell in October, 1883, and discusses volcanic dusts in general. Professor Diller concludes that "so far as definite observations have been made, they warrant the general assertion, that with occasional exceptions, which can be readily explained, volanic dust contains a higher percentage of silica than the lava to which it belongs."

Professor Diller has also described some volcanic material from Knox county, Nebraska, and from the West Blue River, Seward county, Nebraska,* and estimated that about 90% was vocanic dust, there being also numerous rolled quartz grains.

* Proc. U. S. Nat. Mus. 1885, p. 100.

† Science, May 30, 1884.

The description of the material collected by Professor Hill from Wray (B. & L. R. R.), on the south side of the Republican River, occurs in an interesting article by Professor Merrill, 'On the Composition of Certain Pliocene Sandstones from Montana and Idaho.'†

Three figures are given showing the shape of the particles of volcanic glass found in the sandstones. In the material from the Devil's Pathway (No. 35893 [a]) "there are many disc-like bodies on the glass particles, colorless and nearly circular in outline," but the other figures show angular and fluted forms like those above referred to. Merrill gives analyses of three samples of the volcanic dust from Montana and Idaho, and concludes that they are of andesitic or tractytic origin. His analyses include lime and alkali determinations, and the silica contents range from 67.76% to 68.92%.

Merrill also states that some volcanic dust from Krakatoa fell on a ship 885 miles from the source of volcanic activity, so that the existence of a layer of volcanic dust at a given point may not indicate the proximity of the volcano from which the material came, but a deposit forty or more feet thick would hardly form at a great distance from the source.

The volcanic dust obtained by the writer from a layer in the Neocene Lake beds that underlie Mohawk Valley, in Plumas county, California, likewise resembles in the shape of its particles the dusts figured by Diller and Merrill. An analysis of this material by Dr. W. H. Melville showed that it contained 70.64% of silica, and it was there-

* See article by J. E. Todd, Science, Vol. VII., p. 373.

† Am. Jour. Sci., Vol. XXXII., pp. 199–204.

fore presumed to be a rhyolitic glass.* The material obtained by Professor H : 11 closely resembles the Mohawk Valley material. The Texas occurrence is of unusual interest, being in a region where evidences of the former existence of volcanoes are rare.

H. W. TURNER.

WASHINGTON.

CURRENT NOTES ON ANTHROPOLOGY (VI.).

THE CAUCASIC LINGUISTIC STOCK.

COL. R. VON ERCKERT, of the Russian army, already known for an excellent work on the ethnography of the Caucasus, has just published an epoch-making volume on the languages of that region (Die Sprachen des Kaukasischen Stammes, Vienna, 1895). In this he solves the intricate problem which has so long puzzled linguists as to the relationship and place of these tongues. He demonstrates by satisfactory evidence, structural and lexicographical, that these numerous languages and dialects, some thirty in number (the Ossetic, which is Aryan, being of course excluded), belong to one family, which should be called the 'Caucasic.' It is divided in three groups, the Georgian, the Circassian and the Lesghian. The stock stands wholly independent, all similarities to either Ural-Altaic or Indo-European proving accidental or unimportant. Which of the groups is nearest the ancient original tongue he does not pretend to decide; but he offers striking testimony to the persistence of the traits of these languages. The Georgian was written as early as the ninth century A. D., and he gives a letter composed by a bishop in 918. It is quite identical, both in syntax and words, with the current tongue of to-day.

All these facts are the more to the purpose since so much has been made of late years by Professors Sayce, Hommell and their followers, of what they call the 'Alarodian' linguistic stock (*i. e.*, the Georgian), in connection with the pretended 'Sumerian' of lower Babylonia. It is likely that they will have to 'back water,' now that comparisons can really be made.

CUNEIFORM INSCRIPTIONS.

DR. HUGO WINCKLER, in his 'History of Babylonia and Assyria,' tells us that the cuneiform method of writing was in use among eight nations speaking entirely different languages. Whether this is quite accurate or not, we need not stop to consider, as there can be no question that it had a much wider distribution than used to be supposed. Last year the well-known French archæologist, M. E. Chantre, unearthed specimens of it at Pterium and Cæsarea, in Asia Minor, as far west, perhaps, as such inscriptions have been found in place. The excavations continued by the University of Pennsylvania at Niffer have proved rich in finds of tablets. But the champion recent discoveries appear to be those of M. de Sarzec at Tello. A brief account of his eighth campaign in that rich locality appears in the 'Révue Archaéologique' of December last, extracted from the official report of M. S. Reinach. From it we learn that M. de Sarzec opened a small mound some hundreds of yards from that which he had previously worked, and chanced upon the very archives of the old city themselves. They were inscribed on tablets and neatly stored in trenches, where they had rested undisturbed these thousands of years. From these deposits he took out more than *thirty thousand* tablets, about five thousand in perfect condition, another five thousand very slightly injured, and the others more or less defaced. This magnificent discovery will have the greatest importance in revealing the history and character of the ancient Babylonian civilization.

* Bull. Phil. Soc. Washington, Vol. XI., p. 389.

THE ORIGIN OF NATIVE AMERICAN CULTURE.

Among the Americanists of Europe, Dr. Eduard Seler easily ranks in the first class. He is lecturer on American archæology in the University of Berlin, and his numerous writing are of the most solid merit. Two recent articles by him are significant. One in 'Globus' (Vol. 65, No. 20), entitled 'Where was Aztlan?' was inspired by Mr. Wickersham's aticle in 'Science,' December 8, 1893, in which that writer endeavored to discover 'Asiatic analogies' between the Aztecs, the Puget Sound Indians and various Asian tribes. Seler's second article is broader. It is entitled 'On the Origin of the Ancient Civilization of America,' and appears in the Preussische Jahrbücher (Vol. 79, 1895).

In these able and pointed papers he sums up with masterly force the arguments which prove that the culture of ancient America in all its details was indigenous, starting at various centers independently, and in no item or shred derived from instructors from across the ocean or across Bering Straits. 'American science,' he pertinently says, 'can only win by giving up once for all the vain attempts to construct imaginary connections between the cultures of the old and new continents,' and he points out clearly that this independence of historic connection is what lends to American archæology its greatest importance.

In singular and sad contrast to these truly scientific views are the efforts of a local school of American students to rehabilitate the time-worn hypotheses of Asiatic and Polynesian influences in the native cultures of our continent. The present leader of this misdirected tendency is Professor O. T. Mason, whose articles in the 'International Archives of Ethnography' and in the 'American Anthropologist,' bearing on this question do the utmost credit to his extensive learning and the skill with which he can bring it to bear in a lost cause. His latest, entitled 'Similarities of Culture' (Amer. Anthrop. April, 1895), is so excellent an effort that it is all the more painful to see its true intent is to bolster up a moribund chimera. It is to be hoped that they will not influence the younger workers in the field to waste their energies in pursuing these will-o'-the-wisps of science which will only lead them to bootless quests.

ARCHÆOLOGICAL NEWS FROM SWITZERLAND.

Two or three years ago the curious discovery was made in Switzerland that at one time, during the neolithic period, a dwarf race, true pygmies, flourished in Europe. The bones of a number of them were unearthed at Schweizersbild, near Schaffhausen, in connection with polished stone implements and pottery. The average height of the adults was about 140 centimeters, close to that of the Bushmen. They apparently lived along with other tribes of ordinary stature, as the remains of both were found together. The cubical capacity of the skull was about 1200 c.c. Several anatomists have given the skeletons close attention, notably Professor J. Kollman, of Basel, in the 'Verhandlungen der Anatomischen Gesellschaft,' May, 1894, who appends to hispaper a bibliography of articles relating tothe find.

The abundant richness of Switzerland as an archæological field is strikingly shown by an archæological map of the canton Zurich, prepared by Dr. J. Heierli, and just published in the city of the name. It is very neatly printed in colors, showing by the tint the relative age of the station, whether neolithic, Roman, Allemannian, etc. The author has added a pamphlet of explanations and an index, so as to familiarize students with the local sites and what they signify. It is heartily to be wished that some State of our country would follow this excellent example and thus lead to a more intelligent comprehension and a better preservation of the antiquities on our soil.

SOUTH AMERICAN TRIBES AND LANGUAGES.

IN the February number of the Journal of the Anthropological Institute, Mr. Clements R. Markham, republishes his 'List of Tribes in the valley of the Amazon,' which first appeared about twenty years ago. Of course there are many improvements in the enumeration; but it is amazing to note that by far the best recent authorities are not referred to, and their material is ignored. In the 'list of authorities' there is no mention, for instance, of the names of Von Den Steinen, Ehrenreich or Barbosa Rodriguez. For the linguistics he quotes Dr. Latham as still the authority. In fact, the best work done in Amazonian ethnography within the last decade is not mentioned nor utilized.

Some interesting studies in the languages of the Argentine Republic should not be overlooked. The Allentiac was a language, now extinct, spoken in the vicinity of San Juan de la Frontera. A little catechism, grammar and vocabulary of it was printed by Father Louis de Valdivia in 1607, of which only one perfect copy is known. This has been edited with a useful introduction by José T. Medina (Sevilla, 1894), and has been made the subject of a neat study by General Bartolome Mitré (Estudio Bibliografico linguistico de las Obras de Valdivia, La Plata, 1894; pp. 153). He inclines to consider it a separate stock.

The well-known Argentine linguist, Samuel A. Lafone Quevedo, has added another to the list of his valuable monographs by a thorough study of the mysterious Lule language (Los Lules; Estudio Filologico, Buenos Aires, 1894, pp. 145). It is based, of course, on the grammar of Machoni, and reaches the conclusion that the modern are not the ancient Lules, and Machoni's grammar is that of a tongue which belongs with the Quichuan group, and not among those of the Gran Chaco.

D. G. BRINTON.

UNIVERSITY OF PENNSYLVANIA.

CORRESPONDENCE.

A LARGE REFLECTOR FOR THE LICK OBSERVATORY.

MR. EDWARD CROSSLEY, F. R. A. S., of Halifax, England, has offered to present his 3-foot reflecting telescope to the Lick Observatory with its apparatus and dome, complete. The grateful thanks of the Observatory are returned for this generous and highly appreciated gift.

EDWARD S. HOLDEN.

MOUNT HAMILTON, April 4, 1895.

SCIENTIFIC LITERATURE.

Alternating Generations. A Biological Study of Oak Galls and Gall Flies. By HERMAN ADLER, M. D. Schleswig. Translated and edited by CHARLES R. STRATON. New York, Macmillan & Co.

The recent appearance, from the Clarendon press, of an edition of Dr. Herman Adler's celebrated work, which was published some fourteen years ago, on alternating generations among the Cinipidæ, being a biological study of oak galls and gall-flies, will be welcomed by all interested in the subject, especially by those who do not read German or French. The English translation is by Charles R. Straton. The work consists of: (1) an introduction by the editor; (2) the translation proper, to which the editor has added, in brackets and in smaller type, the popular English name of the gall, the particular oak upon which it is found, and a list of the inquilines and parasites that have been reared from each species; (3) as Appendix I., by the editor, a full account of *Cynips kollari* Hartig; (4) as Appendix II., a synoptical table of oak galls; (5) as Appendix III., a classification of the Cynipidæ, and (6) a bibliography.

The synoptical table of oak-galls (Cynipidæ alone included) is based on European species; while the classification includes not only European but a certain number of the older American species, but it is very imperfect in taking no note of the many later described American species, especially those described by Ashmead and Gillette. The classification is based on Mayr's, as was that given in Lichtenstein's translation of 1881, and comparatively few additional species are included.

The introduction is very full and includes a discussion of heredity and a rather full summary of late embryologic work, with a view of getting a clearer conception of the philosophy of alternation in generations. Mr. Straton particularly discusses Weismann's views, but by no means accepts them, though a thorough believer himself in natural selection.

Straton points out "that galls may be arranged in groups of greatly increasing complexity and that they must have arisen by gradual and complete improvements in the initial stages of their formation, acting through natural selection over an unlimited period of time and through numerous consecutive species." Each infinitesimal improvement in the gall itself, internally or externally, which has been of service as a protection against parasites or as favoring the development of the larva, has been preserved. In this view of the case, which is one that certainly seems most reasonable, the various characteristics of galls, such as spines, prickles, glutinous secretions, induration, and even size and coloration, are all acquired characteristics for the protection of the larva within. This theory is certainly justified in a large number of cases, but is equally at fault in many others. It would be hard to conceive that the bright colors which many galls assume in an early stage of development or the succulent character and pleasantly sub-acid or fruity flavor of others which renders them so prone to be invaded and preyed upon by a host of other insects could have any relation to the benefits of the gall-maker within. Here, as in most other natural history phenomena, natural selection can hardly be considered an all-sufficient explanation. Likewise, the assumed protective colors which galls often take on in autumn will find more valid explanation in the same causes which produce the similar changes in the leaves themselves, which can have no reference to the welfare of the plant.

No subject connected with galls has perhaps been more written about than the inciting cause of their formation. Adler and Byerinck effectually disproved the older belief that the exciting poison was inserted by the parent in the act of oviposition, *i. e.*, that the initial force was due either to a chemical secretion injected by the gall-mother or to the mechanical stimulus of traumatic irritation. A fluid is secreted in the act of oviposition, but it is absolutely unirritating and acts primarily as a lubricant to facilitate the arduous mechanical act and probably also as a mild antiseptic dressing to the wound made in the plant. Nevertheless there is an irritating salivary secretion produced by the larva itself and the gall growth is co-incident with the hatching and feeding of this larva. The fact that the influence on the plant tissues sometimes begins before the egg-shell is ruptured indicates that this fluid possesses amylolytic and proteolytic ferments. That the influence should be slightly exerted prenatally is not to be wondered at when we consider the delicate nature of the egg covering which often makes it difficult to observe the dividing line between the egg and newly hatched larva.

While, therefore, it is the larva in the Cynipidæ which causes the gall, this is not the case with the many other gall-producing insects, since many of the gall-gnats (Cecidomyidæ) and most, if not all, of the gall-making saw-flies (Tenthredinidæ) secrete a poison in the plant tissue in the act of oviposition, causing the gall to form before the larva hatches. One must, therefore, in reading Straton's Introduction, bear in mind that he is treating solely of the Cynipidæ. Adler himself recognizes the fact, so far as the *Tenthredinidæ* are concerned, from observations on *Nematus vallisnerii*, which produces a gall on *Salix amigdalina;* but in sweepingly denying it for the gall-gnats (p. 100), on the score that they have no piercing apparatus, he makes one of those generalizations which the facts do not justify, as most of the gall-making species have a very effective and specialized piercing ovipositor. This is, of course, not homologically comparable to that of the Hymenoptera, but is no more exceptional than is the wonderful piercing apparatus of Pronuba among Lepidoptera, being, like this last, a modification of the tubular tip of the abdomen and of the chitinous rods connected therewith.

Adler shows very conclusively that, in spite of the great variation in form, size, appearance and manner of formation, or whether they grow from bud, blossom, leaf, bark or root, galls spring invariably from the zone of formative cells or the cambium ring, just as indeed does the whole life of the plant. These cells are the theatre of actual metabolism. They are not differentiated into stable tissue, but await a period of developmental activity and possess the very conditions essential to gall formation. This explains the fact that Cynipid galls formed from punctures in the leaf almost always begin on the under surface of the leaf, since the cells of the upper surface have become stable and do not respond to any irritation applied to them; while when the eggs are laid in a dormant bud containing rudimentary leaves consisting of unmodified cells, both surfaces may take part in gall formation, the resulting gall, in such case, growing through the leaf substance. Again, when the egg is laid in the cambium ring of the bark, there is a sharp zonal contrast in the resulting gall between the soft and sappy parenchymatous cells and a harder central zone of wood parenchyma corresponding to the bast and to the wood parenchyma, the softer parts of the gall projecting from the bark while its woody base penetrates into the woody tissue.

From the above facts we come to understand why from winter buds, *i. e.*, where eggs are laid during winter in a bud that is dormant, only bud galls are produced, while from buds pierced in spring, when metabolism has begun, we get leaf-galls. Moreover, it has been proved by Adler, and explains the many failures in the efforts to obtain gall growths by confining gall-flies upon the plants, that if the parent fly fails to reach the formative zone of cambium cells the larva on hatching perishes without forming a gall. Another interesting fact which the writer has observed is that where but one bud-gall is usually produced several eggs are nevertheless inserted in the bud by the parent, a prodigality not uncommon in insects under similar circumstances, and which has some profound significances which we cannot discuss in this connection.

On the question as to what determines the ultimate growth of each particular gall so characteristic of its species Adler ventures no theory or explanation; but all the facts would indicate that it depends on the specific quality of the larval secretion, each having its distinct form of morbid poison working in the same pathologic way as the virus of the various eruptive diseases of man. Bacteriology may, in fact, yet come to our aid in this connection, as it has in the study of the pathologic manifestations of higher animals.

The process of oviposition in the Cynipidæ is a very elaborate one and has been much written about. Adler gives a most full and elaborate description of the mechanism of the ovipositor, and particularly of the ventral plates and bundles of muscles by which the terebra is worked. The structure of the ovipositor is well known and its parts homologize with those of the same organ in all Hymenoptera. It consists of a large bristle or seta, and of two spiculæ which mortise into it by means of two tenons and form the channel down which the egg passes. The seta occupies half the area of a transverse section of the terebra, and the two spiculæ occupy the other half. The seta has a central canal which contains an air vessel, a nerve branch and some sanguineous fluid. While appearing like a single piece, it is in reality double or composed of two parts which, indeed, are separated at the extreme base, but otherwise firmly soldered together. The spiculæ are serrate or notched near the tip, and the seta often ends in a slight hook. The two spiculæ play by means of strong basal muscles, longitudinally up and down on the tenons of the seta.

The eggs of Cynipidæ are characterized by having a stalk or pedicel of varying length according to the species, the egg-body proper, according to Adler, being at the apical or anterior end which first issues from the body, and the posterior end being also somewhat enlarged or spatulate. In repose the ovipositor is concealed within two sheaths, but in oviposition, according to Hartig's views, the spiculæ grasp the egg-stalk and push it to the tip, the fluids in the egg-body being pressed back in the operation, so that they come to be distributed along the stalk or to lie at the opposite or posterior pole of the stalk. The spiculæ then slightly separate at the tip from the seta and extend beyond it so that the apical end of the stalk becomes free. Now by pressure the fluid at the posterior end passes back through the stalk into the opposite or apical end which is plunged in the plant, the basal portion becoming emptied, the swollen apical end thus remaining in the plant when the ovipositor is withdrawn, filling the distal end of the puncture, which is somewhat enlarged. The empty basal sack of the egg and a portion of the stalk are often left exposed, looking not unlike the empty egg of some lace-wing fly (Hemerobiid).

In short, Hartig's view, very generally adopted, was that the extensile and ductile egg was driven through the ovipositor itself while this was in the plant, and that the contents of the egg-body were pressed back into the egg-stalk or pedicel during the operation and collected in the posterior end, and only after the apical end had reached the bottom of the puncture did these contents stream back into it. Adler would refute this view and draws attention to his own figures on Plate 3, where the eggs and ovipositor are illustrated side by side, all taken from photographs and drawn from the same amplification. These show that the ovipositor is, in every case, longer than the egg itself, the enlarged head of the egg corresponding in direction to the tip of the ovipositor. He argues from this fact that one end of the egg cannot be in the plant tissue while the other is in the canal. He further argues that it is not possible that the whole egg can be received into the ovipositor and glide through it in the way in which Hartig supposed. The operation of oviposition according to his observations consists of three distinct stages: (1) The canal in the plant is first bored, after which the fly rests; (2) the egg is then passed from the ovarium to the entrance or base of the ovipositor, *the anterior swollen end or egg-body hanging out*, since it is too large to be passed down the channel. It is then pushed along by means of the egg-stalk behind being grasped between the two spiculæ. (3) Finally, when the egg-body reaches the perforation, the ovipositor is partially withdrawn and the whole egg is then pushed in till the egg-body reaches the bottom of the puncture. Adler rightly expresses wonder that this complex procedure should be repeated so often with such great accuracy, and proceeds to describe the tactile hairs connected with the ovipositor which permit the fly to carry out the operation. He further states that, while oviposition in the surface of leaves is in its nature easier, the mechanism of oviposition is exactly the same as in buds.

We thus have two diametrically opposed views as to how the Cynipid egg passes down the ovipositor, the oviduct or passage of which is but one-fourth as wide as the egg-body itself, and into the puncture prepared for it. Hartig gave a perfectly simple explanation, and one generally accepted. While it is difficult to understand how the egg can be pushed into the puncture with the swollen egg-body entering first, yet Adler goes into elaborate details and is so careful that one is scarcely justified in questioning his conclusions. There is, however, good reason for doubting their accuracy as applied to all species and for believing that the method described by Hartig does also obtain and that there are even further modifications of the process.

In controverting Hartig and referring to his figures of eggs and ovipositors, Adler gives no indication whether the eggs were taken from the buds after being deposited, or from the ovaries or from the ovipositor, and my own experience with these and other ductile and extensile eggs with long egg-stalks would indicate a very varying length of stalk according to these varying circumstances. Again, he evidently has misjudged Hartig in assuming that the latter describes the passing of the egg down the minute channel of the seta, for Hartig's figures, as well as his description, make it clear that he had in mind the actual facts, viz., the passage of the egg down the channel formed by the connection of the two spiculæ with the seta. He is quite clear on this point and refers to the seta as the egg-guide (Eileiter) and not as the oviduct. He also elaborately describes and figures the eggs in the ovaries, with the swollen egg-body away from and the stalk directed to the base of the ovipositor.

My own studies of the oviposition of *Callirhytis clavula* O. S. in the buds of *Quercus alba* in April show that the eggs are inserted by the egg-stalk into the substance of the leaf, and that the fluids are first gathered in the posterior end which is not inserted. The fluids are then gradually absorbed from this exposed portion into the inserted portion of the egg and by the time the young leaves have formed the exposed shells are empty, the thread-like stalk has disappeared and the egg-contents are all contained within the leaf tissue. The larva now hatches and young galls rapidly form, the colorless and shriveled egg-shell being still often exposed in position and generally some distance from the position of the larva, a difference doubtless representing the original length of the inserted egg-stalk.*

These observations certainly comport more with the conclusions of Hartig than of Adler, though they indicate a quite different

* This agamic gall-fly produces a hemispherical gall involving both sides of the leaf, the cells in the center being connected by loose spongy fiber, and from it comes the sexual species, *Callirhytis futilis* O. S. This in turn produces the twig gall from which the agamic *C. q-clavula* is derived. Mr. H. F. Bassett (*Psyche*, Vol. 5, pp. 235–8, December, 1889) has connected *Callirhytis futilis* O. S. with a new species which he there describes as *Callirhytis radicis*, reared from a gall which is, practically, a blister-like swelling of the root. There is here either an error as to determination or else we have another interesting discovery in connection with these insects, viz., that the same species may indifferently produce a gall on the root or on the twig. When we remember how readily nature in many cases will convert a root into a twig, and *vice versa* this last explanation will not appear so improbable. I may add that Mr. Ashmead, who has reared the fly from the *clavula* gall, has carefully compared it with those actually observed ovipositing in the buds and agrees with me that they are identical.

method of oviposition from that described by either, in that the fluid egg-contents are not passed from one pole to another rapidly in the act of oviposition as described by Hartig, but very gradually, the process not being completed till just before the hatching. I had the assistance of Mr. Th. Pergande in carefully watching the steps in this particular case (in April 1884) and have put them on record here for the first time. Again, a small black wingless species (*Biorhiza nigra* Fitch, subsequently described as *B. politus* by Bassett), is not infrequently found during winter under the shelter of bark scales and oviposits during late winter in the terminal buds of *Quercus alba* and *Q. obtusiloba*. The ovipositor in this case, as in most cases where eggs are laid in dormant buds, is thrust down between the bud-scales until it reaches the soft latent cell tissue toward the center of the bud. And here it is easy to observe, by removing the scaly coverings, as I have done, that the pedicel or stalk only is inserted in the embryo leaf-tissue and that the enlarged portion or egg-body is at first external, being pressed and somewhat flattened by the surrounding leaf-scales.*

In still a third case of a small black inquiline (*Ceroptus politus* Ashm.) oviposition was observed by Mr. Pergande in the mid-rib of *Quercus rubra*, May 20, 1894 ,and in this case, as my notes show, the egg is thrust down into the puncture made by the terebra in the mid-rib until not a vestige of the egg is visible, the pedicel being very short.

There is, therefore, good reason for believing that oviposition in these insects follows no uniform system, and there is a serious question whether Adler's rejection of Hartig's views are justified. In connection with Adler's views as to oviposition, he concludes from his own studies that the main purpose of the egg-stalk is to supply oxygen to the egg-body in the plant-tissues, but that this is also an erroneous conclusion is, I think, made manifest by some of the facts just stated. That the function of the egg-stalk is, rather, to facilitate the otherwise difficult mechanical operation of the passage of the egg down a narrow and elongate ovipositor in the manner indicated by Hartig is supported by the fact that the puncture is often closed at its mouth as also from what we know of the similar oviposition in other orders of insects. The facts, for instance, connected with the oviposition of *Pronuba yuccasella*, where the egg is thrust deep into the ovarian cavity of the Yucca pistil bear out this view. The egg, in this case, as it passes down the ovarium has not a definite pedicel or stalk, but becomes a mere thread in passing through the ovipositor (the nature of which precludes any external outlet during the passage), and the fluids gradually concentrate in the apical or anterior end as the embryo develops. Moreover, it is passed into the ovarian cavity and has no connection through the pedicel with the exterior wound which is closed long before the larva hatches.*

The great service which Adler rendered in the study of the gall-flies was, however, to establish the fact of alternate generation in so many cases. He thus proved the existence of alternate generation in the following species : (See opposite page.)

The writer established, by breeding, the connection of the agamic *Callirhytis operator* O. S. and *C. operatola* Riley in 1872, the facts and specimens having been communicated to

*This fly produces an undescribed vesicular bud-gall from which issues a small black winged bisexual species (*Dryophanta vesiculoides* M S. mihi). The gall produced by this and from which the apterous agamic generation comes is not yet known, though it will probably be a leaf-gall similar to that of *Acraspis erinaceæ* Walsh.

* *Vide* the Yucca Moth and Yucca Pollination, by Charles V. Riley (from the Third Annual Report of the Missouri Botanical Garden). Issued May 28, 1892.

No.	Parthenogenetic Generation.	Flies Emerge.	Sexual Generation,	Flies Emerge.
1.	Neuroterus lenticularis	April	Spathegaster baccarum	June
2.	" læviusculus	March / April	" albipes	June
3.	" numismantis	April	" vesicatrix	June
4.	" fumipennis	May	" tricolor	July
5.	Aphilotrix radicis	April / May	Andricus noduli	August
6.	" Sieboldi	April / May	" testaceipes	August
7.	" corticis	April / May	" gemmatus	July / August
8.	" globuli	April	" inflator	June / July
9.	" collaris	April	" curvator	June
10.	" fecundatrix	April	" pilosus	June
11.	" callidoma	April	" cirratus	June
12.	" Malpighii	April	" nudus	June
13.	" autumnalis	April	" ramuli	July
14.	Dryophanta scutellaris	Jan. / Feb.	Spathegaster Taschenbergi	May / June
15.	" longiventris	Nov.	" similis	May / June
16.	" divisa	Oct. / Nov.	" verrucosus	May / June
17.	Biorhiza aptera	Dec. / Jan.	Teras terminalis	July
18.	" renum	Dec. / Jan.	Trigonaspis crustalis	May / June
19.	Neuroterus ostreus*	Nov. / March	Spathegaster aprilinus	May / June

H. F. Bassett July 10th of that year, though not published till 1873. The synoptical table by Straton does not add to the list as originally published by Adler. The subsequent discoveries have not been many, it is true,† but their inclusion would have increased its value. The facts incidentally recorded in this review add two other American cases to the list, though the alternate gall in one instance has not yet been discovered. It is not difficult to observe these gall-flies in the act of oviposition and to follow up the investigation until the resulting gall is produced, and there is a wide and most interesting field of inquiry which offers rich results for any American biologist who has the time to take it up seriously. The coupling of the alternate galls with each other is, however, more difficult, by direct observation, and is to be arrived at rather from careful identification of the flies in connection with the galls they have been reared from. Even in an epoch-making work like Adler's, the conclusions respecting some of the most interesting problems connected with the economy of galls and gall-flies may yet be questioned, as indicated in this review, and there is unlimited opportunity for careful and conscientious direct observation in a field where experience shows that analogy and sweeping generalizations are often misleading.

C. V. Riley.

Washington.

*Franz Löw (Verh. Zool.-Bot. Gesellsh. in Wien, XXXIV., 1885, p. 324) has given good reasons for believing that there was an error here, and that the agamic form of *Neuroterus áprilinus* Gir. is *Neuroterus Schlechtendali* Mayr. It should also be noted that Spathegaster is synonymous with Neuroterus.

†I now only recall, besides those already mentioned in this notice, *Chilaspis nitida* Ger. as the agamic form of *C. löwii* Wachtl., and *Dryophanta cornifex* Hart., as the agamic form of *Syntomaspis lazulina* Först..

A Manual of Topographic Methods. By HENRY GANNETT, Chief Topographer U. S. Geological Survey. Washington, Government Printing office. Quarto, xiv+300pp. 18 plates.

Whatever may be thought of the advisability of the publication of scientific manuals or text-books by the government, there is probably little question but that a bureau is justified in issuing volumes or bulletins which are in the nature of instructions to its officers and employees. Some publications of this kind, issued as parts of the reports of scientific bureaus, have been of great value to surveyors and engineers on account of the new facts and methods that they contain. The preface of this work states that it was primarily prepared for the information of employees, and furthermore that it 'describes the stage of development reached at present.' Hence it should presumably be of interest and value to all topographers who are acquainted with the excellent maps issued by the Geological Survey. Of the eighteen plates in the volume twelve give beautiful illustrations of types of topography, and these form its most useful and attractive feature.

The 300 pages of the manual include 130 pages of text, 168 pages of tables and 2 pages of index. Although the form is quarto, the size of the printed page is only $5\frac{1}{4}$ x $7\frac{1}{2}$ inches, and being in large type it includes but little more matter than a common octavo page. Chapter I. devotes 14 pages to historical and general information, chapter II. has 26 pages on astronomical determinations, and chapter V. is an interesting geological essay of 25 pages on the origin of topographic features. Thus only 65 pages remain for the discussion of methods of topography, a space entirely inadequate to do justice to the subject.

On base line measurements with the steel tape the corrections due to inclination, temperature and elevation above sea level are explained, but nothing is said about the sag of the tape, which as well known always makes the recorded distance too long, and the effect of varying intensity of pull is also unnoticed. The subject of primary triangulation is presented more fully than any other topic, the general methods of the Coast and Geodetic Survey being adopted, with somewhat different but excellent instructions for measuring angles. No statement as to the allowable probable errors of angular measurements is made, and the remark that the average length of lines in primary triangulation is 12 or 16 miles, leaves a confused idea as to what class of work is really under discussion.

On topography proper 5 pages are devoted to the plane table, 3 to traverses, $1\frac{1}{2}$ to stadia measurements and 9 to barometers. It is difficult to ascertain from these the details of the methods recommended or used, and it is safe to say that the excellent maps now being issued by the Geological Survey were not made without the application of principles and methods of which this volume gives no adequate explanation. It abounds, however, in useful generalities, such as "Stations for sketching should be selected with the utmost freedom;" "Under certain circumstances it is found advisable to use the stadia method for measuring distances instead of the wheel;" "Constant communication must be had between the chief of party and his assistants," etc.

The main feature of a small-scale topographic map is, of course, the contours. In chapter IV. references to the determination of heights by the barometer and stadia are made, but no forms of field notes are given, and the fact that these heights are to be used for locating contours is scarcely mentioned. In chapter V., however, one page is devoted to the subject, the essence of which is that contours are sketched in the field by the chief of party. It is stated that this 'is artistic work,' that "it is impossible that any map can be an accurate, faithful picture of the country it represents," that the topographer must be able to generalize through his knowledge of geological processes of origin, and that he should be able to decide, "where details are omitted, what to put in their places in order to bring out the dominant features." These are dangerous doctrines. The earth exists, the duty of the topographer is to map it truly, and the study of the origin of its features should come later. It is not a function of the surveyor to interpret nature, and the geologic discussions of Chapter V. seem out of their proper place in a manual of topography.

The book does good service in dwelling upon the important idea that a topographic survey must necessarily be based upon a triangulation, so that an effective control of accuracy may be everywhere at hand. This is set forth with clearness as a sound established principle.

It is difficult to understand why one government bureau should republish tables issued by other bureaus unless they be out of print or not easily accessible. Pages 163–174 and 190–224 give the well-known geodetic and astronomical tables issued by the Coast and Geodetic Survey, and others are taken from the publications of the Corps of Engineers. Of the 168 pages of tables only 24 appear to have been prepared by the Geological Survey. Table XI., for the reduction of stadia readings, gives merely differences of altitude, the reduction to the horizontal being only mentioned in the four lines of text on page 93, where it is said 'tables for this reduction are to be found in Bulletin.' We know, however, of no author of this name who has published stadia tables.

Still more difficult is it to understand why a government bureau should republish a set of logarithmic tables prepared by a foreign author, thus committing a moral if not a legal piracy. Pages 232–298 constitute a reprint of the well-known five-place tables of F. G. Gauss, which are for sale in all bookstores. If the slightest improvement in type or method of arrangement had been introduced some excuse might be seen for this procedure, but as a matter of fact the type employed is far inferior to the original, while the black rules between the columns will prove an injury to the eyes of all who make use of the tables. Moreover, the marks indicating whether the last decimal figures have been increased or not are in all cases omitted; the reprint is thus rendered a most unsatisfactory counterfeit of the excellent original.

This Manual of Topographic Methods is offered for sale by the Geological Survey at one dollar per copy. It is an advantage for many persons to be able to buy a government publication, instead of attempting to beg it through a member of Congress, but in this case it is to be regretted that the value of the contents is so much less than the price demanded. As a presentation of actual field methods, as a manual for the instruction of the employees of the Geological Survey, and as a contribution to science, this volume occupies a low plane compared to what should be expected from a bureau that has done and is doing topographic work of high excellence.

Mansfield Merriman.

Lehigh University.

Degeneration. By Max Nordau. New York, D. Appleton & Co. 1895. 8vo. Pp. 560 + xiii. Price, $3.50.

This is an English translation from the second edition of the original German, the first edition of which was published in 1893, and a French translation of which appeared in 1894.

The author is a pupil of Lombroso, to whom he dedicates his work, and he states that its object is to apply the methods employed by the modern Italian school in the study of weak, imperfect, degenerate men as found among the criminal and mentally disordered classes, to the identification of degenerates among modern authors and artists. Such degenerates, he declares, manifest the same mental characteristics, and, for the most part, the same somatic features, as do criminals, prostitutes and lunatics.

The physical characteristics, or 'stigmata,' as they are called, of degeneracy in man consist of various malformations which have been described and classified by Morel, Lombroso and others, and which are relied upon to some extent in the diagnosis of doubtful cases of insanity, especially in criminals.

The mental stigmata of degeneracy are also, in many respects, well known, and consist in mental asymmetry, more or less lack of the sense of morality, excessive emotionalism, or its converse, *i. e.*, abnormal apathy and sluggishness, morbid despondency, incapacity for continued attention, and lack of will power, tendency to rambling revery, mysticism, intense egotism, abnormal sexual instincts, etc.

Nordau distinguishes between the hysterical and the degenerate, applying the former term to the admirers and followers of the latter. In his sense there are quite as many hysterical males as females. He is not a physician, and his ideas of hysteria do not precisely correspond with those of the ordinary practitioner; he is a literary critic who has made a special study of morbid mental phenomena and attempts to apply this knowledge to the elucidation of the characteristics of certain forms of modern art and literature with which he is remarkably familiar. He takes up in succession the impressionists, the mystics, the Pre-Raphaelists, the symbolists and the decadents and æsthetes, discussing Ruskin, Holman, Hunt, Rossetti, Swinburne, Morris, Verlaine, Mallarmé, Tolstoi, Wagner, Péladan, Maeterlinck, Baudelaire, Oscar Wilde, Ibsen, Zola, Nietzsche and many others. The only illustration of degeneracy in a scientific man which he gives is Zöllner. His criticisms of these are by no means scientifically impartial; they are at times almost vituperative, but they are in the main just, and substantiated by his quotations, and his strong expressions of condemnation and disgust will in the majority of cases meet with sympathy on the part of an intelligent reader, even if he does find some of the adjectives too sweeping and unqualified.

The chief defect of his work considered from the scientific point of view is its want of logical order; it may almost be said to be composed of two different works, composed in two different moods, one of which was strongly pessimistic, the other more calm and impartial; the first an eloquent appeal to the emotions, the second addressed rather to the reason, and these two parts are so arranged and mixed that it is necessary to read the book from cover to cover and to rearrange and classify the matter in one's own mind, before one can be reasonably sure that he knows the views of the author, and this is the more necessary because the book has no index. For example, the first chapter entitled 'The Dusk of the Nations,' is an eloquent piece of pessimism, yet Nordau is by no means a pessimist; in fact, he considers pessimism as one of the stigmata of degeneration, and the reader after finishing the first chapter should next read the last two chapters, which relate to the prognosis and treatment of the disorder under discussion, in which chapters the author points out that the symptoms which he has described pertain mainly to the scum or froth and to the dregs of population, that the great mass of the people are sound, that the degenerates cannot maintain themselves in the struggle for existence, and that humanity as a whole is not yet senile. A degenerate organism can transmit to its offspring the morbid peculiarities, but, as a rule, the stock soon dies out.

In like manner, mysticism is treated with considerable detail as a pathological phenomenon, without a hint that it is ever anything else, and it is only in a succeeding chapter that we are told that "Mysticism is the habitual condition of the human race, and in no way an eccentric disposition of mind," and that the difference between what may be termed normal and pathological mysticism is that "the healthy man is in a condition to obtain sharply defined presentations from his own immediate perceptions, and to comprehend their real connection. The mystic, on the contrary, mixes his ambiguous, cloudy, half-formed liminal representations with his immediate perceptions, which are thereby disturbed and obscured."

In his fourth chapter the author discusses the causes of the disorder, summing them up as alcohol and tobacco, the growth of cities, and excessive fatigue due to the great increase in the number of sense impressions, perceptions and motor impulses which are experienced in a given unit of time. His argument from the supposed increase of insanity has no sound basis, for there is no good evidence that it has increased, and on this point the recent report of the General Board of Commissioners in Lunacy for Scotland is very satisfactory. The argument that the present generation is aging much more rapidly than the preceding one because there are more deaths from heart disease, apoplexy, etc., now than formerly is also fallacious. Deaths from all the causes which chiefly affect persons over fifty years of age are becoming more frequent, because the proportion of persons over fifty years of age is becoming larger, and the death rates of children are becoming smaller.

His therapeutics are not very definite, being mainly the promotion of education, the condemnation of works trading on unchastity, and the branding of the pornographist with infamy. This is rather the treatment of a symptom than of the disease itself.

The real problem of dealing with the degenerate, and of checking their increase, is no doubt mainly connected with the conditions of city life and the increasing use of mechanism, and is to be solved by changes in municipal organization adapted to the new conditions of the day, combined with intelligent direction of the work of private associations of various kinds.

The work of Nordau should be carefully read by every one who is interested in social progress; the translation is excellent, and it is a book well calculated to make one think. His dogmatic statements as to the mechanism of nerve cells in mental phenomena are, for the most part, pure hypotheses based on materialism and taking no account of the persistence of individual consciousness, but they are in many ways suggestive and interesting; and while one must object to some of his premises, his conclusions with regard to the majority of the authors whom he discusses will probably be accepted by the majority of persons who are competent to form a definite opinion on the subject.

J. S. Billings.

Darwinism and Race Progress. By John Berry Haycraft, M. D., D. Sc., F. R. S. E., Professor of Physiology, University College, Cardiff. London, Swan, Sonneschein & Co. New York, Charles Scribner's Sons. 1895.

This is an eminently sensible book, and besides its scientific interest it deserves the study of social reformers and religious teachers. Dr. Haycraft holds that the muscles and brains of a race are not bound to decay, but that the human species in civilized countries is in fact deteriorating because we are breeding from inferior types. The increased knowledge of recent years is being applied to free mankind from those hardships and diseases which have beset them. But although we may improve an individual during his lifetime, both in physical capacity and mental and moral power, this improvement is not transmitted in any appreciable degree to the offspring, who have therefore to begin again where the parents began. Men can leave their full purses to their sons, but no legacies of mental and moral improvement, or not much. Therefore the action of healthy surroundings will never produce a robust race out of a feeble race, nor will the action of the best educational system ever devised develop a race of wise men out of a race of fools.

This leads our author to a dicsussion of the question whether acquired characters are inherited, or whether the reproductive cells remain unaffected by local changes in the body cells, and he sides with Darwin and Weismann rather than with Lamarck and Herbert Spencer. Racial change is brought about by selection, *i. e.*, by the death or nonproductiveness of certain sorts of individuals, so that the others alone remain; and if this remnant is organically superior, then the next generation will be so. But at present we are not perpetuating our best. The gardener perfects his stock by selecting seed only from the best; and improved breeds of cattle are produced in the same way—not by any new method of ventilating the cowshed, nor by any freshly discovered patent fodder—yet we foolishly fancy we can regenerate society by better food and improved dwellings. We must resort to selection rather. Preventive medicine is saving us from small-pox, measles, typhoid fever, etc.; but these diseases previously exercised a selective influence to carry off the feeblest, who are now preserved to become race-producers. Leprosy also exterminates the unhealthy, and must be looked upon as a friend to humanity. The germs of phthisis or scrofula are our racial friends. Sufferers from phthisis are prone to other diseases as well, and are unsuited for the battle of life, yet because of a certain attractiveness of personal appearance they easily marry, and they leave a large progeny. It follows that by exterminating the bacillus of consumption and giving this delicate and fragile type of persons an advantage in the struggle of life we may imperil the well-being of the future of the race. Even drink may be looked upon as a selective agency, constantly thinning the ranks of those who are weak enough by nature to give way to it, and leaving unharmed those with healthy tastes and sound moral constitutions. Besides the diseased and the drunken there are the incorrigibly criminal, the class whose feet take by nature the crooked path, and who at present are allowed to transmit the taint and the tendency.

What is the remedy? The argument might seem to give a moral sanction to the broadcast scattering of the germs of disease, and to the leaving of unlimited whisky on the doorsteps of our weaker neighbors. But no! other ways are open to us. As regards drink, indeed, Dr. Haycraft would not impose any other restraining influence than a man's own conscience and sense of self-respect. But as regards persons tainted with disease, he does not suggest any such merciless measure as a lethal chamber for them or their offspring. He is content that preventive medicine should continue its work, so beneficent to the individual; but he thinks we ought to replace one selective agency by another. There is already a widespread feeling against the marriage of persons with a distinct family history of insanity. He would try to strengthen that feeling and extend it to other forms of weakness and disease. In the course of time public opinion might sanction legislation of a prohibitive character. As to inveterate criminals, we must bring our minds to the remedy of the perpetual confinement of the irreclaimable, so that they may die out and leave no successors.

After discussing the competition of brain against brain and the fact that property is not always acquired by the most capable, and considering the effect of modern democratic attemps to equalize the struggle, as also the question of the relative sterility of the capables and the possible swamping of the capables by the incapables, our author says he cannot doubt that by selection England, in a hundred years, might have its average man and woman as well endowed in body and mind as are the best of us today.

It should be mentioned that Dr. Haycraft has a high regard for the deserving poor and wishes to see the criminal and vagrant class separated from them in our poor-houses and treated differently.

Geo. St. Clair.

Cardiff, Wales.

A Short History of Chemistry. By F. P. Venable, Ph. D. 12 mo. Pp. viii., 163. Boston, D. C. Heath & Co. 1894. Price, $1.00.

What may be called the historical habit of mind is of great value to the student of any science. Many things are constantly met with which can only be understood in the light of their historical setting. This is especially true in the case of a science which has seen so many vicissitudes and so many changes in its point of view as has chemistry. For this reason a book which gives a clear, concise outline of the historical development of the science is sure to find an extensive field of usefulness.

The present author follows, in general, the division into periods as given by Kopp, but discusses the periods of Medical Chemistry and of Phlogiston together under the head of 'Qualitative Chemistry' and adds a period to which the name of Structural Chemistry is given. The opinion is expressed that this period has already passed and that we are entering upon a new and different phase of development for the science. His characterization of the present tendencies of the science is, however, necessarily vague and unsatisfactory.

The book is well written and there appear to be few errors. On page 141 the value of 15.96 for the atomic weight of oxygen is based, incorrectly, on the authority of Stas, instead of on that of Dumas and of Erdmann and Marchand.

For any student who desires more than a very elementary knowledge of the science, the book must, of course, be considered as an outline which is to be filled out by extensive reading of larger works. But, whether used by itself or in connection with other books or lectures, it is hoped that a book which is so easily accessible to every one will give a new impetus to a phase of chemical study which has been too much neglected.

W. A. Noyes.

Rose Polytechnic Institute.

A Laboratory Manual containing directions for a course of experiments in Organic Chemistry systematically arranged to accompany Remsen's Organic Chemistry by W. R. Orndorff. Boston, Heath & Co. 1894.

As indicated by the title, this manual contains directions for the experiments in Remsen's Organic Chemistry in a form suitable for students in the laboratory. The page being printed on but one side, ample room is left for the student's observations and, as the text-book is not open before him, he is led to observe for himself, instead of merely trying to see what the text-book says he should. As stated by Professor Remsen in the preface, "Great care has been taken to determine the best condition for each experiment, and in many cases the directions given are undoubtedly better than those given in my (R's) book." Frequently, however, the only difference in the directions is that in the text-book they are more or less general, whereas in the manual they are given in great detail and, though the student may thus fail less frequently the first time he tries to make a substance, the educational value is diminished. Often more is learned by failure than success. The student must determine the necessary conditions himself. Thus he becomes self-reliant and learns to think chemically. This fault of the manual is to some extent compensated by the questions asked on almost every page. On the whole, the book will be found a valuable aid, especially in those laboratories in which the instructor can not devote much time to each student.

Felix Lengfeld.

University of Chicago.

NOTES AND NEWS.

INVESTIGATION OF THE GOLD AND COAL RESOURCES OF ALASKA.

Congress at its last session ordered a special investigation of the gold and coal resources of Alaska, appropriating $5,000 therefor. The investigation will be made under the direction of the U. S. Geological Survey, and will be under the immediate charge of Dr. George F. Becker, the well known gold expert. With Dr. Becker will be Dr. Wm. H. Dall, paleontologist, who has a superior knowledge of the geography and the general geology of the region. These experts and a single geologic assistant will comprise the party.

The party will leave Washington City, May 15, and it is proposed, with the sum available, to spend three months in actual field work, spending a month in each of three distinct districts along the Alaskan coast. Work will be begun in the Sitka area, where both gold and coal are known to occur. Transportation into and about the various inlets and bays to the north and west of Sitka will be furnished, through the courtesy of Secretary Herbert of the Navy, by the U. S. S. Pinta, which will be stationed in those waters. From the Sitka region the party will go to Kadiak Island and Cook's Inlet by mail steamer. In this region both gold and coal will be looked for also. The district to be visited last is Shumagin, to be reached by mail steamer from Kadiak. In the last named region, as in the other areas, gold and coal will be the main objects of inquiry, though the district is otherwise of very considerable geologic interest on account of its fossil remains and the presence of an active volcano.

The search for coal is one of especial interest to the Navy Department; if coal suitable for use as fuel in the war vessels and revenue cutters in the Pacific were found to be available in quantities, it would be of incalculable advantage to the Government.

It will not be feasible with the limited fund available to carry this investigation of gold and coal resources as far as might be desired. There is demand, for example, for an investigation of the gold placers of the Yucon river, but to do this work effectively the geologist will have to remain in the Yucon region through one summer and through the ensuing winter.

A REDFIELD MEMORIAL.

The botanical section of the Academy of Natural Sciences, of Philadelphia, which had under consideration the subject of a monument commemorative of the services to botanical science of the late John H. Redfield, Conservator of the herbarium of the Academy, has issued a circular, saying:

"It has been decided that no better monument to the memory of John H. Redfield could be erected than to arrange for completing and caring for the work he loved, and to which he gave freely so many years of his life—namely, the Herbarium of the Academy of Natural Sciences. Mainly through his disinterested labors, it stands to-day scarcely second to any in the United States, containing, besides many unnamed, over 35,000 named species of flowering plants and ferns, the half of which have been verified and fastened down.

"No one can probably be found to give the years of time he so freely gave. In order to carry on the work, and add to the collection, as exploring expeditions afford the opportunity, it has been proposed to establish a Redfield Memorial Herbarium Fund.

"Mr. Redfield's will provides that his herbarium, minerals, shells and scientific works shall be sold to help the herbarium, thus furnishing a nucleus for the proposed fund. It is in mind to raise $20,000, but the interest of any sum that may be contributed can at once be made available.

"Statements will be furnished from time to time to contributors, keeping them informed of the progress of the contributions. Checks may be made payable to the order of Thomas Meehan, Director, or Stewardson Brown, Treasurer, and mailed to either at the Academy of Natural Sciences, Nineteenth and Race streets, Philadelphia."

THE MOTION OF CLOUDS.

AT a meeting of the *Royal Meteorological Society*, of London, on March 20th, Mr. W. N. Shaw, F. R. S., delivered a lecture on 'The Motion of Clouds considered with reference to their mode of formation,' which was illustrated by experiments. The question proposed for consideration was how far the apparent motion of a cloud was a satisfactory indication of the motion of the air in which the cloud is formed. The mountain cloud cap was cited as an instance of a stationary cloud formed in air moving sometimes with great rapidity; ground fog, thunder clouds and cumulus clouds were also referred to in this connection. The two causes of formation of cloud were next considered, viz.: (1) the mixing of masses of air at different temperatures, and (2) the dynamical cooling of air by the reduction of its pressure without supplying heat from the outside. The two methods of formation were illustrated by experiments.

A sketch of the supposed motion of air near the centre of a cyclone showed the probability of the clouds formed by the mixing of air being carried along with the air after they formed, while when cloud is being formed by expansion circumstances connected with the formation of drops of water on the nuclei to be found in the air, and the maintenance of the particles in a state of suspension, make it probable that the apparent motion of such a cloud is a bad indication of the motion of the air. After describing some special cases, Mr. Shaw referred to the meteorological effects of the thermal disturbance which must be introduced by the condensation of water vapor, and he attributed the atmospheric disturbances accompanying tropical rains to this cause. The difference in the character of nuclei for the deposit of water drops was also pointed out and illustrated by the exhibition of colored halos formed under special conditions when the drops were sufficiently uniform in size.

THE DISCRIMINATION OF COLORS.

PROFESSOR ARTHUR KÖNIG (*Zeitschrift für Psychologie*, Feb., 1895) has calculated, from experiments previously published, the number of hues or colors that can be distinguished in the spectrum. Differences in hue cannot be perceived beyond $\lambda = 655\ \mu\mu$ and beyond $\lambda = 430\ \mu\mu$; between these limits the normal eye can distinguish about

160 hues. According to König, the dichromatic eye (green or red blind) can distinguish nearly the same number of hues, its accuracy being greater than that of the normal eye in certain regions. The seven colors inherited from Newton should be abandoned. Physically, any three wavelengths, sufficiently separated, suffice to produce all the colors; psychologically, we can distinguish about 160 hues, or, as Leonardo da Vinci stated, there are four distinct colors—red, yellow, green and blue. In the same paper König calculates that about 660 degrees of intensity or brightness can be distinguished between the light that is just visible and the light so intense as to be blinding.

THE KARAKORAM HIMALAYAS.

In a lecture before the Imperial Institute of London, Mr. William Conway described the expedition to the Karakoram Himalayas made in 1892 under the auspices of the Royal Geographical Society, the Royal Society, the British Associotion, and the Government of India. The party consisted of the Hon. C. G. Bruce, Mr. A. D. M'Cormack, the lecturer, and two others, with an Alpine guide. The lecturer stated, according to the report in the London *Times*, that starting from Abbottabad, they went to Srinagar, the capital of Kashmir, thence by the Burzil pass to Astor and Bungi, in the Indus valley. The party followed the road to Gilgit, and a month was then spent in exploring the glaciers at the head of the Bagrot valley, and the great peaks in the neighborhood of Rakipushi. Returning to Gilgit they ascended the Hunza-Nagar valley, and visited the towns. From that point two long expeditions were made into the snowy region to the south and southeast before pushing forward to Hispar, which was at the foot of the longest glacier in the world outside the polar region. Dividing themselves into two parties, they made the first known passage of Europeans up the Nushik pass, and the first definitely recorded passage of the Hispar pass. The two parties united at Askole, in Baltistan, and, proceeding up the Braldo valley, arrived at the foot of the remarkable Baltoro glacier. Having forced their way to the very head of the glacier, they camped for two nights at an altitude of 20,000 ft. The Pioneer peak, which was 3,000 ft. above the camp, was also climbed, thus making, it was said, the highest ascent yet authentically recorded. Returning to Askole, they crossed the Skoro pass to Shigar and Skardo, whence they rode up the Indus valley to Leh, the capital of Ladak, or Western Tibet. The Zoji pass to Kashmir was traversed, and the party returned from Srinagar to England.

GENERAL.

Professor James D. Dana died at New Haven, on April 14th, at the age of eighty-two years.

The sixty-fifth meeting of the British Association for the Advancement of Science will commence on Wednesday, the 11th of September, under the presidency of Sir Douglas Galton, well known for his works upon sanitation, and as an adviser of the Government in matters of sanitary engineering. An invitation is issued to the philosophers of England and other countries, by the Secretary, to support this meeting by personal assistance and written contributions. Americans who have been the guests of the British Association know how admirable the arrangements are for the conduct of these meetings and how, by invitation to the General Committee and the Sectional Committees, a visitor from a foreign country is soon made to feel that he is a part of this great scientific organism.

At the last meeting of the Victoria Institute, of London, Sir George Stokes, Bart, F. R. S., in the Chair, papers by Sir J. W.

Dawson, C. M. G., F. R. S., Professors E. Hull, F. R. S., Parker and Duns, the Rev. G. Whidborne, and Mr. J. Slater, F. C. S., were read upon the questions in regard to natural selection and evolution, treated by Professor Huxley in his recent address on 'The Past and Present.'

On May 4th the Association for the Education of Women is to hold a general meeting in the Schools, Oxford, to consider the question of a petition to the University for the admission of women to the B. A. degree.

Dr. Sherrington, now Superintendent of the Brown Institution, London, has been appointed to the George Holt chair of Physiology at Liverpool, vacant by the removal of Professor Gotch to Oxford.

Dr. H. Weber, Professor of Mathematics in the University of Göttingen, has accepted a call to the University of Strassbourg, and Professor Hilbert, of Königsberg, has been called to the vacant chair in Göttingen.

Dr. E. R. L. Gould has accepted a call to the Professorship of Statistics in the University of Chicago.

Mr. Theodore T. Groom, of St. John's College, Cambridge, has been appointed Professor of Natural History in the Royal Agricultural College, Cirencester, succeeding the late Professor Harker.

Dr. Johannes Brummer, Professor of Agriculture in the University of Jena, died recently at the age of forty-three years.

The death is announced of the Irish Naturalist, Mr. A. G. More.

The Appalachian Mountain Club, of Boston, announces the following excursions for 1895: April 19, Long Walk; May 11, May Walk—Nobscot Hill and Wayside Inn; May 30, Mt. Tom and Mt. Holyoke; July 1–8, Field Meeting—Seal Harbor, Mt. Desert; August, A probable excursion to the Selkirk mountains in British Columbia, occupying an entire month.

A Psychological Index, being a bibliography of the literature of Psychology and cognate subjects for 1894, has been published by Macmillan & Co., as a supplement to the *Psychological Review*. The index has been compiled by Mr. Howard C. Warren, of Princeton College, and Dr. Livingston Farrand, of Columbia College. 1312 titles are given, distributed as follows: General 135, Genetic, Comparative and Individual Psychology 259, Anatomy and Physiology of the Nervous System 190, Sensation 107, Consciousness, Attention and Inhibition 176, Feeling 50, Movement and Volition 116, Abnormal 278.

SOCIETIES AND ACADEMIES.

THE MINNESOTA ACADEMY OF NATURAL SCIENCES, MINNEAPOLIS. JOINT MEETING WITH THE ST. PAUL ACADEMY OF SCIENCE.

March 6th, in the rooms of the St. Paul Commercial Club.

The Physical Features of the Lake of the Woods: Professor Conway MacMillan, State Botanist.

Psychic Effects of the Weather: Edward S. Beals, Observer U. S. Weather Bureau, Minneapolis.

Geology and Flora of the Mountain Region of Northwestern Montana: D. R. McGinnis, Secretary St. Paul Commercial Club.

April 2d in the Public Library, Minneapolis.

Fatigue; its Cause and Social, Religious, Economic and Educational Aspects: H. S. Baker, Ph. D., Principal of the Jefferson School. St. Paul.

Some Queer Forms of Shellfish: Professor H. L. Osborn, Hamline University, St. Paul.

C. W. Hall, *Secretary*.

NEW YORK BRANCH OF THE AMERICAN FOLK-LORE SOCIETY.

On the evening of Saturday, April the 6th, the annual meeting of the New York Branch of the American Folk-Lore Society was held with the following result:

The officers elected for the season of 1895–96 are as follows: President, Mr. E. Francis Hyde; First Vice-President, Mr. George B. Grinnell; Secretary and Treasurer, Mr. William Burnet Tuthill; as members of the Executive Council, Mrs. Henry Draper, Mrs. Mary J. Field and Mrs. E. Francis Hyde. The offices of Second Vice-President and the fourth lady member of the Executive Council were not filled, the places being held vacant for the action of the Executive Council.

It was determined to hold the final meeting of the season on the evening of Tuesday, May the 7th, at the Hotel Waldorf. The speaker for the evening will be Dr. Matthews, of Washington, the subject being Navahoe Myths, illustrated by phonograph. It is also the intention of the Council to have four meetings during the coming season; three of them to be held at the Hotel Waldorf and one at the Museum of Natural History. At the meetings held at the Hotel Waldorf the members of the Society will be entertained after the reading of the paper.

Wm. B. Tuthill, *Secretary.*

THE NEW YORK MINERALOGICAL CLUB.

At the last meeting of the New York Mineralogical Club the following officers were elected for the ensuing year: President, George F. Kunz; Secretary, Professor Daniel S. Martin; Treasurer, J. W. Freckleton; Executive Committee, E. Schernikow, Dr. E. S. Arnold and Professor A. H. Chester; Curators, Professor R. P. Whitfield, Gilman S. Stanton and William Niven; Committee on Admissions, J. McCarthy and Frederick Kato; Committee on Executions, J. S. Walker, Professor D. S. Martin and Frederick Kato; Delegates to Scientific Alliance, George F. Kunz, Professor D. S. Martin and J. W. Schoonmaker.

SCIENTIFIC JOURNALS.

THE ASTROPHYSICAL JOURNAL, APRIL.

Recent Researches on the Spectra of the Planets, II.: H. C. Vogel.

A summary of recent work on Jupiter, Saturn and Uranus. Photographic observations reveal no deviation in their spectra from that of the sun, but in the less refrangible region bands due to the absorption in the atmospheres of the planets have been recorded visually. A comparison of the visual spectrum of Uranus as mapped by Keeler and by Vogel shows little variation. Repeated observations on the red spot of Jupiter indicate no difference between its spectrum and that of the belts. From a study of the red region, the satellites probably have atmospheres similar to that of the primary. The spectra of Saturn and the ansæ of the ring on each side are identical in the more refrangible portion. That there is no absorption band at λ 618 $\mu\mu$ indicates the absence of an atmosphere around the rings.

On the Periodic Changes of the Variable Star Z Herculis: N. C. Dunér.

After discussing various observations upon this variable and giving its ephemeris, the writer concludes that *Z* Herculis is a connecting link between the algol and the Υ Cygni types, differing from algol in having both components bright, and from Υ Cygni in that the components are of unequal brightness. It consists of two stars of equal size, one of which is twice as bright as the other. The stars revolve in 3 days, 23 hours, 48 minutes, 30 seconds, in an elliptical orbit whose semi-major axis is six times the diameter of the stars. The plane of the orbit passes through the sun.

Preliminary Table of Solar Spectrum Wave-Length, IV.: H. A. Rowland.

The table is continued from λ 4266 to 4414.

T. Andromedæ: E. C. Pickering.

A study of later photographs indicate that the period of this variable, which was 281 days during 1891–1894, has changed for 1896.

Eclipse of Jupiter's Fourth Satellite, February 19, 1895: E. C. PICKERING.

A photometric observation before and after eclipse, compared with the second satellite.

Spectrum of Mars: LEWIS E. JEWELL.

A spectroscopic study of the water vapor of the earth's atmosphere shows that, unless the amount of water in the atmosphere of Mars is greater than that in the earth's atmosphere, it is useless to look for it there, with our present instruments. The chances for detecting oxygen and chlorophyl are better.

On a New Method of Mapping the Solar Corona: GEORGE E. HALE.

A method for using the differential bolometer. Evidence is offered that the heat radiation of the corona could be differentiated from that of the adjacent sky. If one member of the bolometer be exposed to a portion of the sky just beyond the coronal region, and the other member set successively on different parts of the coronal image, the galvanometer would indicate the varying radiation of heat intensity. Methods are also proposed for reducing the galvanometer readings to a form suitable for comparison with actual photographs of the corona.

On a New Form of Spectroscope: C. PULFRICH.

A translation from the *Zeitschrift für Instrumentenkunde*, describing a modification of the Littron spectroscope.

Minor Contributions and Notes.

Photographic Correcting Lens for Visual Telescopes: JAMES E. KEELER.

The Color of Sirius in Ancient Times: W. T. LYNN.

On the Variability of Es.-Birm. 281: T. E. ESPIN.

The Displacement of Spectral Lines Caused by the Rotation of a Planet: JAMES E. KEELER.

Dr. Pulfrich's Modification of the Littrow Spectroscope.

A list of the titles of recent publications on astrophysical and allied subjects appearing since the last number is a feature of each issue.

THE PHYSICAL REVIEW, MARCH–APRIL, 1895.

THE leading article in this number of the *Review* is one by Dr. A. S. Mackenzie, *On the attractions of Crystalline and Isotropic Masses at Small Distances* The primary object of the paper is to give in detail the methods and results of an investigation made for the purpose of determining whether, within the errors of observation, there is any deviation from the law of Newton in the case of attracting crystalline matter with reference to its optic axis, and the author gives also the results of some experiments made with a view to testing the application of the same law in the case of isotropic matter at small distances.

Physicists do not yet fully appreciate the value of the ingenious device suggested by Professor Boys through which they have lately been able to use quartz fibres, which furnish a mode of suspending small masses far ahead of anything before made use of in stability or constancy of torsional resistance. Like many other apparently minor discoveries or inventions, the introduction of the quartz fiber has greatly enlarged the opportunities of the experimentalist, in that it provides a ready means of measuring forces so minute as to have been thought until recently quite beyond our reach. The solution of problems relating to near attractions has especially been forwarded by this device, as Professor Boys has himself shown in several able and important investigations. In the paper under consideration Dr. Mackenzie describes the apparatus used in studying the attraction of crystalline masses. It is simple but effective, and so delicate in its indications that the utmost care was necessary to avoid interference for external causes, often difficult to control. Full details are given, as they are of great interest, especially to those who contemplate the use of a quartz torsion fibre. It is interesting to note that the author was never able, throughout a long series of experiments, to control absolutely the zero point of his balance. Although quartz is enormously superior to any other suspension thus far proposed, it is still defective in this respect. For some cause which Dr. Mackenzie is unable to give, the zero was constantly shifting. He does not clearly say whether this partakes of the nature of a 'drift' in one direction or not. In a long series of experiments, made by direction of the writer of this notice, for the purpose of trying to improve the existing form of the vertical force magnetometer, quartz fibres were used. Although apparently well protected from convection currents and changes in temperature, the mirror attached to them *was never actually at rest*. When this shifting and drifting is small, as it usually is, and observations are of the nature of those described by Dr. Mackenzie, that is, not in themselves extending over long periods, the error arising from it may be readily and correctly eliminated.

The apparatus used for observing the attraction of isotropic masses was of the same character, and similar to that used by Professer Boys. The conclusion reached, the experimental results being in agreement within one or two-tenths of one per cent., is that neither in the case of crystalline nor isotropic masses was any deviation from the law of Newton detected. The author fails to note the very ingenious and interesting method of attacking the problem of the attraction of crystalline masses proposed by Poynting in his Adams Prize Essay on the Density of the Earth. Poynting proposes to test the question of there being different properties as to attraction along different axes of crystals by the *directive action* which must exist when one sphere of a crystal is in the field of another. He made some experiments along that line, and his work probably preceded by a year or two that of Dr. Mackenzie. At the present moment, with library out of reach, I am unable to say whether he has published any further results.

The *Influence of Temperature on the Transparency of Solutions*, by E. S. Nichols and Mary C. Spencer, is another prominent article of the Review. Transparency to various wave-lengths was tested and a number of color solutions were examined. There are also papers on the Electric Conductivity of Certain Salt Solutions, by A. C. MacGregory, a continuation of the paper on Forces between Fine Solid Particles totally Immersed in Liquids and among the minor contributions is one interesting and useful on the Variation of Internal Resistance of a Voltaic Cell with Current, by Professor Carhart.

T. C. M.

NEW BOOKS.

Die Chemie des Chlorophylls. L. Marchlewski. Hamburg und Leipzig, Leopold Voss. 1895. Pp. iv + 82. M. 2.

Les Aurores polaires. Alfred Angot. Paris, Felix Alcan. 1895. Pp. vii + 315.

Lehrbuch der Allgemeinen Psychologie. Johannes Rehmke. Hamburg und Leipzig, Leopold Voss. 1894. Pp. 582. M. 10.

Iowa Geological Survey, Vol. III. Des Moines, Published for the Iowa Geological Survey. 1895. Pp. 501.

Magnetismus und Hypnotismus. G. W. Gessman. Vienna, A. Hartleben. 2d edition. Pp. xiv + 205.

Bulletin of the Geological Institution of the University of Upsala. Edited by Hj. Sjögren. Upsala, Almqvist & Wiksells. 1893–1894. Pp. 95, 293.

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NATIONAL ACADEMY OF SCIENCES. REPORT OF THE WATSON TRUSTEES ON THE AWARD OF THE WATSON MEDAL TO SETH C. CHANDLER.

On the recommendation of the Board of Trustees of the Watson Fund, the Academy last year unanimously awarded the Watson medal to Seth C. Chandler, of Cambridge, Mass., for his investigations relative to variable stars, his discovery of the period of variation of terrestrial latitudes, and his researches on the laws of that variation. It is the pleasant duty of the Trustees to set forth the grounds on which this award was recommended.

It is a result of the well-known laws of dynamics relating to the rotation of a rigid body, as the earth is assumed to be, upon its axis, that the poles of the earth may be determined in two ways. Our globe, being a spheroid flattened at the poles and protuberant at the equator, has a certain axis passing between the points of greatest flattening. This axis has no direct connection with the rotation of the earth; it would exist if the latter, retaining its present form, did not rotate at all. It is called the axis of figure, being determined altogether by the shape of the earth.

But the earth has also an axis around which it rotates. Now, assuming the earth to be a rigid solid, there is no necessity that the axis of rotation should correspond to that of the axis of figure just described.

We could take a solid body, pass an axis through it in any direction, and make it rotate on that axis.

It was shown by Euler, more than a century ago, that if a solid body rotated on an axis different from that of figure, the position of the axis of rotation in the body would be subject to a slow change, consisting in a constant revolution around the axis of figure. Were this body the earth, the latitude of a place, as determined by astronomical observation, would change in the same way. The time of one revolution of the pole would depend upon the figure of the earth. The flattening of the earth is such that, were it a perfectly rigid body, the time of revolution would be about 305 days; that is to say, the north pole would make its circuit in a period of 305 days.

There being no necessity that the two poles should coincide, the question was naturally raised whether, perhaps, there might actually be such a difference of the two poles, and, in consequence, a change of latitude of every place on the earth's surface having a period of 305 days. The first to investigate this question with all the refinements of modern astronomy was C. A. F. Peters, who, half a century ago, was an assistant at the Pulkowa Observatory. In his classic paper on the parallax of the fixed stars, one section is devoted to the question of the variability of the latitude in a period of 304 days, which, according to the then accepted value of the flattening of the earth, would be the time of one revolution of the poles. He found a coefficient of 0″.079, with a probable error of 0″.017. This result was so extremely minute that it might have arisen from unavoidable sources of error; and the conclusion therefore reached was that if there was any such separation of the two poles, it was too small to be certainly detected by the most refined observations.

In 1862 our late fellow member, Professor Hubbard, of the Naval Observatory, commenced a series of observations with the prime-vertical transit of that institution, which would be available for the same research. They were interrupted after a little more than a year, by his untimely death, but were continued four years longer by his successors. The result was the same as that reached by Peters; no change having a period of 305 days could be detected.

In 1873 the question was investigated by Nyrén in connection with a longer series of observations on the latitude of the Pulkowa Observatory. His results were somewhat discordant, and the only conclusion that could be drawn from them was that the variation could not be certainly detected by these most refined observations.

Ten years later Nyrén repeated the determination, in connection with his observations for the determination of the constant of abberation. These observations, made with the prime-vertical transit, were carried through with the minutest attention, and the utmost care to avoid every conceivable source of error. Curious discordances were nevertheless found in the results for the constant of abberation.

In 1885 Küstner showed that they could be accounted for by supposing a change going on in the latitude. But nothing could be inferred respecting the law or the cause of the change.

As a result of these investigations, the coincidence of the earth's axes of rotation and of figure has, until within a very few years, been assumed by astronomers as a practically established fact; and all their methods of observation have rested upon the idea of absolute coincidence. This confidence has not been disturbed until within a few years, when the question has been reopened. But it has now apparently been settled upon a new and firmly established basis.

Dr. Chandler's work upon this subject began with observations made by him in

1884–85, using a novel form of astronomical instrument of his own invention. These observations, continued uninterruptedly for thirteen months, revealed a progressive change of a pronounced periodical character in the instrumental values of the latitude. In publishing these results in 1885 he announced his intention to continue the research throughout the remainder of that year. Yet circumstances prevented him from carrying out his intention at that time, and he did not resume his examination of the subject until six years later. Meanwhile Dr. Küstner, at the Observatory of Berlin, in 1888, published a memoir on the Constant of Aberration, as deduced by him from a series of observations also made in 1884–85, simultaneously with Chandler's series, which brought to light anomalies of an entirely analogous character. Küstner's series was not continuous enough to show the periodic nature of the phenomenon; but, by an exhaustive examination of the possible subjective sources of error, he clearly demonstrated that it was no longer permissible to retain the hypothesis of an invariable position of the pole, and he recommended that properly organized observations at various places be instituted to settle the question definitely. It was doubtless this work of Küstner's which compelled the attention of astronomers to the subject. As a result, by the coöperation of three German observatories, under the auspices of the International Geodetic Association, and the independent action of that at Pulkowa, the fact of the variability of terrestrial latitude was placed beyond question, and, by a corresponding series made at the Sandwich Islands, the further fact was established that the variable element is the position of the axis of rotation with respect to the earth's body, and not its position in space.

It was just before this point that a renewal of Chandler's connection with the problem began. The results are published in a series of eighteen papers in the Astronomical Journal (1891–94), exclusive of a series of five papers upon a topic closely related thereto, and involving it; namely, the abberation-constant, which will be separately spoken of later.

The keynote of these investigations, and the undoubted cause of the success which has attended them, lies in the fact that at the outset he first recognized the necessity of deliberately disregarding all teachings of the adopted theory, which had misled previous investigators, and of examining the facts by a purely inductive process, taking nothing for granted, and basing all conclusions strictly upon the observations themselves.

It is impossible to give here more than a bare statement of the principal results thus established, which we arrange in their natural order, and not in the historical order of their derivation.

1. The phenomenon is not a local or a regional, but a terrestrial one; also it is a displacement of the earth's axial rotation with reference to the principal axis of inertia, and not of the direction of the former in space.

2. The axis of rotation, although fixed as regards its direction in space, performs a relative revolution about that of inertia in a period of 428 days. This motion is circular, with an average radius of about fourteen feet, and its direction is from west to east.

3. Simultaneously with the above motion, the actual position of the principal axis of inertia on the earth's surface is in motion about a mean position, in a period of a year. Its direction is also from west to east, but is in an ellipse, three or four times as long as broad, the major and minor axes being about twenty-five feet and eight feet respectively. The major axis is inclined at present, by about 45° to the Greenwich

meridian. The motion is central, obeying the law of proportionality of times to areas described by the radius vector about the center of the ellipse.

4. Both the radius and period in the circular 428 days' revolution are systematically variable; the former between about eight feet and eighteen feet, the latter between about 423 and 434 days; in a long period of apparently about sixty-six years. In this inequality of motion the average angular velocity is attained when the size of the circle is least or greatest when the circle has its mean dimensions.

5. Similarly there are simultaneous changes in the apparent dimensions and velocity in the annual period, which are complementary in their character to those in the 428 days' revolution; but whether they are the result of real changes in the form and dimensions of the ellipse, or the effect of an apsidal motion of long period, cannot at present be determined from the observations available. All that can be said is that observations during five years show that the line of apsides is either fixed, or, if variable, revolving only at a very slow rate.

6. Besides these two motions of relatively short period, there is distinct evidence of a third motion of rotation in a much larger term, probably not far from twelve years, with a radius of ten or fifteen feet, which reconciles similar indications of slow changes which had been pointed out by other investigators. (A. J., XII., 178; XIII., 35, 36.)

The results thus established are the outcome of the examination of an immense number of observations, covering the whole interval since the era of refined practical astronomy began, and in fact practically exhaust the materials which may be drawn for this purpose from existing astronomical annals. The endeavor to make the discussion exhaustive in this respect made it necessary to completely reduce, from the original instrumental readings, extensive older series of observations. It has, incidentally, for example, rescued from almost complete oblivion the series of Pond, 1825–36, and shown that work to be of a character which will compare favorably with the most refined observations made with the meridian instruments of the present day.

Intimately connected with the work on the variation of latitude are five additional papers, containing a redetermination of the value of the aberration-constant from eight different series of observations at the Pulkowa Observatory, with the prime vertical transit and the vertical circle. The correct value of this fundamental element is one of the most important questions occupying the astronomy of the day.

VARIABLE STARS.

THE subject of variable stars was erected into a distinct branch of astronomical science by Argelander, beginning in 1843, and occupied a large share of his activity and interest during a score of years. His classical labors were succeeded or overlapped by those of Schönfeld, who assumed the principal charge of the subject for another score of years, when his devotion to the great work of the Southern Durchmusterung, and later his failing health, left opportunity for other hands to take up and continue the work where they had left it. Since the issue of Schönfeld's Second Catalogue the number of known variables has more than doubled, while the fund of observations pertaining to them has vastly increased. Chandler's work in this direction, besides the incidental work of observation and discovery which he has contributed to it, has involved the collection of all the data in astronomical history, their discussion, and the formulation of the elements of their light-variations into numerical laws. The catalogues of 1888 and 1893, while filling a very moderate number of pages of print, are a crystallization of all the known facts. Especially may be mentioned the investigations of inequalities in the periods of these bodies. While the number of these inequalities known in Schönfeld's time amounted to only about half a dozen, Chandler has detected their existence in about eighty other stars, and has deduced the numerical laws in about fifty of them. This will indicate, in one direction only, how the labor of caring for these objects is increasing.

It would be unjust if, while alluding to these important researches, no mention were made of Mr. Chandler's ingenious and successful device of a new form of instrument for making that class of measurements of position which had previously been made by meridian instrument alone. Both the instrument and the method were novel. In the former, instead of a motion of rotation, determined mechanically by the pivots of a horizontal axis, there was substituted one about a vertical axis determined by gravitative action of an instrument resting in mercury.

As to method, instead of a vertical plane passing through the pole, which is the fundamental plane of reference for meridian instruments, there was substituted a horizontal circle. The value possessed by such an entirely different method consists in substituting a totally different sort of observation, and hence a different set of the systematic errors to which all observations are liable, so that the combined results of the two methods are likely to be freer from them than those obtained by an adherence to a single system of observation. In a memoir of 222 pages Dr. Chandler develops the theory of the instrument and method mathematically, and gives the result of its practical use in observations made with it for a year, and directed to various astronomical problems.

Although not mentioned as forming any part of the grounds for the award of this medal, Dr. Chandler's important labors for many years upon cometary orbits are well known to astronomers. Casual mention may be especially made of his computations relative to the principal component of 1889V, and the action of Jupiter in 1886 upon it, which led to a complete transformation of its orbit; also the definite determination of the relative orbits of the several components into which the comet became separated in consequence of that disturbance.

The Trustees of the Watson Fund feel that this brilliant series of investigations is preëminently deserving of the highest recognition which can be given by the National Academy, and have therefore not hesitated in recommending the award of the medal to Dr. Chandler.

S. Newcomb.
B. A. Gould.
A. Hall.

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*SUMMARY OF CONCLUSIONS OF A REPORT BY DRS. D. H. BERGEY, S. WEIR MITCHELL AND J. S. BILLINGS UPON 'THE COMPOSITION OF EXPIRED AIR AND ITS EFFECTS UPON ANIMAL LIFE.'**

1. The results obtained in this research indicate that in air expired by healthy mice, sparrows, rabbits, guinea pigs or men there is no peculiar organic matter which is poisonous to the animals mentioned (excluding man), or which tends to produce in these animals any special form of disease. The injurious effects observed of such air appeared to be due entirely to the diminution of oxygen or the increase of carbonic acid, or to a combination of these two factors. They also make it very improbable that the minute quantity of organic

* Results of an investigation made under the provisions of the Hodgkin's Fund. Read before the National Academy of Sciences, April 16, 1895, by permission of the Secretary of the Smithsonian Institution.

matter contained in the air expired from human lungs has any deleterious influence upon men who inhale it in crowded rooms, and hence it is probably unnecessary to take this factor into account in providing for the ventilation of such rooms.

2. In ordinary quiet respiration no bacteria, epithelial scales, or particles of dead tissue are contained in the expired air. In the act of coughing or sneezing such organisms or particles may probably be thrown out.

3. The minute quantity of ammonia, or of combined nitrogen or other oxidizable matters found in the condensed moisture of human breath appears to be largely due to products of the decomposition of organic matter which is constantly going on in the mouth and pharynx. This is shown by the effects of cleansing the mouth and teeth upon the amount of such matters in the condensed moisture of the breath, and also by the differences in this respect between the air exhaled through a tracheal fistula and that expired in the usual way.

4. The air in an inhabited room, such as the hospital ward in which experiments were made, is contaminated from many sources besides the expired air of the occupants, and the most important of these contaminations are in the form of minute particles or dusts. The experiments on the air of the hospital ward, and with the moisture condensed therefrom, show that the greater part of the ammonia in the air was connected with dust particles which could be removed by a filter. They also showed that in this dust there were microörganisms, including some of the bacteria which produce inflammation and suppuration, and it is probable that these were the only really dangerous elements in this air.

5. The experiments in which animals were compelled to breathe air vitiated by the products of either their own respiration or by those of other animals, or were injected with fluid condensed from expired air, gave results contrary to those reported by Hammond, by Brown-Séquard and d'Arsonval, and by Merkel; but corresponding to those reported by Dastre and Loye, Russo Gilibert and Alessi, Hofmann Wellenhof, Rauer, and other experimenters referred to in the preliminary historical sketch of this report, and make it improbable that there is any peculiar volatile poisonous matter in the air expired by healthy men and animals, other than carbonic acid. It must be borne in mind, however, that the results of such experiments upon animals as are referred to in this report may be applicable only in part to human beings. It does not necessarily follow that a man would not be injured by continuously living in an atmosphere containing 2 parts per 1,000 of carbonic acid and other products of respiration, of cutaneous excretion, and of putrefactive decomposition of organic matters, because it is found that a mouse, a guinea pig, or a rabbit seems to suffer no ill effects from living under such conditions for several days, weeks or months, but it does follow that the evidence which has heretofore been supposed to demonstrate the evil effects of bad ventilation upon human health should be carefully scrutinized.

6. The effects of reduction of oxygen and increase of carbonic acid, to a certain degree, appear to be the same in artificial mixtures of these gases as in air in which the change of proportion of these gases has been produced by respiration.

7. The effect of habit, which may enable an animal to live in an atmosphere in which by gradual change the proportion of oxygen has become so low and that of carbonic acid so high that a similar animal brought from fresh air into it dies almost instantly, has been observed before; but we are not aware that a continuance of this immunity produced by habit has been previously noted. The experiments reported in the

appendix show that such an immunity may either exist normally or be produced in certain mice, but that these cases are very exceptional, and it is very desirable that a special research should be made to determine, if possible, the conditions upon which such a continuance of immunity depends.

8. An excessively high or low temperature has a decided effect upon the production of asphyxia by diminution of oxygen and increase of carbonic acid. At high temperatures the respiratory centers are affected when evaporation from the skin and mucous surfaces is checked by the air being saturated with moisture; at low temperatures the consumption of oxygen increases, and the demand for it becomes more urgent. So far as the acute effects of excessively foul air at high temperatures are concerned, such, for example, as appeared in the Black Hole of Calcutta, it is probable that they are due to substantially the same causes in man as in animals.

9. The proportion of increase of carbonic acid and of diminution of oxygen, which has been found to exist in badly ventilated churches, schools, theatres or barracks, is not sufficiently great to satisfactorily account for the great discomfort which these conditions produce in many persons; and there is no evidence to show that such an amount of change in the normal proportion of these gases has any influence on the increase of disease and death rates which statistical evidence has shown to exist among persons living in crowded and unventilated rooms. The report of the Commissioners appointed to inquire into the regulations affecting the sanitary condition of the British Army, properly lays great stress upon the fact that in civilians at soldiers' ages in 24 large towns the death rate per 1000 was 11.9, while in the foot guards it was 29.4, and in the infantry of the line 17.9; and shows that this difference was mainly due to diseases of the lungs occurring in soldiers in crowded and unventilated barracks. These observations have since been repeatedly confirmed by statistics derived from other armies, from prisons, and from the death rates of persons engaged in different occupations, and in all cases tubercular disease of the lungs and pneumonia are the diseases which are most prevalent among persons living and working in unventilated rooms, unless such persons are of the Jewish race.

But consumption and pneumonia are caused by specific bacteria, which, for the most part, gain access to the air passages by adhering to particles of dust which are inhaled, and it is probable that the greater liability to these diseases of persons living in crowded and unventilated rooms is, to a large extent, due to the special liability of such rooms to become infected with the germs of these diseases. It is by no means demonstrated as yet that the only deleterious effect which the air of crowded barracks or tenement house rooms, or of foul courts and narrow streets exerts upon the persons who breathe it, is due to the greater number of pathogenic microörganisms in such localities. It is possible that such impure atmospheres may affect the vitality and the bactericidal powers of the cells and fluids of the upper air passages with which they come in contact, and may thus predispose to infections the potential causes of which are almost everywhere present, and especially in the upper air passages and in the alimentary canal of even the healthiest persons; but of this we have as yet no scientific evidence. It is very desirable that researches should be made on this point.

10. The discomfort produced by crowded, ill-ventilated rooms in persons not accustomed to them is not due to the excess of carbonic acid, nor to bacteria, nor, in most cases, to dusts of any kind. The two great causes of such discomfort, though not the only ones, are excessive temperature and

unpleasant odors. Such rooms as those referred to are generally overheated; the bodies of the occupants, and, at night, the usual means of illumination, contributing to this result.

The results of this investigation, taken in connection with the results of other recent researches summarized in this report, indicate that some of the theories upon which modern systems of ventilation are based are either without foundation or doubtful, and that the problem of securing comfort and health in inhabited rooms requires the consideration of the best methods of preventing or disposing of dusts of various kinds, of properly regulating temperature and moisture, and of preventing the entrance of poisonous gases like carbonic oxide, derived from heating and lighting apparatus, rather than upon simply diluting the air to a certain standard of proportion of carbonic acid present. It would be very unwise to conclude, from the facts given in this report, that the standards of air supply for the ventilation of inhabited rooms, which standards are now generally accepted by sanitarians as the result of the work of Pettenkofer, De Chaumont and others, are much too large under any circumstances, or that the differences in health and vigor between those who spend the greater part of their lives in the open air of the country hills and those who live in the city slums do not depend in any way upon the differences between the atmospheres of the two localities except as regards the number and character of microörganisms.

The cause of the unpleasant, musty odor which is perceptible to most persons on passing from the outer air into a crowded, unventilated room is unknown. It may in part be due to volatile products of decomposition contained in the expired air of persons having decayed teeth, foul mouths, or certain disorders of the digestive apparatus, and it is due in part to volatile fatty acids produced from the excretions of the skin and from clothing soiled with such excretions. It may produce nausea and other disagreeable sensations in specially susceptible persons, but most men soon become accustomed to it and cease to notice it, as they will do with regard to the odor of a smoking car or of a soap factory after they have been for some time in the place. The direct and indirect effects of odors of various kinds upon the comfort, and, perhaps also, upon the health of men are more considerable than would be indicated by any tests now known for determining the nature and quantity of the matters which give rise to them.

The remarks of Renk upon this point merit consideration.

Cases of fainting in crowded rooms usually occur in women, and are connected with defective respiratory action due to tight lacing or other causes.

Other causes of discomfort in rooms heated by furnaces or by steam are excessive dryness of the air and the presence of small quantities of carbonic oxide, of illuminating gas, and, possibly, of arsenic, derived from the coal used for heating.

AMERICAN METROLOGICAL SOCIETY.

THIS Society held its annual meeting at Columbia College, on April 22d, at 3 P. M.

The President, B. A. Gould, of Cambridge, Mass., presided. There were present, Wolcott Gibbs, of Newport, R. I.; A. A. Michelson, of the University of Chicago; T. Egleston and J. H. Van Amringe, of Columbia College; T. R. Pynchon, of Trinity College; T. C. Mendenhall, of Worcester, Mass.; George Eastbourn, of Philadelphia; J. M. McKinlay and J. K. Rees, of New York City.

President Gould made an informal address, and called attention to the rapid progress of the zone standard-time system throughout the world. This system the society did important work in introducing. Allusion was made to the report that Turkey had made the Metric System obligatory. The principal countries that do not use the Metric System are England, the United States and Russia. Through the action of the New Decimal Association of England, and of the American Metrological Society, it was hoped that some steps might be taken in the two countries named which would bring about a larger use of the Metric System. It was stated that Utah proposed to adopt the Metric System as the standard when she was admitted to statehood.

The society appointed an important committee on Metric Gauges. This committee consists of the President, B. A. Gould, Wolcott Gibbs, T. C. Mendenhall, A. A. Michelson, and T. Egleston as chairman.

Reports were made by various officers and the following officers were elected for the year 1895–96: President, B. A. Gould, Cambridge, Mass. Vice Presidents, Wolcott Gibbs, Newport, R. I.; T. R. Pynchon, Hartford, Conn.; Sandford Fleming, Ottawa, Canada; T. C. Mendenhall, Worcester, Mass.; T. Egleston, New York City; J. H. Van Amringe, New York City. Treasurer, John K. Rees, New York City. Recording Secretary, John K. Rees, New York City. Corresponding Secretary, O. H. Tittmann, Washington, D. C. Members of the Council, H. A. Newton, New Haven, Conn.; Cleveland Abbe, Washington, D. C.; R. H. Thurston, Ithaca, N. Y.; A. M. Mayer, Hoboken, N. J.; Henry Holt, New York City; W. F. Allen, New York City; Simon Newcomb, Washington, D. C.; S. P. Langley, Washington, D. C.; F. H. Smith, University of Virginia; George Eastbourn, Philadelphia, Penn.

Edward Atkinson, of Boston, was elected a member of the society.

In this connection it may be of interest to state the objects of this society:

1. To improve existing systems of weights, measures and moneys, and to bring them into relations of simple commensurability with each other.

2. To secure universal adoption of common units of measure for quantities in physical observation or investigation, for which ordinary systems of metrology do not provide; such as divisions of barometer, thermometer, and densimeter; amount of work done by machines; amount of mechanical energy, active or potential, of bodies, as dependent on their motion or position; quantities of heat present in bodies of given temperatures, or generated by combustion or otherwise; quantity and intensity of electro-dynamic currents; aggregate and efficient power of prime movers; accelerative force of gravity; pressure of steam and atmosphere; and other matters analogous to these.

3. To secure uniform usage as to standard *points of reference,* or physical conditions to which observations must be reduced for purposes of comparison, especially temperature and pressure to which are referred specific gravities of bodies, and the zero of longitude on the earth.

4. To secure the use of the decimal system for denominations of weight, measure, and money derived from unit-bases, not necessarily excluding for practical purposes binary or other convenient divisions, but maintained along with such other methods, on account of facilities for calculation, reductions, and comparison of values, afforded by a system conforming to our numerical notation.

MODES OF OPERATION.

1. THE society will endeavor to carry out its objects, by appeals to Congress, State Legislatures, boards of education, higher institutions of learning, and to directors and

teachers of schools of every grade throughout the country, urging adoption of measures in their several spheres for diffusing information as to the present state of the world's metrology and recent progress in its reform, and specially for instructing the rising generation in these matters, to the end that our people may be early and fully prepared to act intelligently on the important questions connected with weights and measures.

2. By invoking the aid and coöperation of bodies organized to consider questions of scientific or social interest, boards of trade, chambers of commerce, societies of engineers, industrial associations, professions and trades, in this country and elsewhere.

3. By specially urging scientific bodies to open communications with similar bodies in other countries, with a view to general agreement on values to be henceforth uniformly given to units of measure and points of reference which particularly concern them. *i. e.*, to the so-called constants of science.

4. By memorializing Congress in favor of laws requiring the use, in certain departments of the public service, of metric weights and measures, wherever such legislation may tend to relieve commerce of some of its burdens, to facilitate international communication, to promote international jurisprudence, and to familiarize our own people with the benefits of that system of metrology, with the least interference with their ordinary habits of thought or daily business.

5. By direct appeals to the people through the public press, and by circulating, so far as means allow, books and documents informing the public of the defects of the common system of weights and measures, the means most proper for its amendment, and the great advantages which the acceptance of a universal system would insure to all mankind.

J. K. R.

THE INTERNATIONAL MATHEMATICAL CONGRESS.

Professor A. Vasiliev, President of the Physico-mathematical Society of Kasan, Russia, has sent me a document prepared by him for the Minister of Public Instruction, with a request that I translate such part of it from the Russian as bears on the founding of an International Mathematical Congress, and make it known in America. This is in substance as follows:

After recapitulating the action of the French Association for the Advancement of Science at Caen (August 14, 1894) [already translated by me and published on pp. 21–22 of the Bulletin of the American Mathematical Society, October, 1894], he gives the resolution offered by me that very same day, August 14, 1894, for their signatures to all the members of the American Mathematical Society present at the Brooklyn meeting, and signed unanimously, which was as follows: "The undersigned members of the American Mathematical Society present at its summer meeting, 1894, take this method of expressing their cordial approval of a series of International Congresses of Mathematicians to take place from time to time, as suggested by A. Vasiliev and C. A. Laisant." The names of the signers may be found on page 290 of Vol. I., of the American Mathematical Monthly. I explained the plan as contemplating a *réunion préparatoire* at Kasan in 1896, a *congrès constituant* in Belgium or Switzerland in 1897, which perhaps might fix the First International Congress at Paris in 1900.

Professor Vasiliev then goes on to state the decisive step taken by the *deutsche Mathematiker-Vereinigung* in a reunion at Vienna, September, 1894. It was there unanimously resolved to take part in the organizing Congress. The action was as follows:

"Concerning future International Congresses, the Mathematiker-Vereinigung decides in principle to participate, and charges its bureau to take in regard to this subject the measures that appear necessary. In particular, it leaves to each of its members entire freedom, considering alone as essential that the Society, on this important occasion, may be assured of having the place due it."

Professor Vasiliev expects that the inauguration of the Lobachèvsky monument at Kasan will take place in August or September, 1896, and counts on having there a large number of eminent mathematicians, and will profit by the occasion to propose definitely the organization of the International Congress, and then official calls will be issued to meet for the purpose of final organization in 1897 at a city of Belgium or Switzerland.

GEORGE BRUCE HALSTED.

AUSTIN, TEXAS.

CURRENT NOTES ON PHYSIOGRAPHY (V.).

THE EXTINCT LAKE PASSAIC.

THE annual report of the Geological Survey of New Jersey for 1893 contains a long report on surface geology, in which there is an interesting chapter on Lake Passaic, an extinct glacial lake, by R. D. Salisbury and H. B. Kümmel. First mentioned by Professor Cook in his annual report for 1880, Lake Passaic is now carefully traced by its shore lines and the deltas built in it by streams. Its basin was limited on the west by the slope of the crystalline highlands; on the south and east by one of the curved trap ridges of the Watchung or Orange mountains; while on the north it was enclosed by ice. Most remarkable of all the shore deposits in the lake waters is the great morainic embankment that was built across the basin from Morristown to Madison during the furthest advance of the ice sheet into the lake waters: the lobate front of this bank standing up with great distinctness north of a marshy plain, which now represents part of the lake bottom.

The outlet of the lake was, for a time at least, by a notch in the trap ridge near its southern end, at a height of 331 feet above sea level. Twenty-five miles to the north, the records of the lake level now stand sixty-seven feet above the lowest shore line at the southern end of the basin. Many details of interest are considered in the report; none more surprising than the depth of the drift-filling in the notch of one of the trap ridges at Summit (where the Morris and Essex Railroad crosses the ridge), from which a preglacial discharge of the inner valley at this point is fairly inferred. An excellent map accompanies the report.

LOCAL DISPLACEMENT OF THE MISSISSIPPI.

THE annual report of the Iowa geological survey for 1893, just issued, contains a chapter by C. H. Gordon on a former channel of the Mississippi, now filled with drift. The modern river has cut a narrow rock-bound gorge, five miles to the east of the former valley, and about ten miles long; its lower end being at Keokuk, where the Des Moines river comes in from the west. A general study of the surface and the records of a deep well indicate that the earlier valley was about three times as broad and twice as deep as the new gorge. The gorge being hardly more than in its youth, the earlier valley was certainly not advanced beyond its early adolescence. It therefore clearly indicates that during only a comparatively short preglacial time did the region stand as high as or a little higher than now; most of its preglacial history must have been passed at a less elevation above baselevel. To speak of the preglacial channel as a 'measure of vast denudation' (p. 250) therefore seems somewhat inappropriate; it was only the beginning of a denudation that could in a geographical sense be called vast. The vast denudation is more really shown in the stripping of an unknown thickness of strata from the region, thus preparing the general surface in which the adolescent preglacial valley was eroded.

The relation of displacements of this kind to the location of settlements along the river and to the choice of places for bridge-building across it, would furnish material for an interesting physiographical essay, extending the well-known report by Gen. Warren. The outline map on which the old and new courses of the river are represented, is unfortunately without names, making the careful reading of the chapter a difficult matter for those unacquainted with with such places as Fort Madison and Sand Prairie.

W. M. Davis.

Harvard University.

CURRENT NOTES ON ANTHROPOLOGY (VII.).

RUNIC INSCRIPTIONS IN EASTERN AMERICA.

It is well known that venturous Norwegian navigators in the eleventh century visited at divers times the eastern coast of North America. The ancient sagas of Iceland which narrate the events of these voyages are provokingly meager and obscure; so that it has been quite impossible to decide how often such voyages were made, or how far south the explorers advanced. Of course, it is to be supposed that of some such expeditions we have no account whatever.

The late Professor E. N. Horsford persistently maintained that positive evidence of a pre-Columbian European settlement on the Charles river, Mass., had been discovered by him. The testimony he presented did not convince many, and his daughter, Miss Cornelia Horsford, has done well to pursue and extend the lines of investigation which her father began. The results are said to be confirmatory of his theory, but the only one which has as yet been made public is a neatly illustrated, privately printed pamphlet, of 22 pages, entitled 'An Inscribed Stone,' By Cornelia Horsford (Cambridge, 1895).

The stone referred to was discovered at Weston, Mass., in an uncultivated field, and came under Miss Horsford's notice merely by accident. One of its sides bore a partly obliterated series of lines which Mr. J. B. Woodsworth, of the U. S. Geological Survey, pronounces to be of artificial origin. They are arranged after the manner of a runic futhorc, and simulate certain forms of such writing. Miss Horsford does not offer an interpretation.

A second inscribed stone near New York city is depicted, the runes on which Miss Horsford both transliterates and provisionally translates as referring to a census of the inhabitants by the church officials.

On a loose sheet a large number of runic and ogham inscriptions from Great Britain, the north of Europe and Greenland are given for the purpose of comparison.

The publication is one well worthy the attention of historians.

WHERE WAS THE GARDEN OF EDEN?

We have not yet done with seeking on the earthly plane the pristine Paradise, Eden, 'the land of joy'.

The latest explorer of its whereabouts is the distinguished Professor Paul Haupt, of the Johns Hopkins University, in an article, 'Wo Lag das Paradies?" in the 'Ueber Land und Meer,' No. 15, 1895. He differs from Friedrich Delitsch, who, in his work with the same title, asserted that the description of the locality in Genesis applied directly to the canal and river system of Babylonia; he differs from himself in his opinion as expressed in a paper published last year in the proceedings of the American Oriental Society, and concludes that the four rivers mentioned in the Hebrew record, the Pison, the Gihon, the Hiddekel and the Euphrates, are, reversing the order, the Euphrates, the Tigris, the Karun and the

Kercha. The two latter are small streams flowing, one into the Persian Gulf, and one into the Schott el Arab, near the ancient mouth of the Tigris, both east of it.

Though Professor Haupt supports his opinion with his customary depth of erudition, I doubt if it will be adopted. That part of Genesis was written by the Hebrew author about 650 B. C., and at that period he certainly knew what he was talking about when he mentioned the Gihon and identified it with the river Nile. Professor Haupt's former theory, which recognized this, seems much more plausible.

But all such theories do not touch the kernel of the question. The myth of the Paradise, watered by its four streams, is found in native American mythologies as prominently as in those of the Old World; and no explanation is valid which does not apply to both continents.

The true interpretation is that the four streams refer to the four cardinal points and the four winds, the rain bringers. They are the cosmic and celestial causes of the weather and its changes, and hence of fertility and growth. It were easy to prove this by abundant examples. The Hebrew realist merely endeavored to transport the ancient myth into terrestial geography.

D. G. BRINTON.

UNIVERSITY OF PENNSYLVANIA.

JAMES D. DANA.

WE cannot pay a tribute to the memory of Dana more appropriate than the letter addressed to him by a number of his older colleagues on his eightieth birthday and communicated by Prof. George P. Fisher to the *Evening Post.*

NEW HAVEN, February 12, 1893.

DEAR PROF. DANA: Having had the privilege for many years, of being associated with you as colleagues at Yale, we wish to bring you our cordial congratulations on the occasion of your eightieth birthday.

It gives great pleasure to your friends that after so extended a period of incessant and most faithful activity you are still able with unimpaired mental vigor to carry forward the studies which have contributed so much to the advancement of science and have conferred so great distinction, not on yourself alone, but equally on the University and on the country.

We recall the circumstance that it was only four years after your graduation, in 1833, that the first edition of your work on mineralogy, a work which has remained a classic to this day, was issued. Two years later you embarked on the voyage of discovery, undertaken under the auspices of the government by the American Exploring Expedition, and during four industrious years collected the materials for the subsequent reports on geology, mineralogy, corals and crustacea, which established your reputation at home and abroad as a scientific man of distinguished ability.

It is now well-nigh half a century since you entered upon your labors as an editor of the *American Journal of Science*, your name having first appeared on the title-page of the journal in 1846. The long series of volumes of this periodical are a noble monument of the extent and thoroughness of your labors as a naturalist.

It is in truth surprising that in connection with this continuous employment and with your work as professor you have been able to send forth from the press, in successive editions, the elaborate text-books and other writings, the solid excellence of which is everywhere recognized.

We cannot revert without admiration to the universally broad field of scientific investigation in which you have maintained your place as an acknowledged master.

It would be a signal achievement for any man to hold this position as regards geology, and the branches of zoölogy connected with it; but when, as in your case, the science of mineralogy is added to the list, the eminence which you have attained is quite exceptional.

It is gratifying to know that your services to the cause of science have obtained full recognition from teachers and students of science and from learned bodies in all civilized countries. None will question that the honors which have thus been so abundantly bestowed and so modestly received are well deserved. The consciousness that the motive of your researches has been an unalloyed love of truth and an unselfish desire to enlarge the bounds of human knowledge must give to these testimonials all the value that such marks of honor can ever possess. We congratulate you that your academic relations both with fellow-professors and with pupils have been so uniformly pleasant. The classes which, in long succession, have listened to your instructions, could their voices be heard, would unite in expressions of sincere respect both for the qualities of character and for the talents and learning of their revered instructor. But it is no part of our purpose to enter into a detailed statement of the reasons which render it peculiarly agreeable for us, your old friends and neighbors, to offer to you to-day our heartfelt congratulations. Had it been thought worth while to extend the list of subscribers to this letter, no doubt all the members of the teaching body in the University would gladly have added their names.

But our communication is simply intended as an expression, from a few of your older associates, of interest in this anniversary and of our earnest hope that the blessing of a kind Providence may continue to be with you and with the members of your family.

Very sincerely yours,

Timothy Dwight, George E. Day, George P. Fisher, George J. Brush, William H. Brewer, O. C. Marsh, Franklin B. Dexter, Edward E. Salisbury, William D. Whitney, Hubert A. Newton, Samuel W. Johnson, Daniel C. Eaton, A. E. Verrill, Addison Van-Name, Sidney I. Smith.

CORRESPONDENCE.

THE DISTRIBUTION OF SLEDGES, ETC.

Did anybody ever read or hear of sledges, snowshoes or goggles for the eyes in aboriginal South America? I have traced the skee entirely across Asia, the netted snow shoe from the Amur around to Klamath river, Cal., with extension throughout Canada, New England and our northern tier of States. The ice creeper for the foot covers the region of my migration track from southern Kamchatka around to the Yukon. The built-up sledge is everywhere in the Hyperborean area of two hemispheres, the form depending on the exigencies of timber growth. The great broad skee or snow shoe of the Amur is the flat toboggan of the Dominion of Canada.

Otis T. Mason.

U. S. National Museum, April 20.

SCIENTIFIC LITERATURE.

Memoir of Sir Andrew Crombie Ramsay. By Sir Archibald Geikie, Director of the Geol. Surv. of Great Britain and Ireland. London and New York, Macmillan & Co. 1895. Pp. x + 397.

This is really a charming book and ought to be read not only by every geologist, but by every one interested in the story of a noble life. Indeed, the memoir of such a man as Ramsay by such a writer as Geikie could hardly be otherwise than deeply interesting.

Ramsay's career overlaps on the one hand with the old heroic days of the founders of English Geology—Lyell, Buckland, Sedgwick, Murchison, De la Beche, etc., and on the other with modern times and modern methods. He shared with the former the enthusiasm of grappling with the great general problems of geology; but he himself did much to introduce and urge forward the more accurate methods, if less daring theories, of modern times. The story of his forty year's connection with the British Survey, first as assistant and then as local director for England under De la Beche, then as local director of England and Scotland under Murchison, and finally as Director General himself, is literally a history of the Survey itself. The book is illuminated too and its value enhanced by the pictures of all the principal men of the Survey, whose work every geologist knows, but whose faces are now perhaps seen for the first time.

The story of Ramsay's career is also in no small degree the history of the development of geological science in England. For in the beginning he sat at the feet of the geological Gamaliels, imbibing their spirit, and at the end he gathered about himself all the most ardent and progressive spirits and guided their course. Many modern ideas he himself initiated, while others he carried forward with his characteristic ardor.

In this connection it is interesting to note, in the history of science, the transfer of study from the *remote* to the *near at hand*, from the *abstract* to the *concrete* and often from the *obscure* to the *obvious*. Thus the field of study was Astronomy before Geology, the Science of the Stars before the Science of the Earth. So also it was dead things before living things, and man last of all. This is doubtless mainly due to the fact that the nearest things and things most closely connected with our highest interests are also the most complex and most difficult to reduce to law. But this is not all. There is a fascination in the remote, the hidden and the obscure which piques our curiosity, while we neglect phenomena which lie on the surface and which therefore seem common and trivial because we see them every day. The history of geology is an excellent illustration of this. The early geologists loved to speculate on the interior of the earth and its mysterious forces. Next rock strata, their positions, successions, foldings, faults, etc., engaged attention. In the meantime the surface configuration of the earth, mountains and plains, ridges and valleys, soils and underlying rock surfaces, in fact all the most obvious and obtrusive features were neglected. Now, the change from the study of interior structure alone to the study of surface configurations in relation to interior structures, one of the most fascinating branches of geology, took place during Ramsay's times, and he himself was one of the most active agents in bringing it about. From the first he was deeply interested in the agency of exterior forces as contrasted with interior forces; with destructive as contrasted with constructive agencies. Still later he became interested in the significance of soils and underlying rock surfaces. He it was, therefore, who first gave strong impulse to glacial geology in England. For the seed sown by Agassiz found, at first, but poor soil in England.

Again, it is instructive to note also the effect of physical environment on the course of geological science. The incessant beating of waves on the limited shore line of the 'tight little sea-girt island' of Great Britain, and the ravages produced by these attacks on some parts, early impressed the minds of British geologists with a strong sense of the *power of the sea*. In the study of erosion, therefore, all the early geologists, Ramsay among the number, attributed far too much to marine denudation, while rain and rivers were almost neglected as being of little importance in comparison. It was apparently for the same reason that the iceberg theory of glaciation took so firm a hold and was so hard to displace in England. It was only by travel on the continent of Europe, and especially in the Alps, that Ramsay was led to appreciate the great importance of rain and rivers, as compared with the sea, as a land-destroying and land-sculpturing agent; and of land ice as compared with floating ice as a glaciating agent. But his ardent, candid nature knew no half-measures. His conversion was complete, and some think that he even carried his later views on this subject somewhat too far.

The *work* of Ramsay is well known to geologists. But the readers of Science are not all geologists. It may be well therefore to briefly mention some of the main points on which he contributed to geological knowledge or modified the course of geological thought.

His greatest direct contribution to geological knowledge is undoubtedly that embodied in his admirable map of Wales. The problem of Wales had been attacked successively by Sedgwick, Murchison and De la Beche. But the work of the older geologists was far too cursory. Nothing but the most careful foot-by-foot mapping could unravel its intricate structure. This was first done by Ramsay, and he devoted a large portion of life to its completion. His map is a monument of industry combined with rare geological insight.

Again, he was undoubtedly one of the founders of the study of geographical forms in relation to geological structure. Surely this is one of the most fascinating departments of geology (or of geography, for it may be claimed by both). It is this which constitutes the chief charm of his admirable work on the 'Physical Geology and Geography of Great Britain.'

Again, he was the originator of the idea of other possible glacial periods in the history of the earth and especially of glaciation in Permian times. His ardent uniformitarianism naturally led him in this direction.

Again, finally, he was the originator of the doctrine of the origin of lake basins by glacial erosion. It is possible that in the enthusiasm of the originator, he may have carried this idea a little too far; but it is a misrepresentation to say, as has been done, that he attributed *all* lake basins to this cause. His original paper was entitled 'Origin of *Certain* Lakes by Glacial Erosion.'

So much for Ramsay the geologist. But the greatest charm of the book is found in the vivid picture it gives of Ramsay the man; his intense interest in life in all its phases and in literature in all its departments; his large human sympathy, embracing alike all true men from the rudest country people in their sport and dances to the most eminent scientists in their discussions; his deep love of art, poetry and music; his ardor of temperament, showing itself alike in the intensity of his work and in his keen enjoyment of fun and frolic. I never saw Ramsay but once, viz., at the Montreal meeting of the A. A. A. S. in 1857, when he was in his prime. I remember well on the occasion of a geological excursion in the vicinity the rapid, eager way in which he scrambled over the rocks, hammer in hand, firing all of us with his own enthusiasm. Is it any wonder that he wore himself out prematurely? Although he lived to 77, yet he resigned and quit work ten years earlier, and was already an old man at 63.

In closing this brief account of Ramsay, I cannot do better than quote the closing words of the memoir itself, "But above and beyond the impress of his scientific achievements, Sir. Andrew Ramsay's high positon among his contemporaries was largely determined by his individual personality. His frank, manly bearing, his well-cut features beaming with intelligence and a sweet childlike candor, his ready powers of conversation, his wide range of knowledge, his boyish exuberance of spirits, his simplicity and modesty of nature, his sterling integrity, perfect straightforwardness and high sense of duty, his generous sympathy and untiring helpfulness, marked him out as a man of singular charm and endeared him to a wide circle of friends who, while they admired him for his genius, loved him for the beauty and brightness of his character."

But I cannot close this notice without a final word concerning the memoir itself as a work of art. What we wish to know of great men is not only their achievements, but also all, even the trivial details of their daily life; for these, more than aught else, show character. All things, great and small, must be brought together into a living whole. This Geikie has done in a masterly way. Journals of petty daily occurrences, narratives of more continuous work, discussion of important scientific problems, letters on all kinds of subjects to all sorts of people, some full of weighty scientific matters, some full of fun and jokes and humorous verse, some full of deepest filial or conjugal affection—all these are skillfully woven into a vivid picture of the man as he really lived. Happy is the man who shall have such a biographer.

JOSEPH LE CONTE.

A Text-Book of Invertebrate Morphology. By J. PLAYFAIR McMURRICK, M. A., Ph. D. New York, Henry Holt & Co. 1894.

In preparing this book the author has followed the zoölogical method, and has given us a succinct though general account of the morphology of the different 'types,' classes and orders of the animal kingdom; no special forms under each being described.

Speaking of the word 'type,' we much prefer the older terms, branch, sub-kingdom or phylum, to the the rather meaningless word 'type;' the first and last terms being naturally suggested from the evolutional point of view, the main sub-divisions of the animal genealogical tree being more naturally referred to as branches or phyla. The increase in the number of 'types' from eight to twelve results from dividing the Vermes into several, such as the Platyhelminths, Nemathelminthes and Annelida, which the author regards as of the same rank as the Mollusca. The Arthropoda also, somewhat prematurely, we think, are divided into three types, viz.: Crustacea, Arachnida and Tracheata. That the division is somewhat artificial is indicated by the fact that Limulus is assigned to the Crustacea, though placed in an appendix, whereas it is plainly neither a genuine Crustacean nor a true Arachnidan, and belongs to an independent phylum. And then if we begin thus to manufacture 'types' out of the Arthropoda and out of the Vermes, we can scarcely end at the point the author reaches.

In agreement with some German authors, the Echinodermata, written Echinoderma, are interpolated between the highly specialized Tracheata and the Protochordata. This seems to us in a text-book of this sort a shade objectionable, when we consider how closely allied to the lower worms, both in embryology and in some points in their adult structure, Echinoderms are. Of course this is a matter of individual opinion, but we should look for some expression of the reasons why they are placed so far away from worms, in a situation between such closely circumscribed and specialized groups as insects, and the Chordata. If the position assigned the Echinoderms is due solely to the resemblance of the Tornaria larvæ of Balanoglossa to the larvæ of Echinoderms, this seems a rather slight reason.

While the descriptions of the types and classes are evidently clear and accurate; though not always presented in simple Saxon words, the salient points of resemblance or difference do not seem in all cases successfully brought out. Thus in writing of the Brachiopoda the author speaks of the bivalved shell, 'similar to that of a bivalve mollusk,' but he does not add that the shells are dorsal and ventral, a point in which they differ from any mollusk. On p. 271 it is stated that eyes do not occur in these animals, meaning, of course, the adults, though on the next page the young Argiope is credited with eye-spots ; the fact, however, that they occur in the larva of Thecidium not being mentioned. In the bibliography the papers of Morse on the development of Terebratulina and of Kowalevsky on Argiope, Thecidium, etc., are omitted, although the lower half of the page is left blank, and there was abundant room for the titles.

The treatment of the mollusca is in some respects unsatisfactory, though the anatomical details appear to be correctly and carefully stated. We should decidedly differ from the view that Lamellibranchs, or Pelycypoda, as it is now the fashion to call them, though the name is not nearly so apt or generally applicable as the older term, are intermediate between the Gastropoda and Ceptalopoda. They have no head, and it seems much more natural to suppose that they have more or less directly descended from the Amphineura. The position assigned them by Gegenbaur, next above the last named group and below the Cephalophora, seems to us to be a more natural one. And speaking of the last named group, it is a pity that there should not be more figures of these obscure generalized forms, especially of the ladder-like nervous system of the different genera to show their relationship to Chiton, though the discussion of their affinities is excellent. In speaking of the Gastropods the use of the clumsy German term 'visceral hump' seems objectionable ; we should prefer to call it the visceral mass. The visceral 'hump' in a Cephalopod is in reality all of the body behind the head.

The definitions or diagnoses of the subdivisions of the 'types' placed at the end of each chapter are too brief or defective and not always, it seems to us, happily worded. In those of the Gastropoda and Cephalopoda, the fact that they have a well differentiated head is not mentioned, though the 'visceral hump,' if the student clearly understands what that is, is said to be well developed.

The same lack of completeness applies to the diagnoses of the Crustacea, and particularly to those of the insects, while those of the Arachnida are much better.

The Tracheata (myriopods and insects), as in some other recent works, are not treated with such detail and thoroughness, nor in the case of the present book, so carefully and accurately as the Crustacea. It appears to be wholly a compilation, and not the result of autoptic study. This is not the case in Siebold's excellent Anatomy of the Invertebrates, which, though published forty years ago, is still for Tracheata useful and reliable. Our author's account of the anatomy of insects is somewhat faulty and needs revision in numerous places.

The spiral band of the trachea is said to extend along the tube, whereas it is not continuous, but varies much in length and makes from one to four or five turns, a single tracheal branch thus having many such disconnected spiral bands.

The olfactory organs of the antennæ are not setæ alone, but the pits to which the auther does not refer are far more numerous. The elements of the ovipositor are not situated on the 'last abdominal segment' (p. 414), while the cerci (p. 489) are not regarded by the author as equivalents of the jointed appendages, though they are obviously so, whatever may be said of the parts of the ovipositor. It is also a question whether the 'spring' of Collembola is not the homologue of the legs.

It is rather venturesome to say that in

butterflies and Diptera the thoracic segments seem to be reduced to two, etc., when three segments are easily observed. Vestigial mandibles are attributed to the sphinx, though the structures so called have been shown by Walter not to be such.

The chapter on the Protochordata is well prepared and illustrated. Why, however, Rhabdopleura and Cephalodiscus are, without apparent hesitation, regarded as belonging in this type, should, we think, be carefully explained, the chordate features being so slight compared with those of the Enteropneusta. One also is somewhat startled to find Amphioxus included in a work on invertebrate morphology when its structure and embryology associate it so intimately with the Chordata; and why it should be regarded as a lower or more generalized type than the Tunicata we do not understand. It has been the nearly universal opinion of anatomists that the lancelet is nearer to vertebrates than are the ascidians.

The figures are mostly diagramatic, and carefully drawn, though often coarsely so. We should have preferred, in many cases, exact and not schematic representations. The figures of *Buccinum undatum*, as regards the shell, reminds us more of a Strombus; and the figure of Nautilus should have been credited to Owen; several of the figures are credited to Leunis, and not to the original author or artist. The style cannot always be said to be simple and clear; the tendency being towards the use of long words requiring close attention in the beginner. The typography is fair and there is a praiseworthy absence of typographical errors. But whatever we have said by way of criticism, we desire to commend the book as excellent in its general plan and treatment, usually reliable, and forming a useful manual of the subject.

A. S. PACKARD.

BROWN UNIVERSITY.

The Land-Birds and Game-Birds of New England. By H. D. MINOT. 2d edition, edited by William Brewster. Houghton, Mifflin & Co., April, 1895, 8°, pp. xxiv+492, outline figures. Price, $3.50.

Eighteen years have passed since the first appearance of Minot's 'Land-Birds and Game-Birds' (published in February, 1877). It had a good sale and was soon out of print. Practically the whole book was original—the descriptions of the birds, nests and eggs, and the biographies. The latter are based on the author's own field experience and are interesting, truthful, and in the main well written.

The body of the work is followed by an appendix comprising a bird calendar for eastern Massachusetts, and keys to the Land Birds of New England and the eggs of Massachusetts birds. These keys are based primarily on color and are not likely to prove of much value.

The personality of the author deserves a word. When only a boy of seventeen he had amassed a large quantity of field notes and had written the book now under review. As the editor of the new edition says in his preface: "The author had a clear head, a true heart, and a well-defined purpose, combined with an amount of literary taste and ability very rare in one so young. He was deeply in earnest, full of warm yet reverential love of nature, wholly unconscious of or indifferent to certain conventional methods of investigation and expression, yet in the main careful in observation, temperate of statement, and singularly logical and dispassionate in argument." In his thirtieth year he was chosen President of the Eastern Railroad in Minnesota, and soon after lost his life in an accident on another road.

The new edition is accompanied by a portrait of the author and is an attractive, well-printed volume. The editor, William Brewster, tells us that his 'editorial touches have been of the lightest.' He has substituted current nomenclature for the old, and has added numerous foot-notes, always over his own initials, amplifying or correcting statements made in the body of the work, which has been allowed to stand essentially as in the original edition. Mr. Brewster has also added an appendix comprising additions to Minot's list and containing an abstract of the results of his study of the gyrfalcons—a most perplexing group. He agrees with Ridgway in the number and nomenclature of the forms, and records the authentic New England specimens of each.

It is a great compliment to the worth of Minot's book that one of the most eminent of American ornithologists, and one who could ill spare the time from his own important work, was willing to edit it.

C. H. M.

The Central Nervous System of Desmognathus fusca. By Pierre A. Fish. Reprinted from Journal of Morphology, x, 1, 1895.

Mr. Fish has made an important contribution to our knowledge of the brain of salamaders. His preliminary remarks embrace two statements of interest: (1) That the adult *Desmognathus fusca* lives equally well in the open air or wholly under water, even where no trace of lungs exists; and (2) that the mouth cavity and esophagus are lined with ciliated columnar epithelium. During arial respiration the floor of the mouth is alternately raised and lowered very rapidly, while when the animal was kept under water it was raised and held in that position a long time; the inference being that the blood is oxygenated by means of the epithelium of the mouth.

The simplicity of the amphibian brain renders it, as the author states, "a most admirable object for the study of morphological relations; its general absence of flexure, its successive segmental arrangement and the degree of exposure and differentiation of these segments, give it a great advantage over most other generalized forms." It was found to be remarkable for the large number of 'embryological' features preserved.

About 40 pages are devoted to the brain and cranial nerves, and the paper is accompanied by a bibliography and four plates.

C. H. M.

Introduction to Botany. By Volney M. Spalding, Professor of Botany in the University of Michigan. Boston, D. C. Heath & Co. 1895. Pp. 287.

Professor Spalding has added to his valuable book that which was needed to make it complete, namely, a full glossary, an index, a brief chapter on the organs of flowerless plants, and a chapter on fungi. These added chapters are in keeping with the general plan of the book. The material required is briefly indicated and directions given for its care. Laboratory directions, brief notes directing the student's attention to prominent features, follow. These are extremely good, and it is hoped this feature of Spalding's method of studying plants, corresponding, as it does, with Dodge's method in biology, will be pursued by future makers of text-books, and that we have seen the last of full accounts of what is to be seen, requiring on the part of the student very little thought, and only the attention necessary for the verification of the statements. It is remarkable, when one stops to think of it, how little the inductive method is used in the study of biology. After the directions, comes a little review or summary, giving information not likely to be attained from laboratory practice. This is a very marked feature of the volume and is especially valuable because the information given is so up to date. A very slight examination of the foot-notes will reveal the fact that the very latest research work has been consulted in the preparation of this text-book.

Since the book is only a year old, and since its title is rather misleading, it may not be out of place here to give a short account of it. Its strong point is that along with the study of the morphology of the seed, the root, the stem, the leaf, the flower, the fruit, there is an excellent course of physiological work indicated. Indeed, the whole subject is discussed on the life side, and, although in spite of its title, it is a book adapted to the needs of rather advanced students, yet such a student could easily adapt it to work even in primary schools, according to the most modern pedagogical ideas.

After the general discussion of the life history of the plant, follows a similar work with each of the natural group of flowering plants, the Algæ, Fungi, Mosses, Ferns, Equiseta, and the Club mosses, conducted on the same genaral plan. Then follow the Pines, the Monocotyledons and the Dicotyledons, a special point being made of the relationship of the orders to each other. In this, as in the physiology, a thorough knowledge of the latest thought on the subject is shown, and more than this, the knowledge is given to the student often in a much more logical and understandable way than by consulting the original sources.

Altogether it is the best of the modern text-books on the subject, both in matter and method, and is admirably adapted for use in colleges, either as a basis for advanced work or to give the undergraduate a good general knowledge of the subject.

W. P. WILSON.

UNIVERSITY OF PENNSYLVANIA.

NOTES AND NEWS.

FOSSIL VERTEBRATES OF ARGENTINA.

WE have recently received Part II. of the *Paleontología Argentina*, forming a continuation of the *Anales del Museo de la Plata*, published under the direction of Francisco P. Moreno, Director of the Museum. This sumptuous Memoir in royal quarto size consists of '*Contributions to a Knowledge of the Fossil Vertebrates of Argentina*, by R. Lydekker, in three parts covering the Dinosaurs and Cetacea of Patagonia and the Ungulates of the Argentine. The text is in English and Spanish in parallel columns, and is accompanied by thirty-two large plates which give us some conception of the superb collection of fossils in this Museum. In the first section the author describes the Dinosaurs from Patagonia belonging to Marsh's division of Sauropoda, which have not hitherto been described from South America. The agreement of some of these animals with the North American Dinosaurs seems to be strikingly close, so far as can be judged from Mr. Lydekker's description. The remains, however, are not well preserved. There are several plates principally illustrating the family Titanosauridæ. The Cetacea come from a marine deposit in the Territory of Chubet, and embrace especially three skulls which are far more complete than any of their European congeners and represent the Physodontidæ, Squalodontidæ, Argyrocetidæ and Platanistidæ. The most important section of the Memoir, however, is that relating to the extinct ungulates which are described from the superb collection in the La Plata Museum, belonging to the aberrant Toxodontia and Litopterna, besides the typical Artiodactyla and Perissodactyla. The author gives a clear and concise description of the principal characters of each family and of each genus, and has shown considerable skill and great clearness in matters of priority, for the confusion in South American palæontological literature and reduplication of terms is second only to that which prevails in our own country, and has arisen from the simultaneous and independent publications of Ameghino, Moreno and Mercerat. The author has not gone into the labyrinthine problems of specific priority, but has endeavored to clear up the genera with what appears to be considerable success. Palæontologists everywhere are placed under a great debt both to the author for his most timely review of these forms and to the Argentine Government for the liberal style in which these Memoirs have been published.

VARIATION IN CRABS.

The English monthly, *Natural Science*, under its recent change of publishers, has not lost any of the vigor which has characterized it since its establishment three years ago, and continues to be one of the most interesting of the reviews of progress in biology and geology which come before us. The general editorial attitude is that of entire independence of all traditional theories and authorities. There is shown no bias in the present evolution controversy, either towards the Darwinian or the Lamarckian side, but an impartial consideration of each. In the April number are some comments upon the recent discussion in the Royal Society of the facts brought out by Professor W. T. Weldon's extensive statistical investigation of variations in the shore crabs, from which we take the following: "Although Professor Weldon did not say so, it must have occurred to many listeners that this first result of statistical inquiry upon variation was in direct contradiction to those who asserted that variation is not a matter of 'chance,' but has its course in determined directions. . . . . His results have already established the importance of these methods, and we cannot doubt that wherever the methods are applied with discrimination equally important results will be obtained. . . . . . Pending such inquiry, he may be taken to have shown that there is a relation between selection and minute variation, not that selection operates upon minute variations."

It seems to us too early even to make such guarded inductions as these from these researches, for their significance is very largely diminished, if not completely destroyed by our absence of a knowledge of the conditions under which these seven thousand crabs developed. If the variations were due to congenital tendencies then their selection has a bearing upon the evolution problem, but if the variations were due to varying conditions of development, as is more than probable in a large percentage of cases, their selection has no bearing whatever upon the evolution problem. This is the uncertainty which vitiates this method, and is strangely overlooked by the editors of *Natural Science* as well as by others. None the less, this investigation is a step in the right direction towards a sound inductive basis for the solution of this most pressing biological problem of the day.

REGRESSION AND ORGANIC STABILITY.

Mr. Francis Galton (42 Rutland Gate, London W.) would be glad to receive information regarding:

(1) Instances of such strongly marked peculiarities, whether in form, in color or in habit, as have occasionally appeared in a single or in a few individuals among a brood; but no record is wanted of monstrosities, or of such other characteristics as are clearly inconsistent with health and vigor.

(2) Instances in which any one of the above peculiarities has appeared in the broods of different parents. In replying to this question, it will be hardly worth while to record the sudden appearance of either albinism or melanism, as both are well known to be of frequent occurrence.

(3) Instances in which any of these peculiarly characterised individuals have transmitted their peculiarities, hereditarily, to one or more generations. Especial mention should be made whether the peculiarity was in any case transmitted in all its original intensity, and numerical data would be particularly acceptable, that showed the fre-

quency of its transmission (*a*) in an undiluted form, (*b*) in one that was more or less diluted, and (*c*) of its non-transmission in any perceptible degree.

GENERAL.

AT a meeting of the secretaries of the Scientific Societies of Washington on April 18th, Hon. Gardiner G. Hubbard, President of the Joint Committee, presiding, it was decided to print in SCIENCE regular reports of the meetings of all the societies.

PHILADELPHIA has been selected as the place for the next meeting of the Society of American Naturalists. In conjunction with it will meet the affiliated societies—the American Morphological Society and the American Physiological Society, and probably the Geological Society of America, the Association of American Anatomists and the American Psychological Association.

PROFESSOR WOLCOTT GIBBS, President of the National Academy of Sciences, Professor Herman Knapp of Columbia College and Professor Hugo Münsterberg of Harvard University have been appointed an American committee to collect money for the memorial to Helmholtz to be erected in Berlin.

DR. LOUIS-FLORENTIN-CAMEIL died at Fontenay-sous-Bois on March 11th, at the great age of ninety-seven. He was for many years head physician of the Asylum for the Insane of Charenton, being the successor of Royer Collard and Esquirol.

MR. J. C. SUMNER, of the Royal College of Science, has been appointed Curator of the Port Erin Biological Station.

DR. JOHN FISKE gave, during April, at the Berkeley Lyceum, New York, a course of lectures on 'Lessons of Evolution in Relation to Man.'

THE 'Mazamas,' a society of mountain climbers organized in Oregon last year, propose sending by heliograph a message and reply from British Columbia to Mexico on July 10th. The coöperation of societies and individuals is requested in order that all the intervening mountain peaks may be occupied. Communications should be addressed to Mr. T. Brook White, Secretary, Portland, Oregon.

A NATIONAL ETHNOLOGICAL EXPOSITION will be held at Prague from May 16th to October 12th.

AMONG the papers read at the annual spring meeting of the Institution of Naval Architects on April 3d, 4th and 5th, at London, were 'Notes on Further Experience with First-class Battleships,' by Sir William White; 'On Solid Stream Forms,' by D. W. Taylor, U. S. Navy, and 'On the Method of Initial Condensation and Heat Waste in Steam Engine Cylinders,' by Professor R. H. Thurston.

MR. CHRISTOPHER HEATH, of University College, has been elected President of the Royal College of Surgeons, to fill the vacancy caused by the death of Mr. J. W. Hulke.

MR. HERBERT SPENCER has begun a new series of articles in *The Popular Science Monthly* for May. His general subject is 'Professional Institutions,' one of the divisions of his Synthetic Philosophy, and he will aim to show how each of the professions has been developed out of the functions of the priest or medicine-man.

THE New York Legislature has appropriated $16,000 for scientific work in horticulture. The work will be under the immediate charge of Professor L. H. Bailey of Cornell University.

THE Legislature of California has appropriated $250,000 to erect a building in San Francisco for the professional departments of the University of California.

THE international importance of the work done at the Columbia College Observatory in investigating the subject of variation of latitude has been recently indicated by an offer, from the Royal Geodetic Institute at Potsdam, of a considerable sum of money to be used in employing computers to reduce the results.

A society has been incorporated in the State of New York for the preservation of scenic and historic places and objects. Mr. Andrew H. Greene, to whom the movement is chiefly due, is president of the society, which includes among its trustees a number of the leading citizens of New York.

The University of Kansas will send into the field the present season five different scientific expeditions. Professor Dyche leaves the first of May to collect and study the birds and mammals of Greenland and adjacent regions; Professor Williston will have two expeditions for the collection of vertebrate fossils, one in Western Kansas and one in Wyoming; Chancellor Snow, it is expected will spend the summer in the Southwest with a party collectiug entomological specimens; a fifth party under Professor Haworth will be in the field during the next six months engaged in mapping the Tertiary outcrops of the State. The cost of the three geological expeditions is borne by special appropriations from the State Legislature.

Mr. Mark W. Harrington, Chief of the Weather Bureau, has issued a circular stating that a periodical is proposed, devoted to Climatology and its relation to health and disease, similar in size and general appearance to the monthly weather review. The coöperation is requested of sanitary boards and societies, and of individuals interested in this work.

The Italian Botanical Society met this year at Palermo on the 13th and 26th of April. The German Zoölogical Society will meet at Strasburg on the 4th to the 6th of June.

According to the *Zeitschrift für Luftschriftfahrt* and the *Revue Scientifique*, Herr Berson, on December 4, made the highest baloon ascent on record, attaining an altitude of 9,100 metres. The temperature at this altiitude was — 47.8° C. The highest temperature, 6.1° C., was at a height of 1,400 metres.

The death is announced of Dr. Peck, director of the Museum of Natural History in Gorlitz.

Among recent new appointments in Germany we note that Dr. Himstedt, professor of physics in Giessen, has been called to Freiburg ; Dr. Czermak, professor of ophthalmology in Innsbruck, to Prague, and Dr. Steinmann, professor of minerology in Freiburg, to Tübingen. Dr. Minkowski has been made professor of mathematics in Königsberg.

The mathematician, Dr. E. D. F. Meissel, died at Kiel, on March 11, at the age of sixty-eight years.

The Revue Scientifique of April 13th reports the speeches made at the banquet given in honor of M. Berthelot on April 4th. Speeches were made by MM. Poincaré, Brisson, Perrier, Richet, Zola and M. Berthelot himself.

Professor Ryder at the time of his death had nearly completed the MS. of a book, and left other scientific work of importance which will probably be published shortly under very competent editorship.

The Prince of Wales has formally presented to Sir Joseph Lister the Albert Medal of the Society of Arts for "the discovery and establishment of the antiseptic method of treating wounds and injuries, by which not only has the art of surgery been greatly promoted and human life saved in all parts of the world, but extensive industries have also been created for the supply of materials required for carrying the treatment into effect."

The American Naturalist for March contains illustrations of some remarkable forms of deep sea fishes dredged by the U. S. Steamer Albatross at depths varying from 700 to 1500 fathoms and recently described by Dr. G. Brown Goode and Mr. Tarleton H. Bean in the Proceedings of the U. S. National Museum. The genera have been named Hariotta, Rondletia and Cetomimus.

M. L'ABBÉ MAZE has communicated to the Paris Academy an account of the earliest meteorological observations made in France. They were carried out by the astronomer Boulliau from the 25th of May, 1658, to 19th of September, 1660. The winter was unusually cold, whereas April was warmer than in any recent year, excepting April, 1865. M. Maze also shows that Boulliau used a mercury thermometer 62 years before Fahrenheit's invention.

SOCIETIES AND ACADEMIES.

NATIONAL GEOGRAPHIC SOCIETY.

AT the regular meeting of the National Geographic Society in the large hall of Cosmos Club, Washington, D. C., Friday evening, April 19, Mr. Robert T. Hill, of the U. S. Geological Survey, delivered an address upon the Geography and Geology of Costa Rica and Panama. The fact that he has only recently returned from a tour of scientific investigation of the region, during which he saw a good deal of the prevailing revolutionary spirit, gave special interest to his remarks.

Grateful acknowledgment was made for the opportunity to study the geology of the adjacent continental and island areas furnished the speaker by the enlightened liberality of Prof. Agassiz.

Mr. Hill's lecture, illustrated by a large number of very interesting lantern slides, mostly from photographs taken by him during his recent trip, was partly popular and partly technical in character, descriptive of the topography, vegetation, products, architecture and customs of the widely contrasting regions of the Isthmus of Panama and the modern Spanish American Republic of Costa Rica to the northward.

The Isthmus was discussed as a type of the low-lying costal lands of the tropical region, where Caucasian population could only be maintained by constant immigration, and which would be uninhabited did it not lie in the track of commerce between two oceans. All of its population, except a few unconquered Indian tribes, is concentrated in the two seaports of Colon and Panama, or along the right of way of the railway and canal. On either side it is still an unconquered jungle. The important commercial and political American interests in this region were discussed, showing that its traffic is entirely in the control of Americans, and that it is an important point between our Atlantic and Pacific sea-ports.

Costa Rica, on the other hand, is an example of the higher and better climatic conditions existing in the Tropical American region, where indigenous civilization flourishes under healthy climate conditions. Mr. Hill spoke of this as an ideal country and praised the hospitality and progressive spirit of the people. Illustrations were given of the entire course of the Panama canal, showing the topography, cuttings, machinery and laborers at present working upon the construction. While not committing himself to any preference of canal routes, he said that the affairs of the Panama Canal Company had been painted in this country much darker than they deserved. A far greater amount of work had been accomplished than is supposed. The machinery instead of rotting is kept in the best of condition and the affairs of the Company are not as hopelessly involved as represented. A liberal sum is still in the treasury, and while the concern is in the hands of the courts, it looks as if the French had no intention, after having completed the hardest part of the canal construction, of abandoning it. The terminal port facilities have been completed. Nearly 25 miles of the canal is finished, reducing the distance between the oceans from 47 to 22 miles; about two-fifths of the necessary grading has been accomplished, and every possible machine and tool for its completion is upon the ground. The great problem of controlling and diverting the waters of the Chagres has also been accomplished. It is the general opinion of all Americans who have observed the work, including the engineering of our own famous Cabin John Bridge, that no great obstacle stands in the way of the early completion of this work except the recuperation of its financial affairs from the shameful mismanagement they have suffered.

The lecturer gave interesting accounts of the various zones of vegetation seen in ascending the great volcanoes of Costa Rica, and, incidentally, a general description and classification of the region bordering the Gulf and Caribbean Sea. Especial attention was called to the important bearing of this Spanish American region, between the latitude of the Orinoco and the southern boundary of the United States, upon the great problems of continental development, and its correlated biologic and meteorologic problems; and to the great work Prof. Alexander Agassiz has undertaken at his own expense in studying the marine physiography of the region, especially as regards the origin of its vast areas of coral reefs.

The relief of this portion of the earth's surface, a knowledge of which involves a study both of the land and the submarine topography, was provisionally classified into four great divisions: mountains of accumulation; mountains of corrugation; coastal plains of uniformly uplifted marginal sea-bottom, and land formed by the combined action of coral polyps and wind and tide (as described by Prof. Agassiz). In speaking of the mountains he classified the systems as follows:

1. The southern extension of the Cordilleran region of the United States, which terminates with the great scarp of the Mexican plateau in the latitude of Vera Cruz.

2. The Andes proper, the north and south ridges of which end abruptly in Northern Colombia.

3. A system of more ancient mountains having an east and west trend and composed of folded Mesozoic rocks, with Paleozoic axes, extending along the north coast of South America (between the Caribbean and Orinoco); throughout the Greater Antilles; and through Guatemala, Nicaragua and British Honduras. For this Mr. Hill proposed the name of the 'Antillean System.' It was shown that there were submarine topographic ridges connecting the Honduras peninsula with the islands of Jamaica, Hayti and Puerto Rico, probably also parts of this ancient corrugation.

4. Protuberances of older volcanic accumulation, such as the Windward Islands and Isthmian region.

5. Mountains of recent volcanic accumulation, including the three widely separated groups, with different trends, of southern Mexico, Central America and the northern Andes, all more or less parasitic upon the termini of the antecedent and fundamental mountain systems of corrugation, and to a certain extent (owing to their newness and greater mass) concealing them.

Everett Hayden,
Secretary.

BIOLOGICAL SOCIETY OF WASHINGTON.

At a meeting on April 6th, Dr. Theo. Gill read a paper 'On the Torpedoes.'

The subject was discussed from two points view, taxonomic and nomenclatural.

The family of Torpedoes, or cramp fishes, is well differentiated from all others by the development (from original muscular tissues) of a pair of electric batteries in the region between the cranium and anterior extension of the pectoral fins. The family is divisible naturally into three sub-families which should be called Narcobatinæ, Narcininæ and Hypninæ. These sub-families are differentiated by modifications of the cranium and skeleton generally, disk, tail, position of spiracles and structure of teeth. The nomenclature involves a singular point. The name *Torpedo* was first applied (by Forskal in 1775) as a generic term to the electric catfish of the Nile subsequently called *Malapterurus*, and was accompanied by a tolerable generic diagnosis. (The full history and etymology of the word *Torpedo* was given.) Therefore *Torpedo* must be used for the Nematognath fish. The electric ray must consequently receive another name, and *Narcobatis*, of Blainville, is therefore available. The genera would then have the following names: *Narcobatinæ*, with Narcobatis and Tetranarce; *Narcininæ*, with Narcine, Discopyge, Narbe (Astrabe) and Temera; *Hypninæ*, with Hypnos.

Mr. L. O. Howard cited the name Tarantula as a similar case in which a generic name had long been misapplied. It was first given to a scorpion, and after long service as the name of a spider it has recently been restored to its original meaning. Dr. W. H. Dall and Dr. C. Hart Merriam both agreed that in all such cases the strict law of priority should govern.

Major J. W. Powell spoke on the Classification of the Subject-Matter of Biology and the paper was discussed at length.

FREDERIC A. LUCAS,
Secretary.

ACADEMY OF SCIENCE OF ST. LOUIS.

THE Academy held its regular meeting on April 15 with President Green in the Chair and twenty-nine members and visitors present.

Miss Mary E. Murtfeldt read a paper on 'Habits of Certain Seed Feeding Insects,' giving the result of her observations and experiments with insects which feed upon the seeds of weeds and other injurious plants. Some of these insects were new to science. Miss Murtfeldt stated as her conclusion that the seed feeding insects exercise a very pronounced effect in preventing the spread of weeds, and in many instances almost exterminate them.

A. W. DOUGLAS,
Recording Secretary.

SCIENTIFIC JOURNALS.

BOTANICAL GAZETTE, APRIL.

Issued April 20, 1895. 64 pp., 2 pl.

Present Problems in the Anatomy, Morphology and Biology of the Cactaceæ: W. F. GANONG.

This is the first installment of a paper (to be concluded in the May number) setting forth in brief statement what is at present known of this group in regard to the topics enumerated in the title, and the problems, mainly to be solved by careful field observation and a study of development, which still remain to be worked out.

Flowers and Insects, XIV.: CHARLES ROBERTSON.

In this paper and its predecessor (Bot. Gaz. **20**: 104, Mr. 1895) Mr. Robertson has somewhat changed his plan of contributions to the relations of flowers and insects, in now bringing together his information in regard to the several species of a genus, accompanying it with a voluminous bibliography. Species of Gentiana, Frasera, Phlox, Lithospermum, Physalis and Mimulus are discussed.

Notes From My Herbarium, II.: WALTER DEANE.

The herbarium of Mr. Deane is one of the finest private collections in this country in the excellence and completeness of the plants represented, viz., those of the range of Gray's Manual. It is specially rich in its representation of life histories of plants so far as these can be shown by dried specimens. In this series of notes Mr. Deane is putting on record some of the information gained in the making of this collection. The fruit of *Nymphæa odorata* Ait., a case of teratology in *Apocynum androsæmifolium* L., and *Typha latifolia* L. are discussed in No. II.

Synopsis of North American Amaranthaceæ, II.: Edwin B. Uline and Wm. L. Bray.

This installment of the paper gives a systematic enumeration of the N. Am. species of the genera Acnida and Gomphrena. *Acnida tamariscina prostrata* and *Gomphrena Tuerckheimii* are described as new. To the latter *Telanthera Tuerckheimii* Vatke is probably to be referred.

A Reply to Dr. Robinson's Criticism of the 'List of Pteridophyta and Spermatophyta of Northeastern America:' Frederick V. Coville.

Among *Briefer Articles* Mr. J. Schneck describes and figures the flowering and fruiting of the spider-flower, *Cleome spinosa* L., a subtropical species which reaches up the Mississippi valley as far as S. Ills.; Mr. Geo. H. Shull records some observations on the branching, inflorescence and flowers of *Enslenia albida*, illustrated with a plate; Mr. F. H. Blodgett adds some points to a paper (Bot. Gaz. **19**: 61. F 1894) on the development of the bulb of the adder's tongue, *Erythronium Americanum* Ker.; Mr. Thomas Meehan gives a short biographical sketch of the late John H. Redfield of Philadelphia, and Professor W. W. Bailey does the same for the late Mr. George Hunt of Providence, R. I. In a note on the Systematic Botany of North America, Professor N. L. Britton, the chairman of the Board of Editors, gives a list of the parts at present assigned to the collaborators named.

The editorials deal with the discussion on nomenclature and the progress of the Systematic Botany of North America. In the department of Current Literature Sayre's *Materia Medica* (botanical part), Thomas and Dudley's *Manual of Histology*, and Lister's *Monograph of the Mycetozoa* are reviewed, with briefer mention of several other works. The number closes with six pages of notes and news regarding botanists, their doings and writings.

THE AMERICAN NATURALIST, APRIL.

On the Presence of Fluorine as a Test for the Fossilization of Animal Bones.

Experimental Evolution Amongst Plants: L. H. Bailey.

Observations on a so-called Petrified Man: J. M. Stedman.

On the Validity of the Genus Margaritana: Chas. T. Simpson.

Editor's Table; Recent Literature; Recent Books and Pamphlets; General Notes; Geography and Travels; Mineralogy; Petrography; Geology and Paleontology; Botany; Zoölogy; Entomology; Embryology; Psychology; Archeology and Ethnology; Microscopy.

Proceedings of Scientific Societies; Scientific News.

NEW BOOKS.

The Cambridge Natural History, Vol. III., Molluscs. A. H. Cooke. Brachiopods (recent), A. E. Shipley. Brachiopods (fossil), F. R. C. Reed. New York and London, Macmillan & Co. 1895. Pp. xi + 535. $2.60.

Elements of Mineralogy, Chrystallography and Blowpipe Analysis. Alfred J. Moses and Charles Lathrop Parsons. New York, D. Van Nostrand Company. 1895. Pp. vii + 342.

Steam Power and Mill Work. Geo. W. Sutcliffe. New York, Macmillan & Co. 1895. $4.50.

A Treatise on Bessel Functions. Andrew Gray and G. B. Matthews. New York, Macmillan & Co. 1895. $4.50.

SCIENCE.

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FRIDAY, MAY 10, 1895.

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison on Hudson, N. Y.
Subscriptions and advertisements should be sent to SCIENCE, 41 N. Queen St., Lancaster, Pa., or 41 East 49th St., New York.

CURRENT NOTES ON PHYSIOGRAPHY (VI.).

SURFACE CURRENTS OF THE GREAT LAKES.

A REVISED edition of the atlas of 'Surface Currents of the Great Lakes,' as deduced from the movements of bottle papers during the seasons of 1892, 1893 and 1894, by Professor M. W. Harrington, has lately been issued as Bulletin B of the Weather Bureau. The text describes the method of study, and gives tables of the prevailing winds of the lake-port stations and a list of recovered bottles, 672 being found out of nearly 5000 floated. The chief drifts are: eastward along the south side of Superior, westward along the north side; south along the west side of Michigan and Huron, north along the east side; generally eastward in Erie and Ontario. Many irregular movements are noted, especially near shore. Local and transient currents, formed during severe gales, are sometimes strong enough to drag vessels from their moorings. "There also occurs, occasionally, on the Great Lakes a phenomenon which may be called a *seiche*, namely, a wave of considerable height which travels unaccompanied by other waves, and is seen by navigators as a white wall approaching and rapidly passing them." Following the use of the term *seiche* on the Swiss lakes, where it originated, it would be more properly applied to the rise and fall of the water on the shore, in periods of generally less than an hour; these being well known at our Lake ports, but as yet very little studied. These white-walled waves also call for investigation.

BUCHAN'S CHALLENGER REPORT ON OCEANIC CIRCULATION.

THE latest volume of the Challenger reports contains thirty-eight pages of text and sixteen maps, prepared by Dr. Alexander

Buchan, of Edinburgh, to illustrate the density and temperature of ocean water at different depths; all available material being employed in this elaborate discussion, whose ultimate object is the determination of the oceanic circulation. The charts exhibit the mean annual specific gravity of the surface and the bottom waters, the mean annual surface temperatures, and the temperatures at every hundred fathoms of depth to 1000, then at 1500, 2000 and at the bottom. At 400 and 500 fathoms the South Atlantic and the North Pacific are the colder oceans; the North Atlantic and the Indian are exceptionally warm. At 600 and 700 fathoms the most remarkable feature is the relation of North Atlantic temperature to the warm over-saline water that issues from the Mediterranean; a similar but less marked effect being noticeable in the Indian ocean near the Red Sea. The average at 700 fathoms being 38.°1, the northwestern Indian ocean is 44°, the eastern North Atlantic is 51°, with the maximum centering precisely towards Gibraltar. At 900 and 1000 fathoms the temperatures in low latitudes are symmetrically warmer than in high latitudes; but the difference is less than two degrees.

Dr. Buchan's text summarizes the facts and deals little with theories; but he accepts the winds as the chief cause of the surface currents, and he ascribes deep movements to differences of density, thus indicating the truth of both sides of the Croll-Carpenter controversy of a quarter century ago.

THE EASTERN MEDITERRANEAN.

THE third series of the 'Berichte der Commission für Erforschung des östlichen Mittelmeeres,' recently issued in the memoirs of the Imperial Academy of Sciences of Vienna, contains further physical investigations by Luksch and Wolf on the basis of soundings on the 'Pola' in the Ægæan sea in 1893. The sea consists of a number of separate basins, of which the deepest (2250 met.) lies north of the east end of Candia. Much greater depths occur in the Mediterranean east and west of this island. Charts of temperature and salinity at the surface and at successive depths to the bottom exhibit the distribution of these features with much detail. The surface temperatures are maintained to a depth of about thirty meters; then follows a rapid cooling for seventy or a hundred meters, below which there is a gradual cooling to the bottom, where temperatures a little lower than 13° C. prevail.

AMERICAN GEOGRAPHICAL JOURNALS.

IT is regrettable, but for the present perhaps not surprising, that no American geographical society issues a journal from which a student, teacher or general reader can gather a thorough acquaintance with geographical activity over the world. A journal of thorough and scientific character needs a background of accumulated experience, a large library and exchange list, a good number of active contributors and correspondents, and a large subscription list; and we have not yet been fortunate enough to develop all these conditions under a single control. The best association for such a journal in this country would be with the American Geographical Society of New York, its membership being large, its funds comparatively munificent and its library of long-continued growth and certainly much superior to that of any other similar society in the United States; but, although this society counts explorers, travellers, government officials, professors and a large representation of the general public among its members, the number of its producing geographers is small, and its quarterly Bulletin, now in its twenty-sixth volume, can hardly at present be included among the important geographical periodicals of the world. We understand that plans for greater activity and enlarged form of publication are in consideration. The National Geographic Society of Washington is but a few years old. Its activity at present is greatest in its home city in the matter of geographical lectures, which are very successful. A list of this winter's lectures was given in SCIENCE No. 11. Its Magazine is of irregular publication, presumably on account of lack of funds. While it contains a larger proportion of physiographic matter than any other publication in this country, it gives practically nothing of general news or literature. Appalachia, the organ of the Appalachian Mountain Club of Boston, the Bulletin of the Geographical Club of Philadelphia, the Bulletin of the Geographical Society of the Pacific, and the papers of the Sierra Club, both of San Francisco, complete the list of geographical publications in this country as far as known to the writer. Geographical notes are given in the American Naturalist and in the Popular Science Monthly. All these geographical journals deserve warm support, especially in their own communities, but none of them presents the subject of geography nearly as fully as it is presented by several journals abroad.

FOREIGN GEOGRAPHICAL JOURNALS.

THE small amount of space that can be allowed in SCIENCE to geography makes it impossible to report on the progress of exploration, save when results of especial importance or of immediate physiographic interest are announced. Exploration is, however, fully presented in various foreign geographical journals; and, in the hope of extending their circulation in the libraries of our country, occasional notes of their character and contents will be here introduced. Preëminent among all such publications stand the Geographical Journal of the Royal Geographical Society of London, and Petermann's Geographische Mittheilungen, issued by the great geographical publishing house of Justus Perthes of Gotha and now conducted by Professor Alex. Supan. The Geographical Journal has for the great body of our students of geography the advantage of being in our own language, and it will therefore long continue to reach the larger circle of readers. Besides general articles and current news, ten or twelve pages are given in each number to notes on geographical literature by Dr. H. R. Mill, the entries being conveniently summarized by brief headings in bold type, arranged under countries. Extended reviews are made of important works. But those who can consult German sources—and this ability is now generally demanded of students in higher collegiate and university work—will find in Petermann's Mittheilungen an unrivaled bibliography of the whole range of geographical literature, from the geology of the earth beneath to the meteorology of the air above. Reviews of the more important publications are given in so extended a form that reference to original sources is unnecessary, except for the specialist in some particular division of the subject. Anyone who follows these reviews and the items of monthly news will acquaint himself very fully with the general progress of current geographical work. Other foreign journals will be referred to in subsequent numbers of SCIENCE.

WAGNER'S GEOGRAPHISCHES JAHRBUCH.

THIS indispensable annual, founded in 1866 by Behm and now in its seventeenth volume, is a fitting supplement to the other geographical publications of the house of Perthes in Gotha. The most important reviews and summaries in the Jahrbuch for 1894 are: on terrestrial magnetism by Schering, map projections by Hammer, ethnology by Gerland, geographical meteorology by Brückner, and on the geographical literature of the European countries by various contributors. Several of the latter are of great thoroughness and may serve as guides in ordering the best recent publications for public and college libraries. The most thorough are by Fischer on Southern Europe, Neumann on Germany and Sieger on Austria-Hungary. That by Schlichter on Great Britain and Ireland unwarrantably omits mention of the recent editions of Geikie's Scotland and Ramsay's England. The volume closes with a series of small scale index-maps, giving the state of advance of topographical surveys in Europe, India and the United States up to the autumn of 1894. One may thus determine at a glance whether the sheet for a certain locality in any country is yet published or not. The practical use of these indexes would have been increased if the name and address of the official bookseller from whom the maps may be purchased had been given.

FORSCHUNGEN ZUR DEUTCHEN LANDES- UND VOLKSKUNDE.

The eighth and latest volume of these valuable essays, edited by Kirchhoff of Halle, and published at Stuttgart by Engelhorn, contains studies by Schreiber on the climate of Saxony, Partsch on the glaciation of the Riesengebirge, and Follman on the Eiffel, besides three others on historical and ethnological subjects. Schreiber's essay gives a full account of the periodic values of various climatic factors, but it is deficient in omitting all account of the unperiodic or cyclonic changes, which in winter are dominant, and fully deserve recognition as climatic elements. Partsch presents a careful study of the moraines and associated terraces of the Riesengebirge, which rise a few miles south of the extreme limit ascribed to the northern ice sheet in that region. The height of the snow line during glacial times is placed at about 1200 meters, by means of ratios between length of glaciers and area of snow fields, as determined in the Alps. An older and a younger glaciation are separated by a considerable interval, during which normal valley making was in progress. The author dissents from Berendt's views concerning a more general glaciation of the Riesengebirge. Follman's account of the Eifel is chiefly geological and descriptive, little attention being given to the development of the existing topography or to the explanation of the present courses of the streams. The volcanoes and the *maare*, of course, receive special attention.

PENCK'S MORPHOLOGIE DER ERDOBERFLÄCHE.

This is the most important work on physiography that has appeared during the past year; indeed, in many respects it is a unique work, one that will stand long at the head of works of its class. It is a worthy successor of earlier volumes in the series of geographical handbooks (published by Engelhorn, Stuttgart) to which it belongs—Ratzel's Anthropogeographie, Hann's Klimatologie, Heim's Gletscherkunde, Boguslawski and Krümmel's Oceanographie and others; and in the matter of citations of authorities it is much superior to any of its predecessors. Penck's acquaintance with the literature of his subject is truly remarkable. Each topic is outlined historically, as well as in its present status. A subject relatively so subordinate as the effect of the earth's rotation on rivers has thirty-five citations; sand dunes have fifty-one. Processes of deformation, deposition and denudation are all considered elaborately, with special reference to the forms that they produce, and this part of the book might properly be called *Morphogenie*. The forms themselves are considered afterwards at length. The more general headings in the table of contents are: Form and size of the earth; area of land and water, mean altitude of lands and depth of seas, volume of lands and seas; continents and oceans and their permanence. Land surfaces; weathering and denudation by wind, rivers and ice; deformations of the surface. The forms of the land; plains, hills of accumulation, valleys, basins, mountains, depressions, caverns. The sea; its movements, coasts and bottom; islands.

The chief deficiency of the book is the scarcity of illustrations and the rough quality of nearly all the few cuts that are introduced. Many are merely diagrams, often with excessive vertical exaggeration. This is to be regretted in a subject where graphic aid of the highest quality is necessary for the adequate presentation of the facts. But as the work is in two volumes of 471 and 696 pages, the omission of illustrations has evidently been a matter of necessity.

W. M. DAVIS.

HARVARD UNIVERSITY.

NOTES UPON AGRICULTURE (II.).

MUSCARDINE DISEASE OF CHINCH-BUGS.

ONE of the most serious of insect depredations to wheat and corn is that caused by the chinch-bug, and for years methods of checking it by employing a parasitic fungus have been the subject of research. In Kansas special appropriations have been made by the Legislature to determine the best means of propagating and applying the virus. The latest information upon this subject comes in the shape of a sixty-page bulletin with eight plates (No. 38, March, '95) from the Illinois Experiment Station prepared by Dr. Forbes. The fungus experimented with is *Sporotrichum globuliferum*, Speg., which was cultivated successfully upon a mixture of corn meal and beef broth and afterwards distributed to farmers in the chinch-bug infested portions of the State.

The White Muscardine (Sporotrichium) spreads most rapidly in the field when the weather is moist and the 'catch' is quickest in the low spots in the field and among fallen herbage. Professor Forbes is of the opinion that the disease may be developed without infection by artificially producing the above conditions by trampling down the grain in spots or cutting and stocking small portions as starting points for the infection. It was observed that mites feed upon the Muscardine and in some of the artificial cultures eat up 'the last vestige of the fungus.' The Sporotrichium lives upon many kinds of insects, and a plate is given of the appearance of it upon a leaf skeletonizer (Carnarsia), June Beetle (Lachnosterna), Walnut caterpillar (Datana).

BACTERIOSIS OF RUTABAGA.

THE number of diseases of plants of bacterial origin is rapidly on the increase, or, more strictly writing, the nature of these troubles is in these later days being better understood. A portion of Bulletin 27 of the Iowa Experiment Station is devoted to a disease of rutabagas that Professor Pammel finds, through a long course of bacteriological study, to be caused by a microörganism which he names *Bacillus campestris* n. sp., and figures in details in a plate. This disease is distinguished by its strong odor, the decay usually beginning at the crown of the root, the fibro-vascular zone becomes black, while the softer portions of the root become soft and finally watery. Healthy roots were caused to decay by introducing the Bacilli, previously isolated by cultural methods, into their tissue.

WEED SEEDS IN WINTER WINDS.

IT is well known that winds play an important rôle in the distribution of seeds. Professor Bolley, in the North Dakota Experiment Station Bulletin (No. 17, March, 1895), records that in two square feet of a three-weeks old and three-inch deep snow drift upon an ice pond ten yards from any weeds he found nineteen weed seeds, and and in another drift quite similarly situated thirty-two seeds representing nine kinds of weeds. While the wind was blowing twenty miles per hour a peck of mixed seeds was poured upon the snow crust, and ten minutes after 191 wheat grains, 53 flax seeds, 43 buckwheat and 91 rag weed seeds were found in a trench thirty rods from where they had been poured upon the crust.

BLACK KNOT OF PLUMS AND CHERRIES.

The Black Knot fungus (*Plowrightia morbosa* Schw.) is an old orchard enemy. Professor Lodeman, in Bulletin 81 (December, '94) Cornell Experiment Station, has given the long bibliography of the subject and shows, by means of cuts, how the spores of the fungus may find their way between the adjoining layers of bark in the forks of the small limbs. At these places the bark is thin and the growing layer (cambium) comes near to the surface, thus facilitating the inoculation. Lodgement is also produced at these angles between stems, and besides it is here that knots are most apt to form. Experiments in spraying knotty trees with Bordeaux mixture gave results that were decidedly encouraging.

RECENT APPLE FAILURES.

In another bulletin (No. 84) from the Cornell Experiment Station—and there are many and fine ones—'The Recent Apple Failures of Western New York' are considered by Professor Bailey. A glance at the cuts shows that failures may be due to imperfect pollination, injudicious application of fungicides, but more particularly to the ravages of the Apple Scab (*Fusicladium dendriticum* Fl.), of which Professor Bailey gives a full page colored plate showing the scab enemy in detail from the appearance of the young distorted fruit to the microscopic structure of the fungus shown in leaf sections. That the scab fungus is the leading cause of apple failures is demonstrated by the fact that thorough spraying to check it productiveness has been obtained. The essentials for success in apple culture, as given by the author as his concise summary, are: "till, feed, prune, spray."

DETASSELING CORN.

The removal of the male flowers from a large or small per cent. of the corn plants in a field has been experimented upon at various stations. Thus in Maryland where two-thirds of the tassels were removed the detasseled rows gave a decrease of nearly 10 per cent. At the Kansas Station by detasseling alternate rows of six varieties in every case there was a reduced yield averaging 22 per cent. Delaware obtained under similar circumstances an increase of 6.6 per cent.

Before us is the bulletin (No. 37 Feb., 1895) upon 'Corn Experiments' of the Illinois Experiment Station in which detasseling receives its share of consideration. "In eighteen out of twenty-three comparisons the yield of corn was greater for the rows (alternate) having the tassels removed. For tassels pulled we have an increase of twenty-seven per cent., and for those cut only six per cent. Removed before expanding gives an increase of eleven per cent. The average increase is thirteen per cent." At the Cornell Station one report (1890) gave an increase of fifty per cent. for detasseling, but the next year there was no difference. The results thus far obtained teach that the end of experimentation in this direction is not yet reached.

Byron D. Halsted.

Rutgers College.

LAGOA SANTA.

Such is the title of a memoir published in 1892 by Professor Eugene Warming, of the University of Copenhagen. It is also styled *Et Bidrag til den biologiske Plantegeografi*, and this sub-title sufficiently explains the aim of the work. Lagoa Santa is a small village about 835 meters above the sea and 200 miles north of Rio de Janeiro, in the Brazilian campos, or hilly region beyond the great virgin forests of the coast mountains. Warming spent three years at this place, 1863–66, and made large collections of plants, which have been studied and described by various specialists. Now, after nearly thirty years, the author gives his general conclusions as to the flora of this region, which he considers typical of a great part of the interior of Brazil. The mean temperature is 20.5°C, with a range of 3.5° to 37°C. There are two seasons—*dry*, from April to September, corresponding to our winter, and *wet*, during the rest of the year. Spring opens in August. June is the coldest month, and December and January are the warmest months, but there is no winter in our meaning of the term, the means of the coldest month being only a few degrees below that of the warmest. The annual rainfall is not known, but it is considerable during part of the year, and there are heavy dews in the dry season. The heaviest rainfalls are in November, December and January. The soil is a red clay, very common in Brazil, resulting from the decomposition of the primary rocks. In places cavernous limestones occur.

There are no plains here, but only an interminable succession of hills with narrow valleys through which streams have cut gorges or in which there are lakes or ponds. Forests line the water courses and cover the calcareous rocks. These are a meager continuation of the luxuriant coast forests. The greater part of the country is, however, destitute of trees or bears only scrubby growths. These surfaces are the campos. They consist either of barren, pebbly plateaus and flanks of hills which are subject to washing, covered with scant herbage and often entirely destitute of trees, or of similar areas bearing deeper and more fertile clays and covered more or less densely with herbs, shrubs and small trees. The marsh and water plants form only an insignificant part of the vegetation, and may be left out of account in this synopsis. The contrast between the forest vegetation and that of the campos is very sharp, the plants of the latter resembling desert vegetation in many interesting particulars. Except in very rich parts of the campos the herbaceous vegetation is never dense enough to hide the hard red earth. Grasses are the most important part of the herbaceous covering. There are about sixty species, mostly Panicums, Paspalums and Andropogons. All are perennial and grow in thin scattered tufts, never forming a sod. The Cyperaceæ also grow in the same way. The composites are rich in species, especially the Vernonieæ and Eupatorieæ. The Leguminosæ come next in number of species. There are 554 species of herbs on the campos, but there are no biennials, and the number of annuals is very few, *i. e.*, less than 6%. There are also very few climbers or twiners although the campos bears many forms intermediate between erect herbs and climbing and twining plants. The great dearth of annuals is attributed to the great dryness and hardness of the soil at the time the seeds are shed, to the annual fires which consume seeds and seedings and may perhaps have transformed some annuals into perennials, and to the hard struggle for existence with tall herbs and bushes. Herbaceous shoots develop ordinarily in tufts and are not branched or but slightly, arising in great numbers from subterranean stems or roots. Exclusive of certain grasses, sedges and Bromeliaceæ, herbs with rosettes of basal leaves are almost entirely wanting. Horizontal rhizomes and stolons are absent and horizontal cauline organs always remain very short. Almost all of the perennial Dicotyledons have a short, thick, lignified, irregular, and more or less tuberous subterranean axis. Sometimes a delicate little shoot only ten to fifteen centimeters high arises from a tuberous axis as large as one's fist. Juicy tubers and tender bulbs are very rare on the campos. Typical shrubs are not rare and in some places they form thickets. In other instances unbranched shoots arise in great numbers from a big, lignified, root-shaped axis and form tufts which are often very large. Generally, these tufts are only 0.35 to one meter high, but they cover a diameter of one to three meters and often more. This manner of growth resembles that of the herbaceous perennials, but the shoots are woody. The campos bears 170 to 180 shrubs. The families represented by most species are: Myrtaceæ 40–50, Malpighiaceæ 30, Melastomaceæ 20, Compositæ 15, Euphorbiaceæ and Lythraceæ 6–10, the rest of the species being scattered among twenty-five families. The tallest trees of the campos are three to eight meters high, and the densest growth forms a kind of forest, but this is never close enough to shade the earth. Sometimes the trunks rise obliquely, and both trunk and branches are twisted and stunted with thick, rough, channeled and cross-fissured bark. Many of them are also blackened and charred by the campos fires. There are eighty-six arborescent species on the campos, but many are only one to three meters high, and all resemble stunted fruit trees rather than ordinary arborescent vegetation. Phænogamic epiphytes and epiphytic mosses and lichens are very rare. Lianas are wanting, but some species show a tendency toward such types and these belong to genera which in the forest are developed largely or exclusively as lianas, *e. g.*, there are eighteen species of Serjania in the dense forest, all lianas, while on the campos the one species, S. erecta, is a shrub with lithe slender branches. Cactaceæ and all fleshy plants, exclusive of members of the orchidaceous genus Cyrtipodium, are also wanting and spiny plants are very rare. Certain families very common on the high mountains of Brazil, *e. g.*, Vellosaceæ and Ericaceæ, have no representatives on the campos. Finally the soil bears no mosses, lichens, algæ or fungi. This region is dry. The coast mountains and their virgin forests retain the moisture of the air, and the dryness is increased by the altitude. "The vegetation of the campos, properly speaking, is xerophilos. It is strange to see two forest growths developed side by side and often touching but differentiated in the sharpest possible manner, namely, the wooded campos and the forests. The latter accompany the water and streams everywhere. The trees are close together, tall and slender; lianas twine about them and epiphytes live upon them, and a coolness that is sometimes exquisite reigns in them. Proceeding from the streams the forests have invaded a certain territory on both sides to which, in course of time, they have brought a fertile humus. All at once, the forest stops and we find ourselves on the edge of the campos, where there is neither moisture nor shade, nor humus, and where the red clay earth cracks open in the dry season under the influence of the heat and desiccation. It is the soil conditions which have caused this antithesis. The difference in the quantity of water contained in the soil in the bottom of the valleys and on the summit and flanks of the hills of the campos has brought about these strong and curious contrasts between the two floras. It is certain that the geological formation exhibits no difference. In the campos and under the humus of the forests it is everywhere the same red clay."

The xerophilous character of the campos vegetation is manifest first of all in the shapes of the trees. On account of the dryness of the air these are small, stunted and twisted the same as in the high mountains of Brazil or in the maritime forests of "Restinga," along the sandy shores. Fires have also played a great rôle in developing stunted forms. The strong development of the cortical system and the heavy suberization are due to the dryness of the air and probably also to the fires. The thick, irregular, ligneous, subterranean axial organs (it is often difficult to tell which part is stem and which is root) are also, both in herbs and shrubs, related to the aridity and to the fires. The absence of mosses and of hymenomycetous and other sayrophytic fungi is another indication of the dryness. The leaves show the dryness of the climate in numerous ways. An abundant hairy covering is very frequent, and the leaves of some species have both surfaces covered with a white or greyish felt, while others have only the lower surface felted. The leaves of other species are scabrous, hispid, glandular-hairy, or shining as if lacquered. A few have a waxy covering. Almost always, even in the herbs, the leaves are stiff and coriaceous, unless both surfaces are tomentose, and on some trees they are so stiff as almost to jingle in the breeze. Most of the grasses and sedges have narrow stiff leaves. The direction of the leaves also shows the aridity. Many are vertical or pointed upward, so as to receive the sun's rays at an acute angle. Some species are aphyllous and in others the leaves are much reduced. Usually, the leaves of the forest species are larger and especially broader than those of the campos species, even when of the same family or genus. "The most of the peculiarities which distinguish xerophytes are also found in the plants of the campos, although rarely to such a pronounced degree. The environment does not reach the excessive dryness of the deserts of Africa and Asia, of the high plateaux of Mexico, etc., and this explains the absence of catacеæ and other fleshy plants and the rarity or absence of succulent organs, such as tubers and bulbs. The dryness is never so great that vegetation is forced to disappear or dry up entirely for a longer or shorter period, as happens in the steppe or the desert, and the spring awakening is not so sudden as in these places. The dryness of the campos is also manifest in the fall of the leaves." Every year, when the sun has parched the herbage so that it is almost like hay, the campos are fired so as to get new growths for the cattle. These firings occur most frequently from July to September, but also earlier and later. The fires sweep everything that is close to the ground, including the lower branches of the trees, and cause the leaves to fall by thousands. When they are set too early, *i. e.*, in May or June, the succeeding vegetation is feeble, and when they are set too late in the spring, *i. e.*, after the spring vegetation has begun, they cause immense and lasting injury. When set at the proper time the campos are covered in a week or two with a rich carpet of green. Plants blossom earlier on the burned campos, and many species are seldom found in bloom elsewhere. The rarity of annuals has already been mentioned. The unbranched tufted habit of many shoots and the numerous swollen tuberous axial organs also seem to be due to the fires, and the numerous big underground stubs of trees and shrubs are undoubtedly due solely to this cause.

The forests of Lagoa Santa are not as imposing, as dense or as moist as those of the coast mountains. Those on the calcareous rocks in particular are quite open, dry and light. Tropical forests sometimes pass for being poor in flowers, but this is only an appearance, the blossoms being concealed in the tops of the trees. Most of the trees have small flowers. Like tropical forests in general the ground between the trunks is densely covered, in places impenetrably tangled, with bushes, small trees and lianas. The author observed nearly 400 arborescent species in the forest and thinks the actual number much exceeds this. These trees belong to sixty-seven families, the leading ones including nearly one-half of the species, being Papillionaceæ, Myrtaceæ, Rubiaceæ, Lauraceæ, Artocarpaceæ, Cesalpinaceæ, Euphorbiaceæ, Meliaceæ, Mimosaceæ and Anonaceæ. The individuals of a species are widely scattered and it is often difficult to find more than one or two of a kind. The great number of species is attributed to the uninterrupted development of the forest during many geological ages, the campo-growths being a derived and more recent flora. The height of the trees is rarely more than 20 to 25 meters. The trunks are not scraggy like those of the campos, and the bark is smoother and less corky. The well lighted forests have a dense undergrowth of shrubs 1–3 meters high, most of which bear small white flowers. The soil of the forests is poor in herbaceous and suffrutescent species. There is no carpet of mosses or lichens. Agarics are small and very rare. Grasses form no part of the covering of the soil, and if any exist in the forest they are tall perennials such as Olyra and Bambusa. The forest is rich in climbing and twining plants, in striking contrast to the campos. The big woody lianas belong principally to Bignoniaceæ, Convolvulaceæ, etc., and the herbaceous climbers to Cucurbitaceæ, Passifloraceæ, etc. The Convolvulaceæ of the forests are generally voluble, while those of the campos are erect under-shrubs. The numerous Aristolochias of the forest are also all voluble, while the single species of the campos is an under-shrub with stems 15–30 centimeters high from a woody, tuberous, subterranean axis. The air is so dry that even in the forests there are but few Epiphytes. Cactaceæ and other fleshy plants, and numerous hairy, thorny and stinging plants grow in the more open forests on the calcareous rocks.

Only the forest lands are used for agricultural purposes. The trees are felled, and after the clearing has been subject to the heat of the dry season for some months it is fired and then planted—sometimes to sugar cane and rice, but more generally to Indian corn, with castor bean, perennial cotton, beans, cucumbers, pumpkins, etc., between the hills. After the second year the clearing is abandoned. These neglected clearings are soon covered with a dense growth of weeds, which are quickly crowded out by various shrubs—felted leaved and spiny Solanums, hispid Lantanas, dirty green or brown hairy Crotons, numerous Sidas and other Malvaceæ, dull composites often sticky, tall grasses with large leaves and many other plants, mingled with which are shoots from the tree stumps. Gradually the area becomes once more a forest, twenty or thirty years sufficing. It is said that after the forest has been cleared away three or four times it will not return, its place being taken by bushes, thickets of Pteris aquilina var. esculenta and dense masses of the glandular hairy Panicum Melinis; 43% of the weeds of the gardens and clearings are annuals, and a few of these weeds are old acquaintances, *e. g.*, Chenopodium ambrosioides, Gnaphalium purpureum, Xanthium Strumarium, Erechthites hieracifolia, Sonchus oleraceus, Panicum sanguinale, Eleusine Indica, Argemone Mexicana, Phytolacca decandra, Portulacca oleracea, Physalis pubescens, Datura Stramonium and Solanum nigrum.

The flora of the forest is twice as rich in species as that of the campos. Of the 755 genera observed at Lagoa Santa 82 belong exclusively to the campos, 61 are tributary to the water and 364 belong to the forests, although the latter only occupy a small part of the country. The forest flora is probably much more ancient than that of the campos. Compositæ and Papilionaceæ form about one-quarter of the entire flora of the campos. The flora of the forest is made up chiefly of Compositæ, Polypodiaceæ, Orchidaceæ, Rubiaceæ and Euphorbiaceæ. A large number of genera are common to both campo and forest, but often the species are not nearly related. In other cases the species resemble each other so closely that some botanists regard one as a variety of the others. The Brazilians have also noticed this in case of certain trees and designate one form as *do campo* and the other as *do mato*. Woody species are more common in the forest than on the campos, *i. e.*, 800 to 250. The number of herbaceous species on the campo and in the forest is about the same. Hygrometric conditions determine essentially the anatomy and the morphology of plants. This causes the difference in form and in thickness of bark of the trees of the campos and of the forest. In the campo plants there is a marked reduction of foliar surface to prevent excessive transpiration, and pilosity is most frequent in these species, although common in the forest, where it occurs most abundantly on the foliage of the trees and lianas, the glabrous plants of the forest being the lower and shaded species. A great many of the weeds are abundantly hairy. These grow principally in the clearings in narrow valleys exposed to a burning sun. Plants with lacquered leaves occur both on the campos and in the forest. Spiny plants are rare on the campos, more frequent in the forest, especially on the calcareous rocks, and most common in the clearings. Waxy leaved plants occur in various situations, but are not frequent. Coriaceous leaves occur on the woody plants of the campos and also frequently on the forest trees. They are not so common on the forest shrubs and are still rarer on the marsh plants. Many plants of the forest have large thin leaves, entirely unsuited for the campos. The fall of leaves is brought about by the increasing dryness of the air and soil rather than by any change of temperature. This is much more decided in the trees of the campos than in those of the forest and is most noticeable in the woody plants on the calcareous rocks. Some trees shed their leaves in winter and remain bare for several months, but most of the leaves fall in the spring (August to October) simultaneously with the appearing of new leaves, so that the forest is always green and retains about the same coolness and depth of shade. The trees of the campos as well as of the forest show annual rings, and the author thinks that the same periodicity of growth takes place everywhere, even in the trees on the Amazon. Buds are not generally protected by budscales, although some of the woody plants of Lagoa Santa bear as characteristic buds and budscales as any forest trees in Denmark. The author's principal collections were made from the small area of 170 sq. kilometers, from which he obtained about 2,600 species of vascular plants.

ERWIN F. SMITH.

WASHINGTON.

THE PROGRESS OF PARONYMY.

TEN years ago* I urged the desirability of the general employment of technical anatomic terms consisting, so far as practicable, of one word each (mononyms), and derived directly or indirectly from the Latin, constituting *paronyms* of the originals. Such paronyms might be either identical with the original, *e. g.*, English *pons*, or changed in various ways in conformity with the custom of each language, *e. g.*, French *pont*, Italian *ponte*. The subject was further discussed in connection with Prof. S. H. Gage in 1886† and in 1889,‡ and the principle of

* Paronymy *versus* heteronymy as neuronymic principles. Presidential address at the 11th annual meeting of the American Neurological Association, 1885. *Transactions of the Association*, pp. 21. Also *Journal of Nervous and Mental Disease*, Vol. XII.

† Anatomical technology: an introduction to human, veterinary and comparative anatomy. Second ed., 1886, O., pp. 600, 120 figs., 4 plates.

‡ Anatomical terminology. Reference Handbook of the medical sciences. A. H. Buck, editor, VIII., pp. 24. 1889.

paronymy was approved by the Committee on Biological Nomenclature in the Report adopted by the American Association for the Advancement of Science, August, 1892.

Naturally the application of the principle has been easier with the French and Italian than with the German. Yet nearly all recent works in this language contain paronyms either unchanged (excepting for capitalization), *e. g.*, *Dura*, or with slight changes, *e. g.*, *Hippokamp* for *hippocampus*.

The last example of Germanization to come under my notice is in Eisler's 'Das Gefäss- und periphere Nervensystem des Gorilla," where the customary heteronym, *Herzbeutel*, is abandoned for the regular paronym of *pericardium*, *Perikard*. Curiously enough in English we have hitherto retained the useless termination, but analogy with *pericarp* (from *pericarpium*) not only warrants but demands the abbreviated form, *pericard*.

Burt G. Wilder.

Ithaca, N. Y.

THE MARINE BIOLOGICAL LABORATORY.

The annual announcement of the 'Marine Laboratory' for the eighth season, 1895, has recently appeared.

The officers are as follows: Dr. C. O. Whitman, Director, Head Professor of Zoology, University of Chicago, and editor of the *Journal of Morphology;* Dr. H. C. Bumpus, Assistant Director, Professor of Comparative Anatomy, Brown University.

ZOÖLOGY.

A. Investigation. Howard Ayers, Professor of Biology, University of the State of Missouri; E. G. Conklin, Professor of Biology, Northwestern University; S. Watase, Assistant Professor of Zoölogy, University of Chicago; M. M. Metcalf, Professor of Biology, The Woman's College of Baltimore; C. M. Child, Fellow in Zoölogy, University of Chicago; F. R. Lillie, Instructor in Zoology, University of Michigan; O. S. Strong, Instructor in Zoölogy, Columbia College; H. S. Brode, Fellow in Zoölogy, University of Chicago.

B. Instruction. W. M. Rankin, Instructor in Zoölogy, Princeton College; J. L. Kellogg, Professor of Biology, Olivet College; P. A. Fish, Instructor in Physiology and Anatomy, Cornell University; A. D. Mead, Fellow in Zoölogy, University of Chicago; H. E. Walter, Chicago.

BOTANY.

W. A. Setchell, Instructor in Botany, Yale University; W. J. V. Osterhout, Instructor in Botany, Brown University.

PHYSIOLOGY.

Jacques Loeb, Associate Professor of Physiology, University of Chicago; W. N. Norman, Professor of Biology, University of Texas.

The work of the laboratory is definitely organized with reference to the needs of three classes of workers, namely, (1) students, (2) teachers of science, and (3) investigators. There are regular courses of instruction, consisting of lectures and laboratory work under the supervision of the instructors, given in Zoölogy, Botany, Embryology and Physiology. In addition to these, there will be courses of lectures on special subjects as follows: Embryology, by the Director, Professor C. O. Whitman; on Botanical Museum Development, by J. M. McFarlane, and on Matter and Energy, by E. A. Dolbear.

There will also be evening lectures on biological subjects of general interest. Among those who contribute these lectures may be mentioned: G. F. Atkinson, E. G. Conklin, Northwestern University; J. M. Coulter, President Lake Forest University; A. E. Dolbear, Tuft's College; Simon Flexner, John Hopkins Hospital; E. O. Jordan, University of Chicago; William Libbey, Jr., Princeton College; F. S. Lee, Columbia College; W. A. Locy, Lake Forest University; J. M. MacFarlane, University of Pennsylvania; C. S. Minot, HarvardMedical School;

E. S. Morse, Peabody Academy of Science; H. F. Osborn, Columbia College; W. B. Scott, Princeton College; W. T. Sedgwick, Massachusetts Institute of Technology; William Trelease, Director Missouri Botanical Garden; S. Watase, University of Chicago; E. B. Wilson, Columbia College; B. G. Wilder, Cornell University; W. P. Wilson, University of Pennsylvania.

The laboratory has been considerably enlarged and now consists of four two-story buildings, with forty private rooms for the exclusive use of investigators, and seven general laboratories. It is supplied with aquaria, a steam launch, boats, dredges, and all the apparatus necessary for collecting and keeping alive material reserved for class work or research.

A Department of Laboratory Supply has been established in order to facilitate the work of teachers and others at a distance who desire to obtain material for study or for class instruction. Circulars giving information, prices, etc., may be obtained on application.

The forty private laboratories are distributed as follows: Zoölogy, twenty-two; Physiology, eight; Botany, ten. These rooms are rented at one hundred dollars to colleges, societies or individuals.

The general laboratories for research are for the use of students engaged in special work under the supervision of the Director and his assistants, and for advanced courses preparatory to beginning investigation, such as the course in Embryology. There are forty-two tables, of which Zoölogy has twenty-two, Physiology ten, and Botany ten.

Applications should be made to Professor C. O. Whitman, University of Chicago, Chicago, Ill.

EMBRYOLOGY.

THE course in Embryology extends from July 10th to August 17th. The aim is not only to master the details of development, but also to acquire a thorough knowledge of preparing surface-views, imbedding in paraffin and celloidin, staining, mounting, drawing, reconstructing modeling, etc. The study is mainly confined to the fish egg as the best type for elucidating vertebrate development; but the eggs of amphibia and other vertebrates as well as some invertebrates will receive attention. The fee is $50.

INVESTIGATION.

THE course in Investigation extends from July 3d to August 17th. For those prepared to begin original work, ten tables are reserved in Zoölogy, and the same number in Physiology and Botany.

Special subjects for investigation are assigned to the occupants of tables, and the supervision of the work is so divided that each instructor has the care of but three or four students. In this way all the advantages of individual instruction are secured. The fee is $50.

SEMINAR.

A SEMINAR has been instituted, and, though specially designed for members of the class in Embryology and beginners in investigation, it is open to all. The third volume of the Biological Lectures will be made the basis of discussion. Most of the authors of these lectures will be present; and from two to three mornings will be devoted to the consideration of each lecture and such questions as may be raised.

LABORATORY FOR TEACHERS AND STUDENTS IN ANATOMY.

IN the Laboratory for Teachers and Students in Anatomy, which is open from July 2d to August 30th, two courses are offered: the first, in Invertebrate Anatomy, and the second, a newly arranged course in Vertebrate Anatomy. The fee for either course is $40.

VERTEBRATE ANATOMY.

THE list of lecturers on Vertebrate Anatomy will be as follows: Professor H. P.

Bowditch, Harvard Medical School; Dr. F. S. Lee, College of Physicians and Surgeons; Dr. C. F. Hodge, Clark University; Dr. O. S. Strong, Columbia College; Dr. C. S. Minot, Harvard Medical School; Dr. J. S. Kingsley, Tuft's College; Dr. J. P. McMurrich, University of Michigan; Dr. H. F. Osborn, Columbia College.

Applications for admission to the laboratory for students and teachers should be made to Prof. H. C. Bumpus, Brown University, Providence, R. I.

BOTANY.

THE laboratory work in Botany (July 10–August 17) will be restricted to the study of the structure and development of types of the various orders of the cryptogamous plants, and especial attention will be given to the study of the various species of Marine Algae which occur so abundantly in the waters about Woods Holl.

The following colleges and societies controlled private rooms or tables during the season of 1894:

Boston University School of Medicine, Brown University, Bryn Mawr College, College of Medicine, Syracuse University, College of Physicians and Surgeons, Columbia College, Hamilton College, Harvard University (Professor Farlow), Lake Forest University (President Coulter), Massachusetts Institute of Technology, Miami University, Mt. Holyoke College, Missouri Botanical Garden, Northwestern University, Princeton College, Smith College, University of Chicago, University of Cincinnati, University of Pennsylvania (Provost Harrison), Vassar College, Wellesley college, Williams College, Women's College Baltimore, American Association for the Advancement of Science, American Society of Naturalists, Beta Alpha Chapter of the K. K. G. Fraternity of the University of Pennsylvania, Lucretia Crocker Scholarship, Woman's School Alliance Milwaukee.

THE GENERIC NAMES OF THE THREE-TOED ECHIDNA.

THE three-toed *Echidna* discovered by M. Bruijn in northwestern New Guinea, and described by Peters and Doria in 1876 as *Tachyglossus bruijnii*, has been commonly recognized as belonging to a different genus from the common five-toed *Echidna* of Tasmania and Australia. Although the species was described less than twenty years ago, four generic names have been proposed for it. Early in 1877 Dr. Theodore Gill erected the genus *Zaglossus** for it, and Gervais separated it in November of the same year under the name *Acanthoglossus*;† but a few days later, finding that this name had been pre-occupied, he renamed the genus *Proechidna*.‡ Five years later M. Dubois proposed to replace *Acanthoglossus* by *Bruynia*.§

Of these four names *Proechidna* has come into general use, while *Zaglossus* Gill seems never to have been mentioned by any subsequent author. My attention was first called to it several months ago by Dr. Gill himself, who suggested that it would probably antedate *Proechidna*, but no copy of Gervais' Ostéographie being at hand I could not determine which name had priority. Recently I have had an opportunity of examining a copy of the Ostéographie des Monotrèmes, and find that not only does *Zaglossus* antedate *Proechidna*, but in fact it was the earliest name proposed for the genus, and should be adopted to the exclusion of all the others.

The second chapter of the Ostéographie, apparently the only part of the text ever published, contains the name *Proechidna* on page 43. In the introductory foot-note on

* Ann. Record of Science & Industry for 1876, May 5, 1877, p. clxxi.

† Comptes Rendus, lxxxv., No. 19, séance du 5 Nov., 1877, p. 838.

‡ Ostéographie des Monotrèmes Viv. et Fossiles, Nov. 30, 1877, p. 43.

§ Bull. Soc. Zool. de France, vi. No. 6 (1881) 1882, pp. 267–270, pls. ix–x.

page 41, dated '*30 Novembre, 1877*,' M. Gervais gives the reasons for publishing the second chapter first, and states that the first and third chapters will probably appear during the year 1878. From this statement it is evident that *Proechidna* could scarcely have been published prior to December 1, 1877. The Annual Record of Science and Industry for 1876, on the other hand, was received at the Library of Congress, Washington, D. C., on April 28, 1877. This date, however, may be the date of entry for *copyright*, and does not necessarily show that the book was issued on April 28. A copy of the same volume in the library of the U. S. Patent Office, Washington, D. C., was received early in May, while the publishers, Messrs. Harper & Brothers, give the exact date of publication as May 5, 1877.

The synonomy of the genus should stand:

Zaglossus Gill, May 5, 1877.

Acanthoglossus Gervais, Nov. 5, 1877 (Date of reading, *not* of publication).

Proechidna Gervais, Nov. 30, 1877 (Date of prefatory foot-note).

Bruynia Dubois, ———, 1882.

The evidence seems sufficient to show that *Zaglossus* was published at least as early as May 5, 1877, and, therefore, antedates *Acanthoglossus* by six months and *Proechidna* by nearly seven months.

T. S. Palmer.

Washington.

CORRESPONDENCE.

SPECTROSCOPIC OBSERVATIONS OF SATURN AT THE ALLEGHENY OBSERVATORY.

To the Editor of Science: As certain observations of mine on the spectrum of Saturn have been widely noticed by the daily press, and various reports have been spread, some of which are correct and some incorrect, but none of which were made by my authority, I take this opportunity to explain the real character of the observations. It is hardly necessary for me to say here that I have made no 'claims' whatever respecting them.

The observations furnish a direct proof of the accepted hypothesis that the ring of Saturn consists of a multitude of small bodies revolving around Saturn in circular orbits. The hypothesis is an old one, but its universal acceptance dates from the publication of Maxwell's prize essay in 1859. While the mathematical proofs given by Maxwell and his predecessors are conclusive, a demonstration of the hypothesis by the widely different method of direct observation with the spectroscope is not, I think, without interest.

The proof depends upon an application of the well-known principle of Doppler, by which the motion of a heavenly body in the line of sight can be determined by measuring the displacement of a line in its spectrum. Under the two different hypotheses, that the ring is a rigid body, and that it is a swarm of satellites, the relative motion of its parts would be essentially different; hence, to distinguish between these two hypotheses it is only necessary to find a method of sufficient delicacy, in order to bring the question within the province of the spectroscope. Any method depending on the successive comparison of the spectra given by different parts of the ring would be almost certain to fail. The method which I have employed is explained below.

If two planes, at right angles to each other, are passed through the observer and the system of Saturn, one (A) passing anywhere through the system and the other (B) through its center, the velocity, resolved in the direction of the line of sight, of any point on the surface of the system where it is intersected by plane A can be expressed as a function of the perpendicular distance of the point from plane B. It is only necessary to consider the case when the plane A is parallel to the major axis of the apparent ring. On the assumption that the ball of Saturn rotates as a solid body, and the ring as an assemblage of particles, each of which moves with a velocity determined by Kepler's third law, the expressions for the ball and for the planet are very different, the former being linear, and the latter an equation of a degree higher than the second. I have determined these expressions for the special case above mentioned. They are still further simplified by assuming that plane A also passes through the center of the planet.

Now, if we bring the image of Saturn, formed by a telescope, upon the slit of a spectroscope, with the slit in the intersecting plane A, the expressions above referred to are also the equations to the curves of which the lines in the spectrum of the planet are a part, referred to an undisplaced spectral line and the perpendicular line through its center as axes; for, in these curves, x is proportional to the perpendicular distance from plane B, and, by Doppler's principle, y is proportional to the velocity in the line of sight. The simplest case is, of course, that in which the slit coincides with the major axis of the ring; this is also the condition for which the differential velocity of points on the surface of the ring is a maximum, and it is one which can be approximately realized in observation.

Hence the laws of rotation of the component parts of the system can be determined (within certain limits) by the *form* of the special lines, and the form can be determined with very considerable accuracy by photographing the spectrum with a suitable instrument.

According to the assumptions which have been made above, and which represent the accepted hypothesis, lines in the spectrum of the ball are straight, but inclined; as compared with their direction the general inclination of the (theoretically) curved lines in the spectra of the opposite sides of the ring is smaller, and it is *reversed*. The actual aspect of the lines on my photographs is in exact accordance with that required by the hypothesis.

If the ring rotated as a whole, the lines in its spectrum would be straight, and their direction would pass through the origin; they would be very nearly prolongations of the planetary lines. Such an aspect of the lines as this could be recognized on my photographs at a glance.

The direction of a line free from displacement was obtained by photographing the spectrum of the full moon on the same plate, on each side of the spectrum of Saturn.

For further details, with the numerical results of measurement of the plates, I must refer to the May number of the *Astrophysical Journal*, in which I have described these observations at some length.

James E. Keeler.

Allegheny Observatory.

A GENERAL SUBJECT-INDEX TO PERIODICAL SCIENTIFIC LITERATURE.

The Editor of Science—*My Dear Sir:* I notice that you are printing in Science various replies to the circular of the Royal Society of London relating to the matter of a general subject-index to all scientific publications. Your correspondents have so far been in favor of such an undertaking. As I do not believe it to be practicable, it may be of interest to some of your readers to see my own reply which I venture to send herewith. I have made a few trifling changes in the copy which I enclose.

I am, very respectfully,

Edward S. Holden.

The Lick Observatory,
March 30, 1895.

Mount Hamilton, April 24, 1894.

To Professor M. Foster, *Secretary R. S., Chairman of the Committee on a Subject-Index, etc., etc.*

My Dear Sir: I beg to acknowledge receipt of the circular of April 6 relating to a proposed subject-index of scientific papers, and to express my opinions on some of the points contained therein. I will not burden you with the arguments that might be brought forward in support of the opinions, at this time; but, of course, I am very ready to give my reasons in detail should you desire them.

I. It appears to be of the utmost importance that the Royal Society should continue to issue its author-indexes, *i. e.*, the quarto Catalogues of Scientific Papers. Such indexes can be made at comparatively small expense, and by comparatively unskilled workers, under the direction of a single competent scientific head.

II. It is entirely otherwise with a subject-index. *Here the routine work must be done by the expert.* Professor Helmholtz was none too good to make the subject-index of his Optics. If it had been made by one of his pupils, it would have been less valuable; if it had been made by clerks, it would have been of little use except to beginners. It is perfectly clear that, in general, we cannot expect our bibliographies, etc., to be made by the heads of science, as Helmholtz, Houzeau, etc., and it therefore seems to me that it is unadvisable to attempt a general subject-index to science on any plan whatever.

III. If it is ever attempted at all, it should not, in my judgement, be done by international coöperation, but by a single society responsible only to itself. International coöperation has, I believe, generally failed (the only marked exceptions that I recall are the International Geodetic Association and the International Bureau of Weights and Measures). The Zone observations of the German Astronomical Society are of the highest use and excellence, but they were begun by international coöperation about 1866 and are not yet published.

IV. If the work is attempted, it should be printed in English alone, one would think. If the past is not ours, the future surely is to be.

V. My own opinion, therefore, is that the general subject-index should not be attempted. The Royal Society and other great academies might well subsidize the making of special bibliographies, for example, Houzeau's Bibliographie de l' Astronomie (already printed), or Professor Cleveland Abbe's Bibliography of the Literature of Meteorology (now in MS.), and other undertakings of the kind, when they are directed by men of special learning, and not otherwise.

VI. It, however, appears to me that the Royal Society can do a great work in the direction aimed at, at comparatively litttle expense and trouble, as follows: I would, first, say that it is necessary—essential—that an author-index should be complete. It is very desirable, but by no means essential, that a subject-index should be exhaustive. A subject-index is generally required to set the inquirer on his way, and once fairly started in his reading, the foot-notes will keep him informed. This being granted, the plan I refer to is for the Royal Society to undertake the publication, in one volume, of a subject-index, or guide, to the ten quartos of author-indexes already preprepared. The work could be easily done as follows: Select a scheme of subject-headings, under the advice of specialists. The Melville Dewey plan of library cataloguing* would serve as a basis, and it is capable of indefinite and logical subdivision. This subdivision should be made under the advice of the heads of English science; and, in my opinion, the thing to be avoided is too minute division. A practical point is, also, that the same paper should be catalogued under all the headings under which it might be sought, not merely under the strictly logical and appropriate heading.

* Which is based on the scheme of Dr. W. T. Harris, Editor of the Journal of Speculative Philosophy.

This is a detail, but it is of prime importance.

For each subject, as Astronomy, appoint a Director who should be the best man obtainable, but who may be any competent and faithful astronomer, even if he is without very wide experience and reading. Let each Director go over the author-indexes already in type, and mark each entry there printed with the numerals expressing its class or classes. Many, in fact most, of these papers can be pretty well classified from their titles alone, especially if the subject-index is not too minutely subdivided. All cases of doubt must be resolved by a reference to the original memoir. A clerk follows the Director. He finds under *Newcomb* certain papers which have been marked by the Director as relating to Astronomical Optics—Class XXXII., say. He, therefore, collects these on a card, thus:

XXXII.

Newcomb (*S*): Nos. 1, 11, 19, 26 (vol. I.).

In a subsequent volume he finds other entries belonging under class XXXII. and under *Newcomb*, and makes a separate card for them, noting the volume. The same thing is done by the Director for Astronomy for all his classes and for each author; and by the Directors of other subjects in like manner; and they are followed by copyists. Finally all cards are sorted into one series:

First, by the class—as XXXII.

Second, alphabetically by authors, and then revised and printed thus.

Class XXXII.—Astronomical Optics—Optics of the Telescope; see also classes XCV., etc., etc.

Abbe (*C*): Vol. i., 17, 34; ii., 80; ix., 92, etc.

Albrecht (*T*): Vol. vii., 13; viii., 31.

Auwers (*A*): ii., 7, 23; iii., 18, 37; iv., etc., etc., etc., etc.

By following out this plan under intelligent Directors for the special topics, the Royal Society would very soon have a nearly complete subject-index in one volume, covering its author-indexes, vols. i.–x.; and the plan, once in operation, could be carried on without trouble and at small expense. Such a subject-index would, in my view, supply all real needs in science. It certainly would in my branch of it.

The only objection that I can see to this plan is that it is not perfectly complete and logical to the extremest point. If the preface to the proposed book declares that it is not intended to be so, it seems to me that the Royal Society need not mind. After the book was printed it would, I think, be used by everyone; and it would, I believe, meet the wants of every one as nearly as any practicable plan could do.

If I have extended my remarks too far, I beg you to excuse me. I have desired to show what seems to me to be an easily obtained benefit to science, and I trust my suggestion is not impertinent to your inquiry. I am, My Dear Sir, with high regard,

Very faithfully yours,

EDWARD S. HOLDEN.

SCIENTIFIC LITERATURE.

Ein Geologischer Querschnitt durch die Ost-Alpen, nebst Anhang über die sog. Glarner Doppelfalte von A. ROTHPLETZ, mit 2 Tafeln und 115 Abbildungen im Text. Stuttgart. 1894. Pp. 268.

This valuable contribution to our knowledge of mountain structure is arranged in three parts. The first of these is a statement of the petrography and stratigraphy, and the second an account of the tectonic, of a cross-section of the Alps, in the meridian of Munich, from the plain of the Po to the Bavarian plateau, a distance of about 230 km. The third part is a discussion of the general results of the author's study. The details of the first two parts are well illustrated, both by the fine geologically colored profile on a scale of $\frac{1}{75000}$, and by the numerous excellent cuts throughout the text. Only the conclusions of the author can be adverted to in the present brief notice.

The eastern Alps have an east and west trend and the section is normal to the strike. The highest mountains have an elevation of about 3500 m., and lie towards the northern end of the section. The average elevation is 1800 m. In the northern Alps there are three principal folds, in the middle Alps four, and in the southern three, with many subordinate folds throughout. None of these folds remain in their original continuity. Fractures separate one from another and chop each of them up into a series of blocks. By faulting on these fractures the folded arrangement of the strata is greatly disturbed and obscured.

The special features of the faulting are :

1. The prevalent dislocation of synclines in such a manner that their axial troughs are thrust up and the wings dropped.

2. Anticlines with dropped crests so that the newer strata of the crests appear below the older strata of the wings. Not well exemplified in the section.

3. The occasional downthrow of the axial troughs of synclines with uplift of both wings.

4. The faulting of anticlines on longitudinal axial planes and the conversion of the convexity of the anticlines into concavity by subsequent compression.

5. Thrusts. There are five important overthrusts in the section ranging in inclination from 20° to 70°, the overthrust in all cases being toward the south.

6. Cross fractures. Highly inclined to the longitudinal faults and generally nearly vertical. These are not expressible on the profile, but are of the utmost importance for a proper appreciation of Alpine structure. They are subsequent to the folds and associated longitudinal faults, and are the latest manifestations of the orogenic forces. As such they have exerted a powerful influence upon the topography, giving the Alps, in the opinion of the author, their transverse drainage outlets and many of their lake basins.

7. There are also faults which antedate the period of Alpine folding.

In discussing the age of the folding of the Alps the author makes it clear that there have been at least two chief periods of folding, one pre-Permian, and the other post-Miocene. There were, however, diastrophic movements in the interval. This is proved, first, by the faults which antedate the later folding, and second, by the oscillation of the ocean border in the intervening time. In discussing the latter argument the author gives a series of nine profiles showing the hypothetical relative distribution of land and water over the Alpine region in *old Paleozoic*, *Permian*, *Muschelkalk*, *Rhætic*, *Lias*, *Neocomian*, *Eocene*, *Miocene* and the *Present*. These show a trangression of the sea up to the close of the Triassic, followed by a steady recession from then on to the present time. The sections, considered by themselves, might lend support to the hypothesis of Suess that the oscillation is due to the variation of the surface of the ocean. But other sections in neighboring parts of the Alps give discordant results, and it is concluded that the Alpine region was the scene of diastrophic movement between the Permian and Miocene, whether the ocean surface oscillated or remained constant.

The shortening of the arc of the earth's surface in the line of the author's section is 18 per cent., *i. e.*, the region has, in consequence of the folding, now only about four-fifths of its original breadth. If the folding of the central Alps be assumed to be pre-Alpine, then the shortening is reduced to from 12 to 13 per cent., or about one-eighth. The author contrasts these figures with the much higher values obtained by Heim, who places the shortening of the arc in the north and central Swiss-Alps at one-half. He discredits the structural interpretations which have led Heim to so large a value. He takes issue with the latter, particularly in the interpretation of the so-called Glarner double fold, and discusses this structure at length in an appendix to the volume, interpreting the structure as an overthrust and not a double fold.

In discussing the mechanics of the lateral thrust, to which all are agreed the Alpine structure is due, the author says the earth's crust may be considered a virtual arch. Then the continents must be either arches of less radius than that of the earth as a whole, or they must be superficial masses reposing upon the arch. In the latter case the continental masses would suffer no folding, but would lie as a dead weight upon the laterally compressed and folding arch below. This being contrary to experience, it is rejected, and the alternative is adopted that the continents are arches of smaller radius. The condition of folding of strata by lateral compression is, then, that they must lie below the limiting curve of the continental arch. So long as they lie above this curve they escape folding. Where folding occurs under the dead weight of rocks lying above the curve it is manifest at the surface only as elevation or depression. But the load tends to restrain folding and the latter takes place most readily where the load is least. This occurs where the continental arch merges into the geoid arch. Here is the weakest part of the arch; here the strongest folding should arise. Orogenic folding is most effective on the borders of the oceans. This fact the author finds in accord with his theoretical deductions, for it is on the oceanic borders that the continental and geoid arches intersect.

This principle is resorted to in explanation of the common up-throw of synclinal troughs. The deep synclinal folds will suffer most from the lateral compression. The consequence is that the axial troughs of the synclines are faulted up and the anticlines relatively dropped.

Part of the transverse cleavage of the rocks is ascribable to pre-Permian orogenic forces and part to the later compression which gave rise to the Alps. Most of the pre-Permian strata show this cleavage in a pronounced degree. This cleavage is best developed in the Zillerthaler towards the middle of the section, and least so on the margins of the Alpine region. The author suggests, in explanation of this deficiency of cleavage on the margins, that these parts were folded under a less load than the more central portions and were earlier lifted above the line of compression. The limestones are characterized by suture-like cracks so well known in limestones and marbles the world over. These are held by the author to be due to solution under pressure, and evidence in favor of this view is adduced.

The discussion of the metamorphism is perhaps the least important section of the book, and contributes little of importance to the general subject.

The discussion of the *cause* of mountain uplift and folding is chiefly interesting for the clear and concise statement of the expansion theory as an adequate explanation of the origin of mountain structures and plateau uplifts. The advantages of this theory over the doctrine of the earth's contraction under secular cooling are clearly set forth. The doctrine of secular contraction fails to give an adequate explanation of the phenomena of volcanology; it does not account for the distribution of the force of gravity; and it involves too great a shortening of the earth's radius. The expansion theory does not have these objections. The admissibility of the expansion theory is based on the assumption that the earth magma *may* expand on solidifying as water does. The recent work of Barnes, however, with which our author was probably not familiar at the time he wrote, so invalidates this assumption that it is no longer worthy of serious consideration.

A. C. LAWSON.

UNIVERSITY OF CALIFORNIA.

Mesozoic Plants From Kōsuke, Kii, Awa and Tosa. By METAJIRO YOKOYOMA, Professor in the Imperial University of Japan.

In this paper, illustrated by nine plates of good figures, and published as part III., Vol. VII., of the *Journal of the College of Science*, Imperial University of Japan, Professor Yokoyoma has given us a valuable addition to our knowledge of the lower Cretaceous flora. The plants of this age, known for a long time mostly in their Wealden types, and from a few localities in England and on the continent of Europe, have, by recent discoveries, been greatly increased in number and variety. The extent of the territory known to have been occupied by them has of late been still more notably enlarged. We now know lower Cretaceous plants from such widely separated series of strata as the Potomac of the Atlantic States: the Comanche series of Texas, the coal group of Great Falls, Montana; the Kootanie series of British Columbia; the Shasta group of California; the lower strata of Newton's Dakota group in Dakota and Wyoming. Professor Yokoyoma's investigations add still another region on the Asiatic side of the Pacific, and make it probable that the lower Cretaceous flora was in Asia no less important than it was in North America. These additions are especially gratifying, as the flora of this time was the last one in which angiosperms did not predominate. It is the flora of an era when predominating Mesozoic elements were about to disappear forever. If we are ever to learn what changes caused a flora consisting only of Equiseta, Cycads, Ferns and Conifers to give way to one in which angiosperms overwhelmingly predominate, and in which all these groups, except the conifers, play an insignificant part, we shall most probably find the solution of this as yet unsolved problem from the examination of lower Cretaceous plants.

In 1890 Prof. Nathorst, of Stockholm, examined a number of fossil plants from Shikoku, Japan, and determined their age to be either upper Jurassic or Wealden. Professor Yokoyoma states that he was induced to carry the investigation of this flora farther than the Swedish paleontologist had done, with the hope of fixing more definitely its age. In consequence of this he collected not only from the localities of Nathorst, but from several others showing a similar flora. He succeeded in adding a number of species not seen by Nathorst, and in procuring, in some cases, better specimens of those previously obtained. In this way the total number of species was brought up to 26, with 2 varieties. It is noteworthy that, while the flora is without doubt lower Cretaceous in age, as Professor Yokoyoma determines it to be, it contains no angiosperms. He identifies several of the species with certain ones found in the lower Potomac strata of the eastern United States. He states his conclusion as to the age of the plants in the following words: "I go a step farther than Professor Nathorst and say that the plant-bearing beds of Kozuki, Kii and Shikoku represent the whole Neocomian series, corresponding to the Potomac of America." This statement, so far as the Potomac is concerned, would be more correct if it made the Japanese beds correspond to the *lower* Potomac. American geologists now include in the Potomac the Tuscaloosa group and the South Amboy series of beds, both of which contain few, if any, of the characteristic plants found in the lower strata of the Potomac of Virginia, while angiosperms overwhelmingly predominate in each. Until the Japanese beds show angiosperms they cannot be considered as young as the uppermost portion of the lower Potomac, which, in the Brooke locality, Virginia, and at Baltimore, Maryland, show many angiosperms.

Prof. Yokoyoma has followed Prof. Nathorst in changing from Dioonites to Zamiophyllum, the name of a cycad that, so far, is confined to the lower Cretaceous. This is the species known as *Dioonites Buchianus.* This change does not seem to be called for. The reason assigned by Prof. Nathorst does not seem weighty enough to remove a name so well fixed as this, and, if a change be made, the name Zamiophyllum seems open to more objections than Dioonites. The leaflets of Zamia are articulated at their junction with the rachis and deciduous, characters which are decidedly not found in Dioonites Buchianus. These features seem to be of more importance than the obliquity of the leaflets and their narrowing towards the base, which characters in Dioonites Buchianus Professor Nathorst presents as objections to regarding this plant as a Dioonites. WM. M. FONTAINE.

UNIVERSITY OF VIRGINIA.

Repetitorium der Chemie. By DR. CARL ARNOLD. Sixth Revised and Enlarged Edition. Hamburg and Leipzig, Leopold Voss. 1894. 8°. Pp. x+613. Paper. Price, 6 marks.

This book has been written for medical students and is intended to be used by them as a convenient reference book in connection with lectures upon inorganic and organic chemistry and in preparing for examinations. That there is a demand for such a book is shown by the fact that since it first appeared, in 1884, six editions have been called for.

The work is divided into three sections. In the first one of fifty pages the general principles of the science are considered. Such topics as the laws of stoichiometry, the atomic and molecular theory, the determination of molecular and atomic weights, theory of valence, constitutional formulas and the periodic classification of the elements are here discussed. The treatment of these subjects is necessarily very brief and is not intended to be exhaustive. As far as it goes, however, it is clear and concise, and, on the whole, the views of the author represent fairly well the present position of the science. To a few statements, such as those on pages 6 and 31 that heat, light, electricity and chemical affinity are known to be different forms of motion (*bekanntlich nur verschiedene Bewegungsformen darstellen*), one is inclined to take exception.

The second section of 216 pages deals with descriptive inorganic chemistry. The elements are arranged under two heads, first the non-metals, then the metals. The more important facts as to the occurrence, preparation and properties of each element and its chief compounds are here systematically and concisely presented. Newly discovered facts in this field of chemistry have not been overlooked. Thus, for example, we find here described the preparation of azoimide, HN_3, from inorganic substances; the electrolytic preparation of aluminium and magnesium; the statement that red phosphorus is crystalline, etc.

The last section of 295 pages gives a summary of the more important facts of organic chemistry. After some preliminary paragraphs upon the analysis of carbon compounds, molecular weight determination, constitutional formulas and stereochemistry, the organic compounds are taken up in the usual way. In connection with each class of compounds the general behavior and chemical characteristics of the class are discussed. In this section of the book, as in the earlier ones, the author has endeavored to keep abreast of the times, and we find mentioned here the results of recent synthetical experiments, such as those upon the sugars; and many new substances that in recent years have become prominent because of their medicinal properties have been introduced. While the book is not intended to be a text-book in the ordinary sense, nor to serve as an introduction to the science, it can, nevertheless, be strongly recommended to all students of chemistry, who, in connection with their lecture and laboratory courses, desire to have a convenient and compact reference book—a book containing all the more important facts of general and descriptive chemistry clearly stated and provided with an excellent index. EDWARD H. KEISER.

Field, Forest and Garden Botany. A simple introduction to the common plants of the United States east of the 100th Meridian, both wild and cultivated. By ASA GRAY. Revised and extended by L. H. BAILEY. American Book Co. 1895. 8vo. pp. 519.

The first edition of this useful popular botany was issued in 1868 as a companion book to the author's 'Manual of the Botany of the Northern United States.' The present revision is planned to fill the same place as relates to the sixth edition of the 'Manual,' giving, as it does, concise descriptions of the more common native plants, and of the large number of species cultivated for use or ornament. The number of the latter category has greatly increased during the twenty-seven years which have elapsed since the first issue of the work, and as regards these the treatment is exceedingly complete. The selection of the 'common' native species has been a matter of great difficulty, and in this the book will probably be found unsatisfactory. The more usual plants of the region north of Virginia and Tennessee are for the most part included, but the Southern native flora is almost wholly omitted, so that in this respect the title is misleading. As a guide to the cultivated species it will find its greatest value. It is our opinion, however, that if the scope of the work had been restricted to the domesticated flora, and the descriptions of these plants been more fully drawn out, it would have been more generally serviceable than by treating them with the native species.

The necessity which has been felt of making the book a companion to the 'Manual' has kept up the old and unfortunate arrangement of groups which we find in that work, although we are pleased to find that the Gymnosperms have been brought into their logical position.

N. L. B.

Description des ravageurs de la vigne. Insects et champignons parasites. HENRI JOLICOEUR. 4°. Riems et Paris. 1894. Pp. viii., 236, pl. 20.

This sumptuous volume with large pages and wide margins is one of the latest contributions to the rapidly increasing literature of disease of plants. The French have always taken the greatest interest in diseases of the vine, and quite naturally, because of the extent of the industry in their country. The author of the present volume is the general secretary of the Society of Viticulture and Horticulture of Reims, and while he brings to the subject a knowledge of what various French authors have to say upon the subjects discussed, from its pages there never could be gleaned the fact that the English speaking races had ever done any work upon the various diseases. This is, perhaps, a general fault of the French, since they are so imbued with admiration for their own country that other countries hold a very subordinate place.

The work under notice is divided into two parts, one treating of parasitic animals, the other of parasitic plants. The 'animals' treated of are mainly insects, and the various orders taken up are Lepidoptera, Coleoptera, Orthoptera, Hemiptera and Arachnida. Under each of these heads the species belonging to the orders are discussed, and facts are given regarding their life history, geographical distribution, natural enemies, influence of external conditions on development, means of destruction and bibliography. The cryptogamic enemies of the vine form the subject of the second part, and we have here discussions of Oidium, mildew, anthracnose, pourridie (caused by *Agaricus melleus*), *Vibrissea hypogea*, melanose, black rot and one or two others. There are no especially new facts given in the volume as far as observed. The plates are beautifully drawn and colored and have the merit of being mainly new, only a very few figures having been copied from other authors.

J. F. James.

Icones fungorum ad usum Sylloges Saccardianæ Accommodatæ. A. N. Berlese. Vol. 2, fasc. 1, pp. 28, pl. 45.

This, the first part of a new volume of this sumptuous work, has just been published. It sustains the high character of the first volume. In it Dr. Berlese discusses the species of Saccardo's section *Dictyosporæ* of the *Sphæriaceæ*, giving diagnosis of the species of *Pleomassaria*, *Karstenula* and *Pleospora*. Only two new species are described, viz., *Pleospora parvula* on stems of *Berberis vulgaris*, and *P. magnusiana* on culms and leaves of *Glyceria vahliana*. The latter name is proposed for *P. pentamera* of Berlese's monograph, as the form is now considered distinct from Karsten's species of this name. *Pleospora carpinicola* Ell. & Ever. is transferred to the genus *Karstenula*; and *P. hysteroides* Ell. & Ever. is regarded as a sub-species of *P. andropogonis* Niessl. These are all the changes proposed, which seems quite remarkable in these days. The illustrations are excellent, and while some species seem to be perilously near others, doubtless a carefully discriminating eye would be able to separate them.

Joseph F. James.

Washington, D. C.

NOTES AND NEWS.

General John Newton, U. S. A., engineer, died on May 1, at the age of seventy-two years. He was elected a member of the National Academy of Sciences in 1876.

Dr. Karl Ludwig, professor of physiology in the University of Leipzig, died on April 27, at the age of seventy-nine years.

The *Johns Hopkins University Circular* for April contains the address made by President Low on the Nineteenth Commemoration Day, February 22. The address was entitled 'A City University,' and gives an admirable review of the scope of a great university and its relation to the city in which it is situated. After describing the different plans of the American, German, French and English university, Mr. Low continued: "The aim which the German university has set before itself and which it has very largely realized under the conditions natural to German life, is the aim, in my judgment, which the American university also should set before itself, and which it must realize under the conditions natural to American life. Because, after all has been said, the world is ruled by its thinkers, and civilization is carried forward by the patient investigators of natural laws; the lives of men are largely shaped by the teachings of experience as revealed by historic study; and the literature of men is enriched by every addition to our knowledge of the literature and language of the past. Nature's craftsmen in all these directions will produce results according to their gifts outside of a university if they get no opportunity within it. But the history of Germany clearly shows that the opportunity to serve mankind along such lines is much enlarged if to train such men is the chosen aim of the university; in part, because, in that case, the university affords the material apparatus by the aid of which the natural thinker or investigator can best do his work, and, most of all, because, in a university so constituted, the atmosphere of the place and the spirit of the men who work there are friendly to such labors."

Through the courtesy of the Assistant Secretary of the Royal Meteorological Society, we are informed that at the meeting of that Society on April 17th Messrs. A. C. Bayard and W. Marriott communicated a paper on 'The Frost of January and February, 1895, over the British Isles.' It was stated that the cold period which commenced on December 30th and terminated on March 5th was broken by a week's mild weather from January 14th to 21st, otherwise there would have been continuous frost for 66 days. Temperatures below 10° Farenheit, and in some cases below zero, were recorded in parts of England and Scotland between January 8th and 13th, while from the 26th to the 31st, and from February 5th to 20th, temperatures below 10° occurred on every day in some part of the British Isles. The coldest days were February 8th to the 10th. The lowest temperatures recorded were —17° at Braemar, and —11° degrees at Bucton and Drumlanrig. The mean temperature of the British Isles for January was about 7°, and for February from 11° to 14°, below the average, while the mean temperature for the period from January 26th to February 19th was from 14° to 20° below the average. The distribution of atmospheric pressure was almost entirely the reverse of the normal, the barometer being highest in the north and lowest in the south, the result being a continuance of strong, northerly and easterly winds. The effect of the cold on the public health was great, especially on young children and old people. The number of deaths in London due to diseases of the respiratory organs rapidly increased from February 2d to March 2d, when the weekly number was 1448, or 945 above the average. From a comparison of previous records the authors are of opinion that the recent frost was more severe than any since 1814.

The *Popular Science Monthly* for May prints an interesting account of the naturalist Conrad Gesner, by Professor W. K. Brooks. It is illustrated by twelve photo-engravings taken from the original wood cuts in his work, *Historia Animalium*, published in the latter half of the sixteenth century.

In the *Atlantic Monthly* for May Mr. Percival Lowell begins a series of articles on the planet Mars. He concludes that we have proof positive that Mars has an atmosphere, that the air is thinner at least by half than that on the summits of the Himalayas, that in constitution it does not differ greatly from our own, and that it is relatively heavily charged with water vapor. Professor Holden, on the other hand, in the May number of the *North American Review*, concludes from the observations on the spectrum of Mars made by Professor Campbell, and printed recently in the *Publications of the Astronomical Society of the Pacific*, that there is no more evidence of aqueous vapor nor of an atmosphere in Mars than there is in the case of the Moon.

The American Academy of Medicine met at Johns Hopkins University on May 4th and May 6th, under the Presidency of Dr. J. McF. Gaston.

Mr. Henry Seebohm will write the text for a new work on the eggs of British Birds, to be published by Pawson and Brailsford, of Sheffield, England. The work will contain colored illustrations of the eggs of 400 species.

Professor F. N. Cole, now of the University of Michigan, has been appointed Professor of Mathematics in Columbia College and Barnard College, filling one of the three new chairs recently endowed in Barnard College.

Professor Franz Posepny, known for his researches on mineral deposits, died on March 27th, at the age of fifty-nine years.

The Association of Military Surgeons of the United States will meet at Buffalo, New York, on May 21st, 22d and 23d, under the Presidency of Dr. George M. Sternberg.

The twenty-second National Conference of Charities and Correction will be held in New Haven during the week beginning May 24th.

Gov. Morton has signed the bill incorporating the New York Zoölogical Society and providing for the establishment of a Zoölogical Garden in New York.

Mr. Robert Fitch, antiquarian and geologist of Norwich, England, died recently at the age of 93 years.

The death is announced of Lothar von Meyer, Professor of Chemistry at the University of Tübingen, at the age of 65.

The presidential address delivered before the recent meeting of the American Society of Naturalists by Professor C. S. Minot on The Work of the Naturalist in the World is printed in the May number of the *Popular Science Monthly*.

The tenth annual meeting of the American Association for the Advancement of Physical Education was held at the Teachers' College, New York, on April 25, 26 and 27. The program included a large number of papers of scientific interest.

Dr. Kurt Rumker has been called to a professorship of agriculture in the University of Breslau.

Commissioners are being appointed by Governor Morton with a view to the acquisition of the Hudson River Palisades by the United States.

Mr. M. S. Read, now of Cornell University, has been appointed Professor of Philosophy in Colgate University.

The departments of Mining and Geology of Columbia College will hold their annual summer school in Colorado. The School in Practical Mining will be in Central City under the charge of Professor Peele, and the Geological School will meet at Golden under the charge of Professor Kemp.

Dr. Hans Thierfelder has been appointed Director of the Chemical Department of the Physiological Laboratory in Berlin.

The Amherst Summer School of Library Economy, under the direction of Mr. William I. Fletcher, will be in session from July 1 to August 3.

The April number of the *Bulletin of the Torrey Botanical Club* contains a biographical notice of John H. Redfield by Mr. William M. Canby. There is an excellent portrait and a bibliography containing fifty-four titles.

The presidential address on 'The United States Geological Survey,' given before the Geological Society of Washington, on December 18, 1894, by Mr. Charles D. Walcott, and published in the February number of the *Popular Science Monthly*, has been reprinted. It should be in the hands of all who are interested in the great work accomplished and in progress under the direction of the United States Geological Survey.

With the permission of the Prussian Minister of Education the University of Göttingen has conferred the degree of doctor of philosophy on Miss Grace Chisholm. This is a first degree conferred on a woman since Göttingen became a Prussian university.

Professor Halsted writes to *Garden and Forest* that the late winter has been very trying upon the English Ivy which covers many of the older buildings in New Brunswick, New Jersey. The leaves are mostly brown, many of them dead, and have the appearance of having been scorched by fire. It may be that the plants will revive with warm weather, but these old vines, which have been the pride of the city, are just now anything but attractive.

SOCIETIES AND ACADEMIES.

BIOLOGICAL SOCIETY OF WASHINGTON.

AT the meeting of April 20 Dr. Frank Baker exhibited specimens and gave descriptions of two anomalous forms of human lumbar vertibræ hitherto undescribed.

Dr. Theobald Smith read a paper entitled 'An Infectious Entero-hepatitis of Turkeys, Caused by Protozoa.'

The first intimation of the existence of this hitherto unrecognized disease was given by some diseased organs sent by Mr. Samuel Cushman of the Rhode Island Experimeet Station in 1893. In 1894 the speaker had an opportunity of studying a number of cases in various stages of the disease.

This begins in the cæca and manifests itself by a more or less uniform thickening of the wall. When this has continued for some time an exudate is poured out from the mucous membrane, which coagulates firmly and occludes the tube itself more or less completely. The cause of the thickening of the cæcal wall is a protozoon from 6 to 10 μ in diameter, which multiplies very rapidly within the connective tissue interstices of the mucous and submucous tissue. The irritation produced by these bodies induces proliferation of the connective tissue cells. The thickening is further increased by cell infiltration, due to inflammatory processes which appear later on, and which may be due to the absorption of bacterial products from the denuded mucosa.

In almost every case the liver is secondarily and usually very severely involved by the transportation of these protozoa from the seat of the disease in cæca through the portal system. The liver becomes covered with round isolated and confluent patches of a yellowish or brownish color, which represent necrotic foci in the substance of the liver itself. Within these, in the earlier stages, large numbers of the same protozoa may be found.

The protozoon, as stated above, is a spherical or slightly oval body, of a homogeneous appearance and containing an exceedingly minute ring-like nucleus. It has shown none of the characters of sporozoa. Its rapid multiplication within the tissue spaces, where it may be seen either isolated or in groups of two, three, four or many individuals, as well as the absence of any intercellular stage, has induced the writer to place it, at least provisionally, in the genus Amœba, and, in consultation with Dr. Stiles, to denominate it *Amœba meleagridis.* A detailed account of this investigation is to appear in a forthcoming bulletin of the Bureau of Animal Industry.

Dr. G. Browne Goode read a paper on 'The Horizontal and Vertical Distribution of Deep Sea Fishes.' The paper had for its object to demonstrate that the accepted ideas in regard to the distribution of deep sea fishes, having been founded on incomplete data, are erroneous; and that, contrary to the commonly accepted opinion, no separation of deep sea fish life into horizontal strata is possible. On the other hand, the idea that the fish fauna of the depths of the sea is the same in all parts of the world is without foundation.

Through the application of a percentage method eleven well marked faunal regions were shown to exist, as well as two subregions. The regions proposed were as follows:

1. Boreal Atlantic.
2. Eastern Atlantic or Lusitanian, with a Mediterranean sub-region.

3. Northwestern Atlantic or Virginian, with a Caribbean-Mexican sub-region
4. Southwestern Atlantic or Brazilian.
5. Boreal Pacific or Aleutian.
6. Eastern Pacific or Galapagean.
7. Northwestern Pacific or Japanese.
8. Polynesian.
9. Zealandian.
10. Antarctic.
11. Indian.

M. B. Waite,
Recording Secretary.

BOSTON SOCIETY OF NATURAL HISTORY.

The annual meeting was held on Wednesday, May 1st.

A paper was read by Mr. J. L. Tilton *On the Geology of the Southwestern part of the Boston Basin.*

Reports of the officers were received and officers for 1895–6 were elected as follows:

President, William H. Niles.

Vice-Presidents, Nathaniel S. Shaler, William G. Farlow, Charles P. Bowditch.

Curator, Alpheus Hyatt.

Secretary, Samuel Henshaw.

Treasurer, Edward T. Bouvé.

Librarian, Samuel Henshaw.

Councillors for Three Years, Hermon C. Bumpus, Charles B. Davenport, William A. Jeffries, George G. Kennedy, Augustus Lowell, Miss Susannah Minns, Thomas A. Watson, Samuel Wells.

Samuel Henshaw,
Secretary.

SCIENTIFIC JOURNALS.

AMERICAN JOURNAL OF SCIENCE, MAY.

James Dwight Dana.

Color Relations of Atoms, Ions and Molecules: By M. C. Lea.

Further Notes on the Gold Ores of California: By H. W. Turner.

Some Relations between Temperature, Pressure and Latent Heat of Vaporization: By C. E. Linebarger.

Double Halides of Cæsium, Rubidium, Sodium and Lithium with Thallium: By J. H. Pratt.

Argon, Prout's Hypothesis, and the Periodic Law: By E. A. Hill.

Improved Rock Cutter and Trimmer: By E. Kidwell.

Relation of the plane of Jupiter's orbit to the mean-plane of four hundred and one minor planet orbits: By H. A. Newton.

Chemistry and Physics; Geology; Miscellaneous Scientific Intelligence; Obituary.

BULLETIN OF THE TORREY BOTANICAL CLUB, APRIL.

Notes on Some Florida Plants: Geo. V. Nash.

John H. Redfield: Wm. M. Canby.

A Fossil Marine Diatomaceous Deposit at St. Augustine, Florida: Charles S. Boyer.

New Species of Parastic Fungi: S. M. Tracy and F. S. Earle.

The Systematic Botany of North America; Botanical Notes; Proceedings of the Club; Index to Recent Literature Relating to American Botany.

AMERICAN JOURNAL OF CHEMISTRY, MAY.

On the Two Isomeric Chlorides of Orthosulphobenzoic Acid: Ira Remsen.

I. The Action of Aniline and of the Toluidines on Orthosulphobenzoic Acid and its Chloride: Ira Remsen and C. E. Coates, Jr.

II. Further Study of the Action of Aniline on the Chlorides of Orthosulphobenzoic Acid: Ira Remsen and E. P. Kohler.

III. Separation of the Two Chlorides of Orthosulphobenzoic Acid: Ira Remsen and A. P. Saunders.

The Sugar of the Agave Americana: W. E. Stone and D. Lotz.

The Law of Mass Action: J. E. Trevor.

Chromates of the Rare Earths: Chromates of Thorium: Chase Palmer.

On a New Method for the Separation of Copper and Cadmium in Qualitative Analysis: Allerton S. Cushman.

Reviews.

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FRIDAY, MAY 17, 1895.

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison on Hudson, N. Y.
Subscriptions and advertisements should be sent to SCIENCE, 41 N. Queen St., Lancaster, Pa., or 41 East 49th St., New York.

THE BALLISTIC GALVANOMETER AND ITS USE IN MAGNETIC MEASUREMENTS.

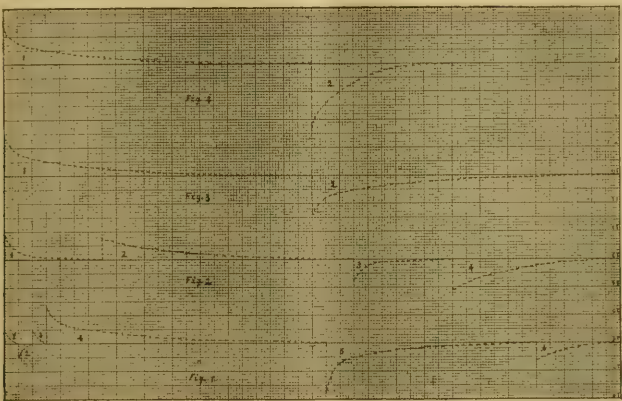
THE ballistic galvanometer gives one of the most convenient and reliable means of measuring the total quantity of electricity conveyed through a circuit by a transient current when the conditions are such as to admit of its legitimate application. It is well known, however, to experienced observers that in a large number of the common applications of the instrument the results are doubtful because the fundamental principle on which the calculations are based is not sufficiently attended to. The object of the present note is to direct more particular attention to the conditions under which accurate results may be obtained.

Most text-books on electrical measurements give formulæ for the calculation of the quantity of electricity required to produce a given deflection, or throw, of the galvanometer needle, and also indicate how the constant of the instrument may be determined, and how the damping effect of the air and of induced currents may be allowed for. The formulæ assume as fundamental that the duration of the flow is negligibly small in comparison with the time which the needle takes to reach its greatest deflection. This fundamental condition is of course implied in the name ballistic, but it does not seem, from the applications which we find continually made of the instrument, that the simple statement, as commonly given, is sufficiently explicit to prevent a vicious use of this method of experiment. For the measurement and the comparison of the capacities of condensers and similar purposes the ballistic galvanometer is generally reliable, providing the constant is properly determined and suitable appliances used for manipulation. In magnetic measurements, however, it not unfrequently happens that the duration of the current is much too great, and not only too great, but variable throughout the series of observations, the results of which are compared. The carelessness with which this method of experiment is recommended by authorities who ought to know better is astonishing. We find, for instance, in one of the most widely used text-books on the practical application of electricity the statement that to measure the total induction across the armature of a dynamo a few turns of wire may be wound round the section of commutation and connected in series with a ballistic galvanometer, and the throw of the needle, when the field circuit is closed or broken, will indicate the induction. For any ordinary galvanometer such statements are simply nonsense.

Let us take, for the purpose of illustration, the measurement of the magnetic quality of iron, according to Rowland's method, or some one of the modifications of it which have come into use. Here the specimen is a ring, which, in most of the recent determinations, is made up of wire or thin sheet iron. The ring is surrounded along its whole length by one or more magnetizing coils, and over a short length by a secondary or induction coil, included in the circuit of a ballistic galvanometer. The inductions produced by different magnetizing forces are then measured by observing the corresponding throws of the ballistic galvanometer needle. Various modes of operation are adopted, as, for instance, the magnetizing force is changed by successive steps from an extreme value in one direction to an equal extreme in the opposite direction, and then back by similar steps, thus passing the iron through a complete cycle of magnetization. The corresponding successive throws of the galvanometer needle are then taken to indicate the increased or diminished magnetic induction, due to the different changes of magnetizing force. In another method the magnetization is changed always from the extreme in one direction as the zero for each observation. The change of magnetization is in this case produced either by diminution, and, if necessary, reversal of the magnetizing force in one magnetizing coil, or by the use of a second coil and a current sent through it in such a direction as tends to reverse the original magnetization. The reverse half of the cycle is then obtained by passing the extreme current through the second coil, then slowly decreasing it to the required value, and afterwards suddenly breaking the circuit. The changes of induction are measured as before by the deflections of the ballistic galvanometer needle. Other methods might be mentioned, but these will serve for our present purpose.

In order to illustrate the variable conditions under which such experiments are made, the curves given in figures 1–4 have been drawn by an autographic recorder showing the actual character of the induced current which is sent through the galvanometer under different circumstances. In figure 1 the numbers 1, 2, 3, 4, 5, 6, give the curves of variation of current with time (the ordinate being current and the abscissæ time) for the following set of operations: Two magnetizing coils being placed on the iron a constant current was established in one of them; next, for curve 1, a small reverse current was sent through the other coil; for curve 2, the second coil was closed across the battery and the battery cut out; for curve 3, the battery put in circuit and the current again established; for curve 4, the current was increased by short circuiting part of the resistance in the circuit; for curve 5, the short circuit was taken off and the current reduced to the same valve as at the end of 3; for curve 6, the coil was closed across the battery terminal and the battery taken out of circuit.

Figure 2 shows the result of a similar series with the magnetizing force for curve 1 greater and the operations 2 and 3 of figure 1 omitted. Figure 3 illustrates the result when the whole of the reverse current was put on in operation 1, and the curve 2 shows the effect of short circuiting the battery in the second circuit. Figure 4 is the same as figure 3 so far as the first operation is concerned, but in the operation which gave curve 2 the second magnetizing circuit was simply broken. The scales of these curves are arbitrary, but are the same for the different curves, and hence the relative magnitudes of the changes of current may be estimated from the curves. The reverse current in the second coil was not at any time adjusted so as to give an equal but opposite magetization to that given by the coil through which the constant current was kept flowing. The two primary objects of drawing the curves were (a) to show the great difference in the time required to produce changes of magnetization as depending on the magnitude of the change, and (b) to show the differences in time for the two cases of short circuit and complete break of the magnetizing coil circuit.



The fact that the time required to produce the change of magnetization is dependent on the amount of change shows that, unless the period of the galvanometer needle be so long that even the longest of these times is short in comparison, the measurements of the higher magnetizations will be more in error than the lower. The effect of this on the magnetization curve of iron is to render the steep parts of the curve less steep. The curves 1 and 2 of figure 4 show the effect of the diminished inductive retardation when the circuit is broken in shortening the time required for the magnetization to change back as compared with the time required to produce it. Curve 2 of figure 3 compared with curve 2 of figure 4 shows the relative times when in the first case the e. m. f. is removed, but the circuit left closed and in the other case the circuit is broken. Comparisons between the deflection due to the application and the removal of magnetizing force should always be made in such a way that the circuit has the same inductive retardation in both cases.

The e. m. f. should therefore be introduced and removed without breaking the circuit.

If we assume no damping action on the needle the equation to its motion is

$$\frac{d^2\theta}{dt^2} + n^2\theta = X$$

where n is a constant depending on the galvanometer and the intensity of the magnetic field at the needle, while X depends on the galvanometer and on the nature of the transient current. If we suppose the impulse given to the needle to be due to the charge or discharge of a magnetic field and take the permeability of the core as constant we may put $X = A\, e^{\alpha t}$ where A is a constant depending on the galvanometer and $a = \frac{R}{L}$ where R is the resistance and L the co-efficient of induction.

We thus get $\frac{d^2\theta}{dt^2} + n^2\theta = A\, e^{-a\, t}$

The solution of this equation is

$$\theta = \frac{A}{n^2 + a^2}\left\{ e^{-a\, t} + \frac{a}{n} \sin nt - \cos nt \right\}$$

or, $$\theta = \frac{A}{n^2 + a^2}\left\{ e^{-a\, t} + \sqrt{\frac{n^2 + a^2}{n}} \sin(nt - \beta) \right\}$$

where term $\beta = \frac{n}{a}$

The constant n is equal to $2\pi/T$, where T is the free period of the needle.

Take, as a particular case, a ring of mean circumference $l = 30$ centimetres, and cross sectional area $S = 2$ square centimetres, and suppose the total number of turns on the magnetizing coil to be $N = 600$, the permeability $\mu = 2000$, and the resistance 1 ohm. Then the increase or decrease of induction per unit current $\times N = L = \frac{4\pi N^2 \mu S}{l \times 10^9} = \frac{6}{10}$ nearly in henrys. Hence we have a or $R/L = \frac{10}{6}$, and the current at time t, after the removal of the e. m. f., the circuit remaining closed, is $C_t = C_o\, e^{-\frac{10}{6} t}$ where C_o is the current just before the e. m. f. is removed. Giving t different values in seconds we have the following values of the ratio C_t / C_o:

t in seconds =	1	2	3	4	5
C_t / C_o =	0.1889	0.03565	0.00673	0.00127	0.00024

If the resistance be taken equal to 10 ohms then the unit of time in the above table is to be taken as one tenth of a second, and so on for different resistances. Precisely the same calculation applies to the case of increasing magnetization, only C_o is then the final steady current, and the numbers in the line C_t / C_o are the differences from unity of the ratio C_t / C_∞, that is, the equation becomes $C_t / C_o = 1 - e^{-\frac{10}{6}} t$.

Hence, remembering the high value which L may have at certain parts of the cycle in the case of iron, we see that to insure the whole quantity of electricity getting through the galvanometer coil in a small fraction of the quarter period the resistance would require to be in the neighborhood of 1000 ohms for a needle of 4 seconds period, and of 100 ohms for a needle of 40 seconds period.

The quantity of electricity which flows through the coil in time t is given by the equation

$$Q = \int_0^t C_0\, e^{-\frac{R}{L} t} = C_0 \frac{L}{R} \left(1 - e^{-\frac{R}{L} t}\right)$$

Hence in the case supposed above the quantity which flows in one second is about $\frac{4}{5}$ of the whole when the resistance is one ohm, and about $\frac{4}{5}$ of the whole in $\frac{1}{100}$ of a second when the resistance is 100 ohms.

The equatim $\theta \frac{A}{n^2+a^2}\left\{ e^{-a\, t} + \sqrt{\frac{n^2+a^2}{n}} \sin (nt - \beta) \right\}$ reduces to $\theta' = \frac{A}{a\, n}$ in the case of a being very great in comparison with n and this form can be readily reduced to the equation commonly given on the supposition of the time of discharge being small in comparison with the period of the needle. Keeping to the case taken above of the period 4 seconds or quarter period 1 second we have the following values of α :—

$\alpha =$	1	10 / 6	2
$\theta / \theta' =$	0.632	0.774	0.810

The middle one of these values corresponds to the ring discussed above when the resistance is one ohm. In these three cases the maximum deflection is reached after 1.54 seconds, 1.45 seconds and 1.40 seconds from the time when the e. m. f. is applied to or removed from the circuit. The conditions here taken may be considered extreme in so far as the period of the needle is concerned, but it is not difficult to find examples of actual measurements in which the period has been equally short.

The examples here given are probably sufficient to direct attention to the care that must be taken in the choice of apparatus and the arrangements of circuits when the ballistic galvanometer is used in magnetic measurements. The method is only applicable when α is so large that θ and θ' are practically equal to each other and this condition is approximated to by making R large and L as small as possible. Hence, high e. m. f. s. should be used with high non-inductive resistance in the circuit and magnetic force should be secured with small numbers of turns by using large currents. It is well always when comparing charge with discharge to keep the induction of the circuit the same in both cases by means of an apparatus which cuts out the battery and at the same time keeps the circuit closed through an equal resistance, instead of breaking the circuit when the discharge is measured. A check on the accuracy of the observations in any particular case may be obtained by observing the successive extreme deflection of the needle. If the first deflection has the proper magnitude the mean ordinate of the curves drawn through the extreme deflections to opposite sides of zero should be at all points zero. When the duration of the current is a large fraction of the time of swing of the needle the mean of the deflections to opposite sides will lie for the first few swings on the same side of zero as the initial deflection.

THOMAS GRAY.
ROSE POLYTECHNIC INSTITUTE.

THE SCIENTIFIC METHOD AND MODERN INTELLECTUAL LIFE.

SCIENCE, as a necessary term, is possibly upon the verge of obsolescence. Within the last half-century it has spread the mantle of its meaning over almost every department of thought until to-day knowledge and science are perceived to be so nearly co-extensive that the newer term might rightly yield to the priority of the older. While twenty-five years ago one heard much about science and the languages as rival claimants for place in the college curriculum, one now listens to the message of that useful *science*, classical philology. Then the polemic between science and religion seemed earnest indeed; now theologians and laymen are alike shocked when Mr. Benjamin Kidd suggests that there can not be a science of religion. Antithesis has softened into synonymy. It is not that the lion of science has devoured the lambs of art, literature and philosophy; it is rather that systematists of opinions and beliefs have determined a generic unity where before variety was supposed eternally to exist. Such condition has arisen, it may be presumed, from the prevalence at least among Western nations of what has come to be denominated the scientific method. This prevalence is not yet universality. It does not yet extend in full measure to every individual; nor does it, perhaps, persistently characterize the intellectual life of any man at the present time. The atavism of superstition must somewhere mar the image and superscription of one's intellectual inheritance. Nevertheless, so widespread and so dominant everywhere is this scientific method that in a broad sense it might be accorded universality. It becomes, then, an important matter to discover, if one can, what effects upon the intellectual life, not only of the individual, but of society in general, are resulting from the method now and will develop in the future.

It is possible to define science as that orderly mass of facts and hypotheses within experience by which we criticise our primitive ideas. Social, not merely individual, experience and the broader implication of criticism are intended. The scientific method is therefore that intellectual process by which facts are recognized, accumulated and arranged, hypotheses framed, tested and exploited and conclusions drawn, verified, accepted and applied where they may seem best to fulfil their function in the enginery of social progress. It would be an error to suppose that any clear demarcation exists between knowledge that is scientific and other knowledge that is not; nor can one, search as he will, discover the birth-place or learn the natal day of the scientific method. As Dr. Osborn has shown, from the Greeks to Darwin there exists a continuity of speculative evolution. Bacon was not the first to make use of induction. Franklin did not discover electricity, nor Lamarck the impermanence of species. Everywhere the older phases of thought merge into the newer, much as one picture seems to follow another in the cunningly presented dissolving views or phantasmagoria of the stage. Yet it will scarcely be gainsaid that while yesterday the scientific method was indeterminate and sporadic, to-day it is definite, characteristic of most that is valuable in thought and in a sense universal.

Carrying farther the definitions which are so useful if one desires to make one's meaning plain, it will appear that the intellectual life is a concept that has enlarged, imperceptibly at first, but surely during these later days. When one sees the phrase in type one does not stop with Hamerton. Insensibly the meaning of the word *life* has expanded in the minds of thoughtful men until the limits of individualism are instinctively transcended and the instant idea is of the greater social, not of the lesser individual organism. No more impressive evidence of an onward movement in thought could be offered, no more conclusive demonstration of some welding, humanizing force unconsciously at work generalizing and extending the point of view. The intellectual life is seen to be not merely an efflorescence of culture; it is not the knowing of the best that has been said and written in the history of the world; it is not the peace of introspective calm, nor serenity in a delightful oasis amid the desert sands of a crass and insentient materialism; it is a strenuous, an austere exertion of those high human powers that command the world of things for the world of thought. Culture, essentially individualistic, is not the concretely social and dynamic intellectual life. It is true one must not altogether forget the traditional meaning of the phrase, but that traditional meaning is after all suggestive principally as a vestigial character. Its peculiar interest lies in the fact that it has been outgrown.

Having indicated the content of such phrases as *intellectual life* and *scientific method*, it remains to show briefly how the latter in its slow but massive development has influenced the former, or rather how the two have unfolded themselves in unison. In the course of the examination, it will perhaps become apparent that the larger modern implication of such a phrase as *intellectual life* is due, above all, to precisely such influences as have been brought to bear upon the texture of society by the progressively larger, though in great part unconscious, activity of what has been termed the scientific method.

Noting first the evident contact points, especially in pedagogics, between the scientific method and the intellectual life of the individual, one cannot but reaffirm in the light of experience what has long been maintained by those who advocate the fundamental position of science in every educational system. In the domain of reason, breadth, grasp and clarity are developed as under no other discipline. Sanity in emotion is secured, and vigor, together with modesty and a reasonable deliberation, tends to distinguish the active life of the man who has brought himself into what may be styled a scientific frame of mind. The accumulation of any mass of facts, if the search be tireless, must stimulate the growth of a certain cosmopolitanism. The Scarabean doubtless found more foreign letters in his mail than did the Autocrat. When one goes farther and attempts an induction or an hypothesis he must hold firmly the facts he has, his eye must be unclouded, his step steady, or he will fail. Still more certainly will his office remain an humble one if, when he ventures to make known to others his discoveries or conclusions, he want in transparency and precision. Nor will the man whose life is truly illuminated by the sun of science lack somewhat of self-control; under less favorable conditions this equipoise may take the guise of unenthusiasm, but at its best it is activity —sympathic, tolerant, enlightened. Such being their recognized educational productivity, the so-called sciences have taken masterful positions in the schools of Europe and America. It will not be necessary here to point out in detail the precise pedagogic adaptability and the importance of the various sciences in a general educational scheme; it will suffice to inquire whether it be not true that whatever branch of learning popularly classed outside of the sciences maintains itself in school curricula, it does so by virtue of the scientific method being possible in its presentation.

Although clearly not so fundamental in their effect upon the individual character as must be these simple reactions where the scientific method is brought into an alembic with nascent intellect, there are some relatively subtle yet far-reaching influences that should not be overlooked. From a number that might be chosen I will bring forward three. A just appreciation and personal application of the scientific method tends to discourage introspective and metaphysical habits of thought, to counteract the insidious pessimism with which so much of modern life is tinged, and to impel one unmistakably toward a rational and sober altruism. I would not be understood to regard metaphysics as altogether pernicious. At its worst it may be as Walter Pater thought it, 'the art of methodically muddling one's self,' but it has its place and its mission. Yet there is an individualistic and almost a selfish tendency in much of what passes for philosophy. One need not pursue the thorny path of dialectics to the end that one denies the existence of all but himself. Whatever intellectual attitude demands, an attentive scrutiny of one's own mental, moral or physical mechanism can not but be self-centered. For this reason, if for no other, the failure of deductive philosophy to carry its influence beyond the lecture room or seminarium might easily have been predicated in advance. The student of the history of philosophy is scarcely more impressed by the cumulative intricacy of philosophic speculation than by its progressive futility as a guide in the every-day affairs of life. Employment of the scientific method discourages on the whole that naive self-inspection which was the badge of the older intellectual *cultus*, just as on the other hand it lends encouragement to the open-eyed, outward searchings of the modern investigator. This objectivity, whether or not it be an indication of intellectual maturity in a nation, is distinctly characteristic of modern Occidental civilization in no less degree than the reverse condition is supposed to mould the thought and life of the Orient. Such objectivity—not without the stigma of materialism—seems to result from the general prevalence of the scientific methods in contemporaneous thought.

If it be protested that the scientific method is blighting in its tendency to suppress metaphysics, not so certain objections will be made to its efficiency as a counterfoil against philosophic pessimism. Whether one professes with Schopenhauer to believe that this is the worst possible world, or joins von Hartmann in that more dismal suggestion that this is the best possible world, but not worth living in; whether one sigh with De Musset, weep with LeConte de Lisle, or rave with Baudelaire, one must give the sanction in so doing to existence, and if to existence then to evolution, by which such existence became possible, and if to evolution then to progress. Therefore, if we have the scientific spirit two escapes are possible from the darkness of pessimism—superficially by occupying one's self with some scientific protocol, or more profoundly by turning one's despairing thoughts aside in the recognition of an indwelling power in the social organism which makes, if not for righteousness, at least for social evolution. If under the leadership of the scientific method one can actually grasp the form of truth there is in positivism; if one can really feel the existence of a social organism and listen to his ideals as did Comte, believing them to be the sealed orders of humanity; if one can learn with Weismann to know the profound sense in which all men are brothers, *for all men are one*, it will make little odds to him whether he be shown with most convincing logic that the constitution of the nervous system makes pain the positive and pleasure the negative and that death is merely an acquired physiological trait useful to insure the permanence of the species at the zenith of its youth and power. But after all, perhaps the most fatal blow that the scientific method strikes to pessimism is, as argued above, in its settled antagonism to introspection. For pessimism as an ethical and metaphysical system is based peculiarly upon self-observation. A man does not despair of the world from what he sees around him, but from what he sees in the secret places of his own heart. By its discouragement of morbid subjectivity the scientific method cuts the very foundation from under the philosophic pessimist.

We are led then to the third postulate—that the scientific method impels us unmistakably toward a rational and sober altruism. This indeed links itself inseparably with the others. If defective this type of altruism is defective in fire and in enthusiasm. Domination by the calm reasonableness of the inductive philosophy does not stimulate one to take up the tambourines and drums of the Salvation Army. He who has ordered his mental processes in accordance with a scientific method is inclined to prefer the charity organization to personal alms-giving; he shrinks a little from the zeal of the social reformer; he is unlikely to be a poet in literature, a rhapsodist in music or a revivalist in religion. He is rather to be sought among the rank and file of the great, silent army which is behind every reform as 'public sentiment' or as the 'moral sense of the community.' But as has been pointed out elsewhere this quiet acquiescence is a necessary factor in social reform, just as underneath every successful revolution there has been a subtile and tacit confession of faultiness in the established order by the very party that storms barricades in the struggle for its maintenance. To sum it up in a word, under the scientific method men may not be so ready to conquer rights and privileges for others, but they are prepared unflinchingly to concede such rights when the request has come with authority.

From this point the transition is easy to the consideration of what influence the scientific method may exert in a general way upon society as a whole. There is not space in the compass of a review article to discuss adequately a matter of so many complications, but it is possible to offer a syllabus for reflection. It must first of all be kept in mind that *world-wideness* is in the fabric of all sceince. Since induction is objective, the scientific method is cosmopolitan. The humble describer of a new species of butterfly must have passed, in orderly fashion, all the butterflies of the earth before his mind ere he ventures to set his own over against the rest as new. The question of the German University laboratory—'Was haben Sie neues gefunden?'—presupposes a knowledge of what the world has done before. This characteristic of the scientific method cannot be too strongly emphasized. What then must be the natural reflex of the method upon social institutions?

Science has bound the world together by its spirit no less than by its discoveries. Interest in others would make communication easy even if the telegraph did not exist. Sympathy is a stronger cable than those that lie along the bottom of the Atlantic. Hence in every region of human intellectual activity one traces the broadening influence of the scientific method. In politics, democracy; in warfare, humanity; in commerce, freedom; in art and in literature, realism; in all the social relations of life, kindliness and charity; in religion, tolerance and dynamic helpfulness—these are the children of this scientific method. Perhaps nowhere better than in the field of religion has the change to the new order made itself felt. Religion is to-day recognized as social rather than as individual. Faith is blended in works, and in place of a pitiful solicitude for the welfare of one's own immortal soul there has been developed a missionary spirit, boundless in its self-sacrifice, a magnificient phenomenon of altruism. It is very remarkable when comparing theological literature of say the Oxford Tractarian movement with that of the present decade, such as the discourses of Washington Gladden or the Unitarian writings of Martineau, to note that the essential difference between the two groups is that in the former everything is discrete and individualistic in tone, while in the latter everything is concrete and social. Under the stress of the scientific method, sanctity has seemed second to helpfulness, just as individual culture has seemed a less noble end than social progress.

On the whole the influence of the scientific method upon society is two-fold. Statically it has added organizability to the social character, and by virtue of this it has dynamically contributed to the advance in social progress. The influence mentioned upon character could scarcely strike more profoundly, for the capacity to take part in organization is possibly the most important trait of all in social character. Precisely as organization becomes most perfect will progress be most rapid. And here one perceives that a veritable intellectual sanction for progress is to be sought. The author of *Social Evolution* has denied that such sanction exists, but apparently without taking into account the very method by which he arrived at this conclusion. There is quite as strong an instinctive quality in science as in religion. Each takes progress for granted, each in its own field contributes to the advance, and in so doing each gives its sanction to the movement. Since progress lies principally within the realm of the social organism, its sanctions are social rather than individual. And the error has been in failing to perceive the strong social nature of a certain type of intellection and in assuming the metaphysical or introspective type to be the only one worthy of consideration. In the phrase 'devotees of science' there is a gleam of true meaning, for in its social quality, its instinctiveness, science is akin to religion. One might term science an intellectual religion and not go wide of the mark. While it may be argued that philosophy in the traditional sense does not sanction progress, it cannot be argued that science withholds either sanction or its encouragement. Science is social thought reflected back into the mind of individuals; metaphysics is individual thought radiated outward upon society. The sanction for social progress is therefore derived rather from society as a whole than from individual introspection. For this reason the intellectual sanction is all the more forceful and takes its place beside the moral sanction offered by religion. There need then be no fear that progress is intrinsically irrational, and there may be a science of religion, as there is a religion of science. It is the function of the scientific method to organize for victorious contest the battalions of the intellect, while religion may bring on the moral forces. Therefore it appears that progress is an open-minded movement onward, of which we are all a part, and to which reason, under the sway of the scientific method, gives sanction no less than does emotion.

CONWAY MACMILLAN.

UNIVERSITY OF MINNESOTA.

THE LIQUEFACTION OF GASES.—A CONTROVERSY.

THE scientific world has been treated during the last few weeks to one of those happily to-day rather infrequent controversies which are always unseemly, the more so when the parties are men of eminent scientific reputation. Polemics in science may sometimes be entertaining, but are always unprofitable and tend to bring discredit upon the participants, if not on their work. The recent discussion* on the subject of liquefaction of gases is no exception to the rule.

Prof. Dewar, in defending his failure to give Prof. Olszewski due credit, has made what might have been looked on as a pardonable omission appear almost as intentional deceit. In taking up the cudgels in Prof. Olszewski's defense, Professor Muir has seemed to make an unjust and almost spiteful attack upon Professor Dewar; while Professor Olszewski, whose work was already too well and favorably known to need any defense, has added nothing to his reputation; indeed, he has rather laid himself open to the charge he prefers against Professor Dewar, inasmuch as in his article in the Engineering and Mining Journal he makes but slighting reference to the work of Pictet and Cailletet, and the name of Wrôblewski is but once, and that incidentally, mentioned. The following is a summary of the more important work of these investigators in this field:

In 1877 two independent experimenters almost simultaneously succeeded in condensing to liquids the so-called permanent gases. Cailletet, the French ironmaster at Chantillon-sur-Seine, used a hydraulic press, and obtained the necessary lowering of temperature by suddenly diminishing the pressure on the compressed gas. A mist appears in the glass tube containing the gas, and, except in the case of hydrogen, condenses to small drops. Pictet, at Geneva, used the pressure occasioned by the generation of the gas in wrought iron cylinders, and cooled his steel condensing tube with liquid carbon dioxid. In experimenting with hydrogen, Pictet obtained an opaque steel blue liquid, which appeared to solidify

*On the Liquefaction of Gases. Charles Olszewski, James Dewar, M. M. Pattison Muir, *Nature*, Jan. 10, 1895, and following numbers. Letters to the Editor. Also in *The Philosophical Magazine*.

on striking the ground. Later researches of Olszewski and Krzyzanowski have shown that this liquid could not have been hydrogen, and that the gas obtained, as Pictet's was, from potassium formate and caustic potash is by no means pure hydrogen. To Cailletet and Pictet belongs the credit of being the pioneers in this field, and to them in 1878 was awarded the Davy medal of the Royal Society.

A few years later (1883) the work was taken up by Wróblewski and Olszewski at the University of Cracow, and after the death of the former in 1886 was carried on by Olszewski alone, and more recently by Olszewski and Witkowski. The apparatus used was derived from that of Cailletet, the production of cold being by the boiling of liquid ethylene in a vacuum.

The aim of Olszewski's researches has been the exact investigation of the properties and conditions of matter at low temperatures. Many physical constants of the so-called permanent gases have been determined, and especially the optical properties of liquid oxygen have been thoroughly studied. More recently Olszewski was entrusted by Lord Rayleigh and Professor Ramsay with the liquefaction of Argon, and the results of this investigation have been widely published. His latest work is the determination of the critical temperature (—233°) and the boiling point (—243°) of hydrogen, the last gas which still resists condensation to a static liquid.

Professor Dewar, in his position at the Royal Institution of Great Britain, has been looked upon, perhaps, rather as a public lecturer and brilliant experimenter than as an exact investigator. In 1884 he delivered an address at the Royal Institution on the work of Wróblewski and Olszewski, during which oxygen and air were liquefied for the first time in public. He later so improved the apparatus, which was founded on the principles used by Cailletet and by Olszewski, that he could obtain with safety and without great difficulty very considerable quantities ('several pints') of liquid oxygen or air, and his public experiments with this liquid are famous. By the use of liquid air he has studied the electrical resistance of metals and alloys at low temperatures, extending greatly the work of Clausius, Cailletet and Bouty, and Wróblewski in this direction, and has undertaken work on the tension of metals at low temperatures. As far as these latter experiments have been carried, they seem to show that the breaking stress of metals increases decidedly at low temperatures (—182°) and hence that there is no decrease of molecular attraction as absolute zero is approached, although the most powerful chemical affinities are in abeyance, as Professor Dewar has shown. He was also the discoverer of the magnetic properties of liquid oxygen.

In his earlier work Professor Dewar certainly did not fail to give Professor Olszewski due and full credit. Of late years he has failed to often refer to him, and the charge that he has sometimes apparently claimed as his own that which he should have attributed to the Polish professor is, perhaps, not wholly unfounded; yet the claim of the latter for priority was so well understood by scientific men that his attack on Professor Dewar was at least unnecessary. That the Englishman, possibly somewhat rankled that his countrymen should have called on a foreigner to assist in their study of Argon, was led to make a spirited rejoinder, to pose as more of an independent investigator than the facts warrant, and to depreciate the work of his opponent, is perhaps not to be wondered at, but certainly not to be excused. Altogether the discussion is profitless and unfortunate.

Jas. Lewis Howe.
Washington and Lee University.

CURRENT NOTES ON ANTHROPOLOGY (VIII.).

A SPELÆOLOGICAL SOCIETY.

Of course, everybody knows what spelæology means—or perhaps there are one or two who do not, considering that the word was manufactured only last year. Its sponsor was M. E. A. Martel, a French scientist distinguished for his numerous and skillful explorations of caves for scientific purposes. In Greek *Spelæus* means a cave, and 'spelæology' is the science of cave-hunting,as it was called by the English. A society has been formed in Paris with that as a specialty, concerning which the curious inquirer can learn more if he addresses M. Martel, No. 8. Rue Menard.

The subject is one richly deserving this kind of concentrated and special study. No localities preserve more perfectly the records of the past than caverns. In their darkness and silence, guarded by their massive walls, layer after layer of deposits have been strown by their occasional visitors, by inundations and by percolation. A stalagmitic floor, clean, hard and imperishable, seals the traces of every occupant in perfect preservation through all time. Some of the most important discoveries in geology and archæology are due to these conditions. I need but mention the labors of Lartet, Christy, Boyd Dawkins, and in this country of Cope and Mercer, to attest this.

But nowhere is ignorant excavation more fatal than in cave-deposits. There is a high science in their examination; and M. Martel has planned an admirable scheme to disseminate valuable instruction on this essential point.

A VALUABLE STUDY IN PRIMITIVE ART.

A study in primitive art of the most satisfactory character has been lately published by the Royal Irish Academy. It is entitled 'The Decorative Art of British New Guinea: A Study in Papuan Ethnography,' by Alfred C. Haddon, M. A., Professor of Zoölogy in the Royal College of Science, Dublin. The author approaches his topic with an extensensive personal knowledge of it, and a thorough appreciation of its bearings on the leading questions of ethnology in general. The memoir is in large quarto, with twelve full-page plates and many cuts inserted in the text. Some of the designs are colored, and all are copied with fidelity and clearness. Their variety is astonishing, considering that we are dealing with the art of cannibalistic savages, and the sense of proportion and harmony often manifested is just and real. The rapid development of conventionalism is evident, and even in such primitive examples one soon loses the traits of the original design. This has often been commented on in American aboriginal art.

Professor Haddon corrects the impression which sometimes prevails, that art decoration, for itself, is unknown to savages. Art is related to ease; as he says, 'Art flourishes where food is abundant.' Another vital conclusion he expresses in these words: "The same processes operate on the art of decoration, whatever the subject, wherever the country, whenever the age, illustrating the essential solidarity of mankind." No truer words have been spoken on the subject, and ethnographers should learn them by heart.

In every respect the memoir is most creditable to the writer and to the institution which publishes it.

D. G. Brinton.
University of Pennsylvania.

JAMES EDWARD OLIVER.

On March 27th, 1895, after an illness of ten weeks, died Professor J. E. Oliver, of Cornell University, universally honored and beloved.

For more than twenty years he has been at the head of the department of mathematics in this great institution.

Born in Maine in 1829, even from his graduation in 1849 he ranked as a mathematical genius, one of the most remarkable America has produced. But he seemed to have no ambition to leave an adequate record of his mental life in print. In personal character he resembled Lobachévsky, whom he intensely admired.

He was spontaneously loyal to the good and the true, enthusiastic, thorough, painstaking. He loved poetry ; he loved Shakespeare ; he was averse to religious creeds. For Professor Oliver goodness was spontaneous. He did the right not because it was right, but because he intensely wished to do just that. The spring of action seemed a combination of sympathy, perception, knowledge, scientific logic.

In mathematics Professor Oliver worked for the love of it and because he was deeply convinced that mathematics affords that fine culture which the best minds seek for its own sake.

He was a pronounced believer in the non-Euclidean geometry.

I vividly recall how he came up after my lecture on Saccheri at Chicago, and expressing his interest in the most charming fashion, proceeded unhesitatingly to give me a profound lecture on stellar parallax, the measurement of the angles of astronomical triangles and the tests of the quality of what Cayley called 'the physical space of our experience.'

Again, after the Brooklyn meeting of the American Association, he took up the same subject with me, explained a plan for combining stellar spectroscopy with ordinary parallax determinations, and expressed his disbelief that C. S. Pierce had proved our space to be of Lobachévsky's kind, and his conviction that our universal space is really finite, therein agreeing with Sir Robert Ball.

GEORGE BRUCE HALSTED.

UNIVERSITY OF TEXAS.

JAMES DWIGHT DANA.

WE take from the authorized account by Professor Edward S. Dana, in the May number of the *American Journal of Science*, the following facts concerning Dana's life. He was born in Utica, N. Y., on February 12, 1813, his father and mother being from Massachusetts. He early showed an interest in natural history, which increased during his course at Yale College from 1830 to 1833. Immediately after graduation, Dana spent fifteen months as instructor in mathematics to the mid-shipmen of the United States Navy, the time being passed in the Mediterranean. He then spent two years at New Haven, being part of the time assistant in chemistry to Benjamin Silliman. The four following years were spent with the exploring expedition sent by the government of the United States under Wilkes to the Southern and Pacific Oceans. The following years were devoted to the study of the material collected. In 1844 he married a daughter of Prof. Silliman, who survives him, and in 1846 became associated with him in the editorship of the *American Journal of Science*. In 1850 Dana was made professor in Yale College. The remainder of his life was spent as teacher, editor, author and investigator.

Dana was President of the American Association for the Advancement of Science in 1852, and was one of the original members of the National Academy of Sciences ; he received the Wollaston Medal of the Geological Society of London, the Copley Medal from the Royal Society, and the Walker Prize from the Boston Society of Natural History. He received honorary degrees from the University of Munich, Edinburgh and Harvard. He was a member of the Royal Society of London, the Institute of France, the Royal Academies of Berlin, Vienna and St. Petersburg, and many other societies.

In addition to a large number of papers

printed in the *American Journal of Science* and elsewhere, he is the author of the following works:

A System of Mineralogy, 1837, 1844, 1850.

Zoöphytes, 1846.

Manual of Mineralogy, 1848, 1857, 1878, 1887.

Coral Reefs and Islands, 1853.

Crustacea, 1852–54.

Manual of Geology, 1862, 1874, 1880, 1895.

A Text-Book of Geology, 1864, 1874, 1882.

A System of Mineralogy, 1868.

Corals and Coral Islands, 1872, 1890.

The Geological Story Briefly Told, 1875.

Characteristics of Volcanoes, 1890.

The Four Rocks of the New Haven Region, 1891.

CORRESPONDENCE.

THE EDUCATION OF THE TOPOGRAPHER.

To the Editor of Science: Part of Professor Merriman's review in Science for April 26 interests me as being the direct opposite of my own opinion. He says, apropos of Mr. Gannett's statement that the topographer must be able to generalize through his knowledge of geological processes: "These are dangerous doctrines. The earth exists, the duty of the topographer is to map it truly, and the study of the origin of its features should come later." I should like very much to learn through the columns of Science the opinions of other geographers and topographers on this question.

It is not alone the earth that exists; a large series of topographical maps of various parts of the earth also exist; and through their study the young topographer can learn much about the kind of work he will have when surveying those separate parts of the earth that are not yet mapped. This kind of knowledge will help him in mapping new regions in about the same way that preliminary study of known forms of plants and animals helps the systematist to describe new forms when he finds them.

It is certainly the duty of the topographer to make true maps; but the truest map is always only a generalization. Something is necessarily omitted, and the topographer has to choose between what he shall omit and what he shall represent. He sees many things that he can not map. How shall he be best aided in making on the small sheet of paper before him an expressive map of the broad surface of country around him? I do not say 'an accurate map,' because the word 'accurate' is so generally misunderstood in this connection. It is often taken to imply that the topographer has actually measured every part of the surface of the country and carefully constructed every line on his paper. As a matter of fact, by far the larger part of all maps is sketched, and in the sketching more facts often have to be omitted than can be represented. Hence, everything should be taught to the topographer that will aid him in really seeing the facts that are before him and faithfully representing such of them as come within the limit of the scale he employs.

Nothing is of more assistance in seeing the facts, and in thus making a good beginning towards sketching them properly, than some understanding of their origin and meaning. Hence I believe that the best course of education for topographers while yet in school should include a careful study of the development of land forms, and that the best practical work by topographers will require a very careful and sympathetic study of the origin of the land forms on the ground before him. The prepossession that contour lines bend up-stream has deceived many a topographer into giving a wrong expression to flat alluvial cones. Indifference to the significance and importance of the sharp edge of a gorge or a cliff has rounded off many a truly angular contour line into an inexpressive curve.

The objection that is sometimes made against this view of a topographer's education and work is that, if he tries to sketch what he thinks he understands, he will sometimes sketch what is not really before him. There may be a certain amount of truth in this, but there are sufficient answers to it. A topographer who is too far guided by his imagination has been badly taught, or else he is of a mental quality that will prevent his ever becoming a good topographer, quite apart from whatever education he has had. The well taught topographer will make no larger share of mistakes on account of being well informed on his subject than will the well taught systematic botanist or zoölogist. The few mistakes of interpretation that the well taught topographer may make will, I believe, be far outweighed by excellence of the other part of his work.

It is perhaps because I have a higher idea of a topographer's work than ordinarily obtains that I should like to see him generally better educated for it. To my mind, a map is so far from being a copy of nature that I should prefer to call it a graphic description of nature, and in the making of this graphic description the topographer should study his subject and his graphic signs with the same care that a writer should study his thoughts and the words he employs to represent them. Instruments, to which some topographers seem to give their first attention, ought to have about the same place in their real work that a typewriting machine has in the work of a literary man.

The chief subject of the topographer's study should be the form of the land before him; and until this is recognized in engineering schools and enforced by a careful course of preparatory physiographical study, I believe we shall not have the best maps that can be made. Even further, it is as impossible to make a good topographer by merely teaching him about plane tables and stadia and logarithms as it is to make an essayist by teaching him about writing and spelling. It seems to me, in fine, that Professor Merriman's interest in the mathematical aspects of the art of topography leads him to place too low a value on the importance of studying the chief subject of the topographer's attention, the forms of the land.

W. M. DAVIS.

CAMBRIDGE, MASS., April 30, 1895.

THE HELMHOLTZ MEMORIAL.

A FEW months ago Hermann von Helmholtz died, one of the greatest scientific geniuses of all time, whose name will not be forgotten as long as men care for the knowledge of Nature. His invention of the ophthalmoscope made the success of the modern oculist possible; his papers on the conservation of energy gave the strongest impulse to modern physics; his books on seeing and hearing became the basis of modern psychology.

It seems a matter of course that the present generation should express its gratitude in a lasting monument. Not only his friends and pupils all over the world, but men of science and physicians everywhere have supported this idea, and so last month an International Committee was formed to collect money for the erection of a great Helmholtz monument in Berlin, where for the past twenty-five years he lived and worked. The plan has nothing to do with local patriotism; America, France, England, Italy and Russia are represented on the Committee; not a decoration of the city of Berlin is in question, but a universal expression of devotion to the spirit of natural science.

No doubt America will take a very high place in the list of givers. There has been seldom such an opportunity to show that the United States does not stand behind any other country in intellectual interests. But America has a special reason for paying her respects to the genius of Helmholtz, since Helmholtz in his seventy-second year paid his tribute of respect to the genius of America. One year before his death he crossed the ocean to study and to enjoy the scientific institutions of this country from the Atlantic to the Rocky Mountains, certainly the most famous European who has visited America for many years, and nobody who saw his noble personality in New York or Boston or Baltimore, in Philadelphia or Washington or Chicago, will ever forget him.

The American members of the International Committee are Dr. Wolcott Gibbs, President of the National Academy of Sciences; Dr. Herman Knapp, Professor of Columbia College; and Dr. Hugo Münsterberg, Professor of Harvard University.

Contributions may be sent before May 25th to the undersigned Secretary and Treasurer of the American Committee. The lists of contributors will be published weekly in Science.

Hugo Münsterberg.

38 Quincy Street, Cambridge, Mass.

SCIENTIFIC LITERATURE.

Manual of Geology. By James D. Dana. Fourth Edition. American Book Co. 1895.

The announcement, a few months ago, of a new edition of Dana's Manual filled geologists with liveliest expectations. It is needless to say that these expectations are more than realized. The Manual is so well known that a full account is wholly unnecessary—geologists need no urging to buy it. They simply must have it; they cannot do without it. I write this, therefore, not to call attention to the book; but partly because I am glad to have this opportunity to express my unstinted admiration for the author and for the book; and partly because I wish to draw attention to the author's position on some important questions which have come into prominence since the last edition.

1. Every geologist will be gratified to see that the author now comes out frankly for evolution; not, indeed, evolution in a materialistic sense, but in a reverent, theistic sense. In a certain Agassizian sense he has always been an evolutionist, but he has been often quoted by the *opponents* of evolution as now understood (*i. e.*, 'origin of organic forms by descent with modifications') as sustaining their position. In this edition his utterances are not to be any longer mistaken; although he is, perhaps, more nearly Lamarckian than Darwinian, or, at least, than Neo-Darwinian. Surely such plasticity and open receptiveness of mind retained even to the very last is a noble evidence of the true scientific spirit.

2. In this edition he separates the Palæozoic into two primary divisions with *Eo-Palæozoic*, including the Cambrian and Lower Silurian, and the *Neo-Palæozoic*, including the Upper Silurian, Devonian and Carbonic. Thus he makes the greatest break occur between the Lower and Upper Silurian. If this be so, would it not be better to use Lapworth's term 'Ordovician' for Lower Silurian, retaining the term Silurian for the Upper Silurian alone? Probably this would violate the priority-rule of nomenclature; but, perhaps in this, as in many other cases, rules too strictly interpreted stand in the way of a rational classification.

3. He accepts the probability of a Permian glaciation, especially in the Southern Hemisphere; and of an elevation and enlargement of an Antarctic continent and its connection with the southern points of South America, South Africa and Australia as a cause of such glaciation. These great changes of physical geography and climate, and consequent wide migrations of faunas and floras, would go far to account for the

enormous and apparently sudden changes in organic forms which took place during and at the end of the Permian period.

4. In connection with the last he accepts also the idea of a land-connection (Gondwanaland) between *India and South Africa*, and perhaps indirectly through the enlarged Antarctic continent—with Australia—in Permian and Triassic times, as evidenced by the great similarity of the plants and the reptiles of that time in these now widely separated countries. It is true that there is very deep sea between these points now; but it is possible that the idea of the permanence of deep sea basins, originated by Dana, may have been pushed a trifle too far by Wallace as a means of separating faunas and floras.

5. He does not accept Algonkian as a system of rocks coördinate with Palæozoic and Mesozoic, but regards these pre-Cambrian strata as the upper part of the Archæan, *i. e.*, as Huronian and upper Laurentian. Perhaps the time is not yet come to settle this question definitely.

6. He accepts as probable the existence in Quaternary times of a greatly elevated and enlarged Antarctic continent, connecting with and connecting together the southern parts of South America, South Africa and Australia similar to that of Permian times, as evidenced by the faunas, and as accounting for the Quaternary glaciation of these regions.

7. He agrees with Hilgard in thinking that the LaFayette formation (many geologists seem to forget that we owe this name to Hilgard) is a torrential *river* deposit of the early Quaternary and not a *marine* deposit of the Pliocene times as maintained by McGee, and that therefore it indicates elevation and not depression of the continent.

8. He does not accept Croll's theory of the cause of the glacial climate; but, along with most American geologists, regards it as mainly due to elevation of northern land. This would not only directly increase the cold in high latitude regions, but would indirectly increase the ice-accumulation by connecting America and Europe in these regions and thus limiting the northward extension of the Gulf Stream, which, circulating around the Atlantic in mid-latitude regions, would furnish abundant warm vapors to be condensed as snow on the elevated northern land.

9. As might have been expected, his discussion of mountain-making is masterly. But one is interested, though not surprised, to observe that he does not accept the recent theories of Reade, Dutten and others as to the cause of mountain formation, but still regards the *contraction-theory* in some form as more probable.

But a reviewer is 'nothing if not critical.' I must vindicate my character as reviewer by finding some faults, even though they be trifling.

10. This edition, we observe, drops out the graphic illustrations of the distribution, in time, of families, orders and classes of animals, which constitutes so conspicuous, and, we may add, so attractive a feature of previous editions. We observe also that the index of authors quoted and of those from whom figures are taken is omitted. This is to be regretted in a work which will be so constantly referred to.

11. We observe also a few errors of oversight or of misunderstanding of authors quoted. On page 359, and again on page 380, he gives, on King's authority, the whole thickness of Wahsatch sediments, from the Cambrian to the Laramie inclusive, as 31,000 feet. In fact, King gives between 31,000 and 32,000 for *the Palæozoic alone*, page 122; and in addition 3,800 feet for Jura-Trias, page 537, and 12,000 feet for the Cretaceous, page 539 (49th parallel, Vol. 1).

Again, he states on page 520 that the oldest known insect—*Protocimex*—is found in the upper part of the *lower* Silurian; but on page 566 he says that the oldest known insect is the *Palæoblattina* of the *upper* Silurian.

We might mention others, but they are all trifling. In fact, the accuracy of the book is extraordinary.

In conclusion, we must heartily and most gratefully welcome the new edition. It is hard to say what American would be without Dana's Manual. Its encyclopedic fullness and yet extreme conciseness makes it hard reading for those who come to it without serious purpose. The word '*Manual*' exactly expresses its purposes and uses. It must be in the hands of every special student; it must lie on the table of every teacher of Geology to be consulted on every subject of doubt.

I had just finished this notice when the sad news of Dana's death was flashed across the continent. All recognized that this event could not be long delayed; but none the less it came as a shock to every man of science in the country. We are thankful that he lived to finish this new edition, for it is indeed the only fitting monument. No monument is worthy of a man of science except that which he erects for himself.

Joseph Le Conte.
University of California.

A Handbook of Systematic Botany. By Dr. E. Warming. Translated and edited by M. C. Potter. 8vo. pp. 620, fig. 610. London, Swan, Sonnenschein & Co. New York, Macmillan & Co. 1895.

This excellent English translation of Professor Warming's important work will be welcomed by all students and it cannot fail to have a wide use as a text-book. The descriptions of the groups are clear, concise and complete, the illustrations capital and many of them original, and the press-work leaves nothing to be desired.

The arrangement of groups is from simple to complex—the only arrangement compatible with our present knowledge. The special application of this principle may be best stated in Dr. Warming's own words as printed in the preface:

"Each form which, on comparative morphological considerations, is clearly less simple, or can be shown to have arisen by reduction or through abortion of another type having the same fundamental structure, or in which a further differentiation and division of labor is found, will be regarded as younger, and as far as possible, and so far as other considerations will admit, will be reviewed later than the 'simpler,' more complete or richer forms. For instance, to serve as an illustration: Epigyny and Perigyny are less simple than Hypogyny; the Epigynous *Sympetalæ*, *Choripetalæ*, *Monocotyledones* are, therefore, treated last; the *Hydrocharitaceæ* are considered last under the *Helobieæ*, etc. Zygomorphy is younger than Actinomorphy; the *Scitamineæ* and *Gynandræ*, therefore, follow after the *Lilliforæ*, the *Scrophulariaceæ* after the *Solanaceæ*, *Linaria* after *Verbascum*, etc. Forms with united leaves indicate younger types than those with free leaves; hence the *Sympetalæ* come after the *Choripetalæ*, the *Sileneæ* after the *Alsineæ*, the *Malvaceæ* after the *Sterculiaceæ* and *Tiliaceæ*, etc.

"Acyclic (spiral-leaved) flowers are older than cyclic (verticillate-leaved) with a definite number, comparing, of course, only those with the same fundamental structure. The *Veronica*-Type must be considered as younger, for example, than *Digitalis* and *Antirrhinum;* these again as younger than *Scrophularia; Verbascum*, on the contrary, is the least reduced, and, therefore, considered as the oldest form. Similarly the one-seeded, nut-fruited *Ranunculaceæ* are considered as a later type (with evident abortion) than the many-seeded, follicular forms of the order; the *Paronychieæ* and *Chenopodiaceæ* as reduced forms of the *Alsineæ* type; and the occurrence of few seeds in an ovary as generally arising through reduction of the many-seeded forms. The *Cyperaceæ* are regarded as a form derived from the *Juncaceæ* through reduction, and associated with this, as is so often the case, there is a complication of the inflorescence; the *Dipsacaceæ* are again regarded as a form proceeding from the *Valerianaceæ* by a similar reduction, and those in their turn as an off-shoot from the *Caprifoliaceæ*, etc. Of course these principles of systematic arrangement could only be applied very generally; for teaching purposes they have often required modification."

While there is wide difference of opinion among botanists as to the relative degree of complexity of some of the families, and the sequence adopted by Engler and Prantl in their 'Natürliche Pflanzenfamilien' will appeal to many students as in some respects more philosophical, all the suggestions contained in this book must be regarded as very valuable.

Plants are here divided into five great divisions: (1) Thallophyta; (2) Muscineæ; (3) Pteridophyta; (4) Gymnospermæ; (5) Angiospermæ. We note in this a departure from some recent views where the divisions 2, 3 and 4 have been grouped under the primary division Archegoniatæ, and from others where the divisions 4 and 5 have been grouped as Spermatophyta.

Dr. Warming does not discuss the relative value of these different views, contenting himself with alluding to them. We may note that the disadvantage of recognizing the Archegoniatæ as above circumscribed is found in the fact that the female organs of the Angiosperms are also archegones. It must be admitted that the grouping here maintained has many points in its favor, but it is our opinion that the term 'sub-kingdom' is more explicit for the primary groups than 'division.'

The Thallophyta are divided into 'subdivisions': (a) Myxomycetes, (b) Algæ, (c) Fungi. It is said of the Myxomycetes that "they occupy quite an isolated position in the vegetable kingdom, and are perhaps the most nearly related to the group of Rhizopods in the animal kingdom." The Bacteria are treated, unphilosophically, it would seem to us, as a family of Algæ, being grouped with the Schizophyceæ under the class Schizophyta. The treatment of the higher Algæ and Fungi is not essentially different from that of other recent authors. (It should be remarked that the arrangement and description of the Thallophytes is largely contributed by Dr. E. Knoblauch.) The Fungi imperfecti are placed at the end of the subdivision, and the only groups admitted to this category are the Saccharomyces-forms, the Oidium-forms and Mycorhiza. Lichens are discussed under Ascomycetes and Basidiomycetes.

The Muscineæ are treated as (1) Hepaticæ and (2) Musci frondosi. Neither in these nor in the Pteridophyta do we find any views very different from those of other recent authors. In the Gymnosperms we find the three classes, Cycadeæ, Coniferæ and Gneteæ, maintained; the Coniferæ are distinctly separated into two families, Taxoideæ and Pinoideæ, which is a suggestion of much importance.

Under the Angiospermæ we find a discussion of the systematic value of the primary group Chalazogams, recently suggested by Treub. It will be remembered that Treub found that in the curious genus Casuarina the pollen-tube entered the ovule near the chalaza, and on this character proposed to divide the Angiosperms into Chalazogames and Porogames, *Casuarina* being the only genus known to him that would fall into his first group. Dr. Warming concludes, from the more recent observations of Nawaschin and Miss Benson, which indicate the similar entrance of the pollen-tube in *Betula*, *Alnus*, *Corylus* and *Carpinus*, that our knowledge of this phenomenon is as yet too meagre to warrant us in maintaining the views of Treub, and so he adopts the usual grouping into Monocotyledones and Dicotyledones. His primary grouping of the Monocotyledones is as follows: (1) Helobieæ, Juncaginaceæ being taken as the lowest type; (2) Glumifloræ, in which he includes the Juncaceæ, a position which we do not believe can be satisfactorilly maintained; (3) Spadicifloræ; (4) Enantioblastæ; (5) Liliifloræ; (6) Scitamineæ and (7) Gynandræ. It will be observed that in this arrangement he differs considerably in detail from that of Eichler and Engler and Prantl. The primary division of the Angiospermæ is into (1) Choripetalæ, beginning with Salicaceæ and ending with Hysterophyta (parasites such as the Loranthaceæ and Santalaceæ), and (2) Sympetalæ, beginning with Bicornes and ending with Aggregatæ.

An appendix, contributed by the translator, gives a useful tabulation of the system of Ray (1703), Linnæus (1733), A. L. de Jussieu (1789), A. P. DeCandolle (1819), Endlicher (1836–40), Brongniart (1843), Lindley (1845), A. Braun (1864), Bentham and Hooker (1862–83), Sachs (1882), Eichler (1883), Engler (1892). N. L. B.

The Story of the Stars. G. F. Chambers. New York. D. Appleton & Co. 1895. Pp. 160.

The Messrs. Appleton have begun with this small monograph their *Library of Useful Stories*, a series of paper covered booklets intended to embrace the ground of science, history, etc. This initial number, by Mr. George Chambers, an English astronomical writer of long experience, proves to be rather better than a first impression would lead one to judge; for the illustrations, which first strike the eye, are for the most part simply execrable. What excuse for the absence of more and better ones, in these days of inexpensive engraving? Its curiously insular mannerisms might readily have been corrected by a half hour's work of an American editor, who should also have toned down those provincial oddities of style which mar this book even more, because of its smaller size, than the same author's large *Descriptive Astronomy.*

Curiously false implications are wrought into the first chapter, though only a page or two in length. If the manifold uses of astronomy are to be competently brought before the public mind to-day, and the reasons for the support of that science from the public exchequer suitably defended, it is only by telling a few simple things exactly as they are. Now, it may be true in England that, if "the staff belonging to either establishment [the Royal Observatory or the Nautical Almanac Office] were to resort to the fashionable expedient of a strike for higher pay," then, among other dire results, "Our railway system would become utterly disorganized. A few trains could run, but the intervals between them would have to be considerable, and they could only travel by daylight and at very low speeds," but we do not exactly see why. Rather the fact is that, if both these establishments were permanently closed henceforth, the present state of astronomy is such that all the public business of determining time for railways and of preparing data for navigating ships could be done for the fiftieth part of the budget now devoted to the Nautical Almanac and the Royal Observatory; and any government maintaining such costly establishments, with their corps of trained observers and expert computers, merely for this simple though important purpose, would be very foolish indeed. Not only would the expenditure be extravagant, but wholly unjustifiable. These institutions are maintained for quite other purposes; and the significant work of the great government observatories (excellently done in England, France and Russia, and which in this country we have been trying for a half century to do, though not succeeding very well because the proper organization is lacking) lies in quite other fields, the immediate serviceableness of which is by no means universally conceded. Blanketing all this under the antiquated plea of utility in time and navigation is clearly wrong and wholly indefensible.

Mr. Chambers's attempt to popularize seems rather hard, and on the whole of doubtful success. Excellent scientific explanations go on for a while, when suddenly the author, seemingly suspecting that he is less interesting than he ought to be, plunges patchily into something purely literary, or indulges in some incongruous expression not exactly ludicrous, but giving an undignified cast to essays on the most dignified subject in the whole range of the sciences. No carelessness or vulgarity in style was ever a compliment to the literary taste of a reader, and neither the cause of literature, science nor anything else is likely to be enhanced by allusions to 'some Germans nibbling' at stellar photometry; or by ponderous anecdotes about hypothetical carrots, "that grew so well that the roots reached right through to the other side of the earth."

The proof revision has been none too carefully done—illustrations on pages 60 and 116 have been interchanged; the incorrect spelling of Palitzsch would not perhaps attract attention, except that the author, being also the compiler of a handy little German-French-English lexicon, we expect better things of him; and while 'Bob' passes current everywhere for Robert, 'Boberts' will scarcely do for Roberts. The general scientific reliability of statement is fully up to the standard expected of Mr. Chambers, and only one or two inaccuracies need be pointed out—at the middle of page 18, where he should have written, 'a *vertical* plane passing through the zenith;' and on page 73, where the exact opposite of what is meant is inadvertently said, regarding the stars 'converging towards' a point in Hercules.

Of course in so small a book one must not expect everything; but some omissions are noteworthy. In even a magazine article about the stars a single page about their distances would be only too brief, but Mr. Chambers gives only this amount in a volume of 150 pages, with no allusion to the name of Bessel in this connection, or Brünnow or Gill. The classic work of Dr. Gould should not have been omitted. The superb advances of stellar photography in the hands of the brothers Henry, Russell, Gill, Barnard, Roberts, Wolf and others are barely alluded to, or left out entirely. The accurate researches on the brightness of stars by the Potsdam astronomers are wholly ignored. If the space of six pages could be given to 'The Stars in Poetry,' and a third of that amount to speculative 'rubbish' regarding the origin of the Milky Way, is it quite the thing to have crowded out completely the nebular hypothesis, which has engaged such master minds as Herschel, La Place, Lord Kelvin and Darwin? Several chapters are almost purely descriptive, or mere geography of the heavens, as if a handbook for the use of small telescopes; a little yeast here would have done no harm; but it should be pervasive and inherent—not added as an afterthought. Mr. Maunder has appended an excellent chapter on the marvels of the spectroscope as applied to the stars and nebulæ.

It is not, however, intended to imply that there is not much that is excellent in Mr. Chambers's *Story of the Stars*, both as to form and arrangement. Its convenient size, clear type and authoritative statements (even with occasional lapses into 'dread' technicalities) render it, on the whole, an intelligible and interesting booklet, which will be a vast help to the student and general reader, and is worth double what the publishers ask for it. But the author has far from succeeded in making the most and best of his opportunity. DAVID P. TODD.

AMHERST COLLEGE.

The World of Matter: A Guide to the Study of Chemistry and Mineralogy. By HARLAN H. BALLARD, A. M. Boston, D. C. Heath & Co. 1894.

The object of this book is apparently to enable those who may not have an opportunity to study natural phenomena in a thorough way to obtain some comprehension of the objects and methods of scientific investigation by means of a few well chosen experiments. The object is a good one; will a study of this book further it?

It is impossible to say definitely, yes or no. The explanations, so far as they go, are generally excellent, but the tendency of the author to preach rather than to guide is often noticeable. After most properly bidding the student accept as fact no scientific statement capable of easy demonstration until he has proved it such, the book contains several chapters with hardly a single one of the statements made supported by experiment. For instance, we find (p. 179) that "we have now become somewhat familiar with," among other elements, "aluminum and iron; and we have incidentally become acquainted with a number of their more important compounds." Experimentally, how? Thus: The student is bidden to look for iron ore in soil, to write down what he already knows about iron, to examine the physical properties of siderite, to heat a piece of pyrite, and to note the physical properties of slate and of feldspar. That is all. Now, this is not experimental chemistry; it is boiled-down encyclopædia.

On the other hand, after having studied Ice, Water, Fire, Air, Earth and Quartz, molecules and atoms and all the other fascinating mysteries are brought in in a chapter called A Lesson in Chemistry (!); later, atomic weights are given and symbols in plenty. After having stated as facts the Laws of Chemical Combination, the author later, without further explanation, gives the following formulæ for some of the minerals the student is to work with—of course, with their names: Fe_7S_8, $(FeMnZn)_2O_4$, $(CaMgAlFe)SiO_3$, $(KFeMgAl)_2 SiO_4$, $Li_6Al_8Si_{15}O_{45}$, $(CaMg)_8(AlFe)_4Si_7O_{28}$.

The directions are in some cases almost tediously explicit, and this is right; frequently, however, they err on the other side. The student is given directions to use phosphorus, and occasionally other dangerous substances, without a word of caution. Considering the inexperience of the student, and the fact of his working probably alone, this is a matter of some importance.

To sum up, if all the theoretical portion of the book, all symbols, atomic weights, etc., had been left out, and a few experiments on the *chemical properties* of substances like iron and aluminium—to mention but two—put in to fill the vacuum, Mr. Ballard's book would have filled a lack. It cannot at present—at least, unassisted.

WYATT W. RANDALL.

NOTES AND NEWS.

At the meeting of the trustees of Columbia College, on May 6th, President Low subscribed one million dollars for the construction of the new library building. He stated that it is to be a memorial to his father, the late A. A. Low, 'a merchant who taught his son to value the things for which Columbia College stands.' The trustees passed the following resolution:

Resolved, That the trustees accept with the deepest sense of gratitude the offer conveyed by President Low in his letter of May 6, 1895, subject to all the conditions therein expressed; and that the Clerk of the Board be instructed to convey to the president the thanks of the trustees for this most munificent and opportune gift, unprecedented in the scale of its generosity, and affording fresh evidence of the president's unbounded devotion to the interest of the College.

President Low's gift was accompanied by the following conditions which add to rather than detract from its value: That twelve Brooklyn scholarships for boys be established in Columbia College, and twelve Brooklyn scholarships for girls in Barnard College; that eight university scholarships, to be known as the President's University Scholarships, be established; that a university fellowship, the Class of '70 Fellowship, be established. President Low graduated in the class of '70.

At the same meeting Mr. W. C. Schermerhorn, chairman of the trustees, subscribed three hundred thousand dollars for the Natural Science Building, or other building or part of building that may be more needed.

CARL VOGT, Professor of Natural History in the University of Geneva, died in Geneva on May 5th, at the age of seventy-seven years. Vogt made important contributions to physiology, zoölogy and geology, but became most widely known through his work 'On Man' (1863), written from a materialistic point of view. He was born at Giessen, July 5, 1817, studied at that place, under Liebig, and at Berne, worked with Agassiz and was made professor at Giessen. After taking a prominent part in the Frankfort Parliament of 1848, he considered it prudent to retire to Switzerland, and from 1852 was professor in the University of Geneva.

MISS CRANE, through her excellent reviews and synopses of current brachiopod literature, certainly keeps the public well informed of the progress made in this department, and from time to time she ventures to make contributions of her own to the knowledge of the class. Her latest paper, *The Evolution of the Brachiopoda* (Geological Magazine, February and March, 1895), is a combination of the results and conclusions reached in the most recent investigations by various authorities, together with a general application of the facts to a scheme of phylogeny. The profound changes which have been made of late in the classification of the Brachiopoda through the application of modern principles of evolution are graphically stated :—"The Brachiopoda now seem to justify the prescience of Darwin. Formerly regarded as one of the most obstinate difficulties in the way of the demonstration of the evolution of the invertebrate life on earth, they now bid fair to become a remarkable illustration in favor of it."

THE building containing the entomological department of the Amherst State College is being enlarged so that the capacity of the laboratories will be doubled.

MONEY has been given to defray the expenses of transporting to Mount Hamilton and erecting there the great reflecting telescope presented to the Lick Observatory by Mr. Edward Crossley, of England. A reflecting telescope was included in the plans for the Lick Observatory made 21 years ago, and before Mr. Crossley presented the telescope to the observatory Professor Holden had been in correspondence with him, with a view to purchasing it. It is hoped that the telescope will be ready for use before the close of the current year.

THROUGH a gift of W. C. McDonald, McGill University has secured 35 acres of land for botanical gardens and an observatory.

THE bill consolidating the Astor, Tilden and Lennox libraries has been approved by Gov. Morton. The present site of the Lennox library will probably be adopted.

DR. GUSTAV HIRSCHFELD, Professor of Classical Archæology in the University of Königsberg, died on April 20th.

A JOINT meeting of the Scientific Societies of Washington, was held on May 10th, on the occasion of the delivery of the annual address of the President of the National Geographic Society, the Hon. Gardiner G. Hubbard. The subject of the address was 'Russia.'

DR. FERDINAND BRAUN, of Tübingen, has been appointed Professor of Physics in the University of Strasburg, succeeding Professor Kohlrausch.

DR. W. S. HALL has accepted the Davis Professorship of Physiology in the Northwestern University Medical School, of Chicago.

THE trustees of the University of Pennsylvania have accepted with regret the resignation of Professor Harrison Allen from the Professorship of Comparative Anatomy and Zoölogy.

ACCORDING to the *American Geologist*, Mr. Warren Upham, recently of the Minnesota

Geological Survey, has removed to Cleveland, Ohio, to accept the position of librarian for the Western Reserve Historical Society, and Mr. H. F. Bain has been elected Assistant State Geologist of Iowa in place of Dr. Charles R. Keyes, who recently resigned to take charge of the Missouri Survey.

THE Provincial Legislative Assembly of Ontario has authorized a grant of $7,500 towards defraying the expenses of a meeting of the British Association at Toronto in 1897, should the Association decide to accept the invitation that has already been received from Toronto.

THE Society of German Naturalists and Physicians will meet at Lubeck from September 16th to 21st.

THE death is announced of Dr. Tomsa, Professor of Physiology in the University of Prague.

IT is stated that Dr. Bertillon has discovered a new method for identifying handwriting by enlarging the letters by photography and measuring the alterations due to beating of the pulse.

THE celebrated Villino Ludovisi, in Rome, has been leased for the new American School of Architecture and Archæology.

ACCORDING to the *Medical Record* 14 of the 140 Medical Schools of the United States now require a four years' course.

SWAN, SONNENSCHEIN & Co. announce for publication next autumn a translation by Professor E. B. Titchener, of Cornell University, of Professor O. Külpe's *Grundriss der Psychologie*.

ACCORDING to a note in the London *Times*, the excavations by the American School at the Heraion of Argos, under the direction of Professor Waldstein, which were resumed this spring, have been very successful. Two hundred and fifty men have been employed on the work. Besides the two temples and five other buildings previously discovered, a large and well-preserved colonnade 45 metres long has now been found 25 feet below the surface south of the second temple. The discoveries include parts of metopes, two marble heads of the best Greek period, a hundred objects in bronze and gold, gems, vases and terra cottas of the Homeric period, as well as numerous scarabs and several Mycenean tombs with Argive inscriptions on bronze, probably of a religious character. The excavations, which are now in the fourth season, will be completed this year. They rival the French excavations at Delphi in magnitude and importance, representing all the periods of Greek life from prehistoric to Roman epochs.

THE residue of the estate of Mary D. Peabody has been left to the Catholic University of Washington, for the foundation of scholarships (probably three or four of the value of $5,000 each) in the chemical and physical sciences.

THE *Medical Record* gives an account of the malarial map of Italy, recently issued by the Italian Bureau of Statistics. It is based upon the death returns during the years 1890–92. The varying intensity of the disease in different sections is shown by modifications of color. In the three years there were 50,000 deaths from malarial causes, or 54 in 100,000. The worst districts, where the mortality is as high as 8 in 1,000, are in southwestern Sardinia, southeastern Sicily, the Pontine marshes, the district at the head of the Gulf of Taranto, and the southeastern slope, from the promontory of Gargano south to the Ionian Sea. Districts where malaria prevails, but not so intensely as to be fatal, are the lower reaches of the Po, Grosseto in Tuscany, the mouth of the Tiber, and the district near Salerno and the temples of Pæstum. In Rome itself malaria has sensibly declined; the deaths in 1881 were 600, in 1892 only

139. The general mortality from this cause in Italy has remained pretty constant; the average is 15 or 18 per 1,000.

PROGRAMS of the School of Applied Ethics, which opens at Plymouth, Mass, on July 8th, may be obtained from the Secretary, S. B. Weston, 1305 Arch street, Philadelphia.

THE Metropolis Law School has been united with the Law School of the University of the City of New York.

ACCORDING to the prospectus of the Cotton States and International Exposition, which opens at Atlanta, Ga., on September 18th, science will be well represented. There will be special buildings for machinery, minerals and forestry, agriculture, electricity and transportation. The United States Fish Commission will supply an aquarium with tanks occupying 10,000 square feet, and the National Bureau of Forestry will exhibit models showing methods of forest cultivation and preservation.

WE learn from a notice by Prof. Ziwet, in the April number of the *Bulletin of the American Mathematical Society*, that the first installment of the *Répertoire bibliographique des Sciences Mathématiques* has been issued. This consists of a set of 100 cards, 14x8 cm., on each of which about 10 titles are printed. The series is published by Gauthier-Villars in Paris and sells for two francs. It was decided at an international meeting held in Paris under the auspices of the French Mathematical Society to prepare a complete bibliography of the literature of mathematics since 1800 and of the history of mathematics since 1600.

MR. CLEMENS R. MARKHAM, President of the Royal Geographical Society, in a paper read before the Royal United Service Institution, urges the importance of an Antarctic expedition from a scientific and naval point of view, and recommends that it be undertaken by the British Government.

THE correspondent of the *Evening Post* announces the following new appointments at Bryn Mawr College: Dr. Florence Bascom, the only woman who has received the Ph. D. from Johns Hopkins University, now of the Ohio State University, Reader in Geology; Mr. Richard Norton, Lecturer in Archæology; Dr. M. L. Earle, Ph. D., of Columbia, Associate Professor of Greek; Mr. P. E. More, Associate in Sanscrit; and Dr. Alfred Hodder, Lecturer in English Literature.

DR. PEAT, of Butler, Pa., has cast a lens 60 inches in diameter for the telescope for the American University (of Washington).

MR. LEONARD T. METCALF has been appointed Professor of Mathematics in the Amherst State College.

The Bakerian Lecture before the Royal Society on May 9th was based upon a research conducted by Messrs. A. Vernon Harcourt and William Esson, on 'The Laws of Connexion between the Conditions of a Chemical Change and its Amount.'

IN a brochure of fifty pages issued in connection with an exhibit at the World's Fair, Mr. Gifford Pinchot gives an account of an attempt to introduce a proper system of forest management upon the estate of Mr. George W. Vanderbilt in North Carolina, together with the result of the first year's work. Biltmore is about two miles from Asheville, on the table land in western North Carolina. The estate includes 3,891 acres of woodland on the banks of the French Broad River. The forest is composed chiefly of young oaks and other deciduous trees, the best timber having been cut away. Fires and neglect have also done much injury. This forest has been divided into suitable blocks and compartments, and put into the care of a competent forester for improvement while at the same time yielding money returns to the owner. The location of the forest, soil, climate, kinds of trees, treatment previous to coming into the hands of the present owner, improvement, cuttings and other topics are discussed. While it was not expected that the forest would be self-supporting from the start, it has been nearly so, the expenditures for the year ending April 30, 1893, being $9,911.76, and the income from sale of ties, cord-wood, lumber and posts, together with the estimated value of stock on hand, amounting to $9,519.36. Part of the tract will be managed on the regular high forest system with a 150-year rotation; the rest, on a selection system. Steps have also been taken to reforest a thousand acres of waste land, using many kinds of native and foreign trees. In connection with this work it is designed to build up an arboretum second to none in the world. This is under the direction of Mr. Frederick Law Olmsted. Already there are in the nursery more kinds of trees and shrubs than in the gardens at Kew, and the number is being steadily increased. This arboretum will form the borders of a drive about five mile long. Careful records are being kept in connection with the work, and a forest botanical library, already of considerable extent, will furnish the necessary aid to study. Accompanying the report is a map of the forest and a number of good half-tones showing original condition, proper and improper methods of lumbering, etc. This is the first time proper forest management has ever been undertaken in the United States, and as time goes on the results will undoubtedly become an object lesson of prime importance, and one badly needed by the American public, whose delight from the earliest settlement of the country has been to destroy trees.

E. F. S.

SOCIETIES AND ACADEMIES.

GEOLOGICAL SOCIETY OF WASHINGTON.

THE following are abstracts of the communications presented at the 33d meeting, April 24, 1895:

W J McGEE. 'The topographic development of Sonora.'

The territory described, lying between the Gila river on the north and the Rio Sonora on the south, and extending from the Sierra Madre to the Gulf of California, is about 400 by 200 miles square. Essentially it consists of an undulating plain with embossed mountain ranges. The plain varies from sea-level to some 4000 feet in altitude; the mountain ranges, commonly 4000 feet or less in height above the plain, are rugged, narrow and generally parallel, trending somewhat east of south. These ranges are remnants of larger mountain areas, shaped by erosion, and sometimes they are connected by transverse ridges which, like the ranges themselves, are residua of ancient masses. The area is one of complete gradation within itself, *i. e.*, the rainfall is so slight that the material degraded from the mountain is aggregated on the intermontane plains, as the storm-waters sink or evaporate—for none of the rivers between the Gila and Yaqui ever reach the sea. Certain peculiarities of the topography grow out of this condition.

The entire plain inclines southwestward, having evidently been tilted in this direction during late geologic time, though the date is not yet fixed. A consequence of this tilting was the stimulation of the streams flowing westward, southward and southwestward, and partial paralysis of the streams flowing in the opposite direction; and by reason of previous adjustments of topographic processes and products under the peculiar climatal conditions of the region these effects were greatly increased. Accordingly the southwestward-flowing streams retrogressed and pushed their headwaters through the parallel ranges and sometimes through the transverse ranges connecting them, while the northeastward-flowing streams practically ceased to corrade. Accordingly the area is characterized by retrogression; the main waterways diverge from the main valleys, and cut through the ranges and athwart the valleys; and the primary and secondary divides do not coincide with the mountain ranges, but traverse the valleys in a singularly erratic manner. By reason of the combination of epeirogenic and meteorologic conditions, the region affords a remarkable example of the retrogression of streams and of the development of unusual topographic forms thereby.

WHITMAN CROSS. 'The Geology of the Cripple Creek Gold Mining District, Colorado.' This important new gold district lies on a granite plateau, some ten or twelve miles southwest of Pike's Peak, at an elevation of 9,000 to nearly 11,000 feet. There is at this point a small volcanic vent, to be regarded as an outlier of an extensive volcanic region to the westward, lying between South Park and the Arkansas River.

While the area of the Cripple Creek volcano is small, there has been a very complete cycle of events at this center. Explosive eruptions in the earlier periods built up a cone of fine tuff and breccia, through which numerous eruptions in narrow fissures and irregular channels took place in later times. Erosion has now removed a large part of the ejected material, though not clearly disclosing the volcanic neck.

The igneus products of the volcano are andesites of several kinds, phonolite, phonolitic trachyte, nepheline-syenite, syenite-porphyry, and several dense varieties of basalt. Phonolite is the specially characteristic rock of the center, and in dike form in granite occurs for several miles about it.

Fumarole and solfataric emanations of chlorine, fluorine and sulphurous gasses undoubtedly characterized certain periods of the volcano, followed by hot waters containing the same agents in solution. By these processes the rocks of the district have been very extensively decomposed. The ore deposits are very intimately connected with the volcanic center.

This communication presented the general geological results of a detailed study of the district made last fall. An examination of the ore deposits was made at the same time by Prof. R. A. F. Penrose, Jr., and the two reports, with a geological map, will be issued by the U. S. Geological Survey during the coming summer.

W. H. WEED. 'The Shonkin sag, an abandoned channel of the Missouri river.' The Shonkin sag is a peculiar topographic feature of the country south of the big bend of the Missouri River in central Montana. It is an abandoned river channel which was formed by the waters of the Missouri River flowing around the margin of an extension of the great Canadian ice sheet (the Laurentide glacier). The sag consists of a winding valley from a quarter of a mile to two miles wide with rocky bluff walls, and holds a succession of lakes, several of them without outlet. The continuity of the channel is interrupted by modern stream valleys cutting it transversely, but their later origin is clearly apparent, and even the settlers of the region recognize the fact that the sag is an old water way. It begins near the mouth of Highwood Creek, east of the Great Falls of the Missouri River, and extends in a general easterly direction over 100 miles to the mouth of Judith River. Throughout its course the northern wall marks the limit of the glacial moraine. Glacial drift is found in a few places a short distance south of the channel, but in small quantity. In general the sag defines the moraine front. It is, therefore, believed that the ice sheet ponding the waters of the Missouri near the mouth of Sun River deflected the stream, which at that time flowed northward, and caused it to flow about the margin of the ice. Upon the recession of the glacier the river abandoned this temporary channel for the old valley to the northward, which was but partially filled by glacial material. The present course of the Missouri, for some distance below the cataracts, is cut in black shales of the Fort Benton period, capped by 100–250 feet of glacial till and silt.

WHITMAN CROSS,
Secretary.

ENTOMOLOGICAL SOCIETY OF WASHINGTON.

THE 108th regular meeting was held May 3d. Mr. L. O. Howard read a paper entitled 'Some New Scale Parasites,' in which he discussed several species of the family Chalcididæ which are new to science, and which are important parasites of destructive scales. A paper entitled 'Two Leaf-beetles that Breed on the Golden-rod,' by F. H. Chittenden, was read by title, and another, 'Sexual Dimorphism in the Scolytid Genus Xyleborus,' by E. A. Schwarz, was also read by title and referred to the committee on publications. Mr. Ashmead presented a communication on Lysiognatha, a new and remarkable genus in the Ichneumonidæ. The form described was an extraordinary one, possessing the head and jaws of the Braconid sub-family Alysiinæ, the wings and remainder of the body resembling those of the Ichneumonid subfamily Ophioninæ. Mr. Ashmead considered it typical of a new sub-family of the Ichneumonidæ. Dr. Theodore Gill expressed himself as of the opinion that the form is really typical of what should be a new family. A note from Mr. H. G. Barber, of Lincoln, Neb., a corresponding member of the Society, was read by the secretary. The note was entitled 'Food-habits of *Hypatus bachmanni.*' This butterfly, which has recently been observed migrating in great numbers in the Southwest, has been previously supposed to feed only on species of Celtis. Mr. Barber considers Symphoricarpos to be probably its favorite food plant. Mr. W. T. Swingle made some remarks on the effects of the December and February freezes in Florida upon the insects injuring the orange. The really important insects, namely, the red scale and the white fly, have been seriously checked. All specimens occurring upon foliage have been killed. In discussing this paper, Mr. C. L. Marlatt called attention to the fact that the serious injury to the trees caused by the cold has already resulted in the appearance of a number of bark-boring beetles, which will undoubtedly do much damage during the next two or three years.

L. O. HOWARD,
Recording Secretary.

NEW BOOKS.

Proceedings of The American Association for the Advancement of Science for the Forty-third Meeting held in Brooklyn, N. Y., August, 1894. Salem, The Permanent Secretary. 1895. Pp. xiii + 486.

Der Gute Geschmack. LOTHAR ABEL. Vienna, A. Hartleben. Pp. vii + 368.

The Geological and Natural History Survey of Minnesota, Vol. III., Part I., Paleontology. N. H. WINCHELL. Minneapolis, Minn., Harrison and Smith. 1895. Pp. lxxv + 474.

John Dalton and the Rise of Modern Chemistry. SIR HENRY E. ROSCOE. London and New York, Macmillan & Co. 1895. Pp. 212. $1.25.

Missouri Botanical Garden. Sixth Annual Report. WILLIAM TRELEASE. St. Louis, Mo., The Board of Trustees. 1895. Pp. 134.

The Origins of Invention. OTIS T. MASON. London, Walter Scott; New York, Charles Scribner's Sons. 1895. Pp. 413. $1.25.

Chemical Analysis of Oils, Fats and Waxes. From the German of PROFESSOR DR. R. BENEDICT. Revised and enlarged by DR. J. LEWKOWITSCH. London and New York, Macmillan & Co. 1895. Pp. xviii +683. $7.00.

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Subscriptions and advertisements should be sent to SCIENCE, 41 N. Queen St., Lancaster, Pa., or 41 East 49th St., New York.

VARIATION OF LATITUDE.*

THE question is frequently asked, "How can latitude change?" There are two ways obviously. First, we may imagine that a portion of the earth slips on the surface of the globe, due say to earthquake shock. Then if the movement of the mass has been toward the equator the latitude of that place is decreased; if toward the pole of the earth the latitude is increased. But suppose that some forces at work on the earth cause it to revolve about a new axis, then we have at once a new equator, and the latitudes of *all* points on the earth's surface change except at those places where the old and new equator intersect.

If, for example, the earth's axis of revolution should be changed so as to pass through this hall, the latitude would be changed from a little over 40 degrees, as it now is, to 90 degrees. There are changes no doubt produced by the slipping of portions of the earth's strata, but we know that these causes are insignificant and local. The only way that latitudes could be made to change *throughout the world* would be by changes in the axis of rotation of the earth, thus changing the position of the equator.

Are there any undisputed evidences of a variation in the latitude of a place and is it large?

To-day the evidence is overwhelming, but the amount is small, so small, in fact, that only the refined instruments of the present day have been able to discover it; though now, that it is discovered, older observations show it.

La Place, in his Mécanique Céleste (Tome V., p. 22), says "All astronomy depends upon the invariability of the earth's

*From a lecture before the New York Academy of Sciences, April 29, 1895.

axis of rotation and upon the uniformity of this rotation."

He considered that down to the beginning of this century astronomical instruments had not been able to show any variation of latitudes. There were differences, but these he thought could be accounted for as errors of observation.*

To-day, however, we feel certain that small variations in latitude are taking place, but so small that practically, in map making, for example, and in navigation, they are of no importance, though scientifically very important.

It might also, in this connection, be stated that there are theoretical reasons which seem to indicate that the earth's *rotation time* is not only changing, but also is not altogether uniform. The effect of the tide-wave as it moves west over the earth is to act as a friction-brake on the revolving earth, and so slow up the rotation time, and as this tide effect is not always the same the retarding effects differ, and theoretically produce a non-uniformity in the rotation time. But the shrinkage of the earth, due to loss of heat, would tend to make it revolve more rapidly. These effects may work against each other. However, observations and calculations to-day do not furnish us with any certain evidence that the rotation time is longer or shorter than it was ten centuries ago.

It no doubt will happen that, when observations and instruments are much improved, astronomers will discover these slight changes in rotation time that theory seems to require.

The idea that the latitudes of places change is not a new one.

Down to about the time that the telescope was invented there were many learned persons who believed that the latitudes of places changed several degrees in the course of centuries. These ideas were based on a comparison of maps made at different times.

A disciple of the illustrious Copernicus considered that the evidence was conclusive, and was satisfied that the pole of the earth was changing its position in a *progressive* manner; he considered that in time the torrid and frigid zones would change places.

However, these views of Dominique Maria de Ferrare were founded on poor data. The latitudes of a few places had been determined, by very imperfect means, in the best way they had, viz., from the shadow cast by a gnomon; but the latitudes of many places on the maps were put in from the accounts of travelers, the time it took to travel from one point to another being used as the basis of calculation.

Even in these enlightened days, as we like to consider them, there is no good map of our own Empire State. The latitudes of a few points only in New York State have been determined with accuracy. But there are many places in the State whose positions are not known within more than a mile.

In the latter part of the 16th century Tycho Brahe, of Denmark, improved the instruments in use (without the telescope), and later, about 1610, the telescope was discovered and applied to astronomical instruments. Then new and more accurate methods were used to determine latitude, and the large discrepancies disappeared. Some observers found differences between latitudes determined in winter and in summer, and they supposed those differences to be due to changes of the pole.

In the latter part of the 17th century J. D. Cassini summed up the state of the problem in his day, and arrived at the conclusion that, notwithstanding the apparent variations in the latitudes, the pole of the

* The writer is much indebted to the paper by Professor Doolittle on 'Variations of Latitude' read before the A. A. A. S., at Madison, Wis., August, 1893.

earth did not change to any *large extent*; that most of the apparent changes in latitude were due to errors of observation and defects in theory, but he thought it probable that *small* changes did occur in the position of the pole; he thought the changes were *periodic*, and did not amount to more than two minutes of arc equal to about 12,000 feet. "Thus, instead of several degrees which were conceded by the astronomers of of previous centuries, but a paltry two minutes was now allowed; but with improved instruments, with the discovery of aberration and nutation and the perfection of the theory of refraction, even this modest allowance was gradually reduced to a vanishing quantity."

The geologists, in their investigations, have found fossil remains in the cold regions of the north, belonging to the Miocene, Upper and lower Cretaceous, Jurassic and other geological periods, which seem to indicate a former temperature much higher than the present. In 1876 Dr. John Evans, then President of the British Geological Society, discussed the problem, and concluded that the amount of polar light and heat in the past must have been much greater than it is now. He invited the attention of the mathematicians to this problem, and asked: Would a considerable elevation and depression of the sea bottoms and continents produce a 'change of 15 degrees to 20 degrees in the position of the pole?'

Sir William Thomson discussed this problem and gave his conclusions in 1876 to the British Association for Advancement of Science. He said: "Consider the great facts of the Himalayas and Andes and Africa, and the depths of the Atlantic, and America and the depths of the Pacific, and Australia; and consider further the ellipticity of the equatorial section of the sea level, estimated by Colonel Clarke at about one-tenth of the mean ellipticity of the meridianal sections of the sea level.

"We need no brush from a comet's tail to account for a change in the earth's axis; we need no violent convulsions producing a sudden distortion on a great scale, with change of axis of maximum moment of inertia, followed by gigantic deluges; and we may not merely admit, but assert as highly probable, that the axis of maximum inertia and the axis of rotation, always near one another, may have been in ancient times very far from their present geographical position, and may have gradually shifted through ten, twenty, thirty or forty or more degrees without at any time any perceptible sudden disturbance of either land or water."

Sir William Thomson gave no account of the calculations made by him as the basis of these conclusions.

In 1877 Mr. G. H. Darwin made a careful and elaborate mathematical discussion of the problem. He showed that, in a *perfectly rigid* globe, the pole could not have wandered more than 3 degrees from its original position, as the result of the continents and oceans changing places. "If, however, the earth is sufficiently plastic to admit of readjustment to new forms of equilibrium, by earthquakes and otherwise, possible changes of ten or fifteen degrees may have occurred. This would require, however, such a complete changing about of the continents and oceans, with maximum elevations and depressions in precisely the most favorable places, as has certainly never occurred in geologic times."

The evidence indicates, in fact, that the continental areas have always occupied about the same positions as now.

Thus it would seem that the geologists must abandon the hypothesis of *great* changes in latitude as a factor in the earth's development, unless a new cause can be found that will move the pole to the extent required by the geologists.

In an address made before Section A, of the British Association in 1892, Professor

Shuster stated that he believed the evidence at hand was in favor of the view that there was sufficient matter in interplanetary space to make it a conductor of electricity. This conductivity, however, must be small, for if it were not, he said, the earth would gradually set itself to revolve about its magnetic poles. However, changes in the position of the magnetic poles would tend to prevent this result. Perhaps the investigator in the near future, working on the suggestion of Dr. Shuster, may find some connection between the earth's magnetism, rotation time and position of rotation axis.

The evidence, then, at this phase of the discussion, is in favor of the view that there is no adequate reason for believing that any *large changes* of latitude, amounting to several degrees, have occurred in geologic times. The evidence shows, however, that there are small changes. Are they *progressive;* does the north pole of the earth wander slowly but surely further and further away from its positions of ages gone by?

At the International Geodetic Congress held in 1883 at Rome, Sig. Fergola, of the Royal Observatory Cappodimonti, Naples, gave a tabular statement which seemed to show that small but *progressive* changes *had* taken place in Europe and America. This table showed, for example, that the latitude of Washington, D. C., had decreased from 1845 to 1865, 0.47″; at Paris, from 1825 to 1853, the decrease was 1.8″; at Milan in 60 years, 1.5″; at Rome during 56 years, 0.17″; at Naples in 51 years, 0.22″; at Königsberg in 23 years, 0.15″; at Greenwich in 19 years, 0.51″. Fergola, at the Congress mentioned, suggested a plan for making systematic observations, and he pointed out the favorable location of several observatories that were on nearly the same circle of latitude, but differing widely in longitude. Unfortunately this suggestion of Fergola's was not carried out in any way until 1892, when the Columbia College Observatory arranged to work in conjunction with the Naples Observatory on the problem. This series of observations was begun in the spring of 1893, and will be continued several years.

The data given by Fergola at Rome in 1883 showed a diminution of latitude in every case; other data showed a similar diminution; however there were exceptions, where the latitudes seemed to increase.

The investigations that have been going on since 1883 throw doubt on the *progressive* changes in latitude, or at least such changes are masked by proved *periodic changes.*

For a long time, since 1765, periodic changes have been looked for, because the theory of a rotating earth, an earth having the form of a sphere flattened at the poles, or, more accurately, an ellipsoid of revolution, demanded such changes; but the theory did not furnish any clue to the amount of changes, except that they must be very small. This theory shows that if the earth was *absolutely rigid* and revolved about its shortest axis (called the axis of figure) at any time it would continue to revolve about such axis forever, unless disturbed by some outside force. If so disturbed, then the axis of rotation would no longer coincide with the axis of figure—the axis of rotation would intersect the earth's surface at points away from the points where the axis of figure comes out. But the theory also showed that the new axis of rotation would revolve about the old one in a period of 304.8 days. This period comes from the knowledge of the magnitude of precession and nutation, and is known very accurately.

We would expect therefore that changes in latitude would show this 305-day period.

Several attempts have been made to determine the distance between the two axes (figure and rotation axis) from changes in latitudes.

The celebrated astronomer Bessel made

the first attempt, and was unsuccessful, it was supposed until recently.*

Observations were also made at Pulkova, Russia, Greenwich and Washington. The Washington observations were made between 1862 and '67, and included six complete periods of 305 days each. A rigorous discussion by Newcomb gave the separation of the axes as 3 feet, or 0.03″.

C. A. F. Peters, of Pulkova, had in 1842, obtained ″.079 = 8 feet.

These figures are small, but fairly accordant. A reinvestigation, however, showed that the various calculations did not agree in showing the same displacement at the same time. This made the whole result doubtful, so that Newcomb (in 1892, March, Mon. Not. R. A. S.) remarked that "the observations showed beyond doubt there could be no inequality of the kind looked for."

It was while investigations of this kind, to determine the separation of the axis of rotation and axis of figure, were going on that Sir Wm. Thomson (now Lord Kelvin) announced, at the Congress of the British Association at Glasgow in 1874, that the meteorological phenomena, the fall of rain and snow, the changes which occur in the circulation of the air and of the sea waters would modify a little the mechanical constitution of the globe, and displace a little the *axis of figure*, *i. e.*, the form of the earth would be changed by the causes mentioned, and so a new shortest axis would be made. The effect of this would be to produce a change in the latitudes of places, evidently. He thought that it might amount to ″.50, which would correspond to a movement of the old axis (at the pole) of 50 feet on the earth's surface. Sir W. Thomson did not publish his calculation, but the authority of the great English mathematician and physicist was such as to make scientific men give the statement great attention. These meteorologic phenomena of which Sir William Thomson spoke are annual in character. When this annual period is combined with the 305-day or ten-month period of Euler we see that complexity results. This was the state of the investigation when Dr. Küstner, of the Berlin Observatory, published the results of his observations made in 1884–1885. Dr. Küstner undertook some observations for the trial of a new method for the determination of the constant of aberration. On reducing his observations he obtained results which were not at all satisfactory. A careful examination of his work led him to make the announcement that the unsatisfactory value for the aberration constant was due to a comparatively rapid, though very small, change in the latitude of the Berlin Observatory—"that from August to November, 1884, the latitude of Berlin had been from ″.2 to ″.3 *greater* than from March to May in 1884 and 1885."

*Tisserand says in Ann. Bur. Long. '95 (P. 42, B. 11) that there is a letter of April 7, 1846, in which Humboldt replies to Gauss that Bessel had told him in 1844 that his observations showed that his latitude had decreased 0. 3″ in two years. Bessel attributed this variation to changes accomplished in the interior of the globe. See also Hagan's letter in Astr. Nach., September, 1894.

In this connection it ought to be noted also that Prof. J. C. Maxwell read a paper April 20, 1857, before the Royal Society of Edinburgh (see Transactions Roy. Soc. Edinburgh, Vol. XXI., Part iv., pp. 559–571), 'On a Dynamical Top for exhibiting the phenomena of the motion of a system of invariable form about a fixed point, with some suggestions as to the earth's motion.' He deduced a period of 325.6 solar days. He examined the observations of *Polaris* made with the Greenwich Transit Circle in the years 1851–54. He found the apparent co-latitude of Greenwich for each month of the four years specified.

"There appeared a very slight indication of a maximum belonging to the set of months, March, '51; February, '52; December, '52; November, '53; September, '54." This result, he says, "is to be regarded as very doubtful, as there did not appear to be evidence for any variation exceeding half a second of space and more observations would be required to establish the existence of so small a variation at all."

This would indicate that from August to November, 1884, the pole of the earth had approached Berlin more closely by 20 to 30 feet than in the time from March to May.

This conclusion was fortified by the examination of other data, obtained from the observations made at Pulkova by Nyrén.

Here, then, was evidence of a comparatively rapid change in latitude. New observations were undertaken at Berlin, Potsdam, Prague, and Bethlehem, Penn. (all by Talcott's method), and all agreed in showing plus and minus changes in latitude for the years 1888–'90.

There were still some doubters. Moreover it was decided to critically test the matter by sending an expedition to the Sandwich Islands, which is 180 degrees (nearly) in longitude from Berlin. If it was known the latitude of Berlin increased, then a point in the northern hemisphere 180 degrees away from Berlin should simultaneously show a decrease in latitude, for if the pole moves toward Berlin it must move from the point on the other side of the earth.

Our own Government joined in the effort. Marcuse of Berlin and Preston of Washington spent more than a year on the Sandwich Islands observing for latitude, while at the same time observations were continued at Berlin, Prague and Strassburg in Europe, and at Rockville, Bethlehem and San Francisco in the United States. The results of all these observations have been published, and show, without a chance of error, that the earth's axis is moving, that the latitudes at the Sandwhich Islands increased when the latitudes in Germany diminished and *vice versa*.

The law of the change was eagerly and industriously sought for by some of the ablest mathematical astronomers of the world. They first worked on the idea that the changes must conform to the 305-day period of Euler, combined with an annual change due to causes set forth by Sir W. Thomson, and which I have previously mentioned. None of these investigations have given a satisfactory formula for the prediction of the latitude of any place.

In 1891 Dr. S. C. Chandler, of Cambridge, Mass., began his investigation of the problem. He remarks:

"I deliberately put aside all teaching of theory, because it seemed to me high time that the facts should be examined by a purely inductive process; that the nugatory results of all attempts to detect the existence of the Eulerian period (of 305 days) probably arose from a defect of the theory itself, and that the entangled condition of the whole subject required that it should be examined afresh by processes unfettered by any preconceived notions whatever. The problem which I therefore proposed to myself was to see whether it would not be possible to lay the numerous ghosts in the shape of various discordant, residual phenomena pertaining to determinations of aberration, parallaxes, latitudes and the like, which have heretofore flitted elusively about the astronomy of precision during the century, or to reduce them to some tangible form by some simple consistent hypothesis. It was thought that if this could be done a study of the nature of the forces as thus indicated, by which the earth's rotation is influenced, might tend to a physical explanation of them."

Dr. Chandler proceeded to examine his own work with the Almucantar at Cambridge, the observations of Küstner, Gyldén, Nyrén, the Washington observations and others. He found that they all seemed to indicate that the pole of the rotation axis was moving from west to east about the axis of figure of the earth in a period of 427 days. Other observations did not seem to confirm this period. Finally he made an elaborate analysis of 33,000 observations between 1837 and 1891, and the result was an empirical law which can be announced as follows:

The pole of the rotation axis of the earth moved with its greatest velocity about the pole of the axis of figure about the year 1774; the period then was 348 days. The velocity has diminished with an accelerated rate since then. In 1890 the period was 443 days. The distance of one pole from the other was about 22 feet = 0.22″.

Further elaborate examination of this material developed the exceedingly important and interesting result that the changes in latitude were the sum of *two periodic* fluctuations superposed on each other. One had a period of about 427 days and an amplitude of 0.12″ The second had a period of a year with an amplitude that was variable between .04″ and .20″

Sometimes these two fluctuations worked together, giving a total range of .33″, and at times they conspired against each other, reducing the range to a minimum of a few hundredths of a second. He compared his theory with the observations, and the result was in the main exceedingly satisfactory.

His conclusions were attacked as to the 427-day term. The annual term could be explained as due to meteorologic causes.

Professor Newcomb, however, in March, 1892, explained in a paper communicated to the Monthly Notices of the Royal Astronomical Society that in deducing the Eulerian period of 305 days the earth, as we have remarked, was considered *absolutely rigid;* that when the effect of the mobility of the oceans and of the lack of perfect rigidity of the earth were taken into account, the mathematics required a time of rotation of the true pole about the axis of figure longer than the previously accepted 305 days. Making certain assumptions Newcomb obtained a period of 443 days.*

An additional interesting conclusion which Dr. Chandler has lately published is that the fluctuation with a period of 427–428 days is a circular one, as theory seems to demand, while the annual fluctuations appear elliptical in character.

An exceedingly interesting and important confirmation of the Chandlerian period of 427 days, or about 14 months, was lately announced by M. Tisserand. Examination has been made of the tide records of the Helder in Holland. These are kept with great accuracy. It has been found that between 1851 and 1893 these tide records show a variation in the average sea level indicating a 14-month period. The greatest divergencies are very small, only 14 mm.= ½ inch about, but they appear unmistakably and are what theory would demand.

In a letter recently received from Dr. Chandler he states that he finds that the annual part of the polar motion is an ellipse three or four times as long as broad, and he expresses the law of the motion of the pole in this ellipse as that the areas described from the centre are proportional to the times.

We can conclude safely, therefore, that no large changes of latitude have taken place for many thousands of years; in fact, in geologic times, that there is no adequate proof of *progressive* changes in the latitude of any place; but finally that very small periodic changes have occurred, and they are such as can be and are observed.

The feeling is growing in the minds of those who have given the subject close attention that we shall find that many and various causes enter into the problem of determining the law of changes. It will, no doubt, take many years of careful observation to obtain the data necessary to fully test Dr. Chandler's or any other hypothesis.

The scientific men abroad are discussing the advisability of establishing several observatories at various places on the earth's surface, for the purpose of collecting the data.

* Professor R. S. Woodward has lately obtained by a new discussion of the theoretical problem a formula that seems to indicate the correctness of Chandler's empirical formula.

Ultimately Dr. Chandler's formula, or a slight modification of it, may be proved correct, and with it we may be able to state what the latitude of any place will be at any time.

The lecture was followed by some illustrations showing that revolving bodies preferred to revolve about their shortest axis or around the axis about which the moment of inertia was a maximum.

Charts and diagrams were exhibited showing the results of observations made at Pulkova, Prague, Berlin, Strassburg, Bethlehem and the Sandwich Islands, etc.

These results were compared with the deductions from Chandler's formula and shown to agree therewith to a remarkable extent.

The preliminary results of the observations made at Columbia College from May, '93, to July, '94, were exhibited.

The lecturer threw on the screen illustrations of several forms of Zenith Telescopes and described the new form made by Wanschaff, of Berlin.

J. K. Rees.

Columbia College.

CURRENT NOTES ON PHYSIOGRAPHY (VII.).

AREA OF LAND AND WATER.

Professor H. Wagner, of Göttingen, contributes to the April number of the Scottish Geographical Magazine an abstract of his recent studies on the land and water areas of the globe for successive latitude belts. He contends that Murray's figures, published in the same magazine for 1886 and 1888 and based on Bartholemew's maps, are inaccurate to a significant extent. Wagner's measures of the better known lands between 80° north and 60° south latitude is 51,147,100, against Murray's 51,298,400 square miles. Taking 250,000 for lands yet undiscovered in the Arctic regions, and 3,500,000 for Antarctic lands, the total land area of the globe would be 55,814,000 square miles. Wagner finds confirmation of his figures in the results independently obtained by K. Karstens, who has recently made a new reckoning of the area and mean depth of the oceans.

THE 'FLY-BELT' IN AFRICA.

The remarkable control over the occupation of Africa, exercised by the little tse-tse fly, whose bite is fatal to horses and cattle, leads to the introduction of cheaply constructed narrow-gauge railways across the belt of country dominated by this pest. The Portuguese district, next south of the Zambesi river on the east coast, with its capital at the little settlement of Beira, attains some commercial importance from its relation to Mashonaland and the gold district of the interior; but in order to connect the two, a railway a hundred and twenty miles long has been made 'to bridge the fly-belt.' The coast exhibits a combination of equatorial and tropical rainfall, having high temperature and heavy rain from October to April, but from June to September 'the weather is almost pleasant.' At Beira the scarcity of water in the dry season threatened a few years ago to be a serious question, as a supply had to be brought from the upper course of the rivers at a considerable cost; but "in 1893 a Scotch plumber was imported, and all anxiety on this score came to an end," as he made galvanized iron tanks in which rain water could be gathered and stored from the roofs (Scot. Geogr. Mag., April, '95).

COLD AND SNOWFALL IN ARABIA.

The ordinary association of heat with the dryness of deserts tends to give the impression that Arabia has no cold weather. Nolde's account of his expedition into the Nefud desert of the Arabian interior, latitude 28 north, altitude 3,000 feet, tells of the severe cold that he experienced there in

February, 1893. The days were warm and pleasant; but the nights cooled to –5° or –10° C; the changes of temperature being extremely sudden. For example, on February 1, at noon, the thermometer read +5°.5, with a cool wind; at 2 o'clock, +6°, at 4, 7.5°; then came a rapid rise to 25.5°, for which no special explanation is given. Just after sunset there was a sudden fall of thirty-three degrees, to –8°; and the minimum of the night was –11°. The cold and blustering wind caused much discomfort in traveling. The greatest surprise that Nolde met was on February 2, when a storm clothed the Nefud far and wide with a sheet of snow several inches deep, making it resemble a Russian steppe rather than an Arabian desert. The Bedouins, however, said that snowfall there was very unusual. (Globus, 1895, No. 11.)

CENTRAL AMERICAN RAINFALL.

PROF. M. W. HARRINGTON shows in an article under the above title (Bull. Phil. Soc. Washington, xiii., 1895, 1–30) that the northeast slope of Guatemala and Honduras has rainfall maxima in June and October, following the zenithal passages of the sun and a moderate winter maximum in January, ascribed to the encroachment even in these low latitudes of cyclonic areas from the westerly winds of the temperate zone. This gives an interesting repetition of the case of northern India, as described by Blanford. The rainfall on the southwest slope of Central America has maxima in June and September-October, corresponding to the two zenithal passages of the sun. The July-August minimum is faintly marked, while that of January and February is very low and for a time almost rainless. It is noteworthy that the zenithal rains here are often accompanied by strong squally winds from the southwest, suspected of being occasional extensions of the southeast trade wind across the equator into our hemisphere. It may be remarked that the association of these winds with the counter current that runs eastward in the Pacific a little north of the equator confirms the suggestion that the equatorial counter currents in general are caused by the extension of the trade winds of one hemisphere across the equator into the other hemisphere. They are thus deflected from a westward to an eastward course, and hence locally produce eastward currents.

THE METEOROLOGISCHE ZEITSCHRIFT.

THE thoroughness so characteristic of German scientific work appears in this excellent journal, the leader of its class, with its able original articles, its rich variety of notes and its exhaustive bibliographic reviews. Originally established thirty years ago by the Austrian Meteorological Society, and edited successively by Jelinek and Hann, of Vienna, it was enlarged eleven years ago by further assistance from the German Meteorological Society, when Köppen, of the naval observatory at Hamburg, became associate editor; his place being lately taken by Hellman of the Prussian Meteorological Institute at Berlin. Dr. Hann, however, still retains his position as leading editor and is a frequent contributor to the pages of the journal. One of his latest essays (January, 1895) is on the rainfall of the Hawaiian Islands, in which he brings together all available material, and discusses it more completely than has hitherto been done. Dutton's explanation of the considerable rainfall on the southwest slope of Hawaii is quoted with acceptance. A meteorological peculiarity of these islands seems to be that their richer windward sides, sloping to the northeast with a plentiful rainfall, are on a large part of the coast with difficulty approached from the sea on account of the cliffs that have been cut along the shore by the strong surf from waves driven by the trade winds.

FOEHN-LIKE EAST WINDS IN AFRICA.

Danckelman, who for some years has made a special study of African meteorology, contributes a note on the foehn-like east winds felt on the southwest coast of Africa, about the southern tropic (Met. Zeitschr., January, 1895). In the interior, temperatures above 27°C are unknown in the winter (April to October); but on the coast in this season, maxima over 30°, and even as high as 39°, are reported, east winds and low humidity occurring at the same time. As so high a temperature cannot be ascribed to heat from the interior, Danckelman explains it as the result of the dynamic warming of the wind during its descent from the interior highlands. This is only one more illustration of the importance of adiabatic changes of temperature in meteorological phenomena; the Swiss foehn and our western chinook, the extraordinary foehn-like winds of west Greenland, the 'hot winds' of India and of Kansas, as well as the ordinary warm or hot southerly cyclonic winds, or 'siroccos,' all owing a greater or less share of their high temperatures to the heat developed by compression during the descent of air from higher to lower levels.

THE AMERICAN METEOROLOGICAL JOURNAL.

The American Meteorological Journal, conducted for a number of years by Professor Harrington at Ann Arbor, and since 1892 edited by R. DeC. Ward and published by Ginn & Co., Boston, is an able exponent of the science of the atmosphere for this country. The closing number (April, 1895) of the eleventh volume opens with a note by the editor, reviewing the recent work of the journal, and making an excellent showing for its continuation. Its original articles make it of value to the investigator; its notes and reviews place much important material before the general student; and its more elementary or educational articles must prove useful to the teacher, for in spite of a recent assertion to the effect that the meteorological aspects of geography are well taught in our schools, there is room for much improvement in this direction. The April number contains notes on signs of a recent change of popular opinion concerning the effect of cultivation on rainfall in Iowa, the proceedings of the last meeting of the New England Meteorological Society—the only society of the kind, we believe, in this country—and diagrams of a curiously curved storm track from the Pilot chart of the Hydrographic office; reviews of the Blue Hill (Mass.) observations for 1893, of Ley's new work on clouds, and of a new Danish series of monthly pressure charts for the North Atlantic. The editor contributes an account of Swiss studies of thunderstorms, and a description of meteorological work in India and Australia. The wind known as the 'southerly burster,' as felt at Sydney, has recently been studied in a prize essay; it recalls in many particulars the 'northers' of our Texan coast.

NOTE ON CROLL'S GLACIAL THEORY.

A brief article by the undersigned (reprinted in Amer. Met. Journal for April from the Trans. Edinb. Geol. Soc., vii., 1894, 77–80) suggests a common explanation for three forms of geologically recent climatic change, namely, the glaciation of many northern lands, the expansion of many interior lakes, and the production of *wadies* by water action in the now dry Sahara. Accepting Croll's theory of the coincidence of glacial conditions with long aphelion winters during periods of great orbital eccentricity, it is argued that the chief cause of snowy precipitation at such times must be the greater activity of cyclonic processes, then intensified by the stronger general circumpolar circulation, in turn accelerated by the increased winter contrast of polar and equatorial temperatures;

Hann's dynamical theory, instead of Ferrel's convectional theory of extra-tropical cyclones, being adopted. All those regions whose precipitation is in large part dependent on extra-tropical cyclonic storms would under these conditions have an increased annual rainfall; and the lakes of interior basins in temperate latitudes would consequently increase in volume. The winter rains of subtropical belts, such as the northern Sahara, would extend further towards the equator, for the equatorward migration of the tropical belt of high pressure in winter is essentially a result of the increased vigor of the circumpolar circulation at such times; thus the formerly greater rainfall indicated by the desert *wadies* might be explained. The coincidence of greater precipitation during the same epochs of time over the glaciated, the lacustrine and the desert areas is, however, not yet independently proved. W. M. DAVIS.

HARVARD UNIVERSITY.

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*GRAVITY MEASUREMENTS.**

RELATIVE measurements of the force of gravity were made in 1894 by the U. S. Coast and Geodetic Survey at twenty-six stations, mostly located along the thirty-ninth parallel from the Atlantic coast to Utah. Points were included on the Atlantic coast, Appalachian mountains, central plains, Rocky mountains (including the summit of Pike's Peak, 14,085 feet in altitude), western plateaus, and the eroded valleys of the Green and Grand rivers. The half second pendulum apparatus designed by Dr. T. C. Mendenhall was used, with methods not before employed with short pendulums. They were swung at a low air pressure (60 mm.), each swing lasting eight hours, and the successive swings covering the entire interval between the first and last time observations, usually forty-eight hours. The two chronometers used were rated by star observations made with a portable transit in the meridian. The flexure of the support was measured and correction applied. The results indicate the entire elimination of errors due to diurnal irregularities of rate, and show that there was practically no wear of the agate knife-edge. Determinations made at the base station (Washington) several times during the year show a range of only .000,004 second in the mean period of the three pendulums, indicating a high permanency of period, and throwing some light on the invariability of gravity. The average time required per station was slightly over five days.

Values of gravity for Washington derived relatively from absolute determinations made in various parts of the world show a considerable discordance, the range being from 980.047 to 980.285 dynes. The results of the past season are based on a provisional value adopted for Washington. As they were carried out with the same instruments and uniform methods, it is probable that their relative accuracy is much higher than that of many of the absolute measures.

The results are discussed principally in connection with the question of reduction to sea level, the distribution of the stations with respect to an unusual variety of continental conditions rendering the series valuable in this connection. This is an important question in the application of pendulum observations to the geodetic problem of the earth's figure, and involves the various theories as to the condition of the

* 'Results of a Transcontinental Series of Gravity Measurements,' by G. R. Putman, read February 2, 1895, Philosophhical Society of Washington, Bulletin Vol. xiii.; preliminary results were presented before the National Academy of Sciences by Dr. Mendenhall, November, 1894. Mr. G. K. Gilbert, of the U. S. Geological Survey coöperated in this work by making a geological examination of the stations. His conclusions and a discussion of the results in connection with the theory of isostasy are published in the same Bulletin.

earth's crust. It has given rise to many diverse opinions, and the apparent anomalies in the force of gravity have been so great with various methods of reduction as to necessitate the rejection of certain classes of stations even in the most elaborate discussions, as those of Clarke and Helmert. Three methods of reduction were applied to these stations, and the effect of latitude was eliminated by comparison with a theoretical formula based on Clarke's figure of the the earth. In each of these methods correction was made for the elevation above sea level and for topographical irregularities near the station, and they differ only in the allowance made for surface attraction, as follows:

1. Bougner's reduction. The vertical attraction of the entire mass above sea level was subtracted. With this method the results show a large defect of gravity on the western mountains and plateaus, closely proportional to the average elevation, but having no relation to the altitude of the particular point of observation or to distance from the ocean.

2. Elevation reduction. No correction was made for attraction. The defect of gravity in general disappears, but there are large residuals in the mountainous regions, gravity being in excess at stations above the average level of the surrounding country, and in defect at those below. The size of the residuals is nearly proportional to the difference in elevation between the station and the average level.

3. Faye's reduction. On the theory that the surface of the earth is in general in a condition corresponding to hydrostatic equilibriun, M. Faye proposed that no correction be made for the attraction of the average mass above sea level, but that account be taken of local deviations from the average level, as, for instance, the attraction of a mountain on a station at its summit. Developing this idea we may consider that all general continental elevations are compensated by a lack of density or other cause below sea level, but that local irregularities of surface are not so compensated, but are maintained by the partial rigidity of the earth's crust. The measure of this lack of compensation will be the attraction of a plain whose thickness is the difference in elevation between the station and the average surrounding country. The latter was estimated within an arbitrarily adopted radius of 100 miles of each point, and the correction applied, positive for stations below the average and negative for those above.* With this reduction all the large residuals disappear. For the fourteen stations (in mountainous regions) where it was applied, the sums of the residuals are: with Bougner's reduction 2.577 dynes, with elevation reduction 0.677 dynes, with Faye's reduction 0.175 dynes, indicating a decided advantage for the latter.

A similar discussion made of former Coast and Geodetic Survey observations on oceanic islands and coasts shows that the excess of gravity that has been found on islands with Bougner's reduction largely disappears on the application of Faye's idea, subtracting the attraction of islands considered as displacing sea water. The residuals with Bougner's reduction are probably a measure of the lack of density below sea level, and with the elevation reduction a measure of the lack of compensation. The general conclusion is that the so-called anomalies of gravity may be largely accounted for on general principles, and that the value of these measurements in connection with the problems of geodesy and the intimately related questions of terrestrial physics will be proportionately enhanced.

By comparing the values of g measured on the summit and near the base of Pike's Peak the value 5.63 was deduced for the

*Mr. Gibert independently applied this method of reduction, using a radius of 30 miles.

mean density of the earth. The attraction of the mountain was computed from contour maps and from information as to its density furnished by Mr. Whitman Cross of the U. S. Geological Survey. A set of quarter-second pendulums designed by Dr. Mendenhall was tested at four of the stations with satisfactory results. This is the smallest apparatus yet made for the purpose, weighing but 106 pounds with packing boxes.

HERBERT G. OGDEN.

WASHINGTON, D. C.

THE ASTRONOMICAL AND PHYSICAL SOCIETY OF TORONTO.

THIS Society, now very widely known, was originally formed in 1884 by a few gentlemen who, while actively engaged in business pursuits, were kindred spirits in their love for scientific study and met at intervals more or less regular at their respective residences for recreative reading, observation and experimentation. The membership gradually increasing, it was finally decided to secure incorporation under a general Act permitting the acquiring and holding of real and personal property, etc., and in 1890 the Society became a corporate body. The first president of the new association was the late Mr. Chas. Carpmael, M. A., F. R. A. S., the Director of the Toronto Magnetic Observatory; the vice-president was Mr. Andrew Elvins, who had indeed been the first to gather together the few friends who had formed the original nucleus, and who is still highly esteemed and honored as the father of amateur astronomy in Toronto. A constitution modeled upon that of the Astronomical Society of the Pacific having been framed and by-laws adopted, a circular was addressed to many scientific societies and distinguished astronomers and physicists throughout the world. Several of the latter became corresponding members, while various scientific bodies contributed many volumes of reports, etc., which formed the beginning of what is now a very valuable library. Without this very material aid the progress of the Toronto Society would have been very slow indeed, but as, at meeting after meeting, the secretary's and librarian's reports were read, it became soon apparent that the heartiest sympathy and support were being extended, without exception, by all who had been addressed.

The first annual report of the Society was an unpretentious little volume of 40 pages, containing abstracts of papers read during the year 1890, and records of the more important work done at the telescope by the various members who were particularly interested in observation. The frontispiece was a drawing of sun-spots and also of hydrogen flames, by Mr. A. F. Miller, who has always taken a keen interest in solar physics. Mr. T. S. H. Shearmen contributed a paper on 'Coronal Photography, in the Absence of Eclipse.' In common with many other enthusiastic observers, Mr. Shearmen is still engaged upon this work. Referring to the objection raised regarding the impossibility of photographing the corona in full sunshine on account of the very slight difference between the intensities of the two lights, Mr. Shearmen cites observations of the inferior planets seen projected on the corona.

The appendix to this volume contains a list of the presents donated by the various observatories and scientific bodies in the United States, and by Mr. John Goldie, of Galt, Ont., a life member of the Society. The list of the Society's exchanges increased very rapidly after the publication of the first report. The volume for 1891 contained papers by Dr. J. Morrison, Mr. J. Ellard Gore and Mr. W. F. Denning. An opera-glass section had been formed which met during the weeks alternating with the regular fortnightly meetings of the Society, and much interest began to be taken in active telescopic work. An essay by Mr. G. E. Lumsden, entitled a 'Plea for the Common Telescope' (subsequently reprinted in the *Scientific American Supplement*), was the means of creating a very general desire for the possession of instruments of moderate aperture, and there are now a great many telescopes ranging to 5-inch among the members of the Society. Mr. Lumsden's own telescope is a $10\frac{1}{4}$-inch, With-Browning reflector. It was with this that he made an observation of a double shadow of Sat. I in transit across the disc of Jupiter, on the night of September 20, 1891. The particulars of the observation and comments upon theories accounting for the possible cause of the phenomenon, which has been seen but three or four times, appeared subsequently in *L'Astronomie*. A drawing of Jupiter made on the night of the observation forms the frontispiece to the volume of Transactions of the Society for 1891.

During this year the Society lost a sincere friend and earnest worker by the death of the Hon. Sir Adam Wilson, Chief-Justice of Ontario. This distinguished jurist, one of the most eminent of Canada's public men, had actively interested himself in scientific matters after retiring from the Chief-Justiceship, and had erected and equipped an observatory at his residence. Shortly after Sir Adam's decease, which was quite sudden, Lady Wilson donated to the Society his telescope, a six-inch reflector, together with other apparatus and many works on science. Sir Adam had intimated that he wished these to pass to the Society at his death. The reflector is now mounted at the residence of Mr. John A. Paterson, M. A., vice-president, and is used by the members in regular observation.

In 1892 McElvins resigned the office of vice-president, in order to have more time at his disposal during which to take up active work on special lines, notably meteorology. The constitution was amended to admit of election of two vice-presidents, and Dr. Larratt W. Smith, Q. C., and Mr. John A. Paterson, M. A., were appointed. During this year also the Hon. G. W. Ross, LL. D., Minister of Education, became Honorary President. The Society was now becoming very extensively known, and its list of correspondents rapidly increasing. The meetings were particularly well attended, and the Toronto press was most courteous and obliging in publishing reports of the Society's work from time to time. Meetings were frequently held at the Toronto observatory, where practical use was made of the large equatorial and other instruments of the equipment. The great magnetic storm of February 13, 1892, was charted by Mr. F. L. Blake, of the observing staff, and a photographic reproduction accompanied the volume for that year. Towards the close of 1892 a committee was appointed to act conjointly with a committee from the Canadian Institute with a view to moving in the matter of a change in astronomical time reckoning. The report of the committee was presented on April 21, 1893, and adopted. It is now widely known that the great majority of astronomers are in favor of reckoning the astronomical as the civil day, from midnight to midnight, and it remains for the Government of the United States to decide whether the ephemeris shall be changed accordingly. The Admiralty in England has expressed a desire to meet the views of other nations.

During 1893 the Society was enabled to further the object always kept in view, the popularizing of science, by the kindness of the University authorities, who gave the use of the physical lecture room for popular lectures, illustrated by experiment. Mr. C. A. Chant, B. A., and Mr. G. F. Hull, B. A., have taken charge of this department of the Society's work with eminent success. A very liberal interpretation of the physics relating to astronomy having been made, there has resulted a keen interest in experimental science; so that he is a welcome addition to the membership who takes interest in any branch of what was formerly styled natural philosophy.

During the years 1893 and 1894 the subject of magnetism and electricity engaged a large portion of the time spent at the regular meetings. Spectroscopy, quite apart from its bearing upon astronomy, has also been a subject of interest. A valuable note, by Mr. A. F. Miller, on the spectrum of the light emitted by insects, appeared in the volume of Transactions for 1893.

In the earlier years of the Society's existence the meetings were held at the residences of members, but it was ultimately found that one central place of meeting would be preferable, and for some time past the regular meetings have been held in the rooms of the Young Women's Christian Guild building. Here the library is kept and the secretary has his office. The Society suffered another loss in October, 1894, by the death of the president, Mr. Carpmael, whose health had been impaired for some time previously. A short sketch of Mr. Carpmael's very active life is appended to the Transactions for 1894.

Dr. Larratt W. Smith, Q. C., succeeded Mr. Carpmael in the presidential chair, and the office vacated by the former is now ably filled by Dr. E. A. Meredith, formerly Deputy Minister of the Interior, and the predecessor of Sir Wm. Dawson in the presidential chair of McGill University, Montreal. The great work always before the Astronomical and Physical Society of Toronto is the founding of a popular observatory, in the true sense of the term; not being too sanguine, it is still hoped that steps will soon be taken to this end. It is a matter of regret that there is no astronomical equipment in Canada able to meet all the requirements of modern astronomy.

Two of the members of the Society, Messrs. Z. M. and J. R. Collins, have been very successful in making silver-on-glass specula, and have figured several of eight-inch; having recently fitted up apparatus for the work, it is confidently expected that they will soon be able to undertake the construction of very large reflectors. It is not too much to hope that they will be able to execute the telescope when the public spirit of the Toronto people demands a great observatory, and this may be in the near future, for, in regard to popularizing science, the Toronto Society has been eminently successful. A branch of the association at Meaford, Ont., has recently been formed, and other similar societies are already spoken of.

THOMAS LINDSAY.

CORRESPONDENCE.

THE RIVERS OF EDEN.

TO THE EDITOR OF SCIENCE: Referring to a note on the 'Garden of Eden' in SCIENCE (May 3, 1895), I desire to point out that in a series of articles, under the heading 'Gold, Bedolach and Shoham Stone,' in the 'Expositor' (London, 1887), I showed that the only possible scientific explanation of the geography of Eden in Genesis is that based on the geological explorations of Loftus, and now advocated by Prof. Haupt, namely, that the four rivers are the Kherkhat, Karun, Tigris and Euphrates. Farther I showed that the geography and geology of this ancient author are more accurate than those of modern maps and popular statements until within a very recent time, and that the local standpoint of the original writer was on the Euphrates, and his date not long after that of the historical deluge, whatever views may be held by critics as to the ultimate editing of the book. Delitsch and others have been misled by their want of knowledge of the condition of the district in the earliest human (Palanthropic) age, whereas this was evidently known to the original writer, though the geographical conditions must have been somewhat changed in his time.

I rejoice that a scholar like Dr. Haupt has advocated a view which will almost for the first time bring this very ancient and very accurate geographical description before the notice of modern biblical scholars in a manner which will be intelligible from their point of view.

I may add that a popular view of the geological argument on the subject will be found in my work, 'Modern Science in Bible Lands,' published in 1888,* where will also be found a sketch-map of the region, illustrating the bearing of the geological and geographical researches of Loftus and others on this much vexed and much misunderstood question.

J. William Dawson.

Montreal, May 7, 1895.

*Harpers, New York.

COLOR-ASSOCIATIONS WITH NUMERALS, ETC. (THIRD NOTE).

To the Editor of Science: In Science, old series, Vol. vi., No. 137, p. 242, I printed the results of some experiments upon the association of colors with letters of the alphabet, with numerals, etc., in the case of one of my daughters. In *Nature* for July 9, 1891, I gave a table exhibiting the results of these experiments in the years 1882, 1883, August, 1885, December, 1887, June, 1889, and June, 1891, a period of about nine years. The table can be readily consulted by anyone interested, so that it need not be reprinted here. In February, 1895, I again questioned my daughter on the subject, and I find that the colors given in her replies of June, 1891, are unchanged except in two cases. The figure 8 was visualized by her as white (August, 1885), cream color (December, 1887), white (June, 1889), cream (June, 1891), and is again seen as white (February, 1895). The figure 10 was noted as brown (1885), brown (1887), black ? (1889), black or brown (1891), and black (1895). With these exceptions there are no material changes. My remarks on the table, given in *Nature*, do not seem to call for any additions or subtractions. The present note, taken with the others cited, seems to be of value, as it records the results of experiments made under exceptionally good conditions and now extending over a period of some thirteen years.

Edward S. Holden.

Mount Hamilton, May, 1895.

UNIVERSITY OF KANSAS STATE GEOLOGICAL SURVEY.

In conformity with the law under which the University of Kansas is now working, the Board of Regents at a recent meeting formally organized the University Geological Survey of Kansas with Chancellor F. H. Snow, ex-officio Director; Professor S. W. Williston, Paleontologist; Professor Erasmus Haworth, Geologist and Mineralogist, and Professor E. H. S. Bailey, Chemist.

In addition to these, other members of the University Faculty will be engaged upon the work of the Survey, as well as the advanced students of the departments of Geology and Paleontology. An effort will also be made to centralize and unify the energies of different geologists in the State who have been doing valuable work along different lines of geological investigations. Already a considerable start has been made and the coöperation of different geologists of the State has been secured.

The policy of the Survey will be conservative, with the expectation that it will be continued and eventually include all other branches of the natural history of the State. The general stratigraphy of the State will first be elaborated in order that it may be used in the further study of various questions of economic and scientific importance, all of which will be taken up as rapidly as existing conditions from time to time will permit.

Work in the Coal Measures of the State has been in progress for two summers, and Volume I. of the Report is now almost ready for publication. Other volumes will appear at irregular intervals. Those already under preparation are: One on Coal, Oil and Gas; one on the Vertebrate Paleontology of the State; and one on the Salt and Gypsum deposits of Kansas.

F. H. Snow,
Chancellor University of Kansas.
Lawrence, Kansas,
April 20, 1895.

SCIENTIFIC LITERATURE.

Our Native Birds of Song and Beauty. By H. Nehrling. 4°, 36 colored plates from originals by Ridgway, Goering and Mützel. Published by Geo. Brumder, Milwaukee. To be completed in 16 parts, $1.00 each.

Part eleven of this excellent work, carrying it nearly half through the second volume, has been delivered to subscribers. It is enough praise to say that the high standard of the first volume is maintained. Mr. Nehrling is a field naturalist of the kind who deem a bird in the bush worth two in the hand. He loves everything in the woods and fields, and in telling about the birds and their lives he tells also of the trees and flowers.

The aim of the book is to give trustworthy accounts, in popular style, of the haunts and habits of our birds. Occasionally it does more and introduces a new fact of scientific interest, as when the breeding of the Pine Grosbeak (*Pinicola*) is recorded for northern Wisconsin. On the other hand, it is not always down to date. For instance, under the Black Rosy Finch (*Leucosticte atrata*), the statement is quoted from Ridgway that "nothing has yet been learned as to its range during the breeding season." As a matter of fact, the species is common in summer in the higher parts of the Salmon River Mountains in Idaho, where it was obtained by the reviewer five years ago (see North American Fauna, No. 5, 1891, 102). Similarly, the Gray-crowned Rosy Finch (*L. tephrocotis*) is said to be 'a resident of the interior of British America, near or in the Rocky Mountains,' and further, that 'none seem to breed in our territory.' If Mr. Nehrling had consulted the 'Report on the Ornithology of the Death Valley Expedition,' by Dr. A. K. Fisher, he would have found the statement that this species "is a common summer resident in the higher portions of the White Mountains and the Sierra Nevada in eastern and southern California," where it breeds abundantly and where nearly 40 specimens were secured by the expedition (North Am. Fauna, No. 7, 1893, 82).

The plates are of two kinds, some show a single species in appropriate surroundings; others showing a number of species grouped together on a background of landscape or dense vegetation. The reproductions, while amply sufficient for purposes of identification, are evidently inferior to the originals, the number of stones used in printing being too small, and the workmanship not of the best. By far the most effective picture in the second volume is one of a group of winter birds—Evening Grosbeak, Pine Grosbeak, Redpoll, White-winged Crossbill, Nuthatch and Chickadee—on top of a spruce tree laden with snow. The combination of colors is striking and is aided by the red berries of a giant mountain ash, which, by the way, forgot to drop its leaves! Among the earlier plates of high merit, both in conception and execution, are several by Robert Ridgway that give charming glimpses of birds in characteristic attitudes and surroundings. Of these, the Golden-crowned Kinglet, Prothonotary Warbler, and Canon Wren are among the best.

By some accident in binding, the two plates of part 10 (pls. 13 and 15) are repeated from the first volume.

The nomenclature is that of the American Ornithologists' Union, except that the authority given is for the combination, not for the species—an unfortunate departure, inasmuch as it does not tell who was the original describer of the species.

To those unfamiliar with the first volume it may be said that the work is not a scientific treatise at all, but a popular book devoted to the life histories of birds, and based mainly on the authors' extensive field experiences, supplemented by quotations—perhaps too lengthy and frequent—from the writings of well-known ornithologists. It does not profess to cover all North American birds, omitting the water birds, birds of prey and a few others, but treats primarily, as its title indicates, of 'Our Native Birds of Song and Beauty.' It is a large, well printed quarto, and of its kind is incomparably the best book yet published in America. C. H. M.

Municipal Government in Great Britain: By Albert Shaw. New York, The Century Co. 1895, 8°, viii + 385.

The modern increase of cities, and of the proportion of urban population as compared with that of rural districts, is, according to Mr. Shaw, to be accepted as a permanent fact for this generation and its immediate successors, and,instead of lamenting over it, it is the duty of thinking men to devise ways and means to do away with or diminish the evils which are at present connected with city life. The author states his point of view as being that a city government should so order the general affairs and interests of the community as to conduce positively to the welfare of its people, or, at all events, to make it certain that for the average family the life of the town shall not be necessarily detrimental. The object of the book is to show how some of the older and larger British cities have dealt with this problem, giving details as to their modern forms of government, method of elections and modes of securing pure water, cleanliness, rapid transit, prevention of contagious diseases, etc.

The cities selected for this purpose are Glasgow, Manchester, Birmingham and London, and for each a vast amount of information is clearly and concisely given.

Taking Birmingham as an example, it is shown that in twenty years the death rate of the city was lowered twenty per cent., and, in some parts of the city, sixty per cent.; that the provisions for the comfort and recreation of the people have been greatly increased, and that, while over forty millions of dollars have been expended in securing these improvements, the taxes have not been increased, because the municipal gas and water works, street railways, markets, etc., have been from the financial, as well as from the utilitarian, point of view completely successful. Surely it is worth while for the citizens of American cities to inquire how this has been accomplished.

The description of the means used by the city of Glasgow for the isolation and treatment of infectous disease is worthy of careful study. The Contagious Diseases Hospital has been given the semblance of a lovely village, and Mr. Shaw truly says that "the difference between popularity and unpopularity in a public hospital for infectious diseases may well mean all the difference between a terrible epidemic and its easy prevention." The sanitary wash houses of Glasgow are a feature of the work of the Health Department which finds no parallel in American cities but which is of great importance. One of these cost $50,000, another $75,000, and they far more than repay their cost.

The author promises a second volume

treating of municipal government in the chief countries of Continental Europe, and if we could be assured of a third volume, prepared with equal care and accuracy, 'On Municipal Governments in the United States, or how not to do it,' it would be, as Artemus expressed it, 'a sweet boon.' Meantime, let Mr. Shaw's first volume be made a subject of special study by the younger professional men in this country, for the time is near at hand when they will be compelled to take some definite line of action with regard to our own cities, each of which presents its own peculiar problems, but problems upon which much light is thrown by the experiences of our transatlantic brothers.

J. S. B.

Theoretical Chemistry. By PROFESSOR W. NERNST, Ph. D., University of Göttingen, translated by PROFESSOR C. S. PALMER, Ph. D., University of Colorado. Macmillan & Co. Pp. 697. Price $5.00.

It has long been evident that the treatment of the physical side of chemistry, in text-books avowedly devoted to chemical theory, is not satisfactory. In the present work Physical Chemistry is the main object in hand, and, correspondingly, chemical theory proper is relegated to a subordinate position. The treatment of purely chemical topics is clear and suggestive, but brief, and occasionally inadequate. Thus the discussion of the stereochemistry of nitrogen is confined to the mere statement of the views of Hantzsch and Werner, with not even the barest mention of the difficulties and exceptions which have led many to regard the spatial conception, so far as it applies to nitrogen, as prematurely developed.

But insufficiency of this kind is to be expected whenever the attempt is made to cover the whole field of chemical and physico-chemical theory within the limits of the same work, and it would be unfair to criticise Professor Nernst's book adversely on the ground of inadequate treatment of purely chemical topics which, presumably, were introduced simply for the sake of completeness. We pass, therefore, to the main subject.

For some time a work has been needed which would give concisely the remarkable results of the new Physical Chemistry, and this want Professor Nernst's work is well fitted to meet. The material is well selected, the sections are well proportioned, the facts are accurately and concisely stated, and the translation has been faithfully made, too faithfully perhaps, by one who is evidently well fitted, on the scientific side, for the task.

It may not be out of place to express the opinion that the almost complete abandonment of the historical method which characterizes Professor Nernst's work is a mistake, even in so small a volume. This is particularly plain in the account of the doctrine of electrolytic dissociation. One who reads the fascinating chapter 'Geschichte der Electrochemie' in Ostwald's 'Lehrbuch der Allgemeinen Chemie,' Vol. I., part II., observes this concept vaguely adumbrated in the minds of Grotthus and Daniell, sees it implicitly present in the remarkable views of Clausius, and finally recognizes it freed from all obscurity in the papers of Arrhenius. In Nernst, on the contrary, one is introduced to the doctrine fully formed, and, looking about him in some bewilderment to ascertain its source, discovers an incomplete justification for its existence in the behavior of aqueous salt solutions.

The student who desires to devote himself specially to Physical Chemistry may read the book with profit, but he would do better, having acquired the necessary physical, mathematical and chemical preparation, to go directly to Ostwald's 'Lehrbuch'; to those who wish simply to obtain a broad view of the present state of the science the work will be decidedly acceptable, and this will be its chief function.

It is not pleasant to be obliged to record the complete failure of Professor Palmer's attempt to 'make the sound German speak good English.' The 'sound German' seems to be unusually refractory in his hands, and frequently refuses not only to 'speak good English,' but also to speak any kind of intelligible English at all.

An unpleasant appearance is given to the pages by the translator's unfortunate practice of introducing phrases from the original, sometimes directly, sometimes in curiously infelicitous translation. Thus, in the section in which the applications of the first law of heat to chemical reactions are discussed we read, to express thermal evolution or absorption, either 'Wärmetönung,' which is clear enough, but out of place, or 'heat toning,' a phrase which one struggles vainly to comprehend. Thus he replaces the word element by the remarkable expression 'ground-stuff.' He advocates the introduction of the term 'Knall gas,' and employs it faithfully himself. Rarely the translation attains to complete unintelligibility, *e. g.*, on page 149:

"The choice of a suitable hypothesis to be advanced can be easily made, now or never, in the case before us."

It must be admitted that Professor Palmer's English is by no means pleasant reading. Those with any feeling for the right use of language will be incessantly irritated by it, and even others will be not infrequently annoyed by the unnecessary difficulties which it introduces.

The defects of the translation are undoubtedly serious. But for this there is much compensation. It is plain that the translator has followed the wonderful development of the new science faithfully, and his own comprehension of the subject is evident on every page. The student who will forgive the obvious defects, which, after all, concern rather the appearance than the substance, and give to the book an earnest, thoughtful reading, can not fail to derive from it a large amount of valuable information.

Robert H. Bradbury.

Proceedings of the Society for the Promotion of Engineering Education, Vol II., Brooklyn Meeting, 1894. Edited by Professors Swain, Baker and Johnson. 8vo, pp. viii., 292. $2.50.

This excellent collection of interesting and helpful papers is issued to the members of the Society; but, as we understand from an inserted slip, copies may be obtained from the Secretary, Professor J. B. Johnson, of Washington University, St. Louis, at the regular price paid by members. The volume is well made up, and its contents justify a good form of make-up. The book contains the usual statement of the objects of the Society, the rules, and the lists of officers and members, followed by the complete papers of the the meeting of 1894. The Society was organized in Chicago in 1893, and its next meeting, at Brooklyn, is that here given record. Its membership, already about 160, includes probably the majority of the recognized leaders among representatives of the department of education to which its belongs. The discussions are mainly on subjects of immediate interest to the teachers in the professional engineering schools, and are necessarily of great importance to them and their pupils, though perhaps less attractive to the average reader than are discussions of educational matters generally. The requirements for admission, the character and designation of the degrees properly conferred, the teachers and the text-books, methods and extent of shop and laboratory work, and forms of curricula suitable to this special work, are the main topics, and they are well and dispassionately treated. The volume is full of useful and instructive matter.

R. H. T.

Steam Power and Millwork: By GEO. W. SUTCLIFFE. Whittaker & Co., London; Macmillan & Co., New York. 12mo, pp. xv. 886. 1895. $4.50.

This book is one of the excellent series for specialists published recently by this firm, and is a very good example of the kind of work now coming to be so common in technical departments. It is written for those who are interested in the design, manufacture and use of steam engines, mill machinery and similar apparatus, and presumably represents the condensed experience of its author. The book gives valuable information relative to the most modern systems of production and transmission of power, and the latest forms of engine boilers and transmitting mechanisms, and their details, including also instructions regarding their proportions and for their maintenance. The 157 illustrations are numerous and good, representing every essential detail of which description is given. Numerous tables are distributed through the pages of text, and afford a condensed presentation of facts and data required in the computation of designs. The discussion relates principally to the steam engine; but considerable space is given to rope and other transmissions, and the customary forms of power-transmission by the older methods. References are freely given, and the book is thus made, not only intrinsically valuable, but a key to the extensive literature of its subject and field. The book will prove an excellent contribution to the library, especially of the young engineer.

R. H. T.

NOTES AND NEWS.

JOINTS IN THE VERTEBRATE SKELETON.

IN the last number of the *Archiv für Entwickelungsmechanik der Organismen* is the completion of Gustav Tornier's elaborate investigation upon 'The Origin of the Forms of the Joints in the Vertebrate Skeleton.' The writer is apparently unaware of the work which has been done upon the same subject by Ryder, Cope and others in this country, and his conclusions are therefore of all the greater interest since, while independently reached, they are in accord with the American Neo-Lamarckians so far as the adaptive power of individual reaction is concerned. He concludes as follows: The forms of the joints arise by the adaptation of the organism to external conditions of life, and are the results of mechanical influences which are directed upon the joint apparatus by the action of the muscular system. These mechanical stimuli act directly upon the joints, and lead not through the reproductive cells, but directly through the transformation of those parts of the body which are under these changing influences. Joints, therefore, arise according to the principle announced by Wilhelm Roux of 'functional adaptation,' and of the 'self formation of the useful,' 'of adaptation of the organism to functions through the exercise of these functions.' Since comparative anatomy affords the surest tests of the truth of these principles, proofs which have not had their inspiration in Roux's declarations, but have led a long way toward them and are still showing the application of these principles in questions of theoretical evolution, how useful it would be were these principles also extended into other fields of research! At the same time these proofs indicate that comparative anatomy united with pathology present two of the routes by which this goal can and will be reached. This number also contains the experimental studies in teratogeny by Mitrophanow, and a continuation of Driesch's experimental work.

This journal has become the medium of publication of the new school in Germany which revolts against the extreme to which Weismann has carried the theory of selection, and represents partly the thought which is independent of all theories, partly that which, as seen in the above quotation from Tornier's paper, is analogous to American Neo-Lamarckism. It differs from the American school in the cardinal point, however, that judgment is suspended as to the inheritance of acquired characters.

H. F. O.

THE PREPARATION OF ARGON.

Since the announcement, by Lord Rayleigh and Prof. Ramsay, of the isolation of a new constituent of the atmosphere, any information as to the nature of this substance has been received with interest by the scientific world. Guntz has recently described, in the *Comptes Rendus*, a modification of the method used by Rayleigh and Ramsay for its preparation. This author has substituted lithium for magnesium, thereby securing the absorption of the nitrogen more readily at a lower temperature. The preparation of pure lithium in quantity has hitherto been a difficult problem, but Guntz has devised a simple method for its preparation in large quantities.

This consists in the electrolytic decomposition of a mixture of equal parts of lithium chloride and potassium chloride, the latter being introduced to lower the temperature at which the decomposition takes place. The decomposition is carried on in porcelain crucibles and the molten lithium poured into molds. It is free from iron and silica, but contains a small amount of potassium chloride.

The experiment showing the presence of argon in atmospheric nitrogen and its absence from chemical nitrogen, the latter term being used for nitrogen obtained from chemical substances by decomposition, consists in introducing the nitrogen into a glass tube containing the lithium in a boat. The glass tube is connected with a manometer to show the change in pressure. Upon heating the metal to dull redness, combination of the nitrogen and lithium takes place with incandescence. The manometer after the operation shows a pressure of about 10 mm. Upon introducing another volume of nitrogen and repeating the operation about the same amount of argon is obtained, and this process can be continued until the tube is filled with argon. If, however, chemical nitrogen is used there is total absorption, showing that atmospheric nitrogen contains some constituents not present in chemical nitrogen.

J. E. Gilpin.

HELION.

Prof. Ramsay has kindly sent us the following abstract of his paper on 'Helion, a Gaseous Consistent of certain Minerals.' Part I., received by the Royal Society on April 27th:

An account is given of the extraction of a mixture of hydrogen and helion from a felspathic rock containing the mineral clèveite. It is shown that in all probability the gas described in the preliminary note of March 26 was contaminated with atmospheric argon.

The gas now obtained consists of hydrogen, probably derived from some free metal in the felspar, some nitrogen and helion. The density of helion, nearly free from nitrogen, was found to be 3.89. From the wave-length of sound in the gas, from which the theoretical ratio of specific heats 1.66 is approximately obtained, the conclusion may be drawn that helion, like argon, is monatomic. Evidence is produced that the gas evolved from clèveite is not a hydride, and a comparison is made of the spectra of argon and helion. There are four specially characteristic lines in the helion spectrum which are absent from that of argon; they are a brilliant red, the D_3 line of a very brilliant yellow, a peacock-green line, and a brilliant violet line. One curious fact is that the gas from clèveite, freed from all impurities removable by sparking with oxygen in presence of caustic potash, exhibits one, and only one, of the characteristic bright red pair of argon lines. This, and other evidence of the same kind, appears to suggest that atmospheric argon and helion have some common constituent.

Attention is drawn to the fact that on subtracting 16 (the common difference between the atomic weights of elements of the first and second series) from 20, the approximate density of argon, the remainder is 4, a number closely approximating to the density of helion; or, if 32 be subtracted from 40, the atomic weight of argon if it be a monatomic gas, the remainder is 8, or twice the density of helion, and its atomic weight if it too is a monatomic gas.

GRAVITY MEASUREMENTS.

AT a meeting of the Philosophical Society of Washington on March 16th Mr. G. K. Gilbert discussed the gravity determinations reported by Mr. G. R. Putnam, an account of which is given elsewhere in the present number of SCIENCE. Mr. Gilbert summarizes his conclusions as follows:

"The measurements of gravity appear far more harmonious when the method of reduction postulates isostacy than when it postulates high rigidity. Nearly all the local peculiarities of gravity admit of simple and rational explanation on the theory that the continent as a whole is approximately isostatic, and that the interior plain is almost perfectly isostatic. Most of the deviations from the normal arise from excess of matter and are associated with uplift. The Appalachian and Rocky mountains and the Wasatch plateau all appear to be of the nature of added loads, the whole mass above the neighboring plains being rigidly upheld. The Colorado plateau province seems to have an excess of matter, and the Desert Range province may also be overloaded. The fact that the six stations from Pike's Peak to Salt Lake City, covering a distance of 375 miles, show an average excess of 1,345 rock-feet indicates greater sustaining power than is ordinarily ascribed to the lithosphere by the advocates of isostacy. It indicates also that the district used in this discussion for estimating the height of the mean plain is far too small; even the radius of 100 miles selected by Mr. Putnam may not be large enough."

GENERAL.

IN a paper read before the Paris Academy on April 29th MM. Hericourt and Ch. Richet announce that they have applied the method of injecting serum in the treatment of cancer. Two patients only have undergone this treatment, one of whom is said to have been completely cured.

REV. J. M. SEELYE, president of Amherst College from 1877 to 1890, died at Amherst on May 12th, at the age of seventy. For nineteen years before his election to the presidency he filled the chair of mental and moral philosophy and retained this chair until his death. His original contributions to philosophy were not important, but he exercised great influence as an educator and teacher.

WE learn that Deputy Surgeon-General John S. Billings, of the army, has requested that he be placed on the retired list; and that in October that distinguished officer will leave the Army Medical Museum, of which he is curator, and the Library of the Surgeon-General's Office, of which he is librarian, and these magnificent institutions, that have been made what they are largely by his ability and zeal, will know him no longer. Before the date he has selected for his retirement he hopes to complete his work on the final volume of the Index Catalogue. In seeking official retirement Dr. Billings does not propose to give up work, as he has accepted the chair of hygiene in the University of Pennsylvania. —*N. Y. Medical Record.*

Dr. Carl Thiersch, professor of surgery in the University of Leipsic, died on April 20th at the age of seventy-three. He was appointed professor of surgery at Erlangen in 1854, and in 1867 proceeded to Leipsic. During the Franco-Prussian war he was attached as senior surgeon to the 12th Army Corps. He was the author of standard works on cholera and embryology.

The number of medical journals at present published in Russia is 38. Of these 20 are published in St. Petersburg, 5 at Moscow, 4 at Warsaw, 2 at Odessa, 2 at Charkoff, and 1 at Kasan, Kieff, Saratoff, Woronesz and Pultawa, respectively. The oldest of them all is the *Medizinskoie Obozrenie*, which is twenty-one years old; next comes the *Russkaia Medizina*, which is in its nineteenth year; the *Vratch*, which is in its fifteenth, being third.—*N. Y. Medical Record.*

We much regret to learn that the publication of *Insect Life* will cease with the next number. Two new series of bulletins will be started from the Division of Entomology of the Department of Agriculture to take its place. The one will contain articies of a general economic and biological character —practically such articles as have been published most frequently in *Insect Life*—and the other will contain results of the purely scientific work of the office force.

There has been established in Leicester, England, a bacteriological institution under the direction of a medical officer in the interests of anti-vaccination.

Edward Burnett Tylor, M. A., Reader in Anthropology in the University of Oxford, has been made Professor of Anthropology.

Prof. W. M. L. Coplin, who holds the Chair of Pathology at Jefferson Medical College in Philadelphia, has accepted the call tendered him by the Trustees of Vanderbilt University, Nashville, Tenn., to take charge next fall of the departments of Pathology, Biology and Bacteriology, for which they have just completed a new building.—*N. Y. Evening Post.*

Brigadier General Thomas L. Casey, having reached the age requiring retirement from the active list, has relinquished command of the corps of engineers and charge of the engineer department. He is succeeded by Col. William P. Craighill.

We learn through the *N. Y. Medical Record* that the Medical Department of the State University of Minnesota was granted $40,000 by the Legislature for a laboratory building, making a total of $150,000 appropriated for buildings alone in a period of four years. The medical law was likewise amended to require of all graduates of later date than 1898 'attendance upon four courses of medical lectures, in different years, of not less than six months' duration each.'

The trustees of Williams College have accepted the legacy of $20,000 from Mme. Souberville, in memory of her father, Horace F. Clark, D. D. The College has also received a gift of $3,500 from ex-Governor Pennoyer, of Oregon, to found a scholarship in memory of his son.

Dr. Ernst Ritter, of the University of Göttingen, has been elected Assistant Professor of Mathematics in Cornell University.

The death of Mrs. Henry C. Lewis, of Coldwater, Mich., leaves the art collection possessed by her late husband, valued at $300,000, at the disposal of the University of Michigan. At present the university has not accommodation for the bequest, but President Angell expects an art building to be erected by private contributions. *N. Y. Evening Post.*

An exhibition of California food products will be held in Berlin from the 5th of May to the 5th of July.

THE *Scientific American* for May 11th contains an interesting illustrated account of Purdue University, Lafayette, Indiana.

THEODOR JOHANN CHRISTIAN AMBDERS BRORSEN, the astronomer, died on April 3d at Norburg in Schleswig at the age of 76. He was director of the observatory of Seuftenberg for twenty years.

THE death is announced, at the age of 64, of James Price, President of the Society of Civil Engineers of Ireland, Professor in the University of Dublin and Engineer in Chief of the Midland and Great Western Railway Company.

THE third International Congress of Zoölogy at Leyden is divided into six sections, as follows: (1) General Zoölogy, Geographical distribution, including fossil faunas. (2) Classification of Vertebrates, Geographical distribution. (3) Comparative Anatomy of Vertebrates, living and fossil. Embryology. (4) Classification of Invertebrates, Geographical distribution. (5) Entomology, (6) Comparative Anatomy and Embryology of the Invertebrates.

THE Craven Studentship at Cambridge has been awarded to Mr. R. C. Bosanquet. This is an endowment for advanced studies abroad in the languages, literature, history, archæology, or art of ancient Greece or Rome, or the comparative philology of the Indo-European languages.

IN a demurrer filed by Mrs. Jane L. Stanford in the United States Circuit Court at San Francisco it is contended that, since no valid claim was ever presented to Leland Stanford during his life or to his widow since his death, any claim the United States Government might have had on the Stanford estate is vitiated.

HON. ECKLEY B. COXE, a prominent mining engineer and writer, at one time President of the American Institute of Mining Engineers, died at Hazleton, Pa., at the age of fifty-four years.

BRIGADIER GENERAL CHARLES SUTHERLAND, formerly Surgeon-General of the Army, died at Washington, on May 11th, at the age of sixty-five years.

THE first *conversazione* of the Royal Society for the season was held on the evening of May 1st in Burlington-house, and there was a very large attendance of guests. The exhibits were exceptionally numerous, electric science and applied mathematics being well represented, while some interesting exhibits were also shown in the department of chemistry, astronomy and biology.—*London Times.*

PRINCIPAL PETERSON, of Dundee College, has been offered the presidency of McGill University, Montreal.

DR. J. H. HYSLOP has been made professor of logic and ethics in Columbia College, and Dr. Frederick S. Lee, adjunct professor of physiology.

LÉOPOLD TROUVELOT died on April 22d at the Observatory of Meudon at the age of 68. After the *coup d'état* he left France and came to America, living in Cambridge until 1882. His first published work appeared in Boston in 1866. At this time he was a student of natural history, but later he obtained a position as astronomer at Harvard College. His most important work was on the planet Venus, published in 1892. He was well known for his drawings, many of which still remain unpublished. He leaves an unfinished memoir on the planet Mars, and at the time of his death was engaged on a study of Jupiter.

DR. JOHN W. BYRON, who died on May 8th at the age of 34, was known for his researches in bacteriology carried out at Havana during the yellow-fever epidemic, later in the laboratories of Berlin and Paris, and during the last five years in the Loomis Laboratory, where he occupied the position of bacteriologist. Dr. Byron is said to have contracted the disease of which he died in carrying out his experiments on tubercle bacilli.

The American Forestry Association proposed holding its annual peripatetic meeting in southern New Jersey from May 16th to May 19th. The privileges of this expedition are open to all members of the American Forestry Association, New Jersey Forestry Association and Pennsylvania Forestry Association. On May 15th Prof. B. E. Fernow was to deliver an illustrated lecture at Camden, from which place the party would start, going down the Delaware by steamboat, visiting all places of interest along the shore from Cape May to Atlantic City and in the pines. On the evening of May 17th an illustrated lecture was to be delivered in Atlantic City by Prof. Joseph Rothrock, Forestry Commissioner for Pennsylvania.

At a meeting of the Fellows of the Royal Botanical Society held in the Societies' gardens at Regent's Park, London, the question of the desirability of opening the gardens to the public on Bank holidays was discussed. It was stated at the same meeting that unless some fresh source of income could be obtained the gardens could not be kept up.

At the spring meeting of the Iron and Steel Institute the Bessemer gold medal of 1895 was unanimously awarded to Henry Marion Howe, of Boston, in recognition of his contributions to metallurgical literature. Among the previous recipients of the medal were Peter Cooper, Abram S. Hewitt, Alexander L. Holley and John Fritz. Mr. Howe's most important work is a treatise on the 'Metallurgy of Steel,' which was published in 1890 and for which he received a prize of $500 from the Société d' Encouragement of Paris.

The 66th anniversary meeting of the Zoölogical Society of London was held on April 29th. The chair was taken by Sir William H. Flower. The report of the Council stated that the silver medal had been awarded to Mr. Henry H. Johnston, Commissioner for British Central Africa, for his distinguished services to all branches of natural history. The total receipts of the Society for 1894 amounted to £25,107, a decrease of £1,110 being attributed to the unfavorable weather of the past year. The expenditure amounted to £23,616, a decrease of £1,661. The number of animals in the Zoölogical Gardens on December 31st last was 2,563, of which 669 were mammals, 1,427 birds and 467 reptiles. About 30 species of mammals, 12 of birds and one of reptiles had bred in the gardens during last summer. Sir William H. Flower was re-elected president.—*London Times.*

SOCIETIES AND ACADEMIES.

SCIENTIFIC SOCIETIES OF WASHINGTON.

A joint meeting of the Scientific Societies of Washington was held May 10th, on the occasion of the delivery of the annual address of the President of the National Geographic Society, Hon. Gardiner G. Hubbard. Dr. G. Brown Goode presided, and in the introductory remarks briefly outlined the development of the Societies and their joint commission.

Mr. Hubbard's subject was 'Russia.' He considered it in the light of his own observations while making an extensive journey through that country in 1881. Its climate, physiographic features, government and the customs and conditions of its people were all graphically portrayed. At the close of the address a series of views were shown upon the screen.

In response to a motion by Prof. Simon Newcomb, seconded by Postmaster General Wilson, the large audience gave Mr. Hubbard a hearty vote of thanks for his address.

J. S. Diller, *Secretary.*

BIOLOGICAL SOCIETY OF WASHINGTON.

At the meeting on May 4th, Mr. Charles Torrey Simpson read a paper 'On the Geographical Distribution of the Naiades,' an abstract of a paper on classification and distribution soon to be published.

After stating that the classification adopted by most authors, in which the family *Unionidæ* is founded on forms without siphons, and the *Mutelidæ* on those in which they are developed, cannot stand, since these characters vary in the same genus or species, the writer showed that von Ihering's new definition of the families, in which the former was based on the embryonic state being a *glochidium* and the latter by its larvæ being a *lasidium* agreed with the shells. In the *Unionidæ* these are *schizodont*, in the *Mutelidæ* they are irregularly *taxodont*. The new arrangement shows the former family to be world-wide; the latter as belonging essentially to the southern hemisphere.

The Naiads are distributed in Geographical Provinces whose boundaries may be mountain chains which act as watersheds between river systems, deserts or oceans, but these do not always divide regions, which sometimes have no tangible barriers. In the Old World and South America these provinces essentially agree with those established by Sclater and Wallace; in North America they do not.

The Palæarctic Region includes all Asia south to the Thibetan Plateau, and all the western part of the continent, all Europe and northern Africa, and all of North America west of the Great Cordillera; an area of 16,000,000 square miles, with only a few, not over 50, simple forms. The Oriental Region includes all of Asia south of the Himalayas, north to the Amoor, west to the Indus, Japan and the Malay Archipelago to the Salomon Islands. The forms are numerous, often heavy, distorted, elegantly sculptured, and closely related to those of the United States.

The Australian Region includes Australia, Tasmania and New Zealand, with a few simple unios related to those of South America. Africa south of the Desert is another great region, the Ethiopian, containing the African *Mutelidæ* and small unios allied to those of India. South America is all included in another province, the Neotropical, the Andes proving a barrier to the passage of all forms except unios, which have crossed to the western slope. All the central United States drainage from West Florida to the Rio Grande, including, for the most part, the Great Lakes and the Mackenzie System, constitutes a wonderfully rich region of naiad life, having the finest and most varied forms of the globe. The waters of North America draining into the Atlantic are peopled by simple forms, which may have descended from those of the Mississippi Valley. Mexico and Central America constitute another region of naiad life, having three distinct faunas, an ancient one derived from the United States, a more recent one from that region, and a few immigrants from South America.

Mr. Simpson attempts to trace the development and past history of the naiads, and their evidence regarding past changes of land and sea and the Glacial Epoch.

The paper was illustrated by a sketch-map in colors, showing the different regions.

The second paper of the evening, 'The Other Side of the Nomenclature Question,' was by Dr. Erwin F. Smith, who spoke, in reply to a previous paper by Mr. F. V. Coville, against the unfounded claims put forth in behalf of the Botanical Club Check List. This list has introduced many radical changes into our existing botanical nomenclature without sufficient reason. The revival of the long disused generic names of Rafinesque *et al.*, and the retro-active application of the rule "Once a synonym always a synonym," whereby many generic names of long standing have been discarded, are specially objectionable, and will not bear the light of criticism. Only a few people are urging the adoption of these ultra rules. The best systematic botanists of the world are opposed to them, and there is such a widespread and determined opposition to them in the botanical fraternity generally, both in this country and in Europe, that the movement is certain to amount only to a lamentable schism. It has been claimed that nine-tenths of our American botanists are in favor of these rules, but such statements are wide of the mark. Some of these rules are in conflict with the Paris Code, and others claim to be a strict interpretation of it; but de Candolle himself, the author of this code, considered such interpretations of it as 'abuses,' and urged that the Paris Code of 1867 be so amended as to prevent the swamping of our nomenclature by ultra theorists.

One fact lost sight of by the movers of this new American system, for it has no following in Europe, is that science is an international affair, that the bulk of the botanical work of the world is done outside of the United States, and that even if we were all agreed on this side of the water, which is far from true, it would still be necessary to gain consent of botanists elsewhere before giving to these rules any more weight than mere suggestions. It will be time enough for American botanists to put them into practice when they have received the sanction of an International Botanical Congress. Another very strong objection to making radical changes in our botanical nomenclature is the extent to which botanical names are used in agriculture, forestry, horticulture, floriculture, pharmacy and medicine. There is nothing comparable to it in zoölogy. Only intolerable confusion can result from calling a plant by one name in botany and by another in horticulture or pharmacy, and it is surprising that the force of this argument was not perceived long ago. Finally, the Botanical Club rules do not have the sanction of the A. A. A. S., as might be inferred from some statements which have been made, and the organization of the Club is so loose as to be a fatal objection to regarding its doings or recommendations as in any sense binding on American botanists, when these are opposed by counter-recommendations proceeding from the most famous botanists in the world.

F. A. Lucas, *Secretary.*

BOSTON SOCIETY OF NATURAL HISTORY, MAY 15.

Notes on the Dissection of a Chimpanzee, with Especial Reference to the Brain: Prof. Thomas Dwight.

The Conditions of Escape of Gases from the Interior of the Earth: Prof. N. S. Shaler.

Samuel Henshaw, *Secretary.*

THE MINNESOTA ACADEMY OF NATURAL SCIENCES, MINNEAPOLIS, MAY 7.

I. An Observation on Ants: O. W. Oestlund.

II. Remarks on Some Birds New to Minnesota: Dr. Thos. S. Roberts.

III. An Amine Compound of Gold: H. B. Hovland.

IV. The Chemical Characters of the Minnesota Sandstones: Chas. P. Berkey.

V. Miscellaneous Business.

C. W. Hall, *Secretary.*

NEW BOOKS.

Zur Psychologie des Schreibens. W. Preyer. Hamburg and Leipzig, Leopold Voss. 1895. Pp. 230. M. 8.

The Female Offender. Cæsar Lombroso and William Ferrero. New York, D. Appleton & Co. 1895. Pp. xx + 313. $1.50.

Story of the Innumerable Company. David Starr Jordan. Stanford Univ. Press. 1895. Pp. 38.

Short Studies in Nature Knowledge. William Gee. London and New York, Macmillan & Co. 1895. Pp. xiv + 313. $1.10

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(1) '*ON THE ELECTRIFICATION OF AIR.*'*

§ 1. CONTINUOUS observation of natural atmospheric electricity has given ample proof that cloudless air at moderate heights above the earth's surface, in all weathers, is electrified with very far from homogeneous distribution of electric density. Observing, at many times from May till September, 1859, with my portable electrometer on a flat open sea-beach of Brodick Bay in the Island of Arran, in ordinary fair weather at all hours of the day, I found the difference of potentials, between the earth and an insulated burning match at a height of 9 feet above it (2 feet from the uninsulated metal case of the instrument, held over the head of the observer), to vary from 200 to 400 Daniell's elements, or as we may now say volts, and often during light breezes from the east and northeast it went up to 3,000 or 4,000 volts. In that place, and in fair weather, I never found the potential other than positive (never negative, never even down to zero), if for brevity we call the earth's potential at the place zero. In perfectly clear weather under a sky sometimes cloudless, more generally somewhat clouded, I often observed the potential at the 9 feet height to vary from about 300 volts gradually to three or four times that amount, and gradually back again to nearly the same lower value in the course of about two minutes.* I inferred that these gradual variations must have been produced by

*Two communications by Lord Kelvin, P.R.S., to the Philosophical Society of Glasgow, meeting in the Natural Philosophy lecture-room of the University of Glasgow, March 27, 'On the Electrification of Air'; 'On the Thermal Conductivity of Rock at Different Temperatures.' Printed from proof sheets for *Nature* contributed by the author.

* 'Electrostatics and Magnetism,' Sir William Thomson. xvi. §§ 281, 282.

electrified masses of air moving past the place of observation. I did not remark then, but I now see, that the electricity in these moving masses of air must, in all probability, have been chiefly positive to cause the variations which I observed, as I shall explain to you a little later.

§ 2. Soon after that time a recording atmospheric electrometer * which I devised, to show by a photographic curve the continuous variation of electric potential at a fixed point, was established at the Kew Meteorological Observatory, and has been kept in regular action from the commencement of the year 1861 till the present time. It showed incessant variations quite of the same character, though not often as large, as those which I had observed on the sea-beach of Arran.

Through the kindness of the Astronomer Royal, I am able to place before you this evening the photographic curves for the year 1893, produced by a similar recording electrometer which has been in action for many years at the Royal Observatory, Geeenwich. They show, as you see, not infrequently, during several hours of the day or night, negative potential and rapid transitions from large positive to large negative. Those were certainly times of broken weather, with at least showers of rain, or snow, or hail. But throughout a very large proportion of the whole time the curve quite answers to the description of what I observed on the Arran sea-beach thirty-six years ago, except that the variations which it shows are not often of so large amount in proportion to the mean or to the minimums.

§ 3. Thinking over the subject now, we see that the gradual variations, minute after minute through so wide a range as the 3 or 4 to 1, which I frequently observed, and not infrequently rising to twenty times the ordinary minimum, must have been due to *positively* electrified masses of air, within a few hundred feet of the place of observation, wafted along with the gentle winds of 5 or 10 or 15 feet per second which were blowing at the time. If any comparably large quantities of negatively electrified air had been similarly carried past, it is quite certain that the minimum observed potential, instead of being in every case positive, would have been frequently large negative.

§ 4. Two fundamental questions in respect to the atmospheric electricity of fair weather force themselves on our attention:—(1) What is the cause of the prevalent positive potential in the air near the earth, the earth's potential being called zero? (2) How comes the lower air to be electrified to different electric densities whether positive or negative in different parts? Observations and laboratory experiments made within the last six or eight years, and particularly two remarkable discoveries made by Lenard, which I am going to describe to you, have contributed largely to answering the second of these questions.

§ 5. In an article 'On the Electrification of Air by a Water-jet,' by Magnus Maclean and Makita Goto,* experiments were described showing air to be negatively electrified by a jet of water shot vertically down through it from a fine nozzle into a basin of water about 60 centimeters below it. It seemed natural to suppose that the observed electrification was produced by the rush of the fine drops through the air; but Lenard conclusively proved, by elaborate and searching experiments, that it was in reality due chiefly, if not wholly, to the violent commotions of the drops impinging on the water surface of the receiving basin, and he found that the negative electrification of the air was greater when they were allowed to fall on a hard slab of any material thoroughly wetted by water than when they fell on a yielding surface of water several

* 'Electrostatics and Magnetism.'

**Philosophical Magazine*, 1890, second half-year.

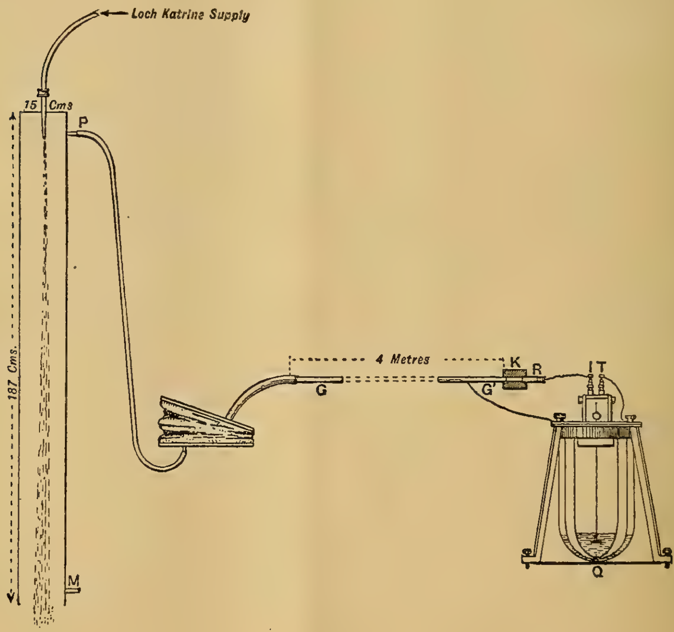
centimeters deep. He had been engaged in studying the great negative potential which had been found in air in the neighborhood of waterfalls, and which had generally been attributed to the inductive action of the ordinary fine weather electric force, giving negative electricity to each drop of water-spray before it breaks away from conducting communication with the earth. Before he knew Maclean and Goto's paper, he had found strong reason for believing that that theory was not correct, and that the true explanation of the electrification of the air must be found in some physical action not hitherto discovered. A less thorough inquirer might have been satisfied with the simple explanation of the electricity of waterfalls naturally suggested by Maclean and Goto's result, and might have rested in the belief that it was due to an electrifying effect produced by the rush of the broken water through the air; but Lenard made an independent experimental investigation in the Physical Laboratories of Heidelberg and Bonn, by which he learned that the seat of the negative electrification of the air electrified is the lacerated water at the foot of the fall, or at any rocks against which the water impinges, and not the multitudinous interfaces between air and water falling freely in in drops through it.

§ 6. It still seems worthy of searching inquiry to find electrification of air by water falling in drops through it, even though we now know that if there is any such electrification it is not the main cause of the great negative electrification of air which has been found in the neighborhood of waterfalls. For this purpose an experiment has been very recently made by Mr. Maclean, Mr. Galt and myself, in the course of an investigation regarding electrification and diselectrification of air with which we have been occupied for more than a year. The apparatus which we used is before you. It consists of a quadrant electrometer connected with an insulated electric filter* applied to test the electrification of air drawn from different parts of a tinned iron funnel, 187 centimeters long and 15 centimeters diameter, fixed in a vertical position with its lower end open and its upper end closed, except a glass nozzle, of 1.6 mm. aperture admitting a jet of Glasgow supply water (from Loch Katrine) shot vertically down along its axis. The electric filter (R in the drawing), a simplified and improved form of that described in the *Proceedings* of the Royal Society for March 21, consists of twelve circles of fine wire gauze rammed as close as possible together in the middle of a piece of block tin pipe of 1 cm. bore and 2 cm. length. One end of it is stuck into one end of a perforation through a block of paraffin, K, which supports it. The other end (G) of this perforation is connected by block tin pipe (which in the apparatus actually employed was 4¾ meters long, but might have been shorter), and india-rubber tubing through bellows to one or other of two short outlet pipes (M and P) projecting from the large funnel.

§ 7. We first applied the india-rubber pipe to draw air from the funnel at the *upper* outlet, P, and made many experiments to test the electricity given by it to the receiving filter, R, under various conditions as to the water-jet; the bellows being worked as uniformly as the operator could. When the water fell fairly through the funnel with no drops striking it, and through 90 cm. of free air below its mouth, a small negative electrification of R was in every case observed (which we thought might possibly be attributable to electrification of air where the water was caught in a basin about 90 cm. below the mouth of the funnel). But when the funnel was slanted so that the whole shower of drops from the jet, or even a small part of it, struck

* Kelvin, Maclean, Galt, 'On the Diselectrification of Air.' *Proc.* Roy. Soc., March 14, 1895.

the inside of the funnel the negative electrification of R was largely increased. So it was also when the shower, after falling freely down the middle of the funnel, impinged on a metal plate in metallic communication with the funnel, held close under its mouth, or 10 or 20 cm. below it. For example, in a series of experiments made last Monday (March 25), we found .28 of a volt in 15 minutes with no obstruction to the shower; and 4.18 volts in five minutes, with a metal plate held three or four centimeters below the mouth of the funnel; the air being drawn from the upper outlet (P). Immediately after, with P closed, and air drawn from the lower outlet (M), but all other circumstances the same, we found .20 of a volt in five minutes with no obstruction; and 6.78 volts in five minutes with the metal plate held below the mouth as before.



§ 8. These results, and others which we have found, with many variations of detail, confirm, by direct test of air drawn away from the neighborhood of the waterfall through a narrow pipe to a distant electrometer, Lenard's conclusion that a preponderatingly strong negative electrification is given to the air at every place of violent impact of a drop against a water-surface or against a wet solid. But they do not prove that there is *no* electrification of air by drops of water falling through it. We always found, in every trial, decisive proof of negative electrification; though of comparatively small amount when there was no obstruction to the shower between the mouth of the funnel and the catching basin 90 cm. below it. We intend to continue the investigation, with the shower falling freely far enough down from the mouth of the funnel to make quite sure that the air which we draw off from any part of the funnel is not sensibly affected by impact of the drops on anything below.

§ 9. The other discovery * of Lenard, of which I told you, is that the negative electrification of air, in his experiments with pure water, is diminished greatly by very small quantities of common salt dissolved in it, that is brought to nothing by .011 per cent.; that positive electrification is produced in the air when there is more than .011 per cent. of salt in the water, reaching a maximum with about 5 per cent. of salt, when the positive electrical effect is about equal to the negative effect observed with pure water, and falling to 14 per cent. of this amount when there is 25 per cent. of salt in the solution. Hence sea-water, containing as it does, about 3 per cent. of common salt, may be expected to give almost as strong positive electrification to air as pure water would give of negative in similar circumstances as to commotion. Lenard infers that breaking waves of the sea must give positive electricity to the air over them; he finds, in fact, a recorded observation by Exner, on the coast of Ceylon, showing the normal positive electric potential of the air to be notably increased by a storm at sea. I believe Lenard's discovery fully explains also some very interesting observations of atmospheric electricity of my own, which I described in a letter to Dr. Joule, which he published in the *Proceedings* of the Literary and Philosophical Society of Manchester for October 18, 1859. * "The atmospheric effect ranged from 30° to about 420° [of a heterostatic torsion electrometer of 'the divided-ring' species] during the four days which I had to test it; that is to say, the electrometric force per foot of air, measured horizontally from the side of the house, was from 9 to above 126 zinc-copper water cells. The weather was almost perfectly settled, either calm, or with slight east wind, and in general an easterly haze in the air. The electrometer twice within half an hour went above 420°, there being at the time a fresh temporary breeze from the east. What I had previously observed regarding the effect of east wind was amply confirmed. Invariably the electrometer showed very high positive in fine weather, before and during east wind. It generally rose very much shortly before a slight puff of wind from that quarter, and continued high till the breeze would begin to abate. I never once observed the electrometer going up unusually high during fair weather without east wind following immediately. One evening in August I did not perceive the east wind at all, when warned by the electrometer to expect it; but I took the precaution of bringing my boat up to a safe part of the beach, and immediately found by waves coming in that the wind must be blowing a short distance out at sea, although it did not get so far as the shore . . . . On two different mornings the ratio of the house to a station about sixty yards distant on the road beside the sea was .97 and ·96 respectively. On the afternoon of the 11th inst, during a fresh temporary breeze of east wind, blowing up a little spray as far as the road station, most of which would fall short of the house, the ratio was 1.08 in favour of the house electrometer—both standing at the time very

* 'Ueber die Electricität der Wasserfälle.' Table xvii. p. 228. *Annalen der Physik und Chemie*, 1892, vol. xlvi.

* Republished in 'Electrostatics and Magnetism.' 'Atmospheric Electricity,' xvi. § 262.

high—the house about 350°. I have little doubt but that this was owing to the negative electricity carried by the spray from the sea, which would diminish relatively the indications of the road electrometer."

§ 10. The negative electricity spoken of in this last sentence, 'as carried by the spray from the sea,' was certainly due to the inductive effect of the ordinary electrostatic force in the air close above the water, by which every drop or splash breaking away from the surface must become negatively electrified; but this only partially explains the difference which I observed between the road station and the house station. We now know, by the second of Lenard's two discoveries, to which I have alluded, that every drop of the salt water spray, falling on the ground or rocks wetted by it, must have given positive electricity to the adjoining air. The air, thus positively electrified, was carried towards and over the house by the on-shore east wind which was blowing. Thus, while the road electrometer under the spray showed less electrostatic force than would have been found in the air over it and above the spray, the house electrometer showed greater electrostatic force because of the positively electrified air blowing over the house from the wet ground struck by the spray.

§ 11. The strong positive electricity, which as described in my letter to Joule, I always found in Arran with east wind, seemed at first to be an attribute of wind from that quarter. But I soon found that in other localities east wind did not give any very notable augmentation, nor perhaps any augmentation at all, of the ordinary fair weather positive electric force, and for a long time I have had the impression that what I observed in this respect, on the sea-beach of Brodick Bay in Arran, was really due to the twelve nautical miles of sea between it and the Ayrshire coast, east-north-east of it; and now it seems to me more probable than ever that this is the explanation when we know from Lenard that the countless breaking waves, such as even a gentle east wind produces over the sea between Ardrossan and Brodick, must every one of them give some positive electricity to the air wherever a spherule of spray falls upon unbroken water. It becomes now a more and more interesting subject for observation (which I hope may be taken up by naturalists having the opportunity) to find whether or not the ordinary fine weather positive electric force at the sea coast in various localities is increased by gentle or by strong winds from the sea, whether north, south, east or west of the land.

§ 12. From Lenard's investigation we now know that every drop of rain falling on the ground or on the sea,* and every drop of fresh water spray of a breaking wave, falling on a fresh water lake, sends negative electricity from the water surface to the air; and we know that every drop of salt water, falling on the sea from breaking waves, sends positive electricity into the air from the water surface. Lenard remarks that more than two-thirds of the earth's surface is sea, and suggests that breaking sea-waves may give contributions of positive electricity to the air which may possibly preponderate over the negative electricity given to it from other sources, and may thus be the determining cause of the normal fair weather positive of natural atmospheric electricity. It seems to me highly probable that this preponderance is real for atmospheric electricity at sea. In average weather, all the year round, sailors in very small vessels are more wet by sea-spray than by rain, and I think it almost certain that more positive electricity is given to the air by breaking waves than negative elec-

* 'Ueber die Electricität der Wasserfälle.' *Annalen der Physik und Chemie*, 1892, vol. xlvi., p. 631.

tricity by rain. It seems also probable that the positive electricity from the waves is much more carried up by strong winds to considerable heights above the sea than the negative electricity given to the air by rain falling on the sea; the greater part of which may be quickly lost into the sea, and but a small part carried up to great heights. But it seems to me almost certain that the exceedingly rapid recovery of the normal fair weather positive, after the smaller positive or the negative atmospheric electricity of broken weather, which was first found by Beccaria in Italy 120 years ago, and which has been amply verified in Scotland and England, *could not be accounted for by positively electrified air coming from the sea. Even at Beccaria's Observatory, at Garzegna di Mondovi in Piedmont, or at Kew or Greenwich or Glasgow, we should often have to wait a very long time for reinstatement of the normal positive after broken weather, if it could only come in virtue of positively electrified air blowing over the place from the sea; and several days, at least, would have to pass before this result could possibly be obtained in the centre of Europe.

§ 13. It has indeed always seemed to me probable that the rain itself is the real restorer of the normal fair weather positive. Rain or snow, condensing out of the air high up in the clouds, must itself, I believe, become positively electrified as it grows, and must leave positive electricity in the air from which it falls. Thus rain falling from negatively electrified air would leave it less negatively electrified, or non-electrified or positively electrified; rain falling from non-electrified air would leave it positively electrified; and rain falling from positively electrified air would leave it with more of positive electricity than it had before it lost water from its composition. Several times within the last thirty years I have made imperfect and unsuccessful attempts to verify this hypothesis by laboratory experiments, and it still remains unproved. But I am much interested just now to find some degree of observational confirmation of it in Elster and Geitel's large and careful investigation of the electricity produced in an insulated basin by rain or snow falling into it, which they described in a communication published in the *Sitzungsberichte* of the Vienna Academy of Sciences, of May, 1890. They find generally a large electrical effect, whether positive or negative, by rain or snow falling into the basin for even so short a time as a quarter of a minute, with however, on a whole, a preponderance of negative electrification.

* 'Electrostatics and Magneticism,' XVI., § 287.

§ 14. But my subject this evening is not merely natural atmospheric electricity, although this is certainly by far the most interesting to mankind of all hitherto known effects of the electrification of air. I shall conclude by telling you very briefly, and without detail, something of new experimental results regarding electrification and diselectrification of air, found within the last few months in our laboratory here by Mr. Maclean, Mr. Galt and myself. We hope before the end of the present session of the Royal Society to be able to communicate a sufficiently full account of our work.

§ 15. Air blown from an uninsulated tube, so as to rise in bubbles through pure water in an uninsulated vessel, and carried through an insulated pipe to the electric receiving filter, of which I have already told you, gives negative electricity to the filter. With a small quantity of salt dissolved in the water, or sea water substituted for fresh water, it gives positive electricity to the air. There can be no doubt but these results are due to the same physical cause as Lenard's negative and positive electrification of air by the impact of drops of fresh water or of salt water on a surface of water or wet solid.

§ 16. A small quantity of fresh water or salt water shaken up vehemently with air in a corked bottle electrifies the air, fresh water negatively, salt water positively. A 'Winchester quart' bottle (of which the cubic contents is about two litres and a half), with one-fourth of a litre of fresh or salt water poured into it, and closed by an india-rubber cork, serves very well for the experiment. After shaking it vehemently till the whole water is filled with fine bubbles of air, we leave it till all the bubbles have risen and the liquid is at rest, then take out the cork, put in a metal or india-rubber pipe, and by double-acting bellows draw off the air and send it through the electric filter. We find the electric effect, negative or positive, according as the water is fresh or salt, shown very decidedly by the quadrant electrometer; and this, even if we have kept the bottle corked for two or three minutes after the liquid has come to rest before we take out the cork and draw off the air.

§ 17. An insulated spirit lamp or hydrogen lamp being connected with the positive or with the negative terminal of a little Voss electric machine, its fumes (products of combustion mixed with air) sent through a block-tin pipe, four meters long, and one centimeter bore, ending with a short insulating tunnel of paraffin and the electric filter, gives strong positive or strong negative electricity to the filter.

§ 18. Using the little biscuit-canister and electrified needle, as described in our 'communication' * to the Royal Society 'On the Diselectrification of Air,' but altered to have two insulated needles with varied distances of from a half a centimeter to two or three centimeters between them, we find that when the two needles are kept at equal differences of potential positive and negative, from the enclosing metal canister, little or no electrification is shown by the electric filter; and when the differences of potential from the surrounding metal are unequal, electrification, of the same sign as that of the needle whose difference of potential is the greater, is found on the filter.

When a ball and needle-point are used, the effect found depends chiefly on the difference of potentials between the needle-point and the surrounding canister, and is comparatively little affected by opposite electrification of the ball. When two balls are used, and sparks in abundance pass between them, but little electricity is deposited by the sparks in the air, even when one of the balls is kept at the same potential as the surrounding metal. [The communication was illustrated by a repetition of some of the experiments shown on the occasion of a Friday evening lecture * on Atmospheric Electricity at the Royal Institution on May 18, 1860, in which one-half of the air of the lecture-room was electrified positively, and the other half negatively, by two insulated spirit lamps mounted on the positive and negative conductors of an electric machine.]

* *Proceedings* of the Royal Society, March 14, 1895.

* 'Electrostatics and Magnetism,' xvi., §§ 285, 286.

(2) '*ON THE THERMAL CONDUCTIVITY OF ROCK AT DIFFERENT TEMPERATURES.*'

EXPERIMENTS by Lord Kelvin and Mr. Erskine Murray were described, and the apparatus used in them was shown, by which it was found that the thermal conductivity of specimens of slate, sandstone and granite is less at higher temperatures than at lower for each of these rocks. The last tested was Aberdeen granite, for which experiments of fairly satisfactory accuracy showed the mean conductivity for the range from 146° C. to 215° C. to be 86 per cent. of the mean conductivity in the range from 81° C. to 146° C. They hope to send a communication to the Royal Society describing their work before the end of the present session.

A DYNAMICAL HYPOTHESIS OF INHERITANCE. †*

THE doctrine of the preformation of an organism in the germ is as inconsistent with the fact as with the requirements of dynamical theory. The effects of the preconceptions of preformationism have been only too apparent in framing hypotheses of inheritance. The now dominant hypothesis is simply an amplification, in the light of numerous modern facts, of the preformationism of Democritus. He supposed that almost infinitesimally small and very numerous bodies were brought together in the germ from all parts of the body of the parent. These minute representative corpuscles were supposed to have power to grow, or germinate, at the right time, and in the right order, into the forms of the parts and organs of the new being. In this way it was supposed that the characteristics of the parent were represented in a latent form in the germ, which might grow as a whole, by the simultaneous and successive development of the germinal aggregate composed, so to speak, of excessively minute buds, or rudiments of the organs. In such wise also did the successors of Democritus, namely, Aristotle, Buffon, and Erasmus Darwin, suppose that the inheritance of parental likeness by offspring was to be explained. The later and greater Darwin greatly amplified this hypothesis and proposed, provisionally, to account for the phenomena of inheritance by its help. Conceiving the process somewhat as above supposed, he consistently gave to his provisional hypothesis the name of *pangenesis*, since the minute latent buds of the germ were supposed to come from, and thus represent potentially every part of the bodies of the parents, and possibly of still remoter ancestry.

With the discovery of the presence of germinal substance in multicellular organisms, from the embryonic stages onwards, by Owen, Galton, Jäger, Nussbaum and others, the theory of continuity of germinal matter came into vogue. Upon this basis Weismann distinguished two kinds of plasma in multicellular beings, namely, the germ-plasm and the body-plasm, and at first assumed that because of this separation the latter could not modify the former, since the fate of the respective sorts of plasma was predetermined by virtue of this separation. The one kind was the mere carrier of the other, and the germ-plasm was immortal because it was produced in each species from a store of it which always existed, either in a latent or palpable form, from the very beginning of development. He seems, however, in recent years, to have admitted that this germ-plasm could be indirectly modified in constitution through the influence of the body-plasm that bore and enclosed it. Beyond this point Weismann again becomes a preformationist, as truly as Democritus, in that he now conjectures that the supposed innumerable latent buds of the germ, representative of the organs of the future being, are minute masses which he sees as objective realities in the chromosomes of the nuclei of the sex-cells. These chromosomes of the germ he calls 'ids' and 'idants,' according to their condition of sub-division, and supposes them to grow and become divided into 'determinants' and 'biophors' in the course of embryonic development. To these he ascribes powers little short of miraculous, in that he asserts that these infinitesimal germinal particles grow and divide just at the right time and order, and control de-

* From 'The Biological Lectures' of the Marine Biological Laboratory, Vol. III., 1895. Printed from the proofs by the courtesy of the editor, Professor Whitman.

† It is interesting to note that the views developed in this lecture lead to conclusions in some respects similar to those held by Professor Whitman in his discourse entitled: 'The Insufficiency of the Cell-Theory of Development,' published in the series of lectures delivered in 1893.

volopment so as to build up anew the arrangement of parts seen in the parent type. This elaborate system of preformationism is bound to produce a reaction that is already becoming apparent; in fact, it is probable that its very complexity, its many inconsistencies, as well as the numerous subsidiary hypotheses that must be worked out to support it, will be fatal to it as a system.

The path along which the solution of the problem of heredity is to be effected lies in a wholly different direction, namely, in that of the study of the mechanics and dynamics of development, and in the resolute refusal to acknowledge the existence of anything in the nature of preformed organs or of infinitesimal gemmules of any kind whatsoever. Such devices are unnecessary and a hindrance to real progress in the solution of the questions of inheritance. They only serve to divert the attention of the observer from the real phenomena in their totality to a series of subordinate details, as has happened in Weismann's case. All this while he has been watching the results of an epigenetic process, as displayed by an inconceivably complex mechanism in continuous transformation, and out of all of this the most essential thing he has witnessed has been one of the *effects* of the operation of that contrivance in the mere splitting of chromosomes that are his 'ids,' 'idants,' 'biophors,' etc. The potentiality of the part has been mistaken for that of the whole.

We must dismiss from our minds all imaginary corpuscles as bearers of hereditary powers, except the actual chemical metameric or polymeric molecules of living matter, as built up into ultramicroscopic structures, if we wish to frame an hypothesis of heredity that is in accord with the requirements of dynamical theory. The 'discovering' and naming of 'ids,' 'biophors' and 'pangenes' time will show to have been about as profitable as sorting snowflakes with a hot spoon. We must also dismiss the idea that the powers of development are concentrated in some particular part of the germ-cell, nor can we assume the latter to be homogenous.* This we are compelled to deny on the ground of the organization of the egg itself. Nor is it possible to deny the reciprocal effects of cells upon each other; the parts are reciprocals of the whole, as the latter is reciprocal to a part. The organism during every phase of its existence is a molecular mechanism of inconceivable complexity, the sole motive force of which is the energy that may be set free by the coördinated transformation of some of its molecules by metabolism. An appeal to anything beyond this and the successive configurations of the molecular system of the germ, as a whole, resulting from the changing dynamical properties of its molecules, as their individual configurations and arrangement change, must end in disappointment. We must either accept such a conclusion or deny that the principle of the conservation of force holds in respect to the behavior of the ultimate molecular constituents of living substance. But to deny that that principle is operative in living creatures is to question direct experimental evidence to the contrary, since Rübner has been able to actually use an organism as a fairly accurate calorimeter.

The initial configuration or mechanical arrangement and successive rearrangements of the molecules of a germ, the addition of

*The writer finds himself unable to agree with Haacke, if he has properly understood that author's assumption as to the homogeneity or monotonous character of living matter, as set forth in his admirable work *Gestaltung und Vererbung*, 1893. Nor does it appear that anything is gained by the acceptance of Haacke's theory of Gemmaria that is not easily understood upon the far simpler grounds that will be set forth here, though there is much in the book cited with which epigenesists must agree, aside from the weighty character of its criticisms and its pregnant suggestiveness.

new ones by means of growth, plus their chemical and formal transformation as an architecturally self-adjusted aggregate, by means of metabolism, is all that is required in an hypothesis of inheritance. The other properties of living matter, such as its viscosity, free and interfacial surface-tension, osmotic properties, its limit of saturation with water, its segmentation into cells, in short, its organization, must be the result of the operation of forces liberated by its own substance during its growth by means of metabolism. We cannot exclude external forces and influences, such as chemism, light, heat, electricity, gravity, adhesion, exosmosis, food, water, air, motion, etc., in the operation of such a complex mechanism. It is these agencies that are the operators of the living mechanism, which in its turn makes certain successive responses in a way that is determined within limits by its own antecedent physical structure and consequent dynamical properties. The parts of the whole apparatus are kept in a condition of continuous 'moving equilibrium' by external agencies, to borrow a phrase of Mr. Spencer's.

This view, it will be seen, leads to a determinism as absolute as that of the Neo-Darwinists, but upon a wholly different basis. It leads to the denial of the direct mutability of the germ by any means other than the transformation, chemical and structural, through metabolism of the germinal mechanism. It not only compels us to deny that the germ can be at once so effected by external blows as to transmit changes thus produced hereditarily except under exceptional conditions, as we shall see later. It denies also, by implication, that the cytoplasm can be so modified, except indirectly, or through architectural transformations of its ultramicroscopic structure.

It is also compelled to deny that spontaneous or autogenous characters can either arise or be transmitted without involving the principle of the conservation or correlation of force, since no transformation of such a mechanism can take place without involving forces directly or indirectly exerted by the external world. In short, the energy displayed by a living molecular system from within must be affected by energies coming upon it from without. All characters whatsoever were so acquired, so that the truth is that there are no others to be considered. Characters acquired through the interaction of inner and outer forces are the only ones possible of acquirement.

That through reciprocal integration (fertilization and formation of an oösperm) this rule may have apparent exceptions, through the compounding of two molecular mechanisms of different strengths, dynamically considered, it is impossible to deny in the face of the evidence of breeders. Such exceptions are apparent, however, and not real, as must follow from dynamical theory.

The sorting process, called natural selection, is itself dynamic, and simply expresses the fact that, by an actual operation with a living body of a certain kind, something more than a balancing of forces is involved between internal and external energies whenever a survival occurs. The principles of dynamics therefore apply in all strictness to natural selection.

What it is that makes crosses or hybrids more variable and often more vigorous than inbred forms must also have a dynamic explanation, since there can be no increased activity of metabolic processes without an increased expenditure of energy and an increased rate of molecular transformation.

Variations cannot be spontaneous, as Darwin himself was aware. The only way in which they can be supposed to have arisen is by the blending of molecular dynamical systems of differing initial potential strengths, by the conjugation of sex-cells (reciprocal integration), and by means of variations in the interactions of such resultant systems with their surroundings. This, however, Weismann and his followers deny, though no proof whatever has been offered that such is not the fact. Indeed, it is probable that, so long as the ultimate machinery of metabolism is beyond the reach of ocular demonstration, there can be no proof or disproof of the position assumed by the performationists or Neo-Darwinists. Such proof or disproof is, however, non-essential, since we are forbidden by the first principles of dynamics to assume that transformation of any living physical system whatever can occur without involving some forces or influences that emanate from the external world.* The separation and evaluation of the internal and external forces incident to the manifestation of life, in the present state of our knowledge, and from the very nature of the case, plainly transcends the capacity of present available experimental methods in biology. The discussion as to whether 'acquired characters' are inherited can, therefore, have but one outcome, since external forces can never be excluded in considering the life-history of any organism.

Nägeli, in seeking to account for the phenomena of growth, gave us a most ingenious physical hypothesis of the constitution of living matter. This, later on, he modified so as to develop an hypothesis of hereditary transmission. But the micellæ that were representative of the germinal matter of a species he isolated in the form of rows or chains of micellæ traversing the rest of the living substance of the organism, and called it *idioplasm*. Here again the germinal matter was conceived as separate from that forming the rest of the body. Mr. Spencer supposed "that sperm-cells and germ-cells are essentially nothing more than vehicles in which are contained small groups of the physiological units in a fit state for obeying their proclivity towards the structural arrangement of the species they belong to." These 'physiological units' are neither chemical nor morphological in character, according to Mr. Spencer's system, but it is admitted that their properties and powers must be determined in some way by their own constitution, conditions of aggregation, and relation to the outer world. The views of Nägeli and Spencer are akin in certain respects, but they still retain a certain amount of resemblance to the older ones, namely, those hypotheses which assume that the forces of inheritance are lodged in certain very small corpuscles forming part only of the germ or organism. These hypotheses are also dynamical in nature, and have been worked out with the consciousness, in both cases, that the mechanism of inheritance must also be the one through which metabolism operates. Indeed, these two authors seem to be the first to have distinctly recognized the necessity for such a supposition.

Later still, with the advent of the discovery that the male nucleus was fused with the female nucleus during sexual reproduction, it was assumed that the nuclear contents were the only essential material bearers of those hereditary forces that shape the growing germ into the likeness of the parentage. With the development of this idea the name of Weismann is perhaps most closely associated. He has utilized the facts of development, nuclear cleavage, expulsion of polar bodies, halving and subdivision of chromosomes, etc., as the founda-

*"Some of the exponents of this [preformation] theory of heredity have attempted to elude the difficulty of placing a whole world of wonders within a body so small and so devoid of structure as a germ, by using the phrase structureless germs (F. Galton, Blood-relationship, *Proc. Roy. Soc.*, 1872). Now one material system can differ from another only in the configuration and motion which it has at a given instant. To explain differences of function and development of a germ without assuming differences of structure is, therefore, to admit that the properties of a germ are not those of a purely material system."—JAMES CLERK-MAXWELL, article Atom, *Encycl. Britan.*, 9th ed., Vol. III., p. 42, 1878.

tion of his hypothesis of inheritance. Its extreme elaboration is its greatest weakness, and in it, no less than in all preceding hypotheses, the theory of a separate category of particles carrying hereditary potentialities again appears.

The one criticism that holds of all these hypotheses is that they are one-sided and ignore a most important set of factors in inheritance, namely, the purely statical ones, or those arising from the mere physical properties of the living matter of the germ viewed as if it were a dead, inert mass, subject to the operation of the reciprocal attraction for one another of its constituent particles. All of these hypotheses, moreover, assume that it is only *some* of the matter of the germ that is concerned in the process of hereditary transmission, and that the remainder may be regarded as passive. The entire germ, on the contrary, or all of it that undergoes development, must be considered as a single whole, made up of a vast number of molecules built up into a mechanism. Such a molecular mechanism, it must be supposed, cannot set free the potential energy of its parts except in a certain determinate order and way, within certain limits, in virtue of the initial physical structure of the whole. If the germ is free to do that, as must happen under proper conditions, as a mechanism, its parts, as they are thus formed by their own metabolism, it may be assumed, will inevitably and nearly recapitulate the ancestral development or that typical of the species. It must do this as a mere dynamical system or mechanism, the condition of which at one phase determines that of the next, and so on, to the completion of development.

In the present state of our knowledge we are not prepared to frame a purely mechanical hypothesis of inheritance that shall answer every requirement, in spite of the fact that no other is possible. Herbert Spencer and Professor Haeckel long ago pointed out that such an hypothesis is a necessity growing out of the very requirements that must be satisfied in any attempt to coördinate the phenomena of biology with those of the not-living world. The material basis of life is always a chemically and mechanically compounded substance. To the very last molecule, such a body must betray evidence of arrangement or structure of its parts that should make it a mechanism of the utmost complexity and requisite potentiality as a transformer of energy through the mere transposition and rearrangement of such parts. We find indeed that living matter is chemically the most complex and unstable substance known. It is composed largely of carbon, a quadrivalent element that stands alone in its power to combine with itself and at the same time hold in chemical bondage groups of atoms representing other chemical bodies. Such groups are probably held together in great numbers metamerically by the reciprocal or otherwise unsatisfied affinities of the large number of carbon atoms entering into the composition of the proteid molecule. In this way the massive and structurally complex molecule of protoplasm may be supposed to have arisen. We may thus trace the genesis of the peculiarities of living matter to this singular property of the carbon atom. On such a basis we may suppose that the ultimate molecular units are identical with the physiological units, so that their structures may not only determine the nature of the metabolism they can undergo, but also be the ultimate units of form or morphological character.

What especially gives color to these suspicions is the extraordinary variety of changes, alteration of properties or powers, and the vast variety of living matter, as represented by the million or more of known distinct living species of organisms. It is as if the permutations, transformations, and the dynamical readjustment of the metameres of the molecules of living matter were the source of its varying potentialities as manifested in its protean changes of specific form and function. That some mechanical and consequently dynamical interpretation of these transformations may yet be forthcoming is, I take it, distinctly foreshadowed by the advances in the newer theories of stereo-chemistry developed by LeBel and Van't Hoff. If this is the case we may yet hope for a mechanical and dynamical explanation of the phenomena of life and inheritance. Especially is this true if we further suppose that the large molecules of living plasma are rather feebly held together by a force almost of the nature of cohesion. We may be permitted thus to find an explanation of that phenomenon which is always so characteristic of living matter, namely, the large and relatively fixed amount of water it contains, and also the mobility of its molecules in respect to one another, its jelly-like character at one instant, its fluidity and power of motion at another. It is indeed probable that the amount of water contained in living matter is controlled within certain limits by the forces of cohesion exerted between adjacent molecules against the osmotic pressure or capillary action of water tending to drive them asunder, as supposed by Nägeli, in his hypothesis of micellæ. Such an hypothesis enables us to explain much that is otherwise quite unintelligible in relation to living things. It renders us an explanation of amœboid motion, of the surface tensions of protoplasm and lastly of metabolism itself through osmosis and the specific characters of the chemical transformations that must take place in each kind of living substance.

Such an hypothesis may also afford us mechanical constructions of atoms, grouped into very large metameric or polymeric molecules of the utmost diversity of powers, capable of undergoing a long series of successive transformations, so as to manifest in the long run, starting with a molecular germinal aggregate, what we call ontogeny or development. These transformations, we must suppose, are effected by the metabolism incident to growth, and moreover, that starting with an initial configuration of a system of molecules, as a mechanical and consequently a dynamical system of determinate powers, in the form of a germ, it cannot undergo any other transformations except such as lead to an approximate recapitulation of the ancestral development or phylogeny. This supposition follows from the rule that must hold of determinate systems of molecules, as well as of systems formed of larger masses, namely, that the initial changes in the configuration of such a complex system must dynamically determine within certain variable limits, under changing conditions, the nature of all of its subsequent transformations, including those due to growth and consequently increased complexity. We thus escape the necessity of invoking certain 'proclivities' of physiological units, or the necessity of appealing to the growth and fission of 'biophors' or the scattering of 'determinants' at the proper times and places in the course of development. We thus escape, too, the mistake of assuming that a part of a germ controls the whole, a proposition that has been so long advocated by one school of biologists that it is astounding that its fallacy has not long since been more generally understood. Such a doctrine is not credible in the face of the fact that we know of no development except that which takes place in intimate association with cytoplasm, which seems to be the principal theater of metabolism and growth. We cannot conceive of the transformations of a germ without considering the metabolism of all its parts, such as nucleus, cytoplasm, centrosomes, archoplasm, chromatin, spindles, astral figures, microsomata, etc.

'Tendencies' and 'proclivities' are words that have no legitimate place in the discussion of the data of biology any more than they have in natural philosophy or physics. Karyokinesis, now admittedly inseparable in thought from the idea of multicellular development, is a rhythmical process so complex in its dynamical aspects as to some extent lead one unwittingly to underestimate the absolute continuity of the accompanying processes of metabolism. But that is no reason why the importance of nuclear metamorphosis should be exaggerated at the expense of the far more important forces developed by metabolism and growth. In fact, the 'ids,' 'idants,' etc., of that school of biologists are not causes but mere effects, produced as passing shadows, so to speak, in the operation of the perfectly continuous processes of metabolism incident to development. Reciprocal relations are sustained between nucleus and cytoplasm of such importance that the transformation or fission of the one is impossible without the other.

The so-called 'reducing divisions' probably have nothing but a passing and purely adaptive physiological significance in every ontogeny of ova and sperms. The far-fetched and extraordinary teleological significance given by some to the reducing divisions would lead one to suppose that the clairvoyant wisdom of the original egg that thus first threw out the excess of its ancestral 'germ-plasm' in order to save its posterity from harm through the fatality of reversion thus entailed was greater than anything human, if not god-like. The complete parallelism of the 'reducing division' in the sperm and egg has never been established. The comparison of these processes in the two is still only approximate, because in the truly holoblastic egg there is, in some cases, an apparent temporary substitution of the male nucleus for the female, as is shown by the former's assuming a position of equilibrium at the center of the ovum (*Ascaris*), a condition of things that does not and could not occur in the sperm cell.

A still more important contrast is the almost incredible difference of volume of the two kinds of sex-cells of the same species. In man the ratio of volume of the male cell to the female cell is as 1 to 3,000 approximately. This extreme contrast of volume is associated with corresponding contrasts in their properties. There can hardly be any doubt that the mature male cell is in a nearly potential or static state of metabolic transformation of its substance, and is characterized by an almost complete want of stored metabolizable reserve material. The egg is in a similar static state, but, on the other hand, contrasts with the male element in that the development of a more or less voluminous mass of reserve material within it has seemingly been also associated with its loss, as a rule, of the power to begin an independent development. The power of the male cell to begin its transformation and growth through metabolism appears to be arrested until it finds the material in which its mere presence will set up transformations. This it must do by in some way setting free and diffusing some of its own molecules osmotically and mechanically through the egg. The substance of the egg appears therefore to be complementary to that of the spermatozoön. The power to set up transformations within the egg leading to the development of a new being is not manifested aside from the presence of the male element except in cases of parthenogenesis. Even the expulsion of the polar cells is not initiated until the stimulus of the presence of the male element is experienced by the egg.

Another contrast is found in the times of the advent of the 'reducing division' in the two kinds of sex-cells. In the male cell the 'reducing division' occurs earliest, or while it is still in more or less close nutritive relation to the parent; in the egg the 'reducing division' or expulsion of polar cells does not occur till the egg is freed, as a rule, from the parent gonad, and generally as a consequence of the stimulating effect of the presence of the male cell. These differences of behavior of the two sorts of sex-cells seem to be correlated with their differences in size.

We may contemplate the sex-cells as molecular mechanisms which, in virtue of their mechanical structure, are rendered capable of controlling the order and manner of rearrangement of their constituent molecules, because of the new successive attractions and repulsions set free, amongst the latter, immediately upon the completion of conjugation. The new forms of metabolism thus initiated enable us to conceive a mechanical theory of fertilization. At any rate, the two sorts of sex-cells are potentially the reciprocals of each other, and their initial or *statical* states cannot begin to set free their energy and thus pass into the successive kinetic states of formal change until the two mechanisms are reciprocally and mechanically integrated into a single one by means of conjugation. The parts of this new single body now act in unison. Even the manner in which the two conjoined molecular mechanisms operate can actually be to some extent traced, as expressed in the complex movements associated with fertilization, the division of the chromosomes and centrosomes. The effect of conjugation is to afford opportunity also for new and various combinations of molecular mechanisms, though the reciprocal integration of pairs of cells having a widely different parentage.

The great size of the egg-cell provides an extensive reserve material that enables the embryo thus built up usually to reach a relatively great size without entering for a time into competition for food in the struggle for existence. Sexuality is therefore altruistic in nature, since it has led in both plants and animals to the evolution of a condition of endowment, or the storage of potential energy in the germ, so that the latter is the better able to cope with natural conditions. While it may be assumed that sexuality has arisen, in the main, under conditions determined by natural selection, once sexuality was attained, the added power thus accumulated potentially in large germs of double origin enabled the latter the more easily to overcome untoward natural conditions. Natural selection thus becomes altruistic or dotational in that it tends through sexuality to defeat the deadliness of the struggle for existence, just as we may also assert that the theory of superposition to which the mechanical theory of development is committed is also finally altruistic. It may be remarked that the greatest mortality of a species, under the conditions of the struggle for existence, also takes place in the egg and embryonic stages, or before organisms can experience acute pain; so that here again we have a result that must materially ameliorate the pains and penalties of the struggle for life.

These details are, however, of minor import for us just now. The important thing to bear in mind is that all of the forces of development are ultimately metabolic in origin, and that the wonderful order and sequence of events in any given ontogeny arise from the transformation or transposition of the parts of a molecular system that also thus increases in bulk by the addition of new matter. The steps of this transformation are mechanically conditioned by dynamical laws with as much unerring certainty of sequence as those that control the motions of the heavenly bodies. The consequence of such a view is that we can thus free our minds of all traces of belief in a theory of preformation. The embryo is not and cannot be preformed in the germ, as observation and physiological tests prove; nor is such preformation necessary if a mechanical hypothesis is adopted.

JOHN A. RYDER.

(*To be concluded.*)

CURRENT NOTES ON PHYSIOGRAPHY (VIII.)

CROWLEY'S RIDGE.

CROWLEY'S RIDGE, rising above the alluvial lowland of the Mississippi in Missouri and Arkansas, has long been a subject of discussion. Branner (Geol. Surv. Ark., Ann. Rep., 1889, ii., p. xiv.) has suggested that the lowland to the west of the ridge was excavated as an early path of the Mississippi, from which it was diverted into its present course east of the ridge by the Ohio; but it is difficult to understand how the smaller of the two rivers could divert the larger one. A new explanation of the ridge has recently been offered by C. F. Marbut (Proc. Boston Soc. Nat. Hist. xxvi., 1895), to the effect that the ridge is homologous with the Chunnenugga ridge of Alabama, and that it belongs to a family of geographical forms frequently found on coastal plains during the mature stages of their development. These ridges or uplands normally run parallel to the coast line; they mark the outcrops of comparatively resistant strata, dipping toward the coast; they descend inland by a relatively rapid slope, often strong enough to be called an escarpment, towards an inner lowland which has been eroded on an underlying and weaker member of the coastal formations; they descend more gently on the coastal side. The inner lowland is drained by longitudinal streams, which enter transverse streams that cut their way through the ridge or upland on the way to the sea. In a region of uniform uplift all these features of relief and drainage have a regular rectangular system of trends; but where the former shore line or the uplift is irregular the trends will depart more or less from a rectangular towards a curved pattern. Marbut regards Crowley's ridge as a portion of an inland-curving ridge of this kind. The master stream of the region is the Mississippi, which bisects the inland curvature of the ridge. The upland along whose eastern base the Tennessee river flows northward in an adjusted subsequent course forms the eastern part of the curve; while Crowley's ridge forms the western part. The lignitic strata by which the ridge is determined weaken southwestward, and hence the ridge soon disappears in that direction. The lowland west of Crowley's ridge, ascribed by other writers to erosion by the Mississippi, is explained by Marbut as comparable to the lowland on the inland side of the Chunnenugga ridge of Alabama, and the rivers which follow this lowland are thought to be adjusted subsequent rivers.

THE CUSPATE CAPES OF THE CAROLINA COAST.

THE systematic repetition of certain features in Capes Hatteras, Lookout and Fear is explained by C. Abbe, Jr. (Proc. Boston Soc. Nat. Hist., xxvi., 1895) as the result of a number of backset eddying currents, turning from right to left between the Gulf Stream and the coast. The generally southward movment of the sands along the shore being well known, some special explanation is needed for the acutely pointed capes between the smooth concave curves of the sand bars. Although this is a conspicuous feature of the coast, it seems to have been little considered. Shaler, in his recent general account of Harbors (U. S. Geol. Survey, 13th Ann. Rept., 1893, 180), suggests that the greater inflow of the tides in the middle of the curved bays between the capes would cause a lateral current in either direction, and that the cusps would form where the outward flow from two curves became confluent; but this is contradicted not only by the general southward movement of sands along the shore, but also by certain minor features to which Abbe gives special attention, and which indicate an outward movement of the prevailing currents on the north side of each cape, but an inward movement on the south side. The V-shaped bars on the shore of ancient Bonneville (Monogr. I., U. S. Geol. Survey, 57) seem to correspond with the cuspate capes in essential features, but their relation to eddying currents is not clearly brought foward by Gilbert. Penck, in his recent *Morphologie der Erdoberfläche*, mentions backset shore currents as of frequent occurrence, and suggests that the V-shaped bars of the Bonneville shore may have been produced by such movements (II., 485, 486), but he does not refer to other examples of this kind. Yet cuspate sand-bar capes of moderate size are certainly not rare, as may be seen by consulting the maps of our coast in the lower part of Chesapeake Bay.

Dungeness, on the southeastern coast of England, seems to be a similar form; but no other examples are known of so great a size as those of our Carolina coast, nor has any other instance been adduced of so pronounced a control exerted by the general oceanic circulation upon the form of the continental shore line.

THE MIGRATION OF CAPE CANAVERAL.

IN connection with the foregoing, mention may be made of the southward migration of Cape Canaveral, as indicated by the Coast Survey Charts (Nos. XIII., and 159–163). Like the capes further north, Canaveral is a sand-bar cusp, the details of its form indicating a control by two adjacent eddying currents, after the manner described by Abbe. Its history appears to have been in brief as follows: The position taken by the first blunt cusp between the adjacent eddies seems to have been about ten miles south of Mosquito inlet and forty miles north of the present cape; this being, as it were, a provisional location adopted by the currents before much work had been done in shaping the coast by building long bars for the transportation of sand. As an improved and continuous bar grew from north to south, its relation to the general curvature of the Carolina bight was such that it ran past the first-formed cape, and a new location for the cusp was then chosen thirty miles farther south, the outline of the old cape being still faintly traceable inside the newer bar. But a still better adjustment of the currents to the shore brought another bar down from the north, this one running past the apex of the second cape in much the same way that the second bar ran past the first cape; and thus the third cusp, the present Canaveral, was formed ten miles south of the second. The southward migration of the cape appears to be still continued, as indicated by the arrangement of the sand dunes; but it is now going on with a slowly progressive, creeping advance, and not by a leap, such as that which shifted the second cape from the first, or the third from the second. All this, however, is based only on a study of the charts. Those who have opportunity for a study of the cape on the ground might make it the subject of fruitful observation.

W. M. DAVIS.

HARVARD UNIVERSITY.

ANNUAL MEETING OF THE CHEMICAL SOCIETY (LONDON).

IN the course of his address at the anniversary meeting of the Chemical Society of London, the President, Professor Armstrong, after referring to the notable growth of the Society in the twenty years during which he had been a member, stated that the Council had decided to break through the practice which had always obtained and by which the Faraday Lectureship has invariably been filled by some foreign scientist, and had bestowed the Faraday Medal upon Lord Rayleigh 'in recognition of the services rendered to chemical science by the discovery of argon.' The President added that the Medalist would address the Society on the subject of argon.

Lord Rayleigh said that, in returning his thanks to the Society, he was somewhat embarrassed, because he felt that there ought to be another standing at his side. It was true that his researches, to which the President had referred, upon the densities of gases had rendered it almost certain that a new gas of some sort was concerned, and probably that the new gas was in the atmosphere. But from this point to the isolation and examination of argon was a long step, and the credit must be shared equally between Professor Ramsay and himself. In some quarters there had been a tendency to represent that antagonism existed between chemists and physicists in the matter, though such a thought never entered his mind. Professor Ramsay was a chemist by profession, while he himself had dabbled in chemistry from an early age, and had followed its development with a keen interest.

During the course of the same meeting Professor Ramsay and Mr. Crookes spoke of the isolation and spectroscopic examination of the gas containing helium derived from clèveite.

At the anniversary dinner in the evening of the same day the principal address was made by the Rt. Hon. A. J. Balfour. The following extracts from this will be of interest. Speaking of the attitude of the statesman towards science, he said: "For my own part, though the last thing I wish to do is to suggest that the work of a practical politician is other than a work which taxes the highest qualities of a man, still I have to admit, on looking back at the history of civilization, that if we want to isolate the causes which more than any other conduce to the movements of great civilized societies, you must not look to the great politician of the hour, on whom it may be all eyes are fixed; you must look to those, often unknown by the multitude, whose work, it may be, is never properly realized by the mass of their countrymen till after they are dead. You must look at them, and at their labors, to find the great sources of social movement. We, who are carrying on a work which I hope is not useless, which, I am sure, receives its full meed of public recognition, do, after all, not belong to that class to which the community is most beholden for all that is to improve the lot of man upon earth. It is to those who, very often with no special practical object in view, casting their eyes upon no other object than the abstract truth and the pure truth which it is their desire to elucidate, penpenetrate ever further and further into the secrets of Nature and provide the practical man with the material upon which he works. Those are the men who, if you analyse the social forces to their ultimate units, those are the men to whom we owe most, and to such men, and to produce such men, and to honor such men, and to educate such men, the Society whose health I am now proposing devotes its best energies. * * *

"I should like to do what I can to dispel the prejudice which certainly exists at this moment in many influential quarters against technical education properly understood. Technical education, properly understood, suffers greatly under technical education improperly understood, and there is so much nonsense talked upon this subject; there is so much money uselessly spent; there are so many things taught to persons who do not want to learn them and who, if they did want to learn them, could by no possibility turn them to practical account; that it is no matter of astonishment that some persons are disposed to say that 'technical education is only the last bit of political humbug, the last new scheme for turning out a brand new society; it is worthless in itself; not only is it worthless, but it is excessively expensive.' I am sure Mr. Bryce * would agree with everything I have said upon this point, and everything I am going to say upon it—for I shall not go into controversial matter—because, while I think that those who object to technical education have their justification, it yet remains true that if you include, as you ought to include, within the term technical education the really scientific instruction in the way of turning scientific discoveries to practical account, if that is what you mean—and it is what you ought to mean by technical education—then there is nothing of which England is at this moment in greater need. There is nothing which, if she, in her folly, determines to neglect, will more conduce to the success of her rivals in the markets of the world, and to her inevitable abdication of the position of commercial supremacy which she has hitherto held."

"I do not deny that, if manufactures and commerce have an immense amount to gain from theoretical investigations, on the other hand—as everybody will admit that has even the most cursory acquaintance, let us say, with the history of the discoveries in electricity and magnetism—pure science itself has an enormous amount to gain from industrial development. While both these things are true, I am the last person to deny that it is a poor end, a poor object, for a man of science to look forward to, merely to make money for himself or for other people. After all, while the effect of science on the world is almost incalculable, that effect can only be gained in the future, as it has only been gained in the past, by men of science pursuing knowledge for the sake of knowledge, and for the sake of knowledge alone; and if I thought that by anything that had dropped from me to-night I had given ground for the idea that I looked at science from what is commonly called the strictly utilitarian standpoint—that I measured its triumphs by the number of successful companies it had succeeded in starting, or in the amount of dividends which it gave to the capitalist, or even by the amount of additional comfort which it gave to the masses of the population—I should greatly understate my thought; but I know this great Society, while it has in view these useful objects, still puts first of all the pursuit of truth, which is the goddess to which every man of science owes his devotion. And truth, not profit, must necessarily be the motto of every body of scientific men who desire to be remembered by posterity for their discoveries. These things can only be done through a disinterested motive, and it is because I believe that societies like the great Society I am addressing do more than any other organization to attain that great object: because I think they bring together men engaged in congenial pursuits; because the stimulus of mind brought close to mind, and the honorable ambitions and the honorable rivalries of men engaged in the same great task must lead to an enormous extension of our knowledge of the secrets of Nature; that I, as an outsider, not belonging to your body, do, in the name of a public for which I venture to speak, wish you all success and wish you all prosperity." W. W. R.

* The Rt. Hon. James Bryce, President of the Board of Trade.

CORRESPONDENCE.

HAECKEL'S MONISM.

Editor of Science: In reponse to your kind note of recent date concerning Haeckle's 'Monistic Creed,' I may state that I find myself in the fullest sympathy with the views expressed by Professor Brooks.

I may perhaps be permitted to add the following:—

These nses of man, as of other animals, yield certain impressions which so far as they go are of the nature of truth. We know truth only through approximation, the revision and extension of these sense impressions. These impressions and the inductions from them serve as guides to action. In this relation these common impressions must be true, because trust in them has been safe. Wrong action must have led to the destruction of the actors. One test of truth, perhaps the only one, is the safety that comes from trusting it. The power of choice implies that right choice must be made. Only those who in the narrow range of choice choose safely can survive. To this end of safe choice, sensations, desires and reason must coöperate. The adaptation to complex conditions rests on the ability of the individual to receive the degree of truth he needs to make safe choice possible, and no more. For truthfulness in sensation exists only in the range within which action and choice are dependent on it. Beyond this range truth would have no value as an aid to adaptation. Our senses tell us something of truth as to bread and fruit and stones, which we may use or touch or avoid. They do not give us just impressions of the stars or sky, which we cannot reach,nor of the molecule, which we cannot grasp. Our sense powers, as well as our powers of reasoning, are eminently practical. They are bounded by the needs of the lives of our ancestors, to whom any form of *hyperæsthesia* would have been destructive and not helpful.

The methods and the appliances of science serve as an extension of the truthfulness of the senses into regions in which truth was not demanded for the life-purposes of our ancestors. These methods yield truth of a similar kind, which can be measured by the same test. We may trust the information given by the electrometer or the microscope or the calculus just as implicitly as we receive what our own eyes have seen or our own hands have felt. We may depend on the truth given by these instruments of precision to a greater degree than on that which the common senses furnish us, because the guards and checks on scientific appliances are more perfect. The information gained by observation and sifted by reason constitutes science. In the struggle for existence, knowledge is power. Our civilization rests directly on the growth of scientific knowledge and on the availability to the individual of its accumulated power. Its basis is the safety of trusting to human experience. The 'Laws of Nature,' as we know them, are generalizations of such experience. Their statement may form part of a 'scientific creed' to those who have tested them, if such feel that 'I believe' adds force to 'I know.'

The essence of the 'Monistic Creed' as set forth by Haeckel is not, as I understand it, drawn from such sources. It is an outgrowth from Haeckel's personality, not from his researches. So far as I know, no change has taken place in it as a result of any discovery its author has made. If its details have been changed at any time since it was first formulated, the reason for such change must be sought for in Haeckel, not in Science.

Perhaps, indeed, there is "one spirit in all things, and the whole cognizable world is constituted and has been developed in accordance with one fundamental law." But this is no conclusion of science. It rests on no human experience. If it be the induction resulting from all human experience, that fact has not been made plain to us. The hyperæsthesia of the microscope or the Calculus brings one no nearer to it. Its place is in the boundless realm of guesswork. It value lies in the stimulus which clever guesses give to the otherwise plodding operations of scientific men. It seems to me that 'Monism' belongs to the domain of speculative philosophy, a branch of thought which, according to Helmholtz, deals with such 'schlechtes stoff';' that its conclusions, however brilliant, can have no value as guides to life or as guides to research, which is the second power of life. The theory of Monism has no interest to Science, until men can come to deal with the 'Stoff' on which its speculations rest. Every conceivable theory of life, its nature, origin and destiny, can be traced back to the pre-scientific philosophy of the Ancients, Monism with the rest. What we have found to be true was not unknown to the Greeks. But that which we find to be false had equally the weight of their authority. It is the business of Science to test by its own methods the value of the supposed basis of these theories. The use of logic is one of these methods. The only logical necessity Science can recognize, as Dr. Brooks has well said, is "that when our knowledge ends we should confess our ignorance."

I have myself not the slightest objection to 'Monism' as philosophy. As a dogma it is certainly more attractive than many others which have been brought like lightning from the clouds, as a stimulus to creeping humanity. My objection lies against the use of the divining rod in connection with the microscope. These instruments do not yield homologous results. If both yield Truth, then Truth is a word of double meaning. This method seems to carry us back to the days when truths were made known to the spirit without the intervention of the body. When some theologian of the past brought to Luther the revelations his spirit made to him, the sturdy Reformer said, "Ihren Geist haue ich über die Schnautze" (I slap your spirit on the snout). Scientific men may have as individuals their own visions and guesses and formulæ of Universal Philosophy. Spiritual gymnastics are not without value to any worker, and men of science have often suffered from their neglect. But this suffering is purely individual. The running high jump does not hasten the progress of knowledge. Science will have none of it. Nor will she tolerate a divining rod even in the hands of her wisest devotees. In other words, where the facts stop Science stops also.

DAVID STARR JORDAN.

STANFORD UNIVERSITY.

THE GENUS ZAGLOSSUS.

TO THE EDITOR OF SCIENCE: Mr. T. S. Palmer's article in SCIENCE of May 10th fixes the synonymy of this genus with precision; but one statement he makes is incorrect, namely, that '*Zaglossus* Gill seems never to have been mentioned by any subsequent author.' The Century Dictionary has three articles from my pen on the subject. 1. *Zaglossus* is defined as 'the proper name of that genus of prickly ant-eaters which is better known by its synonym *Acanthoglossus* (which see).' 2. Under *Acanthoglossus* the genus is characterized, with the statement that this name 'is antedated by *Zaglossus* of Gill.' 3. Under *Echidnidæ* the animal is figured with the legend '*Zaglossus* or *Acanthoglossus bruijni*.'

ELLIOTT COUES.

SCIENTIFIC LITERATURE.

The Cambridge Natural History, III., Molluscs: By the REV. A. H. COOK; *Brachiopods* (Recent): By A. E. SHIPLEY; *Brachiopods* (Fossil): By F. R. C. REED. New York, Macmillan & Co. 1895. XIV., 536. Pp. 8°. Illustrated.

This work is one of a series intended especially for intelligent persons without scientific training, but in which the attempt is made to combine popular treatment and untechnical language with the latest results of scientific research.

Mr. Cooke, who is known as a painstaking and well informed conchologist, has endeavored to unite in one general classification the views of specialists in the various groups, such as Hoyle for the recent, Foord and Fischer for the fossil Cephalopods, Bergh for the Nudibranches, Pelseneer for the Pelecypoda, etc.; but, in conformity with the general purpose of the work, much more space is devoted to the geographical distribution and general natural history of mollusks than to the details of systematic arrangement or technical discussion. Twelve chapters of 377 pages are devoted to generalities, and four, comprising 66 pages, to classification.

The work deserves high commendation for the thorough manner in which Mr. Cooke has foraged for fresh data, bringing together a vast number of facts on the biography, distribution, growth, anatomy and reproduction of mollusks. The style is clear and easy, and the facts are well selected and agreeably presented. For the audience for which the book is intended it seems admirably adapted, and so far as we know there is no work available at present which can be more cordially recommended to a beginner or the general reader.

It would be easy to criticise details of classification here and there, and on many points the opinions of experts will differ in the present state of our knowledge; but in recognizing the aim of the author and publishers it must be conceded that it has been well carried out.

It does not appear to have been necessary to separate the recent from the fossil brachiopoda, and recent efforts at a revised classification of the group have been so successful and complete that Mr. Reed's work appears already somewhat antiquated and too brief, but this perhaps was inevitable from the necessity of preserving due proportion between the parts of the series. Mr. Shipley's account of the anatomy and embryology is good, and his conclusions as to the relations of the class are conservative and reasonable.

The book is fully illustrated with rather unequal woodcuts, many of which are good and others rather 'wooden,' but an unusually large proportion of them are original and fresh. There are four very good maps of geographical distribution and an excellent index.

W. H. DALL.

A Laboratory Guide for a Twenty Weeks' Course in General Chemistry. By GEORGE WILLARD BENTON, A. M., Instructor of Chemistry, High School, and Chemist for the City of Indianapolis. Boston, D. C. Heath & Co.

This book might be better termed 'A Guide for a Course of Test-Tubing,' since nearly all the reactions are performed in a test-tube, and the sole object of the book seems to be to acquaint the unfortunate pupil who uses it with 'Tests' for the various elements and compounds.

The manual is supposed to be put into the hands of beginners in the subject, and yet before a single element is considered or anything is said about elements, compounds or formulas, quite a number of formulas and reactions are given. As an illustration of what the author calls compounds, a piece of wood and granulated sugar are taken and the equation $C_{12}H_{22}O_{11} + H_2SO_4 = 12C + 11H_2O + H_2SO_4$, is written out. Then the student is asked to explain the equation and to define a compound. And yet the author, according to his preface, is one of those 'who see in the Laboratory (with a big L) the means of high development on approved pedagogical grounds.'

It would require more space than the book is worth to point out all its faults. It will, perhaps, be sufficient to state that directions are given for making dangerous compounds without any mention of the danger connected with the work. The pupil is asked, for example, to determine the odor of carbon monoxide, and not an intimation is given that it is one of the most poisonous gases known to the chemist.

Altogether, the book is one that can be most cordially recommended as the kind of a book for both teachers and students to avoid using, if possible.

W. R. O.

NOTES AND NEWS.

THE HELMHOLTZ MEMORIAL.

The following subscriptions have been paid to Prof. Hugo Münsterberg, Secretary and Treasurer of the American Committee, Cambridge, Mass. Further subscriptions should be sent to him at an early date:

A. Agassiz, Cambridge,	$ 25
S. P. Avery, New York,	10
Clarence J. Blake, Boston,	20
Francis Blake, Boston,	50
H. P. Bowditch, Boston,	20
W. N. Bullard, Boston,	10
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Clarence N. Clark, Philadelphia,	25
A. Donner, Boston,	25
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Society of Eye Surgeons, San Francisco,	25
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O. F. Wadsworth, Boston,	20
H. C. Warren, Cambridge,	25
D. Webster, New York,	5
Henry W. Williams, Boston,	25
N. Wilmer, Washington,	10
Total,	$970

THE GEOLOGICAL SOCIETY OF AMERICA.

The Geological Society of America will hold its seventh Summer Meeting at Springfield, Mass., Tuesday and Wednesday, August 27 and 28. The Council will meet Monday evening and the Society will convene Tuesday morning at 10 o'clock.

The Fellowship of this Society includes nearly all the working geologists upon the continent. The roll now contains 223 names of Fellows.

The former Presidents of the Society have been James Hall, James D. Dana, Alexander Winchell, G. K. Gilbert, J. William Dawson and T. C. Chamberlin.

The officers for 1895 are as follows:

President, N. S. Shaler, Harvard University.

Vice-Presidents, Joseph Le Conte, University of California; Charles H. Hitchcock, Dartmouth College.

Secretary, H. L. Fairchild, University of Rochester.

Treasurer, I. C. White, Morgantown, W. Va.

Editor, J. Stanley-Brown, Washington, D. C.

Councillors:

F. D. Adams, McGill College, Montreal.
R. W. Ells, Geological Survey of Canada.
I. C. Russell, University of Michigan.
E. A. Smith, University of Alabama.
C. R. Van Hise, University of Wisconsin.
C. D. Walcott, U. S. Geological Survey.

The Society has just completed the sixth volume of its Bulletin, which is a handsome octavo, with 528 pages and 27 plates. This volume includes twenty-one brochures.

Information concerning the Society and its publications can be obtained by addressing the Secretary, H. L. Fairchild, Rochester, N. Y.

NOMINATIONS BEFORE THE ROYAL SOCIETY.

The following fifteen candidates were selected by the Council of the Royal Society to be recommended for election into the Society: J. Wolfe Barry, civil engineer, Vice-President of the Institution of Civil

Engineers; Alfred Gibbs Bourne, Professor of Biology in the Presidency College, Madras; George Hartley Bryan, Fellow of Peterhouse, Cambridge, and Lecturer on Thermodynamics on the University list; John Eliot, Meteorological Reporter to the Government of India; Joseph Reynolds Green, Professor of Botany in the Pharmaceutical Society of Great Britain; Ernest Howard Griffiths, physicist Private Tutor; Charles Thomas Heycock, Lecturer on Natural Science, King's College, Cambridge; Sydney John Hickson, biologist, Fellow of Downing College, Cambridge; Henry Capel Lofft Holden, Major Royal Artillery, electrician; Frank McClean, astronomer; William Mac Ewan, Professor of Surgery, University of Glasgow; Sidney Martin, Assistant Physician, University College Hospital and Hospital for Consumption, Brompton; George M. Minchin, Professor of Mathematics in the Royal Engineering College, Cooper's Hill; William Henry Power, Assistant Medical Officer, H. M. Local Government Board; Thomas Purdie, Professor of Chemistry in the University of St. Andrews.

JOHN A. RYDER.

A JOINT meeting of members of the University of Pennsylvania, the American Philosophical Society and the Academy of Natural Sciences was held in the hall of the Academy of Natural Sciences on the evening of Wednesday, April 10, in memory of the late Professor John A. Ryder. General Isaac J. Wistar presided and Philip P. Calvert acted as secretary. Addresses were made by Dr. Harrison Allen on 'Dr. Ryder's Relation to the Academy of Natural Sciences;' Dr. Bashford Dean, of Columbia College, on 'Dr. Ryder's Work in the U. S. Fish Commission'; Dr. Horace Jayne, on 'Dr. Ryder and the School of Biology'; Prof. E. D. Cope, on 'The Evolutionary Doctrine of Dr. Ryder;' Dr. H. F. Moore, on 'Dr. Ryder as a Teacher,' and Dr. W. P. Wilson, on 'Dr. Ryder as a Collegian.' The speakers all bore testimony to Professor Ryder's merits as an investigator and as a teacher and to his amiability and honesty as a man.—*American Naturalist.*

GENERAL.

THE *Gesellschaft für Erdkunde* at Berlin has just issued the first volume of a bibliography of geographical science entitled Biblioteca Geographica, edited by Otto Baschin with the assistance of Dr. Ernst Wagner. The volume covers 1891 and 1892 and the society proposes to continue the publication annually. The scope of the work is in full accord with the widest understanding of the word geography. The editor, Otto Baschin, Berlin, W. Schinkenplatz 6, requests that authors send titles and works relating to geography to him.

THE *Imprimerie Polytechnique* at Brussels announces an important Egyptological work by G. Hagemans, which will include a history of Egyptian civilization, a summary of Egyptian literature and a discussion of the Egyptian writing, including a comparison between its hieroglyphs and those of Yucatan; this is to be followed by a Copto-Egyptian grammar, an Egyptian-French and a French-Egyptian dictionary. The entire work will appear in sixty parts at 25 cents per part.

WE learn from *La Nature* that at the annual meeting of '*Le Congrès des Sociétés Savantes*' at the Sorbonne, Paris, on April 20th, under the presidency of M. Poincaré, M. Moissan called attention to the rapid progress and brilliant discoveries of modern chemistry, and their practical outcome in stimulating national industries. He passed under review the processes of manufacturing iron, steel, aluminium, etc., the artificial production of the diamond, the crystalization of metallic oxides, and the use of electricity in the decomposition of those oxides hitherto regarded as irreducible. At the close of the meeting M. Poincaré was elected president for a second term. The Legion of Honor was conferred on MM. le comte d'Avenal, O'Ehlert and Herluison.

The honorary degree of D. Sc. has been conferred on Mr. Francis Galton by the University of Cambridge.

The statute establishing degrees for research at Oxford has now been finally approved by Congregation, with the adoption of several amendments, principally of a technical nature.

The University of Aberdeen is about to confer the degree LL. D. on Miss J. E. Harrison in recognition of her researches in Greek archæology. Miss Harrison will be the first woman to receive this degree from a British university.

Dr. Richard Hanitsch, demonstrator of zoölogy at University College, Liverpool, has been appointed to the curatorship of the Raffles Museum, at Singapore.

The *Evening Post* states that the Herbarium of Rousseau, composed of fifteen quarto volumes in cardboard and containing about 1,500 plants, is about to be sold at Orleans.

At a recent sale in London, Gilbert White's Natural History of Selborne, the author's original manuscript, in the form of letters to Thomas Pennant and Daines Barrington, first printed in 1789, was sold for £294. The manuscript contains many passages not printed in the several editions, and has never before been out of the possession of the lineal descendants of the author.

A catalogue of the Philosophical Transactions of the Royal Society from 1824 to 1893 has been issued by Dulau & Co., London. A large number of separate articles are included. Especially worthy of note is a paper on 'Observations on the Parallel Roads of Glen Roy * * * with an attempt to prove that they are of Marine Origin' (1839), by Darwin, as also articles by Sir Humphrey Davy, William and Sir F. Herschell, Sir E. Sabine, Sir David Brewster, Faraday, Sir Richard Owen and Cayley.

Mr. Arthur M. Wellington, the well known engineer, died in New York at the age of forty-eight.

Prof. E. Ray Lankester is giving a course of four lectures at the Royal Institution on 'Thirty Years' Progress in Biological Science.'

Mrs. Robert E. Peary delivered an illustrated lecture based on her experiences in the North on May 23. This lecture was given under the auspices of the National Geographic Society, which aided Lieut. Peary in his first enterprise. The proceeds of the lecture will be devoted to a fund which is being raised to defray the expenses of an expedition that will enable Lieut. Peary to return to America. It is not believed, however, that he is in any immediate danger. The expedition (which will cost from $9,000 to $12,000, of which about $7,000 has already been raised) will probably start about July 5th, so as to reach Lieut. Peary's headquarters before September 1st.

At the meeting of the Boston Scientific Society, was held on May 28th, an address on 'Some Problems in the Use of Water Power as Applied to the Electrical Transmission of Power' was delivered by Allan V. Garratt.

Professor Dyche, of Kansas University, is starting for Greenland in search of specimens of mammals and birds to add to his collection.

Chancellor James Hulme Canfield has accepted a call to the presidency of the Ohio State University, Columbus.

An infirmary in connection with Harvard University, which is proposed as a memorial to Dr. Peabody, is projected, costing not less than $12,000. President Eliot, in the name of the overseers of Harvard University, has offered a site for the infirmary, providing the money to build it can be raised.

DR. JAMES E. RUSSELL has been made professor of pedagogy in the University of Colorado.

THE American Institute of Archæology, which had already given a fellowship of $600 to the American school at Athens, voted a second fellowship of $600–$800 at the semi-annual meeting of the committee held at Middletown, Conn., on May 17th. These scholarships will probably be awarded to students and graduates of the coöperating colleges on competitive examination. The first examination will probably be held at the end of a year.

PROF. E. S. HOLDEN has been made a commander of the Order of the Ernestine House of Saxony in recognition of his services to science.

DR. P. DANGEARD has been appointed professor of botany to the Faculty of Sciences at Poitiers.—*Nature*.

WE learn from the *Naturwissenschaftliche Rundschau* that Prof. Overbeck of Greifswald has been appointed professor of physics in the University of Tübingen as successor to Professor Braun. Dr. Hermann Struve, astronomer in the Observatory of Pulkowa, has been made professor of astronomy in the University of Königsberg; Prof. Koken of Königsberg, professor of geology and mineralogy in Tübingen; Prof. Hauser of Erlangen, Director of the Erlangen Anatomical Institution; Prof. Brauns of Karlsruhe, professor of geology and mineralogy in Giessen, and Dr. Schutt of Kiel, professor of botany in the University of Greifswald.

PROFESSOR V. KNORRE has been called to the new chair of electro-chemistry in the technical High School at Berlin-Charlottenburg.

THE death is announced on May 4th of Surgeon-Major Carter, F. R. S., also of Prof. Manuel Pinheiro Chagas, General Secretary of the Royal Academy of Sciences at Lisbon, at the age of fifty-three.

IT is announced that Dr. J. P. D. John, who resigned the presidency of De Pauw University a few days ago, will be asked by the trustees to reconsider his resignation.—*Evening Post*.

THEOBALD SMITH, M. D., has been elected professor of applied zoölogy, and Henry Lloyd Smythe assistant professor of mining, in Harvard University.

AT the semi-annual meeting of the trustees of the American University it was announced that $127,300 had been subscribed towards the erection of the first building (the Hall of History), but that $150,000 were required. Those present at the meeting subscribed and assumed the entire deficiency.

DR. ROB. SACHSSE, assistant professor of agricultural chemistry in Leipzig University, died on April 26.

SCIENTIFIC JOURNALS.

THE ASTROPHYSICAL JOURNAL, MAY.

The Modern Spectroscope, XII: WILLIAM HUGGINS.

Dr. Huggins here describes the Tulse Hill ultra-violet spectroscope. An earlier arrangement of telescope and spectroscope had consisted in exchanging the small mirror of an eighteen-inch Cassegrain telescope for a spectroscope with its slit in the principal focus of the large mirror. Difficulties of adjustment and the sacrifice of either light or purity due to the restricted size of the spectroscope led to the abandonment of this form. The small speculum was replaced and the collimator was then inserted in the hole through the large mirror. The long equivalent focal length of the Cassegrain form is of advantage where it is desirable to have images of considerable dimensions upon the slit, while the instrument itself and the building may remain of moderate size.

On the Spectrographic Performance of the Thirty-inch Pulkowa Refractor: A. BÉLOPOLSKY.

The work of the great refractor with a spectrograph not well adapted to it compares unfavorably with that of the new thirteen-inch photographic telescope.

Note on the Spectrum of Argon: H. F. NEWALL.

A line spectrum obtained last year under peculiar conditions of low pressure has been identified as that of argon. A glass bulb was sealed to a mercury pump and the air exhausted. Two photographs, with an exposure for each of thirty minutes, differed in that the second showed the nitrogen bands much weaker than the first, besides containing lines since identified as those of argon.

Preliminary Table of Solar Spectrum Wave-Lengths, V: HENRY A. ROWLAND.

The table is continued from λ 4414 to λ 4674.

On Martian Longitudes: PERCIVAL LOWELL.

A series of observations on the positions of thirty-six points on Mars with a view to the construction of a map. A discrepancy of five degrees between present longitudes and those determined by Schiaparelli in 1879 suggests that the received time of rotation of the planet is too small.

A Combination Telescope and Dome: A. E. DOUGLASS.

The article describes a novel plan of mounting a telescope within a hollow sphere supported like an ordinary globe, but with much of the weight taken off from the supports by floating the sphere in water. The plan is the result of an effort to reduce the instability of the usual mounting by flotation, and the application of the motive power as far as possible from the axes of rotation.

Stars Having Peculiar Spectra; Eleven New Variable Stars: M. FLEMING.

Some Arequipa photographs show eleven peculiar star spectra and eleven new variables.

A Spectroscopic Proof of the Meteoric Constitution of Saturn's Rings: JAMES E. KEELER.

The spectrum of the planet was photographed with the slit parallel to the major axis of the rings. The inclination of the spectral lines of the ansæ show that the inner part of the ring is moving faster than the outer portion, which would not be the case were the rings moving as a solid. The indicated velocities of the different parts satisfy Kepler's third law.

Remarks on Professor Pickering's 'Comparison of Photometric Magnitudes of the Stars,' in A. N. 3269: G. MÜLLER and P. KEMPF.

A criticism of the Cambridge catalogues, translated from the Astronomische Nachrichten.

The Short Wave-Lengths of the Spark Spectrum of Aluminium: C. RUNGE.

A Large Eruptive Prominence; On a Photographic Method of Determining the Visibility of Interference Fringes in Spectroscopic Measurements; Note on the Exposure Required in Photographing the Solar Corona Without an Eclipse: GEORGE E. HALE.

Terrestrial Helium (?).

A Large Reflector for the Lick Observatory: EDWARD S. HOLDEN.

S. B. BARRETT.

NEW BOOKS.

The Natural History of Plants; their Forms, Growth, Reproduction and Distribution. From the German of Anton Kerner von Marilawn, by F. W. OLIVER, with the assistance of MARIAN BUSK and MARY F. EWART. With almost 1,000 original wood-cut illustrations and 16 plates in colors. New York, Henry Holt & Co. 1895. 40, Vol. I., in two parts. Pp. 777. Price $7.50.

Twentieth Annual Report of the Secretary of the State Board of Health of the State of Michigan. Lansing, Robert Smith & Co. 1894. Pp. cxlvi + 416.

SCIENCE.

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SCIENCE.

FRIDAY, JUNE 7, 1895.

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison on Hudson, N. Y.
Subscriptions and advertisements should be sent to SCIENCE, 41 N. Queen St., Lancaster, Pa., or 41 East 49th St., New York.

A DYNAMICAL HYPOTHESIS OF INHERITANCE (II.).

THE egg cannot be isotropic—as follows from observation as well as experiment—in the sense in which the word isotropy is used by physicists of repute. If the egg is a dynamical system it cannot be isotropic or absolutely the same throughout, or along every possible radius from its center, as is proved by its reactions in respect to its surroundings. It may, however, be potentially æolotropic in directions parallel to a certain axis, as experiment has shown by separating the cells that result from segmentation of the egg. Such fragments, if in excess of a certain minimal size, will undergo a larval development of apparently normal character. But this result is fatal to the ordinary corpuscular hypotheses, according to which every future part is represented in the chromosomes by certain hypothetical corpuscular germs. It has, indeed, been shown by Loeb that larval development of portions of an egg can go on whether the divisions be equal or unequal or in any radius. This seems to indicate that an egg is not necessarily isotropic in the undivided state, but that the moment that separation of its mass has occurred there is a readjustment of the relations and potentialities of its molecules simulating that of the original entire egg. The very definition of isotropy, as given by one author (Lord Kelvin), states that it may be assumed only of a spherical mass of matter whose properties are absolutely the same along every one of the infinite number of radii drawn from its center outward, and, as tested by any means whatsoever, shows that such a condition cannot be assumed, on the ground of observation alone, of any known egg. The condition of the egg we must therefore also assume from its known properties to be æolotropic, or different along every one of the infinite number of radii drawn from its center. When we make this assumption, however, we need not necessarily assume that nucleated fragments that will still develop into larvæ after division of the oösperm, natural or artificial, must be isotropic. They may be æolotropic from the beginning, but in precisely the same way in each case, as a result of the successive cleavages of the germ-mass, by means of planes that cut each other at right angles, as in the diagram Fig. 1, where each of the four segments are precisely alike from the pole *a* to that of *b*.

Fig. 1.

The unlikeness of the pole *a* from *b* is indicated by the stippling. This unlikeness would manifestly be unimpaired by segmentation of the germ into four quadrants by the first two cleavages, as shown in the diagram. The same might hold of octants of the spherical germ. Here the initial æolotropy of the whole egg determines that of its segments; that must therefore become four or eight molecular mechanisms, each with precisely the same type of potentiality as that of the whole egg. (See concluding note.)

There may, according to the foregoing view, be such a thing as perfect isotropy in every radius lying in a plane cutting the line from *a* to *b* at right angles. This would not, however, be the perfect isotropy of our definition that we are compelled to accept in the form in which it comes to us from the physicist.

As development proceeds, moreover, we have reason to believe that this æolotropy becomes more and more marked, so that eventually the huge metameric molecules become arranged in definite linear, parallel systems, as in the axis cylinders of nerve cells and in muscular tissue. Here the characteristics of the system become the the same in parallel lines, and in any directions at right angles to an axis parallel to these parallel lines of molecules. That is, in certain rectangular directions there is an approximation toward homogeneity. But the completest homogeneity is found to occur in only one direction in parallel lines extending through the mass. This condition we may designate as monotropy. Starting with the extreme æolotropic condition of the germ, we must, therefore, assume that as organization becomes more and more complete, in the progress of development, in the specialized systems of tissues and organs, the molecules become more and more definitely monotropic. Therefore they at last become incapable, as dynamical systems, of exhibiting a complex development such as is manifested by a germ, but capable only of manifesting the special physiological functions entailed by their dynamically and mechanically evolved monotropism.

We can now understand why it is that the germinal matter of a species always remains in an æolotropic state. Since germinal matter is always relieved of specialized functions in the body of the parent, it must perforce remain in its primitive condition of germinal potentiality as a molecular mechanism. Since the germ is material that has been produced in excess of the needs of metabolism of the parent body, as supposed by Haeckel and Spencer, it can do no work for that body. The unbroken continuity of the processes of metabolism has provided the conditions for the continuous or interrupted production of germinal matter.

The nearest approach to a condition of continuity of germinal matter is found in the tissue of the 'growing points' of plants, where, as in the banana, it has maintained its unabated vigor for probably not less than two thousand years without the help of sexual reproduction. In many organisms the germinal elements must grow and become mature. While in the immature state they do not, for the moment, have the latent potentiality of germs that can, then and there, develop, but may even be destroyed phagocytically, or absorbed by other non-germinal tissues. In still other cases there is no proof that the germinal matter is differentiated, as a complete mechanism, from the first stages of ontogeny onwards, so that the theory of its continuity is not only not always true but is also of small importance. At any rate, it is of far less importance than the fact of continuous metabolism and the gradual advent of monotropism, from a state of germinal æolotropism, effected by the dynamical process of tissue metamorphosis and specialization.

This development of monotropism cannot take place except through the sorting and grouping of specialized molecules, under the domination of forces, the operation of which remains to be discovered in the laws of physiological chemistry and molecular mechanics, and not by an appeal to an unworkable hypothesis that merely covers up our ignorance and impedes our progress by invoking the help of 'gemmules' or 'biophors' that grow and divide like cells. There is no evidence that will enable us to conceive the growth of the molecules of living matter in this way, since we are now dealing with very complex metameric molecular bodies, the growth and disintegration of which is probably essentially similar to the growth and solution of crystals, during the process of metabolism, with this difference that growth and disintegration go on at the same time in living bodies. We do not even know the real nature of the chemical changes that go on in these molecules and determine their structure. That the forces that do determine this are of a chemical nature, operating under very peculiar conditions, we may be certain. The complexity of these bodies, and their complex relations to one another, give us all the mechanism we need in order to account for the phenomena of heredity.

One-half or one-quarter, or an uneven part of the oösperm (Loeb), will operate in the same way as the whole. If we accept the dynamical hypothesis here proposed we are relieved of going to the length of the absurdity of assuming that by dividing a germ we multiply its 'biophors' as many times by two as we have made divisions, or of postulating 'double' or 'quadruple determinants.' The arithmetical impossibility of multiplying by a process of division is, as we see in this case, too much for any non-dynamical corpuscular hypothesis. Where the division of the germ is unequal, as in some of Loeb's experiments, we should, on the basis of a preformation hypothesis, be compelled to suppose that the 'double determinants' were unequally divided.

Regeneration is also to be explained upon the basis of a dynamical theory, as well as polymorphism, alternation of generations, reversion, and so on. We find indeed that it is only the same kind of tissue that will regenerate the same sort after development has advanced a considerable way. Monotropism has been attained by each kind of tissue, and this prevents the production of anything else but the one sort, in each case, after tissue differentiation has proceeded a little way. Polymorphic or metagenetic forms are to be accounted for in the same way as constantly repeated ones. Like the latter they are produced by the operation of a molecular mechanism, the story of the transformation of which is not told off in a single generation, but in the course of several distinct ones. Sex itself is thus determined and must in some way depend upon subtle disturbances of the transformation of the molecular mechanism of the germ, the nature of which is still quite unknown to us.

Equally remarkable are the phenomena of heteromorphosis described by Loeb, whose experiments prove that some animals, like most vegetable organisms, may adjust the molecular machinery of their organization in any new direction whatever that may be arbitrarily chosen, so as to realize the continuance by growth of the same morphological result as that which characterized them normally. These experiments would at first thought seem to prove that some organisms were isotropic, but such a conclusion is exceedingly doubtful. It may be that such organisms are, as molecular mechanisms, when subjected to new geotropic and heliotropic conditions, capable of correspondingly new adjustments of their molecular mechanical structure. But this would not be proof of isotropy—only proof of the assumption of a new condition of æolotropy, adjusted in respect to a new axis of reference, that also coincides with some part of the earth's radius prolonged into space. This readjustment of the molecular mechanism may be effected in some way by gravity, as Loeb himself has suspected. It is certainly not due to the control of any lurking 'biophors,' since it is a purely mechanical readjustment of an ultra-microscopic structure to new conditions which cannot be effected in any other than a mechanical way.

The production of monstrosities also may be explained by a dynamical hypothesis, provided we assume that the forces of ontogeny must operate against the statical equilibrium of the parts of the germ at every step. Especially if we assume in addition, as is born out by facts, that the æolotropy and consequent recapitulative power of the germinal substance is most marked in certain regions of the embryo. These regions, if their molecular equilibrium be mechanically or otherwise disturbed by division during development, will assert their germinal potentiality and produce an embryo, the relations of which to that already formed alongside of it will be modified by the statical conditions of surface-tension afforded by the adjacent embryo or the underlying yoke, or by both combined. This is beautifully illustrated by a host of facts. Double toes must have so arisen, as is proved by the direct experiments of Barfurth, some of which I have repeated, as well as by what happens when the toes of an Axolotl are persistently nibbled off by another animal, when duplication may not only take place in the horizontal plane of the foot or hand, but also in the vertical one. In this way a number of supernumerary toes may be caused to arise from a single stump, provided the re-growth of the toe be so interfered with as to compel regeneration from two terminal regenerative surfaces instead of one. This must follow from the law demonstrated by Barfurth's experiments, namely, that the regeneration of an organ tends to occur uniformly over and in a direction normal to the regenerating surface. In this way it is possible to mechanically determine the direction in which a regenerated part shall be reproduced by merely making changes in the angular relations of the plane of the regenerating surface to that of the axis of the body, as indicated by the diagram in Fig. 2 of the regenerated tail of a tadpole. Here

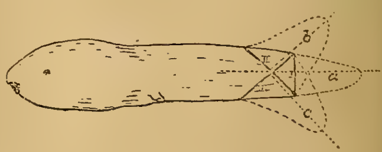

Fig. 2.

the line I indicates the plane along which the tail has been removed, upon which regeneration will restore the tail straight backward to the dotted area *a*. If the plane of section is along the line II the tail will regenerate upward so as to be restored over the area indicated by the dotted line enclosing *b*. If the plane of section of the tail be along III the tail will be regenerated downward to the dotted line enclosing the area *c*. It is therefore evident that Barfurth's law determines the inclination of the axis of the regenerated part to the body-axis, through the different conditions of surface tension that must be set up over regenerating surfaces, whenever the inclination of these to the axis of the whole organism is changed.

New equilibria of surface tension established reciprocally between the cohering but independently developing segments of the oösperm of the sea-urchin, that have been imperfectly separated by mechanical or other means, also cause changes to be produced in the forms of the single larvæ of such coherent groups, and in the spicular skeleton, for the same reason, as is proved by Figs. 23 to 25 given by Professor Loeb.* Those figures also illustrate the thesis that the æolotropy of the distinctly developing segments of the egg must be nearly the same, and that component or resultant equipotential surfaces are developed by the interacting molecular machinery of such coherently developing or compound larvæ.

The angular divergence of duplicated tails and toes as well as the axes of monstrous embryos is explained by Barfurth's discovery, taken together with the principle that division of a germ does not change the æolotropy of its segments. If this interpretation is the correct one, the origin of supernumerary digits must be traced back to mechanical disturbances of the processes of ontogeny. The rationale of the manner in which divergent supernumerary toes may be produced is shown in Fig. 3, representing the regenerating toes of the foot of a salamander.

FIG. 3.

If the toes were cut straight across at the points I., II., III., IV., the toes would regenerate normally. If, however, the regenerating surfaces were divided into two areas in each case by a line along which regeneration were prevented, two toes would arise from each surface. The angular divergence of the pairs of supernumerary toes thus produced would be measured by the angular inclination to one another of the two areas at the end of each original toe that was thus doubly regenerated. In other words, supernumerary digits are the results directly or indirectly of something akin to mutilations. That such duplications may be produced by mutilations there can be no doubt, and of their transmission by inheritance to offspring there is also no doubt. These facts make it probable at any rate that regeneration of distal parts, and the likelihood with which they reappear in duplicate, is due to causes similar or identical in character with those that lead to the production of double monsters, by shaking, mutilation or other physical interference with the normal development of the oösperm. The question of the inheritance of mutilations is consequently far from being concluded as viewed from this new standpoint. Much evidence might be adduced in support of my contention did space allow. The hereditary transmission of such monstrosities as supernumerary digits

Biological Lectures (No. III.). Delivered at Woods Holl, Mass., in 1893. Ginn & Co., Boston.

is well known, and it is a singular fact that it is only the outer digits, *i. e.*, minimus and pollex, or hallux, or those most exposed to the liability of injury during development, that are, as a rule, duplicated. If the foregoing view is correct, the origin of supernumerary digits is not always to be ascribed to reversion. It must not be understood, however, that the theory is here defended that mutilations effected after adolescence is reached are likely to be transmitted.

The 'mutilations' here referred to are hardly to be regarded as such, but rather as the results of mechanical interference or disturbance of the statical equilibrium of those parts of the developing germ that are duplicated, as we see, in obedience to the principle discovered by Barfurth.

Another dynamical factor in development is so generally ignored that it must be especially referred to here. I now refer to the statical properties of the germinal substance in modifying development. Some of its effects we have already taken note of above. Karyokinesis has been shown by Hertwig to be dominated by the principle that the plane of division of a cell is always at right angles to its greatest dimension, a fact readily verified. The greatest dimension of the cell in turn is also often, if not usually, determined by the conditions of free and interfacial surface-tension manifested between the members of a cellular aggregate composing a segmenting egg. This appears to have a determining effect upon the plan of the cleavage. How far and in what way the remarkable movements of the centrosomes that occur during cleavage, and that have been most exhaustively studied by Professor E. G. Conklin, regulate segmentation still remains to be determined. There can, however, be but one explanation of such movements, and that must be a mechanical one, but its nature is entirely unknown. Wilson has shown that the conditions of free and interfacial surface-tension in Amphioxus vary in different eggs from some unexplained cause, so that the earlier cleavages of this form also vary to a corresponding and remarkable degree. In other cases surface-tensional forces operate under similar recurring conditions. In the fish-egg I have witnessed the reappearance of the same or similar interplay of statical energies thrice in succession, so as to produce three similar successive sets of formal changes in the egg that are traceable to the action of similar statical agencies. In *A*, Fig. 4, the

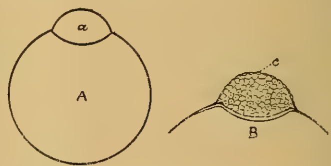

FIG. 4.

germ *a* has assumed a lenticular form of statical equilibrium; after segmentation of the same disk has proceeded some way, as in *B*, the disk, as a cellular aggregate, has again assumed the lenticular form of equilibrium, while the outermost row of cells, *c*, are individually in a similar condition of equilibrium.

These facts are quite sufficient to establish the general truth of the statement that at no stage is the ontogeny of a species exempt from the modifying effect of the surface-tensions of its own plasma acting between the cells as if they were so much viscous dead matter. Such statical effects are not overcome at any stage of the development, or even during the life of any organism. On account of the universal presence and effect of this factor in both the plant and animal worlds, as a modifier of form, we are obliged to consider it as an agent of the first importance in the possible development of the future science of exact dynamical morphology. Its action is so constant an accompaniment of development that the forces of the latter may be divided into the kinetogenetic, or those that develop movement, and the statogenetic, or those that develop rest or equilibria, amongst the parts of the germ. The kinetogenetic forces are the consequences of metabolism, but the statogenetic forces, though dependent upon metabolism, are produced as a consequence rather of the interaction of the surface layers of the plasma of the cells, contemplated as if they were small cohering masses of viscous dead matter. These masses are separated, in the organism or germ, by interfacial planes, free and interfacial curved surfaces that are the results of segmentation and growth, and the extent of the areas of which obey a law first pointed out in relation to soap-bubbles by the blind physicist Plateau, who showed that such bubbles tended to form interfacial films and surfaces, wherever in contact with each other, of an area that was the minimal consistent with their statical equilibrium.* In this connection it may also be remarked that, inasmuch as the cells of a germ or organism are always in statical equilibrium, their surface layers of molecules also always represent complex systems of equipotential surfaces, no matter how intricate the form of the organism may be. Since the equilibria between the molecules of the surface layers of cells can normally be disturbed only by the metabolism incident to physiological activity, it is evident that the figure of the organism must ultimately be ascribed to the action of metabolism or to the functions of the organism as affecting the physical properties of its plasma.

A statical equilibrium in a living cell may be one in which it is not in contact with others at any point on its surface, as in the case of blood-corpuscles or disks. Or a cell may be greatly extended in one direction, as in the case of the axis-cylinder of a nerve-cell, owing to very unequal surface-tensions developed in one or more directions so as to draw it out into a condition of equilibrium, in assuming which it acquires a great length. Formal changes in cells, no matter how irregular these may become, must be due to alterations of surface-tension due to molecular transformations at certain points on the surface of globular or polyhedral embryonic cells. The final mature form of a cell is a consequence of the assumption of a statical equilibrium amongst its parts, due to the nature of its metabolism and its consequent molecular structure. The statogenetic factors of development are therefore of just as much importance as the kinetogenetic, or those involving motion. The statical forces that are developed in individual cells also act reciprocally between all of the cells of the organism, so that in this way the effect of statogeny extends throughout the entire organism.

If there were no such statical forces to be overridden by the purely kinetic ones developed by the molecular transformations and consequent motions incident to metabolism, provided the latter, together with assimilation, took place, during development, with great rapidity, the ontogeny of an organism would take place with such swiftness that it could not be successfully studied by embryologists. In other words, ontogeny would take place in the twinkling of an eye, and organisms as large as whales might even mature in an instant, provided the coefficients of viscosity and surface-tension of their plasma were to fall nearly to zero, while assimilation and metabolism proceeded with infinite rapidity.

It follows also from what has preceded that we can now form some idea why apparent rejuvenescence occurs in every on-

* Some interesting applications of the geometrical theory of radical axes and centers also apply here that have never been studied in connection with the phenomena of segmentation.

togeny. Every germ must, for assignable reasons, begin its existence in the original, highly complex, æolotropic condition of the plasma of its species. It must therefore begin its career somewhat in the guise of the mechanically unspecialized plasma of a remote unicellular ancestor. Unlike that ancestor, however, the cells that result from its growth and segmentation cohere until a multicellular aggregate results, the different regions of which fall into certain statical states in relation to one another and to the earth's centre, in virtue of the action of the forces of cohesion, friction, gravitation, etc. The different regions of such an aggregate now adjust themselves to the surroundings in such a way that nearly constant effects of light, heat, etc., begin to control or affect the functions of such an aggregate dynamically through its metabolism. Function, thus conditioned, asserts itself under the stress of mechanical adaptation or adjustment that becomes increasingly complex with every advance in ontogeny. Every step in ontogeny becomes mechanically adaptive and determinative of the next. It is thus only that we can understand the wonderful molecular sorting process that goes on in ontogeny, for which others have invoked infinite multitudes of needless 'gemmules,' 'biophors' and 'determinants.'

It is the whole organism that develops in continuity or coördination; not its nuclei, centrosomes, and asters only. The whole organism, molecularly considered, is as fixed and immutable, within variable limits, as a crystal. Its development, moreover, becomes intelligible only if we contemplate its ontogeny somewhat as we would the growth of a crystal, with the additional supposition that its growth is not conditioned by forces operating along straight lines having a constant angular divergence as in the latter. On the contrary, living matter is capable of developing curved bounding surfaces in consequence of the permanently mobile nature and cohesion of its molecules, that, as a complex dynamical mechanism, can operate so as to tell off the tale of its transformation in but one way, in consequence of the order and way in which the energy of its constituent molecules is set free during ontogeny. Upon the completion of ontogeny a phrase is reached in which the income and outgo of metabolism is in equilibrium. The duration of life depends upon the length of time that this equilibrium can be maintained without fatal impairment of the harmonious operation of its mechanism under the stress of the dynamical conditions of life. This may be considered the cause of death, so that the length of the life of the individual is determined by the possible number of harmonious molecular transformations of which its plasma is capable as a mechanism.

The doctrine that cells undergo differentiation in relation to other adjacent cells, or that the destiny of a cell is a function of its position (Driesch), is no doubt true. Nevertheless, we have in organisms machines of such complexity, dynamical potentiality, and power of transformation, that in comparison a study of the theories of crystallography is simplicity itself. In organisms we have the polarities of head and tail, stem and root, right, left, dorsal and ventral aspects, as definitely marked out as are the relations of the axes of crystals. In the organism we have diffuse, intussusceptional growth in three dimensions, by means of the osmotic interpolation of new molecules, whereas, in the crystal, growth is superficial, but consequently also tri-dimensional. In the organism the molecules are mobile within limits; in the crystal they are fixed. Nevertheless, we may justly regard organisms as developing after the manner of crystals, but with the power of very gradually varying their forms by means of variation in the structure, forms and powers of their constituent molecules, in the course of many generations of individuals.

This variation may be directed by the concurrence of a series of natural conditions operating dynamically (natural selection). Or, interbreeding and crossing, with care or under Nature, may unite by means of reciprocal integration (fertilization) two molecular mechanisms whose total structure and sum when thus united, as in sexual reproduction, may vary by the mere combination of the two dynamical systems (egg and sperm), differing slightly from one another in potentiality. Finally, adaptive changes may be called forth dynamically in the internal structure of such developing reciprocally integrated systems that must be traced back to changes in the mechanism of metabolism of the parent as well as in the germs it gives off. Such changes produced in the germ must become visible in the effects they produce, as transmitted formal changes exhibited in the course of development.

The tendency or trend of development, however, of a given form must be pretty constant, and controlled within comparatively narrow limits by the initial adult or attained structure. That is, what has been attained must formally affect that which is to be attained in future. This is the idea that underlies the *Vervollkommnungs-Princip*, principle of perfecting, of Nägeli. This view also tacitly recognizes the theory of change of function proposed by Dohrn, as well as the theories of substitution, superposition and epimorphosis of Kleinenberg, Spencer and Haacke. Once a condition of stable equilibrium has been reached in the series of transformation of the molecular mechanism represented by the germ, during the development of an organism, we may have what Eimer has called *Genepistasis*, resulting in the fixity or stability of an organic species, under stable conditions.

The cell is a complete organism, but it loses its physiological and morphological autonomy when combined with other cells. We may regard the nucleus, cytoplasm and centrosome as reciprocally related parts; one of them not much more important than the others. The observed behavior of the centrosome would indicate, as Verworn has held, that it is the important agent in cellular metabolism. If this is true, metabolism has certain centers in the cell to and from which molecular transformations are effected rythmically in every direction, with the centrosome as focal points. This view agrees perfectly with the facts, since the rays of the asters may be regarded as the morphological expression of a dynamical process of intermolecular diffusion due to metabolism, as Kölliker has suspected (*Gewebelehre*, 6th ed.).

Such a process would not only serve to alter the surface and interfacial-tensions of the cells during ontogeny, but also vary the osmotic pressure within them. Consequently, we may conceive that all of the phenomena of development, including the appearance and disappearance of cavities within a germ by changing conditions of osmosis, may receive a dynamical explanation. The centrosomes may, moreover, be conceived to lie at the foci of very complex material figures, the boundaries of which are finite equipotential cellular surfaces. These focal points are clearly near or within the nuclei. The equipotential surfaces developed by the sorting or readjusting process that goes on during segmentation in order continually and rythmically to restore the dynamical equilibrium of the molecular germinal aggregate as a mechanically constructed system during life and development, through growth and metabolism, must maintain the shapes of organisms as we see them. The epigenetic theory of inheritance therefore promises us a secure basis upon which to found a theory of the mechanics of development, as well as a theory of the origin of morphological types.

The theory of life may indeed be regarded as having its foundations in cellular, inter- and intra-cellular mechanics and dynamics as conditioned by ontogentic metabolism. The fact that centrosome, nucleus and cytoplasm are represented almost coextensively with the presence of life itself is proof that the fundamental machinery of organization must be the same in the principles of its action, no matter how widely its forms may differ from one another.

The theory that the surface layer of molecules of organisms, whether interior or exterior, are in equilibrium also carries with it the idea that the configuration of all organs and organisms are merely the material expression of gradually built up equipotential surfaces. This gives us a far more rational foundation for a theory of general morphology than the hypothesis of gemmaria proposed by Haacke. During growth and metamorphosis these equipotential surfaces undergo formal changes in size and shape, due to the internal processes of molecular transformation or metabolism. But such changes are continuous, and one stage or form passes into the next palpable one through an infinite number of slightly different forms. Examples of such surfaces may be seen in any organism, vegetable or animal, and at any stage of the same. The principle is therefore of universal application.

SUMMARY.—Preformation of any organism in the germ has no foundation in fact.

All that it is possible to account for upon the basis of a theory of preformation may be much more logically and scientifically accounted for upon the ground of dynamical theory. Such a theory must deny the existence of separate corpuscles or gemmules of any sort in the germ, whose business it is to control development. All that is required is the assumption of a determinate ultra-microscopic molecular mechanism, the initial structure of which determines all of its subsequent transformations. The present theory also denies that there is or can be anything passive in the germ that enters into its composition.

A dynamical hypothesis of inheritance is correlated with all the facts of physiology. It is in harmony with the dynamical theory of sex, that sees only in sexuality the means developed by another dynamical process (natural selection) that increases the powers of a compound germ to survive and vary. It is consistent with the facts of morphological super-position, with the dynamical theory of the limit of growth, and duration of life of organic species. It is also consistent with the view that the initial or potential states of the germs of species are those that must result whenever they are relieved from physiological service to the parent organism. The apparent continuity of germ-plasm is, in many cases, only an effect of the equilibration of the forces of the organism, and has no further significance. It must also deny any assumed isotropy of the germ as inconsistent with fact. It assumes that the æolotropy of the molecular structure of the germ is followed by a gradually increasing simplification of molecular structure of organs as these are built up. Metabolism is assumed to be the sole agent in effecting the mechanical and dynamical rearrangement or sorting of the molecules into organs during development. Specially endowed corpuscles or 'biophors' are not only needless as conditioning form or function, but also out of the question, dynamically considered. No creature can be supposed to have its life or germinal properties associated only with certain corpuscles within it, since we cannot suppose an organized whole dominated by a portion of it; it is not possible, for example, to conceive of individual life except from the entire organism that manifests it. There can be no 'biophors'—bearers of life; the whole organism must do that as an indivisible unit. Corpuscular doctrines of inheritance are merely a survival in philosophical hypothesis of a pre-Aristotelian *deus ex machina*. The dynamical hypothesis rejects the *deus ex machina*, but finds a real mechanism in the germ that is an automaton, but that is such only in virtue of its structure and the potential energy stored up within it. Every step in the transformation of such a mechanism is mechanically conditioned within limits by what has preceded it, and which in turn so conditions within limits what is to follow, and so on forever through a succession of descendants. The theory of equipotential surfaces, as here applied to organisms, leads to a theory of general morphology that holds of all living forms, and that is at the same time consistent with the facts of development.

EXPLANATORY NOTE TO PARAGRAPH ON PAGE 618.

It now appears that the statement that the quarters or eighths of an oösperm are to be regarded as 'molecular mechanisms of precisely the same type of potentiality' as the whole egg, must be taken with considerable qualification. Loeb (Ueber die Grenzen der Theilbarkeit der Eisubstanz, *Archiv für Ges. Physiologie*, vol. LIX., 1894) has shown that the eggs of echinoderms, if artificially divided, by means of a method of his devising, into quarters or eighths, lose the power of developing beyond the blastula stage. This would appear to indicate that if the egg is subdivided so as to have its parts fall below a certain size, these parts no longer have locked up within them, as molecular mechanisms, as Loeb points out, enough potential energy to transform themselves into completely equipped larvæ. Or, perhaps, the initial æolotropy of the egg does not permit of its subdivision into quarters and eighths without impairing their structure and powers of development.

My own recent experiments have shown that it is possible to incubate for some time the germ of the bird's egg outside of the egg-shell in a covered glass-dish. These experiments also show that restraints to growth developed by the dying of a film of albumen over the germ causes it to be most extraordinarily folded, with many abnormal tumor-like growths from both entoderm and ectoderm, that differ, however, in histological character from the cells of both these layers. These experiments also prove that it is possible to mechanically divide the germ of the warm-blooded Avian type into halves or quarters, and to have these continue to develop for a time.

The converse of the process of mechanical division of the germ we have in Born's remarkable experiments in cutting recently-hatched Amphibian embryos in two, and placing the separated halves again in contact under such conditions as to cause them to grow together, or even to thus graft the half of a larva of one species upon that of another. That such grafting is possible, I can testify, as a result of a repetition of some of the experiments. See Born's paper in *Schlessischen Gesellsch. f. väterländische Cultur: Medicinische Section*, 1894. pp. 13. Supplementing Born's results are Roux's experiments on *cytotropism*, or the reciprocal attraction of isolated blastomeres of Amphibian eggs (*Archiv f. Entwickelungsmechanik*, I., 1894), if brought close together, though at first not in actual contact. There is also some evidence of asexual *caryotropism* as witnessed in the conjugating nuclei of the cells of the intestinal epithelium of land-Isopods (Ryder and Pennington, *Anat. Anzeiger*, 1894).

The experiments of O. Schultze (*Anat. Anzeiger*, Ergänzungsheft zum Bd. IX., pp. 117–132, 1894), by very slowly rotating in a mechanically fixed position the segmenting eggs of Amphibians on a specially constructed clinostaf, with the result of disorganizing and killing them, show that such eggs are not isotropic. His production of double monsters in such ova by disturbing, for a time, their geotropic relations, is also significant, while his conversion of the meroblastic amphibian egg into a holoblastic, evenly segmenting one by merely rotating it through 180° out of its normal geotropic relation, and allowing it to complete its segmentation in an inverted position, proves that the egg can be made structurally homogenous by mere mechanical means, but at the expense of its power to complete its development. This is further proof that the egg is not isotropic in the sense in which that word is used by natural philosophers.

Since the appearance of the short but important paper by Prof. E. B. Wilson and A. P. Mathews (*Jour. of Morphology*, Vol. X., No. 1, 1895), in which they deny the existence of the centrosome, it becomes necessary for me to explain that the word 'centrosome' is used in the text in the sense in which they use the expression 'attraction spheres.' Their discovery that the ovocenter, or attraction sphere of the egg, disappears after the expulsion of the two polar cells in echinoderm eggs, to be replaced by the sperm-center, is of the greatest significance, and may explain the reason why parthenogenetic eggs develop, namely, as a consequence of their retention of an ovocenter. The new facts that these two able workers have disclosed are entirely in harmony with a dynamical theory of fertilization and sex.

JOHN A. RYDER.

SCIENCE IN CANADA.

The awakening from long indifference as to the constant wasting, from various causes, of the timber resources of this continent, which some dozen years ago gave rise to a series of forestry congresses, has produced a considerable mass of literature, mainly economic, but to some extent also scientific, in Canada as well as in the United States. Not only the Dominion, but the provincial authorities as well, took action on the matter for the purpose of at once arresting wanton destruction of still existing forests, of re-afforesting denuded areas and of planting trees in the scantily timbered region between the Great Lakes and the Rocky Mountains. Something has also been done in the introduction of varieties, for sanitary and ornamental uses, from the like climates of the Old World. The scientific societies have done their share in keeping alive the interest created by this far-reaching movement. The latest of the monthly meetings of the Natural History Society of Montreal was devoted to this subject, the Hon. J. K. Ward having read a comprehensive paper on 'Canada's timber resources and lumber industry.' Mr. Ward's paper was largely historical and economic. He gave an interesting sketch of the lumber business from the year 1667, when the first timber ship was despatched from Canada to Europe; spoke of the relations between lumbering and colonization and touched on the great wealth of precious timber growing in Canada west of the Rockies. The lecture was scientific indirectly only and in its suggestions.

In view of the agitation for the admission of the island of Newfoundland into the Dominion, it may be of interest to recall that Mr. B. L. Robinson and Mr. Hermann Schrenk, of Harvard University, made a botanical exploration last July and August through the Exploits Valley and other parts of that island. They obtained more than 7,000 specimens of flowering plants and vascular cryptogams, as well as (incidentally) a number of thallophytes. What is especially noteworthy, as parallel phenomena are well known in Canada, is that though the Exploits Valley is more than 200 miles north of St. John's it 'showed a richer and more advanced vegetation, indicative of a deeper soil and milder climate.' The report was published in the *Harvard Graduates' Magazine.*

A society that is destined to give a fruitful impetus to botanical research in the Dominion is the Botanical Club of Canada, which originated in a recommendation of the Fourth Section (Biology and Geology) of the Royal Society of Canada, at the annual meeting held in Montreal, in May, 1891. It is, however, entirely independent of that Society, with which it holds only the relations common to the other associated scientific societies of the Dominion. "The objects of the Club are to adopt means, by concerted local efforts and otherwise, to promote the exploration of the flora of every portion of British America, to publish complete lists of the same in local papers as the work goes on, and to have these lists collected and carefully examined in order to arrive at a correct knowledge of the precise character of our flora and its geographical distribution." This Club comprises Newfoundland (as does the Royal Society of Canada), not only in the scope of its operations, but by official representation. Prof. George Lawson, Ph. D., LL. D., of Halifax, N. S., is president; Dr. A. H. MacKay, B. Sc., Halifax, is general secretary-treasurer. Prof. D. P. Penhallow, B. Sc., McGill University, is secretary for the province of Quebec; Dr. J. A. Merton Wingham, for Ontario; Dr. A. H. MacKay, for Nova Scotia; Mr. G. U. Hay, M. A., Ph. D., St. John, for New Brunswick; Mr. Francis Bain, North River, for Prince Edward Island; Rev. A. C. Waghorne, St. John's, for Newfoundland; Rev. W. A. Burman, B. D., Winnipeg, for Manitoba; Mr. T. N. Willing, Calgary, for Alberta; Rev. C. W. Bryden, Battleford, for Saskatchewan; Mr. A. J. Pineo, B. A., High School, Victoria, for British Columbia. The foregoing officers were elected on the 25th of May, 1894.

An interesting report of the work of the year 1893–94 was presented at last year's May meeting of the Royal Society at Ottawa, and is published in the *Proceedings*. What is most striking in it is the evidence which it affords that the creation of the Society has proved an incentive to increased industry in field work in distant and out-of-the-way places—in Newfoundland (special attention being called to Mr. Waghorne's work), in the Territories, in British Columbia and on Prince Edward Island. In British Columbia 100 members had been enrolled through Mr. Pineo's efforts, and 1,400 species (of which 30 were new) collected under the direction of Prof. Macoun. In Nova Scotia the work was largely associated with phenological observations. Besides excellent local work, the operations in Ontario included a series of papers by Mr. James Macoun on the plants in the Herbarium of the Geological and Natural History Survey at Ottawa, which appeared in the *Canadian Record of Science*. In Quebec the most important work done was that of Prof. Penhallow, in the determination of the species of American Coniferæ by the structure of the stem, a research of recognized importance in the development of phanerogamic botany. In all the provinces the creation of the Club has already had a marked educational effect, the more intelligent teachers in many localities having engaged with energy in the work. Before the formation of the Club the only Canadian institution whose operations covered the Dominion was the Survey just mentioned, to the botanical work of which Mr. Robinson makes laudatory mention in his Exploits Valley report. In all the older provinces, however, there have long been scientific societies of whose objects botanical exploration formed a leading feature.

The gift by Mr. W. C. McDonald, of Montreal, of thirty-five acres of convenient and suitable land for the formation of a Botanic Garden in connection with McGill University, must very materially aid in the promotion of botanical research in Montreal and will prove a prized boon to Prof. Penhallow and his students. This gift, the deeds for which were formally signed on the 3d inst., is only one of many substantial proofs that Mr. McDonald has given of his interest in scientific education. At the convocation of the University on the 30th ult. the vice-principal was able to announce that, during the session just closing, the students had for the first time surpassed the thousand. That this augmentation is largely due to the increased attendance of the Scientific Faculties (medicine, comparative medicine and applied science) is an open secret. Ten years ago the attendance did not reach five hundred. As the vice-principal (Dr. Alexander Johnson) pointed out, increase of numbers, though desirable, is not the *summum bonum*. He hoped the time would come when all graduates would be first of all graduates in arts. Prof. Callendar, without decrying Latin or Greek, deprecated the neglect by scientific students of their mother tongue, which every student of science should be able to write correctly and clearly.

Professor Bovey, D. C. L., M. Inst. C. E., Dean of the Faculty of Applied Science, after saying that the students enrolled in his Faculty this year numbered 187, an increase of 15 per cent. over the previous year, mentioned among recent improvements a course in Kinematics (Professor Nicholson); the addition of practical mining and underground surveying to the course in Mining Engineering (Professor

Carlyle); the establishment of graduates' courses and arrangements made to facilitate the prosecution of research work, so as to take advantage of the splendid equipment for that end now possessed by the University. This consists of laboratories of mathematics and dynamics, fully provided with instruments of measurement, gravity balances, machines for experimenting on the laws of motion, etc.; three chemical laboratories for qualitative and quantitative work and for original investigation, and supplied with Becker & Son (4) and Bunge (1) balances; a Trœmner bullion-balance; a Laurent polariscope, Dubosq spectroscope, etc.; the McDonald physical laboratory of five stories, each 8000 square feet area, including elementary and special laboratories for heat and electricity; rooms for optical work and photography; two large laboratories arranged for research, with solid piers and the usual standard instruments, etc.; the electric laboratory, with Kelvin electric balances, a Thomson galvanometer, two dynamo-meters (Siemens), voltmeters, ammeters, etc.; the magnetic laboratory, the dynamo room, the lighting station, the accumulator room, geodetic, hydraulic testing, thermo-dynamic and mechanical laboratories. The McDonald Engineering Building and its equipment were the gift of the same generous friend of scientific education whom McGill University has just thanked for its botanic garden. Mr. McDonald also contributed liberally towards the erection of the workshops built on the endowment of the late Thomas Workman, merchant, of Montreal. These consist of machine shop, foundry, smith shop and carpenter, wood-turning and pattern-making departments, and are intended, under the direction of the professor of mechanical engineering, to familiarize the student with the materials and implements of construction.

Although Prof. Milne (whose recent loss every friend of science deplores) and other seismologists are wont to class the earth movements of the United States and Canada under a common head, Canada has had a fair proportion of such disturbances all to herself. Every student of Canada's annals has had his attention drawn to the series of earthquakes which caused such consternation in the year 1663, and its extraordinary moral effects. On the 17th ult. a shock varying from severe to slight or barely perceptible was felt on both sides of the St. Lawrence, though mainly on the south side in what are called the Eastern Townships. Nearly two years ago a somewhat similar shock was felt, and nearly at the same hour, between eleven and noon. This earthquake was distinctly felt in Montreal. The most formidable visitation of the kind in recent times occurred twenty-five years ago. It cleared even the court rooms and filled the streets with frightened groups.

The Royal Society of Canada met at Ottawa on the 15th inst. A programme of considerable scientific interest was gone through.

The death of Mr. Walter H. Smith, well known in Montreal for more than twenty years as an astronomer and publisher of Smith's Planetary Almanac, is sincerely regretted by all who knew him. He was for many years connected with the Montreal *Witness*, in which paper his contributions on astronomical subjects were always read with interest, and were widely reproduced. He died on the 3d inst., in his forty-third year. He was a native of Wiltshire, England, but had lived more than half his life in Canada.

J. T. C.

CARL LUDWIG.

Within a few months Germany and the world have lost three great men, Helmholtz, Freytag and Ludwig. Of these three Carl Ludwig, the physiologist, and the intimate friend of the other two, died in Leipsic on April 27th, 1895, at the age of seventy-eight, after a life rich in scientific achievement.

The world at large can never realize the great debt that the world of science, and through it the world at large, owes to the tireless brain and the skilful hand of this modest Leipsic professor. Ludwig combined, in an almost ideal manner and inseparably, great investigating power and great teaching power. An investigator himself, throughout the course of his busy life he trained between two and three hundred investigators, and more than any other man since Johannes Müller he has directed the course of physiological research. The numberless publications from his laboratory bear the names of his pupils and rarely his own, but the inscription, 'Aus dem physiologischen Institut zu Leipsic,' is the seal of their worth.

Ludwig was a man of the broadest sympathies and culture, restless and eager for knowledge within or without the boundaries of his own science. But he was content to study specific problems and to refrain from baseless and sweeping hypotheses. In the fifty-three years of his constant labor he left untouched few fields of the physiology of his time, and he never delved lightly or superficially. A record like his is rarely equalled. To the end he maintained his interest and activity fresh, and at the age of seventy-five he wrote to an American friend, "Ueberall liegt so viel brach, überall giebt es so viele Lücken, dass man bald mehr Aufgaben als Kräfte besitzt."

It was a memorable day for biology when Ludwig conceived the idea of the kymograph, the instrument used for recording physiological movements, for the invention of the kymograph marked the introduction of the graphic method into physiology. Ludwig once wrote, "Observation and experiment alone bring the light that illuminates the secret ways of nature." The graphic method has made observation and experiment exact, and has revolutionized the biological sciences. Ludwig is responsible for much of the apparatus of precision now in use in physiological laboratories. To him must be ascribed also the fruitful method of separating single organs from the rest of an animal body, and maintaining them for study in a vital condition, a process indispensable to the understanding of function in a complicated organism.

Besides these additions to method, among the more noteworthy of his many contributions to physiology, either alone or in conjunction with his pupils, may be mentioned: numerous facts and principles regarding the dynamics of the circulation of the blood; the details of the heart's action; the location of the vaso-motor centre; the discovery of the depressor nerve; the mutual relations of respiration and circulation; the blood gases; many anatomical and physiological advances regarding the lymphatic system; the secretory function of the chorda tympani nerve; the mutual relations of gland secretion and blood circulation; gas exchange and production of heat in tissues; the presence of inosit, uric acid and other substances in the animal body; numerous facts regarding the metabolism of specific tissues; the course taken by the food-stuffs in absorption; the minute physiological anatomy of the kidney, the liver, the intestine, the pancreas, the salivary glands, the heart, the skin, etc.; many facts regarding general muscle and nerve physiology, the central nervous system and the special senses.

The leading events in Ludwig's life are as follows: Carl Friedrich Wilhelm Ludwig, the son of a Hessian army officer who served in the Napoleonic wars, born in Witzenhausen December 29th, 1816; studied in Erlangen and Marburg; M. D., Marburg, 1839; prosector in anatomy, Marburg, 1841; privat-docent in physiology, Marburg, 1842; extraordinary professor of comparative anatomy, Marburg, 1846; professor of anatomy and physiology, Zürich, 1849; professor of physiology and zoölogy, Vienna, 1855; professor of physiology, Leipsic, 1865.

Probably few American physiologists received the news of Ludwig's death without a feeling of sadness far beyond that occasioned by the loss to science. Ludwig liked America and Americans, and many of his colleagues upon this side of the Atlantic have been his pupils and have found in him a warm personal friend. His wit, his sympathy, his breadth of mind, his love of books and of music, were conspicuous. To work with him was to receive the undying stimulus of a master mind and to feel the charm of a simple, sweet, winning personality.

FREDERIC S. LEE.

COLUMBIA COLLEGE.

CORRESPONDENCE.

THE FROG WAS NOT BRAINLESS BUT DECEREBRIZED.

IN the report of the meeting of the Association of American Anatomists last December in SCIENCE for March 15, 1895, p. 297, it is said that 'Dr. Wilder exhibited a Brainless Frog, etc.' The animal shown had been deprived of his cerebrum Dec. 7, 1894, for demonstration to my class in physiology of the points first, I believe, observed by Goltz. The brain was transected at the diencephal (thalami) and the entire cerebrum removed as described by me in 1886.* The frog was unusually large and vigorous, and was exhibited partly on that account, and partly because when it dies the condition of the brain will be determined and reported to the Association. At this writing, however, it is still living and has been photographed in various attitudes, amongst others while maintaining its balance on a cylinder *by 'backing' instead of going forward as usual.*

The object of the present note is to reprobate the use of *brainless* and *decerebrized* as interchangeable terms. The latter alone was used by me at the meeting, and was accessible in type-writing to all who were present. Nevertheless, both at that time and afterward, there appeared many newspaper paragraphs as to 'Dr. Wilder's brainless frog.' An attempt to correct the misapprehension through the Associated Press only made the matter worse, for I was promptly credited with 'another brainless frog.'

Perhaps, however, we ought not to condemn the popular confusion of terms too strongly in view of the following example among professional anatomists. At the Tenth International Medical Congress in Berlin, August 5, 1890, Professor Sir William Turner, F. R. S., etc., delivered an address, the official title of which, as printed in the *Journal of Anatomy and Physiology* for October, is 'The Convolutions of the Brain;' the real subject is The Fissures of the Cerebrum.

BURT G. WILDER.

ITHACA, N. Y., May 25, 1895.

* Remarks upon a living frog which was decerebrized more than seven months ago. *Amer. Neurol. Assoc. Trans.*, 1886. *Jour. Nerv. and Mental Dis.*, XIII., p. 30. (Abstracts in *N. Y. Med. Record*, July 31, 1886, SCIENCE, Aug. 7, 1886, and *Medical News*, Aug. 7, 1886.)

TEXT-BOOK OF INVERTEBRATE MORPHOLOGY.

TO THE EDITOR OF SCIENCE: A reply to a book review is undoubtedly in many cases inadvisable, but there are certain statements in the review of my Text-book of Invertebrate Morphology in your issue of May 3d which seem, as a matter of justice, to call for some comment. A reviewer has a perfect right to express his opinion concerning the views set forth by an author, but the latter has a right to expect that his statements will not be misrepresented either directly or by implication, and I wish to call attention to certain misrepresentations contained in Professor Packard's review.

In the first place the following statement is made: "Thus in writing of the Brachiopoda the author speaks of the bivalved shell 'similar to that of the bivalve mollusk,' but he does not add that the shells are dorsal and ventral, a point in which they differ from any mollusk." Professor Packard must have read my description of the Brachiopoda very perfunctorily; otherwise he would have seen fifteen lines further on the statement: "Since the mantle-lobes are dorsal and ventral in position, so too are the valves of the shell," and a little further on still he would have found an express statement that there are important differences between the shells of the Brachiopods and those of the bivalve mollusks.

Secondly, it is implied in the review that I state that the thoracic segments in the butterflies and Diptera 'seem to be reduced to two, etc.' If my entire statement had been quoted my meaning would have been clear. The concluding words of the sentence, replaced in the review by 'etc.,' reading 'owing to the close association of the metathorax with the first abdominal segment.' The reviewer implies that I state that but two segments occur in the insects mentioned, whereas I distinctly imply that all three are present.

Thirdly, the reviewer implies that I state on p. 414 that the elements of the ovipositors (in insects) are situated on the 'last abdominal segment.' As it happens at p. 414, it is the Isopods, and not the Insecta, which are under consideration. My statement regarding the ovipositors of insects are: (1) "Cerci, ovipositors and copulatory organs are frequently borne by the posterior abdominal segments" (p. 489); (2) "The genital orifice is situated on the ventral surface of the ninth abdominal segment and is usually surrounded by a number of papillæ, or sometimes by long processes which serve as ovipositors, and are to be regarded simply as processes of the segments from which they arise, and not as modified limbs" (p. 497). In both cases I use the word 'segments' and not 'segment,' and in neither case do I state that the ovipositors are on the last segment.

There are several other points which might be similarly commented upon, but I do not desire to occupy space by multiplying examples of inaccuracies in the review. Surely, in the review of a scientific book evidence of ordinary care in the preliminary perusal of it is to be expected.

Yours truly,

J. PLAYFAIR McMURRICH.

UNIVERSITY OF MICHIGAN, May 7th, 1895.

[IN reply to Professor McMurrich I regret to say that I did overlook the words on p. 269, to which he draws attention, although I still think the dorsal and ventral relations of the valves had better have been emphasized in the beginning of the last paragraph of the preceding page. In regard to the second point, I still think that the expression 'seem to be reduced to two' is unnecessary and a grain misleading. Third, on p. 489 ('p. 414' is a printer's error, for which the reviewer is not responsible) the sentence in question still seems to me to be vague, inexact, and in part incorrect. The cerci *are* the homologues of the other jointed appendages of the body, as may be seen in the cockroach and other orthoptera, as well as Lyda, and the Cinura (Machilis). This and the few other errors noted by us are blemishes which can easily be corrected in a second edition. The charge that 'ordinary care' was not exercised by the reviewer is a gratuitous one. In conclusion, I may say that I regard the book as a most excellent and useful one, and wish it every success, as it fills a vacancy hitherto existing in our literature.

A. S. PACKARD.]

SCIENTIFIC LITERATURE.

The Mechanical Engineer's Pocket Book. By WM. KENT. New York, J. Wiley & Sons, 1895. 168 illustrations, pp. xxxi.; 1087, 16 mo. $5.00.

This 'pocket-book,' although altogether too large for the pocket—as are, in fact, all these books, when meeting the requirements fully—is the most important and valuable accession to the portable library of the engineer that has recently appeared. Its scope is purposely confined to those subjects which are of main interest to the mechanical engineer, including the electrical engineering branch, and matters assignable to civil engineering, distinctively, are omitted; it being assumed that the interested engineer will find them in his 'Trautwine.' The author of the new 'pocket-book' is a distinguished engineer and metallurgist, and has had a peculiarly fruitful and fortunate variety of experience in those departments. He supplemented a mercantile education and some experience with a course of study in mechanical engineering, and subsequently had charge of iron and steel works in Pittsburgh, edited a technical journal, was the responsible laboratory assistant in charge of important work of the 'United States Iron and Steel Board,' and has enjoyed a most unique and helpful experience as a consulting engineer. No one could better comprehend precisely what is demanded of the author of such a book.

Throughout a period of now many years material was in process of accumulation, as advised by Nystrom: 'Every engineer should make his own pocket-book.' The construction of the book in hand was commenced at the request of the publishers, who selected the presumably best prepared man for the work, and the result of four years of labor is an admirable, an extensive and a 'meaty' volume.

The section devoted to the materials of engineering, their strength and their properties, is peculiarly valuable and complete. It is a department in which the author is thoroughly at home and with which he has all his life been familiar. The revision of the old formulæ and their constants has been very carefully and completely performed, and this work in itself constitutes a great boon to the engineer. The wide range of difference of proportions of parts of engines and machines observed among contemporary builders and 'authorities' has been the subject of long and conscientious labors. When it is said that sizes of important parts, in the best practice, for 'low-speed' and 'high-speed' engines, respectively, average as four in the one to seven in the other, and when it is known that variations of ten to one, in certain proportions of parts, among well-known makers, are known to exist, the importance of this revision becomes appreciable.

Experimental data are collated to date, and in immense quantity in all departments, and the theory of construction, as far as required and appropriate to such a book as this, has been well condensed and revised, not only by the author, but by specialists whose aid has been sought by him.

The book is especially rich in matter relating to the steam-engine and steam-boilers, stationary, marine and locomotive, and a moderate amount of space is well utilized by a very condensed resumé of principles and practice in electrical engineering. It may perhaps be fairly anticipated that this section will grow somewhat with the rapidly succeeding editions of the book which, it is safe to predict, will follow. Refrigerating machinery here, for the first time, finds space in some degree commensurate with its growing importance, and theory and practice are judiciously presented with data derived by the best experiments yet reported.

This book has more importance, and deserves much more space, than so incomplete a notice would indicate; but it is only practicable here to give the briefest possible indication of its contents, and to advise everyone interested in the subjects treated to examine the work and judge it for himself. Mr. Kent and his publishers —who have put up the book in excellent shape in all respects—are to be heartily congratulated on the outcome of their long struggle with the most difficult task that authorship knows—the condensation of a a great mass of useful special information into manageable and compact form. The product of their efforts is a mechanical engineer's pocket-book covering the field with remarkable completeness, correct as to theory, rich in data, supplying all the tables, 'constants of nature,' and results of scientific research in its department, required by the practitioner, and in marvellously compact form.

In size, type, paper and presswork, binding and finish, the book is fully up to the established standard for such publications. It seems remarkably free from printers' and other errors—although it must undoubtedly fail of absolute perfection in this respect in a first edition—and is a credit to all concerned in its production. It is a great work well done.

R. H. THURSTON.

Birdcraft, a Field Book of Two Hundred Song, Game and Water Birds. By MABEL OSGOOD WRIGHT. Pp. 317. 15 double plates, mostly colored. New York and London, Macmillan & Co. 8°. May, 1895. Price, $3.00.

On opening Mrs. Wright's *Birdcraft*, fresh from the press, one is likely to exclaim 'what horrible pictures!' and wonder how a reputable publisher or author could permit such atrocious daubs to deface a well printed book. But in spite of these staring eyesores, which certainly prejudice one against the work, the text contains much of interest and, taken as a whole, is well written. The spirit of the book is in touch with the popular and growing fashion of studying birds in the field, and its chief purpose seems to be to interest the novice and aid in identifying birds 'in the bush.' It contains introductory chapters on 'the spring song,' 'the building of the nest,' 'water birds,' 'birds of autumn and winter,' and 'how to name the birds.' The book proper begins with a 'synopsis of bird families,' followed by popular descriptions and short biographies of 200 species—mostly well-known eastern birds—and ends with keys for the ready identification of males in spring plumage. The utility of such keys can be tested only by actual use. These are simple and look as if they would be helpful to the beginner, though it almost takes one's breath away to find the robin classed with the cardinal and tanager under 'birds conspicuously red.'

Most of the biographies are based on the author's field experience in southern Connecticut, and as a rule are interesting and accurate. Now and then misleading statements creep in, particularly with reference to the geographic ranges. For instance, the white-eyed vireo, chat, orchard oriole, and other Carolinian birds are said to inhabit the 'eastern United States,' while, as a matter of fact, they are absent from the northern tier of States and New England, except in the southern parts. Other surprising statements may be traced to popular prejudice. Thus the author says of the Blue Jay: "Here is a bird against whom the hand of every lover of song-birds should be turned * * * for the Jay is a cannibal, not a whit less destructive than the crow. * * * Day by day they sally out of their nesting places to market for themselves and for their young, and nothing will do for them but fresh eggs and tender squabs from the nests of the song-birds; to be followed later by berries, small fruit and grain." The same sweeping ignorance and prejudice characterizes her account of the crow, of which she says: "This is another bird that you may hunt from your woods, shoot (if you can) in the fields and destroy with poisoned grain. Here he has not a single good mark against his name. He is a cannibal, devouring both the eggs and young of insect-destroying song-birds." As a matter of fact, the eggs and young of wild birds and poultry together form less than one per cent. of the food of the crow, as determined by the examination of about a thousand stomachs in the U. S. Department of Agriculture. So with grain; sprouting corn forms only two per cent. of the entire food, most of the corn eaten by crows being waste grain picked up, chiefly in winter, in fields and other places where its consumption is no loss to the farmer. On the other hand, mice and other injurious mammals form $1\frac{1}{2}$ per cent. of the food of crows; and insects no less than $23\frac{1}{2}$ per cent.

The colored plates are execrable. Most of them are cheap, coarse, dauby caricatures, taken second-hand from Audubon, who would turn in his grave if he saw them. In addition to these, there are five uncolored process reproductions of water birds and birds of prey. The latter are from Dr. Fisher's *Hawks and Owls of the United States* (published by the U. S. Department of Agriculture) and, though poor, are by far the best illustrations in the book.

Excepting the plates, the book is neatly gotten up and well printed. A novel and useful feature is the insertion of the common name of the bird in heavy-face type at the top corner of the page, in the place usually occupied by the pagination.

On the whole, Mrs. Wright's '*Birdcraft*' may be recommended as a source of pleasure and assistance to the many lovers of nature who are trying to learn more about our common birds. C. H. M.

Anleitung zur Microchemischen Analyse: Von H. Behrens, Professor an der Polytechnischen Schule in Delft. Mit 92 Figuren im Text. Hamburg, Leopold Voss. 1895. 224 pp.

Professor Behrens first wrote this book in French, and it was published in 1893. An English translation by Professor Judd appeared soon after. That the author published a German edition so soon speaks for the value of the book. Professor Behrens' text-book is the only one, as indeed he is the chief authority, on this new and important subject. The first half of the book describes the reactions of the elements, giving plates of the crystalline precipitates as seen through the microscope. Part Second treats of the systematic analysis of water, rocks, ores, alloys, and compounds of the rare elements. The chapter on the micro-chemical examination of rocks, by study of slides and of powdered rock is very interesting; indeed, for petrographic research the manual is invaluable, but it is also of great value to the metallurgist in the study of ores and alloys, and to the general chemist in the ordinary run of chemical analysis. E. Renouf.

NOTES AND NEWS.

THE AMERICAN ASSOCIATION.

The preliminary announcement of the forty-fourth meeting of the American Association for the Advancement of Science, to be held in Springfield, Mass., August 28 to September 7, 1895, has now been issued. The arrangements promise an interesting and successful meeting.

The first general session will be held on the morning of Thursday the 29th. This will give Friday, Monday, Tuesday and Wednesday as the four days entirely devoted to the reading of papers in the sections. Saturday will be given to excursions in the vicinity of Springfield, and more distant excursions have been arranged to follow the close of the meeting.

At the first general session the President-elect, Prof. E. W. Morley, will be introduced by the retiring President, Prof. D. G. Brinton. Addresses of welcome will be made by his Honor, Mayor Chas. L. Long, and Hon. Wm. H. Haile, President of the Local Committee.

The addresses of the vice presidents before the sections are as follows:

W. LE CONTE STEVENS, before Section of Physics: *The Problem of Aerial Locomotion.*

F. H. CUSHING, before Section of Anthropology.

JED. HOTCHKISS, before Section of Geology and Geography: *The Geological Survey of Virginia, 1835–1841. Its history and influence in the advancement of Geologic Science.*

B. E. FERNOW, before Section of Economic Science and Statistics: *The Providential Function of Government in relation to natural resources.*

MCMURTIE, before Section of Chemistry.

J. C. ARTHUR, before Section of Botany: *The Development of Vegetable Physiology.*

WILLIAM KENT, before Section of Mechanical Science and Engineering.

In the evening the address of the retiring President, DR. DANIEL G. BRINTON, on *The Aims of Anthropology* will be given, followed by a reception by the Ladies' Reception Committee of Springfield.

The affiliated societies meeting in conjunction with the Association are:

The Geological Society of America; August 27 and 28. PROF. N. S. SHALER, Cambridge, *President;* PROF. H. L. FAIRCHILD, Rochester, *Secretary.*

Society for Promotion of Agricultural Science; August 26. PROF. WILLIAM SAUNDERS, Ottawa, *President;* PROF. WILLIAM FREAR, State College, Pa., *Secretary.*

Association of Economic Entomologists.

Association of State Weather Service. MAJ. H. H. C. DUNWOODY, Washington, *President;* JAMES BERRY, Washington, *Secretary.*

Society for Promoting Engineering Education; September 2, 3, 4. GEO. F. SWAIN, Boston, *President;* PROF. J. B. JOHNSON, St. Louis, *Secretary.*

American Chemical Society; August 27 and 28. EDGAR F. SMITH, Philadelphia, *President;* PROF. ALBERT C. HALE, Brooklyn, *Secretary.*

American Forestry Association; September 3. HON. J. STERLING MORTON, Washington, *President;* F. H. NEWELL, Washington, *Secretary.*

The Botanical and Entomological Clubs of the Association will meet as usual during the Association week.

For information relating to membership and papers PROF. F. W. PUTNAM, *Permanent Secretary*, Salem, Mass., should be addressed. For all matters relating to local arrangements, hotels, railway rates and certificates, MR. W. A. WEBSTER, *Local Secretary, A. A. A. S.*, Springfield, Mass., should be addressed.

THE BRITISH ASSOCIATION.

THE arrangements are now completed for the meeting of the British Association, to be held at Ipswich from September 11 to 19, under the presidency of Sir Douglas Galton. The following is the list of sectional presidents nominated by the Council: Section A (Mathematical and Physical Science), Professor W. M. Hicks, of Firth College, Sheffield; B (Chemistry), Professor R. Meldola, of the City and Guilds Technical College; C (Geology), Mr. W. Whitaker, of the Geological Survey; D (Zoölogy, including Animal Physiology), Professor W. A. Herdman, of Liverpool University College; E (Geography), Mr. H. J. Mackinder, Reader at Oxford; F (Economic Science and Statistics), Mr. L. L. Price, Bursar of Oriel College, Oxford; G (Mechanical Science), Professor L. F. Vernon Harcourt, of University College, London; H (Anthropology), Professor W. M. Flinders Petrie, of University College, London; K (Botany), Mr. W. T. Thiselton-Dyer, Director of the Royal Botanic Gardens, Kew. The new President will deliver his inaugural address on September 11th. The two evening discourses will be given by Professor Silvanus Thompson, on 'Magnetism in Rotation,' and by Professor Percy F. Frankland, on 'The Work of Pasteur and its Various Developments.' There will be, as usual, two soirées, and also excursions to places of interest in the neighborhood of Ipswich.

MECHANICAL INTERPRETATION OF VARIATIONS OF LATITUDE.

Under this title, in No. 345 of the Astronomical Journal, Professor R. S. Woodward seeks to deduce the law of variations of latitudes from dynamical considerations. Starting with the hypothesis that the earth is a body of variable form, the general differential equations of rotation of such bodies are derived by means of the Lagrangian method. These equations are then shown to admit of considerable simplification when applied to the earth by reason of the fortunate circumstances that the variations of latitudes are very small, and that the principal moments of inertia of the earth vary exceedingly slowly. The integrals of the resulting equations give the rectangular coordinates of the instantaneous pole of the earth with respect to its pole of figure. The characteristic motion of the instantaneous pole is found to be the resultant of three distinct parts, namely, motion in a circle about the pole of figure with two series of elliptical motions superposed. This characteristic motion is subject, nevertheless, to some fluctuations arising from volcanic and similar impulsive disturbances, as well as from irregularities in meteorological processes.

The general features of latitude variations thus deduced from a purely theoretical basis agree with those arrived at inductively by Dr. Chandler in his elaborate researches. Only one difficulty, in fact, seems to stand now in the way of a satisfactory accordance of theory and observation, and that is the prolongation of the period of the Eulerian cycle from 305 to 428 days. A considerable amount of space is devoted by the author to a discussion of this difficulty. He attacks the validity of the method of deriving the period of that cycle from the ratio furnished by precession, and concludes that the period so derived 'can no longer be maintained as a dogma of dynamical astronomy.' Of several causes which may modify this period, he considers the principal one to be the tide entailed by the motion of the instantaneous axis of rotation about the axis of figure. The order of magnitude of such a cause essential to account for the discrepancy is shown to be very small. The main object of the paper, however, is to obtain a correct specification of the analytical form of the variations in question, leaving to observation and subsequent investigation the determination and reconciliation of the constants which enter that form.

THE MISSOURI BOTANICAL GARDEN.

The Sixth Annual Report of the Missouri Botanical Garden,* issued on May 3rd, is an octavo volume of 134 pages, with 6 halftone views taken in the Garden, and 56 plates illustrating plants described in the report.

The report of officers of the Board of Trustees shows that the receipts for the year 1894 were $95,555.97, and the disbursements $75,800.69, of which $35,483.39 was spent on the maintenance and improvement of the Garden and the performance and publication of scientific work; $3,692.29 was for banquets, exhibition premiums, a

* *Missouri Botanical Garden.* Sixth Annual Report. St. Louis, Mo., Published by the Board of Trustees. 1895.

sermon on flowers, and other designated annual bequests of the founder of the Garden, the late Henry Shaw; $21,334.85 went for taxes, and the remainder for office and other expenses incident to the administration of the trust. The report shows an invested or cash reserve of $35,405.03.

From the report of the Director it appears that the herbarium was increased by the addition of 9,307 sheets of specimens, making a total of 231,527 sheets; and 752 books and 1,165 pamphlets were added to the library, making a total of 7,631 books and 9,822 pamphlets. Attention is called to the establishment of a 'Henry Shaw medal for the introduction of a valuable plant,' open to competition in any line of decorative horticulture at the annual flower show held in St. Louis, and to the provision now made for receiving additional pupils in gardening at the nominal charge of $25 per year for tuition.

The scientific papers, which constitute the bulk of the volume, consist of a revision of North American species of Sagittaria and Lophotocarpus, by Jared G. Smith, with habit and detail illustrations of all of the species; a study of Leitneria Floridana, by William Trelease, illustrated by 15 plates showing the structure of this curious tree, the wood of which has a specific gravity of only 0.207, which is much lower than that of any other described wood, or even cork (0.240); studies of the dissemination and leaf reflexion of Yucca aloifolia, made in Florida, by Herbert J. Webber, and illustrated by three plates; notes and observations on new or little known species, by Jared G. Smith, accompanied by nine plates, and describing six new species from the Southwest; and notes on the interesting mound flora of Atchison county, Missouri, by B. F. Bush.

THE ROYAL ASTRONOMICAL SOCIETY.

At the last meeting much of the interest of the evening centered in a comparison of two photographs of a well-known nebula—that near 15 Monocerotis—the one by the American astronomer, Professor E. E. Barnard, with a six-inch portrait lens, the other by Dr. Roberts, of Crowborough, with his 20-inch reflector. The exposures given and the ratio of aperture to size of image were practically the same in both cases. But the results were very different. Dr. Roberts' photograph showed a great amount of very delicate and beautiful detail in the nebula; Prof. Barnard's, when enlarged to the same scale, was of a much coarser character, but traced the nebula over a wider area. Dr. Roberts argued strongly against the reality of these faint extensions of the nebula shown in Professor Barnard's photograph, but the president showed, by a detailed comparison of the two photographs projected on the screen, that the contention was unfounded, and that the smaller instrument, though inferior to the larger for the exhibition of minute detail, had decidedly the advantage in the detection of faint nebulous masses. Another photograph by Dr. Roberts of the well-known crab nebula in Taurus also gave rise to some discussion, as it differed from the drawing made of the nebula by the late Lord Rosse in 1844. Mr. Chambers, however, pointed out that later visual observations had thrown doubt on the reality of some of the filaments shown in the sketch referred to. A paper from Professor Barnard gave a most convincing proof of variation having occurred in a nebula, that known as Hind's, in Taurus. Mr. Newall presented some recent observations of Phobos, the inner satellite of Mars, showing that the orbit of the satellite was distinctly elliptical, and the ephemeris some ten minutes in error. Mr. Stanley Williams contributed a very remarkable paper showing that spots in different longitudes of Saturn had different rotation periods. Mr. Wilson, of West Meath, described his method of determining the heat radiation from the nucleus of a sun spot.—*London Times.*

GENERAL.

President Cleveland has extended the Civil Service Rules in the Department of Agriculture so as to include all officers and employees, excepting the Secretary and the Assistant Secretary of Agriculture and their private secretaries, the Chief of the Weather Bureau and his private secretary, the chief clerk of the department, and the laborers and charwomen.

In congregation at Oxford a new statute has been promulgated, adding anthropology to the list of subjects in the Honor School of Natural Science.

A memorial tablet to John Couch Adams, the Cambridge astronomer and mathematician, was unveiled in Westminster on May 9th.

The province of Ontario is to have a great reservation for the preservation of its animals and plants. The Algonquin Natural Park will comprise about a million acres of forest land. No hunting, trapping or destruction of animal life will be allowed within its precincts.—*American Naturalist.*

It is announced that the sum of $250,000 or more has been given to the University of the City of New York for the purpose of erecting a central building on University Heights to contain the library, commencement hall, museum, and offices of administration. In accordance with the wish of the donor the name is not announced.

Maj. J. W. Powell is announced as lecturer on the 'History of Culture,' and Prof. Otis T. Mason as lecturer on the 'Origin of Culture,' in Columbian University.

According to the accounts of Oxford University for 1894, recently presented to convocation, the revenue amounted to £63,760 and the payments to £64,390.

Sir George Buchanan, M. D., F. R. S., died on May 3d, at the age of 64. He was one of the first medical officers of health in London, having been appointed to St. Giles's in 1856. He originated methods of inquiry in sanitary matters not before attempted, working at the relation of overcrowding and other insanitary conditions of disease, at the prevention of smallpox, typhus fever, cholera and consumption, and originating a system of collecting statistical information of the public health of the district. He was chairman of The Royal Commission on Tuberculosis, which reported shortly before his death.

The death is announced of Mr. Arthur Edward Durham, a member of the Council and late vice-president of the Royal College of Surgeons, of England. He was the author of *Sleeping and Dreaming* and *The Physiology of Sleep.*

Francis P. Harper announces *The Expeditions of Zebulon Montgomery Pike* to the headwaters of the Mississippi River, the interior parts of Louisiana, Mexico and Texas, in the years 1805–6–7, reprinted in full from the original Philadelphia edition of 1810, with full explanatory, geographical and scitific notes to the text, compiled from many unpublished sources of information and including the results of a canoe voyage of the editor to the sources of the Mississippi River, a new memior of Pike and an index to the whole by Dr. Elliott Coues.

An Austrian expedition for polar research under the direction of M. Julius von Payer will start for Greenland in June, 1896.

At the recent meeting of the Boston Scientific Society it was stated that Dr. Percival Lowell would observe the opposition of Mars in December, 1896, from a suitable location, not yet decided on. For this purpose a telescope of twenty-four inches' aperture has been ordered.

The Spectacle Makers' Company recently presented Mr. W. H. M. Christie, Astronomer Royal of England, with its honorary freedom. The Master, in opening the ceremony, said that the spectacle makers claimed to be identified with those trades which, by the instruments they made, notably telescopes, microscopes, compasses, &c., enabled astronomers to pursue their studies and researches. In his reply, the Astronomer Royal said he could not but acknowledge what had been done for astronomy by opticians. It was true that a great deal was done by the early astronomers with very inefficient means. He might particularly mention Tycho Brahe, who, coming after the Greek, Chaldean and Jewish astronomers, besides others, had made great advances without the aid of the telescope. Astronomy and astrology continued to be one science up to the time when the telescope was invented.—*London Times.*

Dr. Morris Henry, a well known surgeon, died recently in New York at the age of seventy. He was the founder and editor of the *American Journal of Dermatology.*

The May *Forum* contains an interesting article by Professor R. H. Thurston on *Our Debt to Inventors—Shall We Discharge Them?* Professor Thurston says: "The promotion of the arts and manufactures by suitably rewarding inventors and providing that they shall be permitted to collect profits, as in all other departments of business, as large as the business will yield, and in due proportion to the value to the country of the invention or discovery, is one of the most important features of an enlightened public policy; and it is the duty of every intelligent and patriotic citizen, and especially of every one in any manner connected with any department of engineering, of manufactures, or of the mechanic arts, to exert every power and to apply all his influence to promote the perfecting of the patent system, to increase the facilities of the Patent Office, and, especially, to insure the inventor of new and valuable devices a liberal period of possession of the products of his genius."

The Microscopical Society of Washington held recently its annual exhibition. A large number of microscopical specimens and microscopes were exhibited.

Professor O. C. Whitman was announced to lecture on *The Utilities of Biology* at Mount Holyoke College on May 28th.

In the Massachussetts Institute of Technology four instructors have been made assistant professors—Frederick S. Woods, Ph. D., in mathematics; Theodore Hough, Ph. D., in biology; Williom Z. Ripley, Ph. D., in sociology, and Richard W. Lodge in mining engineering. Samuel P. Milliken, Ph. D., was made instructor in organic chemistry, in place of Dr. Evans, resigned. The following assistants were raised to the position of instructors—W. Felton Brown, free-hand drawing; Simeon C. Keith, Jr., S. B., biology; Ervin Kenison, S. B., mechanical drawing; Frederick H. Keyes, S. B., mechanical engineering; Charles L. Norton, S. B., physics; Kilburn S. Sweet, S. B., civil engineering.

The following instructors in the Sheffield Scientific School of Yale University have been made assistant professors: S. E. Barney, Jr., civil engineering; Dr. F. E. Beach, physics; Dr. W. A. Setchell, botany; Dr. Percy F. Smith, mathematics.

Dr. O. S. Strong has been appointed tutor in comparative neurology, and Dr. Hermann S. Davis assistant in astronomy, in Columbia College.

W. S. Matthew has been made assistant in the American Museum of Natural History.

It is reported by telegraph from Naples that Mt. Vesuvius is in an unusually active state of eruption.

It is stated that S. A. Andée's plan for reaching the North Pole by balloon under the auspices of the Royal Swedish Academy of Science will be assisted by a subscription of 30,000 kroners by King Oscar.

SCIENTIFIC JOURNALS.

THE AMERICAN CHEMICAL JOURNAL FOR MAY.

The principal articles in this number are those containing reports of the investigations carried on by Remsen and others, on the chlorides of orthosulphobenzoic acid. Early in the investigation it was found that when the chloride was treated with aniline two products were obtained, which were most easily explained on the hypothesis that the chloride is a mixture of two isomeric chlorides corresponding to those of phthalic acid. This was afterwards shown to be the fact. Two chlorides were isolated and studied, and the results led to the conclusion that the so-called higher-melting chloride (melting point 76°) is the symmetrical one, having the formula

$$C_6H_4<^{CO\,Cl}_{SO_2Cl}$$

and the other, the lower-melting chloride (melting point 21.5°–22.5°), the unsymmetrical one, with the probable structure

$$C_6H_4<^{CCl_2}_{SO_2}>O$$

Both chlorides give ordinary orthosulphobenzoic acid when treated with water, but act differently when treated with ammonia, the symmetrical one forming benzoic sulphinide thus:

$$C_6H_4<^{CO\,Cl}_{SO_2Cl}+4NH_3=C_6H_4<^{CO}_{SO_2}>N.\,NH_4+2NH_4Cl$$

while the unsymmetrical one forms the ammonium salt of orthocyanbenzenesulphonic acid,

$$C_6H_4<^{CCl_2}_{SO_2}>O+4NH_3=C_6H_4<^{CN}_{SO_3NH_4}+2NH_4Cl.$$

As the unsymmetrical chloride is acted upon much more readily than the symmetrical one, it is only necessary to treat the mixture, under certain conditions, with ammonia, to obtain the symmetrical one in pure condition. The action of benzene and aluminum chloride, on the mixture or on the pure symmetrical chloride, leads to the formation of two products,

$$C_6H_4<^{CO\,C_6H_5}_{SO_2Cl} \text{ and } C_6H_4<^{COC_6H_5}_{SO_2C_6H_5}.$$

The latter breaks down when treated with potassium hydroxide, yielding diphenylsulphone and benzoic acid:

$$C_6H_4<^{COC_6H_5}_{SO_2C_6H_5}+KOH=C_6H_5SO_2.C_6H_5+C_6H_5\,COOK.$$

Besides these articles there are several shorter ones, one by Stone and Lotz showing the identity of the sugar called Agavose, with Sucrose, and one by Trevor on 'The Law of Mass Action.' Chase Palmer gives the results of an investigation of the chromates of thorium, and Cushman describes a method of separating copper and cadmium, which is more satisfactory than the method depending upon the precipitation of the cadmium in presence of the copper. He finds that cadmium sulphide is easily soluble in warm dilute hydrochloric acid in the presence of an excess of alkaline chlorides, and is easily precipitated, after filtering to remove the copper sulphide, which is unacted upon. There are also two very interesting reviews, by Professor Mallet, of the Reports on Chemical Industry at the World's Fair, prepared by the German and French chemical representatives.

J. ELLIOTT GILPIN.

THE BOTANICAL GAZETTE.

Issued May 18, 1895. 48 pp., 2 pl.

The Development of Botany in Germany during the Nineteenth Century: EDUARD STRASBURGER.

Professor Strasburger wrote an account of the progress of botany in Germany during the present century for the sumptuous work, *Die Deutschen Universitäten*, prepared under the direction of the imperial government for the educational department of the

World's Columbian Exposition at Chicago. This work is so costly and so inaccessible that Dr. Geo. J. Pierce has translated the paper into English, and, with the approval of Professor Strasburger and the editor of the work named, it is being published in the *Gazette*. It is particularly valuable in that it forms a supplement to Sachs's History of Botany, in a measure bringing it down to date. The conclusion will appear in the June number.

The Embryo-sac of Aster Novæ-Angliæ: CHAS. J. CHAMBERLAIN.

In this study of the structure of the embryo-sac of one of the highest spermaphytes the author shows that the formation of the secondary nucleus of the sac has no relation to a sexual process; comments on the remarkable uniformity in size of the nucleoli of the egg apparatus and endosperm; finds the number of the antipodal cells varying from 2 to 13 and the number of nuclei in each from 1 to 20 or more; and, most remarkable of all, announces that he has found an undoubted egg in the antipodal region.

Present Problems in the Anatomy, Morphology and Biology of the Cactaceæ: WM. F. GANONG.

Professor Ganong continues his account of these plants, in this concluding installment indicating the problems connected with the flowers; the relation of form-conditions to climate; the internal anatomy and its relation to external conditions; the newness of the family and its geographical distribution; and briefly discusses the bearing of the solution of these problems on adaptation and natural selection.

Some Recent Cell Literature: J. E. HUMPHREY.

At the request of the editors Dr. Humphrey has prepared a review of recent cell literature and a summary of our present knowledge of the nucleus and centrospheres.

In *Briefer Articles* DR. C. R. BARNES notes the retention of vitality in the spores of *Marsilia quadrifolia*, whose sporocarps had been continuously for nearly three years in 95 per cent. alcohol; MR. G. E. DAVENPORT adds stations for his new New England species, *Aspidium simulatum*, which is likely to be in many collections under the name *A. Thelypteris* or *A. Noveboracense;* DR. J. C. ARTHUR condenses a biographical sketch of the late Dr. Joseph Schrœter; and MISS ALICE E. KEENER notes that the peculiar protection of the nectar gland in *Collinsia bicolor* by the free bearded tips of the wings of the filaments is a good diagnostic character which occurs in no other Collinsia except (less strikingly) in *C. franciscana*. The *Editorial* is on the recent transfer of the National Herbarium to the care of the Smithsonian Institution. In *Current Literature* appear reviews of 'Field, Forest and Garden Botany;' the second edition of Spalding's 'Introduction to Botany;' the 'Bushberg Catalogue and Grape Growers' Manual;' together with notices of several short papers. The number closes with four pages of *Notes and News*.

THE PSYCHOLOGICAL REVIEW.

The Psychological Review for May is devoted to experimental work. The first article is a 'Preliminary Report on Imitation' by Professor Josiah Royce. He reports the first-fruits of an attempt to submit the imitative functions to an experimental test by giving adult subjects series of rhythmical sounds, such as taps by an electric hammer, which it is their task to reproduce exactly in rhythm and sequence by second series of taps. He promises in a future communication to report on the results, which he finds sufficiently encouraging. The main body of this paper is further devoted to an acute discussion of the definition of imitation and the demarcation of the truly imitative functions. A large part of the number is taken up by a series of 'Studies from the Princeton Psychological Laboratory,' by J. Mark Baldwin, H. C. Warren and W. J. Shaw, five papers in all, giving the output of this new laboratory for the first year. Among the results of most interest reported in these studies may be mentioned the following: The relative falling off in the accuracy of memory after intervals of 10, 20 and 40 minutes is shown by curves, the thing remembered being square magnitudes exhibited to large classes of students. A contrast effect of squares of different sizes shown simultaneously to the eye was discovered, as is reported in a detailed research. It was found that the distance between two squares of different sizes can not be accurately bisected by the eye. There is a constant error in judgment toward the smaller square, whether the two be arranged horizontally or vertically. And the error in finding the midpoint increases as the disproportion between the two squares becomes greater, but always in the same direction. This was tested by different methods, one of which was designed to rule out the effect of eye-movements. Another 'Study,' on 'Types of Reaction,' reports two cases of reagents who give shorter 'sensory' than 'motor' reactions. Professor Baldwin, the author of this paper, accounts for these cases, and earlier ones reported by Cattell and Flournoy, on the general view of mental types founded on recent cases of aphasia. 'Shorter Contributions,' by H. C. Wood, on the 'Haunted Swing Illusion,' and H. R. Marshall on 'Heat Sensations in the Teeth,' make up the rest of the articles. The usual section on 'Psychological Literature' is full and varied. Many readers will be interested by the review in this section of Nordau's book on *Degeneration* by Professor William James, who also reviews several other recent works on 'Degeneration and Genius.'

NEW BOOKS.

Canyons of the Colorado. J. W. Powell. With many illustrations. Meadville Flood and Vincent. 40. Pp. 400.

A Brief Descriptive Geography of the Empire State. C. W. Bardeen. Syracuse, N. Y., C. W. Bardeen. Pp. viii + 126. 75 cts.

Engineering Education. Proceedings of the Second Annual Meeting of the Society for the Promotion of Engineering Education, Vol. II. Edited by Geo. F. Swain, Ira O. Baker, J. B. Johnson. Columbia, Mo. 1895. Pp. vi + 292. $2.50.

Birdcraft. Mabel Osgood Wright. New York and London, Macmillan & Co. 1895. Pp. 317. $3.00

Familiar Flowers of Field and Garden. F. Schuyler Mathews. New York, D. Appleton & Co. 1895. Pp. vii+308. $1.75.

Articles and Discussions on the Labor Question. Wheelbarrow. Chicago, Open Court Publishing Co. 1895. Pp. 303. 35 cts.

Crystallography. N. Story-Maskelyne. Oxford, The Clarendon Press, New York, Macmillan & Co. Pp. xii+521. $3.50.

Official Year-Book of the Scientific and Learned Societies of Great Britain and Ireland. London, Charles Griffin & Co., limited. 1895. Pp. iv+ 254. 7s. 6d.

Complete Geography. Alex. Everett Frye. Boston and London, Ginn & Co. 1895. iv+175.

The Horticulturalists' Rule-book. L. H. Bailey. New York and London, Macmillan & Co. 1895. Pp. ix+302. 75 cts.

The Diseases of Personality. Th. Ribot. Authorized translation. Second revised edition. Chicago, The Open Court Publishing Co. 1895. Pp. viii+163. Cloth, 75 cts; paper, 25 cts.

Major James Rennell and the Rise of Modern English Geography. Clement R. Markham. New York, Macmillan & Co. 1895. Pp. vii+ 232. $1.25.

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*THE LOWEST OF THE VERTEBRATES AND THEIR ORIGIN.**

IN many seas have been found—and in almost all temperate and tropical seas may be found—small animals of peculiar appearance and habits and of extraordinary interest. They have a translucent, compressed and elongated fusiform body attenuated at both ends, and therefore have received one of their names—*Amphioxus*; this form may be superficially modified, however, by the development of a membrane around the caudal portion of the body and the extension downwards of cirri from an oral ring. The existence of these cirri and the erroneous attribution to them of a respiratory function have given rise to another name for the group—*Branchiostoma*. *Lancelet* is a semi-popular equivalent of *Branchiostoma* and *Amphioxus*.

The animals thus distinguished externally are unique in their organization. The nervous system is manifest in an elongated tube without any expansion forwards into an *externally* specialized brain, and with its anterior portion only distinguished by the fact that there are (in front of the first myotome) two symmetrical pairs of sensory nerves which innervate the snout and have no corresponding ventral roots. A skeleton is represented by a simple notochord extending to both ends of the body, and there is no rudiment of a cerebral case or of sense capsules; the only other hard parts are developed around the anterior aperture, where

* *Columbia University Biological Series. II. Amphioxus and the Ancestry of the Vertebrates.* By ARTHUR WILLEY, B. Sc., Tutor in Biology, Columbia College. With a preface by HENRY FAIRFIELD OSBORN. Macmillan & Co. 1894. 8vo, xiv+316. Frontispiece. $2.50.

a ring is formed by a number of subcartilaginous segments, which give rise to as many processes for the support of cirri just before their posterior ends, which connect with the succeeding segments. A specialized heart is also wanting, and so likewise are paired eyes, as well as auditory and olfactory organs. The sense of sight or light is subserved imperfectly by a median 'eye spot' sessile on the forward end of the nerve tube between the foremost pair of nerves. Immediately behind the eye spot is 'a small pit in the body-wall, reaching from the outer surface of the body to the anterior wall of the brain. This is known as *Kölliker's olfactory pit*, after its discoverer' (p. 19). Every other feature of the organization of the animals in question is noteworthy, and Mr. Willey tells about them in detail in the work which is here noticed, and to that reference may be made for further information.

The species of Lancelets are few; only nine or ten at most are known. They are of small size, ranging from about half an inch to little more than three inches in length. Most of them are found along sandy shores and are prone to bury almost the entire 'body in the sand, leaving only the mouth with the expanded buccal cirri protruding' (p. 9). Nevertheless one specimen was described in 1889 in the 'Report on the pelagic fishes' of the Challenger Expedition (p. 43), and affirmed to have been taken 'a few degrees north of Honolulu,' from 'a deep haul 1,000 fathoms' of an open-mouthed dredge.

Diverse are the views that have been held respecting the affinities of the Lancelets. From a single small specimen obtained on the Cornish coast, Pallas in seven lines described the species in 1774* (not 1778, as Mr. Willey states) and called it *Limax lanceolatus* or *lanceolaris*. Under the name *Limax*, Pallas included naked gastropods, whether broad or narrow, having a flat foot, and he mistook the metapleural folds and intermediate area of the new species for a foot. No further notice was taken of the species till 1834, when Costa described it anew as *Branchiostoma lubricum*, and in 1836 Yarrell redescribed it, and, with the assistance of John Edward Gray, identified it with the long-neglected *Limax* of Pallas and called it *Amphioxus lanceolatus*.

Both Costa and Yarrell thought that it belonged with the Lampreys and Hags. J. Müller first recognized how important were its peculiarities and in 1844 gave it subclass rank. Isidore Geoffroy St. Hilaire in 1852 and C. Bonaparte in 1856 first elevated it to class rank. Haeckel in 1866 advanced still further and contrasted the class of lancelets as a subphylum (Acrania) with all the other vertebrates (Craniota). This last view is adopted by Mr. Willey who however prefers the later name, Cephalochorda, for the 'division.' The family name, *Branchiostomidæ*, was first given by Bonaparte in 1846.

With so much interest attached to them, the lancelets naturally have received much attention, and many elaborate memoirs on various parts of their structure have been published. Of the 140 (133 + 7) titles recorded by Mr. Willey in his bibliographical 'references' (pp. 295—309), 66 are under the head 'Anatomy of Amphioxus,' and 37 under the caption 'Development of Amphioxus.' Mr. Willey very properly adds that "this bibliography does not by any means include all that has been written on the anatomy of Amphioxus." Indeed, the titles could be more than doubled, but without material advantage to the value of the work for most readers. Really Mr. Willey has prepared a very useful and well made list and mainly with well considered restrictions. In view of such an abundant literature the need for a general work embodying

*Spicilegia Zoölogica [etc.]. Fasciculus decimus. Berolini [etc.], 1774. (p. 19, pl. 1., fig. 2.)

the most important data respecting the lancelets was urgent. Mr. Willey's volume to a very large extent administers to this need. He has judiciously combined the observations of himself and others and classified them under (I.) 'Anatomy of Amphioxus' and (II.) 'Development of Amphioxus;' under the former caption, he has data 'Historical,' on 'Habits and Distribution,' 'External Form,' and 'Internal Anatomy;' under the latter he treats of the 'Embryonic Development' and 'Larval Development,' enunciates certain 'General Considerations,' and concludes with a comparison of 'Amphioxus and Ammocœtes.' We need only refer specially to the section on 'The Excretory System' (pp. 55–75), because it contains information on 'organs' which were long undiscovered, or at least not appreciated. Mr. Willey is 'convinced as to the essential identity of the excretory tubules of Amphioxus with the pronephros of the craniate vertebrates.' The information respecting other structural features are up to date and the inferences as to homologies and functions reasonable and judicious, although there may be occasion sometimes (but rarely) for dissent. But we could have wished that the radical differences between the lancelets and true fishes had been emphasized by the use of terms indicating that analogous parts were not homologous. For instance, Mr. Willey correctly states that there is a dorsal fin 'supported by a series of gelatinous *fin-rays*, each of which lies in a chamber of its own,' and further says that 'the ventral portion of the fin in the region between atriopore and anus is supported by a similar series of fin-rays, but there are two of them placed side by side in each compartment.' Such structures are very unlike the specialized rays of teleostomous fishes, and to avoid the misleading tendency of such terms it has been recently proposed to designate the so-called rays of the lancelets *actinomimes* and their inclosing chambers *actinodomes*, while the compound ventral fin has been designated as the *sympodium*. Such terms will be useful in systematic zoölogy as well as morphology.

The ground is now prepared for further advance, and one of the first of the problems that need examination is the amount of variability among the Branchiostomids. The first preliminary is the differentiation of known variation into generic and specific characters, instead of confounding all under one generic name, as Mr. Willey has done.

Applying the mode of valuation current for the higher groups, we have several modifications of different systems that are available for genera. Such are the development of the hinder end, the unilaterality or bilaterality of the gonads or sexual organs, the coördinate development of the metapleural folds, the presence or absence of a sympodium, and the development of the dorsal fin, and especially the relative extent of the actinomimes and actinodomes. Variations in these structures are expressible under five generic terms already named, *Branchiostoma*, *Paramphioxus*, *Epigonichthys*, *Asymmetron* and *Amphioxides*. Two of the genera (*Paramphioxus* and *Epigonichthys*) have recently been combined in one to which the new name *Heteropleuron* has been given, but even if such a union is favored, *Epigonichthys* should be used as the first distinctive name given to a member of the group; the two, however, appear to be sufficiently distinguished by the fins. *Epigonichthys* has an unusually high dorsal membrane and contracted actinomimes.

Eight species of lancelets are recognized by Willey, as had been previously by Dr. E. A. Andrews; one described in 1889 ('*B. pelagicum*') was overlooked and another (*Paramphioxus Singalensis* or *Heteropleuron Singalense*) has been described since.*

*On the species of Amphioxus. By J. W. Kirkaldy. Rep. Brit. Ass. Adv. Sc., 1894, pp. 685–686.

These have been simply distinguished by Mr. Willey by the number of myotomes, but most of them may be distinguished by other characters. Of the ten species, five belong to *Branchiostoma*, two to *Paramphioxus*, one to *Epigonichthys*, one to *Asymmetron*, and one to *Amphioxides*.

What are specific characters in any Branchiostomoid genus is a question as yet undetermined. The only one that has been generally used (exclusive of what are rather of generic importance) is the number of myotomes as a whole and in different regions. Even such a character has not been constantly adhered to. For example, in Dr. E. A. Andrews' useful and able memoir on 'An Undescribed Acraniate' two eastern American species are recognized, '*B. lanceolatum*' and '*B. caribæum*.' The former has an average of (35.6+13.6+11.8=) 61 myotomes, and the latter an average of (34.8+14+8.9=) 57.8 myotomes, but one individual from the Chesapeake Bay, referred to '*B. lanceolatum*,' has (36+16+7=) 59 myotomes, and another Florida, referred to '*B. caribæum*,' has (35+17+7=) 59 myotomes. Inasmuch as no other differential characters have been given, it is evident that Dr. Andrews was mainly influenced by the consideration of association or geographical distribution rather than morphological characters in the identification of the different specimens. The relations of the forms of our coast, indeed, still remain to be determined, and it is doubtful whether any American forms will prove to be conspecific with the European. Specific characters may perhaps be found in numerous details, *e. g.*, the number and proportions of the dorsal and ventral or sympodial rays, the development of the cirri and skeletal bases; details of the velar tentacles and gillbars, form of the caudal, relative proportions of the various regions, etc. But numerous as have been the memoirs on *Branchiostoma lanceolatum*, no detailed study of variations has yet been published. Until this is done much is left undone. The material now in museums, however, is generally insufficient for such studies and should be especially prepared therefore. If the labor of students, so often frittered away in verifying oft-repeated observations, could be in part directed to such preparation and observation, a boon to systematic zoölogy would be realized and certainly no less would be the benefit to the student. We may hope that Mr. Willey will continue studies so well begun and enlighten us on some of the many points still obscure. That we are ignorant as to the questions in point is not his fault.

The ancestry of the vertebrates is a fascinating subject for consideration, and the search for their nearest relatives began early in the century. Before the lancelet was known—at least as a vertebrate—Etienne Geoffroy Saint-Hilaire endeavored to homologize constituents of the bodies of insects and vertebrates. (We may here remark that Mr. Willey has repeatedly referred to the French naturalist as Saint Hilaire, but Saint Hilaire was only an agnomen, the true cognomen or family name being Geoffroy.) Long after the lancelet had been carefully investigated, and indeed very recently, a naturalist trained in modern methods, but who did not exercise a 'scientific use of the imagination,' actually contended that the vertebrates had arachnoid or rather limuloid ancestors! A less extravagant view has been that Annelid worms were nearest of kin to the vertebrate ancestors, and this has gained several followers. But the highly specialized character of annelids and still more of arthropods appears to forbid the serious consideration of such conceptions. Much more probable is the view that the nearest relatives of typical vertebrates are the Tunicates. This is the idea adopted by Mr. Willey, who has accepted a 'group' called 'Protochordata' and included thereunder three divisions, (1) HEMICHORDA, or *Balanoglossids, Cephalodiscids* and *Rhabdopleurids;* (2) UROCHORDA, or *Ascidians,* and (3) CEPHALOCHORDA, or *Lancelets.* It is the present fashion to consider this affiliation as established, but it has not been proven beyond cavil. As a provisional hypothesis, however, it is the best of those that have been proposed, and there is no need to offer here any objections. Nevertheless, we should recall the fact that the lancelets and all other so-called 'Protochordata' must have very widely diverged from their common ancestors and that some of the characteristics of the first are probably the result of degeneration. When, for example, we find a specialized heart and auditory organs in Tunicates, as well as in many true invertebrates (even though they be not homologous), it is difficult to resist the inference that their absence in the lancelets is due to loss rather than to original failure of development. But now, with the necessary precautions and much hesitancy, we may assent to the possibility of the conclusions with which Mr. Willey closes his work.

"For the present we may conclude that the proximate ancestor of the Vertebrates was a free-swimming animal intermediate between the Ascidian tadpole and *Amphioxus,* possessing the dorsal mouth, hypophysis, and restricted notochord of the former; and the myotomes, cœlomic epithelium, and straight alimentary canal of the latter. The ultimate or primordial ancestor of the Vertebrates [or Chordates] would, on the contrary, be a worm-like animal whose organisation was approximately on a level with that of the bilateral ancestors of the Echinoderms."

The length to which this notice has already extended forbids attention to various other features of Mr. Willey's work. It must suffice to add that the fourth and fifth sections are devoted respectively to 'the Ascidians' (pp. 180–241) and 'the Protochordata in their relation to the problem of vertebrate descent' (pp. 242–293). For these we owe further thanks, and for all we feel assured future students of the groups in question will be grateful.

THEO. GILL.

SMITHSONIAN INSTITUTION.

CURRENT NOTES ON ANTHROPOLOGY (IX.).

THE RITUAL CALENDAR OF CENTRAL AMERICA.

IN the *Globus,* No. 18, 1895, Dr. E. Förstemann has one of his ingenious studies of the Central American Calendar, this time that portion of it called by the Nahuas the *Tonalamatl,* or Book of Days. This consisted of a period of 260 days, and strenuous efforts have been made by Mrs. Zelia Nuttall and other writers to treat it as a time-count, that is, as an aliquot part of the computation of astronomical years and cycles.

In this article Dr. Förstemann shows that this certainly does not hold good for the *Tonalamatl* as it constantly recurs in the Mayan manuscripts. In them it appears to be introduced for exclusively divinatory purposes, a basis for predicting events relating to persons or tribes, or else the weather, wars, disasters, etc. Not unfrequently a multiple of the period is embraced in the forecast, and very generally reference is made to the divinities assigned to the subdivisions of the *Tonalamatl.* Or, again, it is occasionally divided into its fourths, fifths or tenths; and what is noteworthy, the manuscripts present numerous similarities in these respects, proving that their writers were working on a like system of horoscopy.

I may add that the result of this investigation corroborates the position that I took in my 'Native Calendar of Central America and Mexico' (Phila. 1893), in which I maintained that the *Tonalamatl* was invented for and practically exclusively applied to divination, and not to the cyclical measure of astronomical time.

THE TREE AND THE CONE.

EVERY one who has given the least attention to works on ancient Assyria is familiar with the engraving which shows a winged deity, holding in one hand a small basket or bucket, and in the other something like a pine cone, which he is generally presenting toward a tree. This used to be construed as the 'cherub' offering the cone, a symbol of reproduction, etc., to the 'sacred tree' of Babylonian mythology.

A few years ago Dr. E. B. Tylor advanced the explanation that the true meaning is a representation of the fertilization of the female date palm, artificially, by the agriculturist impregnating its flowers with the inflorescence of the male tree. This was at once accepted by many writers, while others withheld their assent, asking why a winged cherub instead of a mortal should be depicted; and still further pointing out that this same ceremony is not rarely shown where there is no tree at all, but, say, the gate of a city, or some exalted personage, like a king.

These arguments have been repeated with emphasis by Dr. E. Bonavia in his recent work, 'The Flora of the Assyrian Monuments' (London 1894). He shows that the bucket or basket is certainly a *bucket*, intended for fluids, and inappropriate to carrying flowers. He offers the very plausible theory that it was designed to contain holy water, and the cone was an *aspergillum*, as it still is in the East. The winged cherub is the rain-bringer typified, etc.

This is far the most satisfactory interpretation which has yet been offered, and allies itself closely with numerous rites and myths of ancient Mesopotamia.

NATIONAL VERSUS INDIVIDUAL DEVELOPMENT.

THE French have a knack of putting their conclusions in an aphoristic form, which, whether they are right or wrong, impresses the mind. An example is the following from a memoir by M. Dumont, published by the Paris Society of Anthropology last year:

"The increase of a nation in numbers is in inverse ratio to the efforts of its individual members toward personal development."

Now, if this is true, it means the discovery of a momentous law in sociology, which, among other consequences, will do away with all fears of over-population in free and enlightened states. Its corollaries would also dismiss both the dread of socialism and likewise of unscrupulous individualism, which two are the Scylla and Charybdis of modern political economists.

Of course, 'efforts toward personal development' must be construed as sensible and properly directed efforts towards a development which is really such, according to the highest criteria we now have. The reasons why such efforts would necessarily limit the numerical increase of a nation are evident enough. Whether these in the long run might not work as badly as the *laissez faire*, or 'go as you please' policy, is the question underlying this sociological puzzle.

THE SOURCES OF PERUVIAN CIVILIZATION.

IN a paper in the Denison Quarterly, Vol. III., Dr. George A. Dorsey discusses 'The Character and Antiquity of Peruvian Civilization.'

He is inclined to assign it a greater age than has usually been allowed it. He would place its earlier periods contemporaneous with 'the golden age of Greece, or when the people of the Nile valley were in the zenith of their power.'

Generally, the historic or even the traditional cycles of the Quichuas are not supposed to carry us beyond about 1000 A. D. One historian, Montesinos, who names dynasties far more remote than this, has been generally discredited, though he claimed native sources for them; and it is fair to add that we have no positive certainty how great the value of the mnemonic system of the Quichuas, their knotted and colored cords, the *quipus*, may have been. It has also been more than once argued that there must have occurred important modifications in climate since the great temples and cities on the cold plateaus were built, and harbored the large populations which must have dwelt in them. This would require a long period.

As Dr. Dorsey speaks from personal observations and extensive archæological explorations in Peru, his opinion, however at variance with that usually entertained, merits careful consideration.

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CURRENT NOTES ON PHYSIOGRAPHY (IX.)

THE GLACIAL ORIGIN OF LAKE BASINS.

As long as lakes are regarded simply as locally deepened valleys, their explanation by glacial erosion may be fairly maintained; for when the problem is thus vaguely stated, the requirements to be met by the theory are so simple that the hypothesis of glacial erosion finds perhaps better reasons for acceptance than any other hypothesis. But as the facts to be explained are more carefully observed, they generally become more highly specialized and more peculiarly correlated; and their glacial origin may then be either confirmed or excluded. The peculiar association of features described by Lincoln (Amer. Jour. Sci., xliv., 1892, 290) and by Tarr (Bull. Geol. Soc. Amer., v., 1894, 339), regarding Cayuga Lake, seems on the one hand to demonstrate the glacial excavation of this basin; but, on the other hand, the extraordinary correlation of facts determined by various observers around Lake Zurich does not seem to be within reach of explanation by so simple a process as glacial erosion. In spite of so good a general argument for the competence of ice action as has been presented by Bohm (Verein zur Verbreit. Naturw. Kenntnisse in Wien, xxxi., 1891, 477), and in spite of the emphatic disapproval by J. Geikie of various other processes that have been suggested for the production of Alpine lakes (Great Ice Age, 3d ed., ch. xix.), the origin of Lake Zurich is certainly not to be accounted for by generalizations at a distance, but only by a special process that will fit all the facts found on the ground. Evidence tending to this end has gradually been accumulating for a number of years; but at an accelerated rate since Heim and Bodmer interpreted the meaning of the rock terraces on the valley sides, and since Penck, DuPasquier and others deciphered the records of the several glacial epochs on the north slope of the Alps.

THE ORIGIN OF LAKE ZURICH.

The problem of Lake Zurich is presented in a masterful manner by Aeppli in the thirty-fourth number of the *Beiträge zur Geologischen Karte der Schweiz*, in brief as follows: The valley of the Limmat, in which the lake lies, was eroded in broad upland over which the *Deckenschotter* of the first glacial epoch had been previously spread. That the erosion of the valley was performed in the normal fashion by weather and water, and not by ice, is shown by the graded terraces or rock benches, traceable more or less continuously along its sides; these terraces being independent of rock structure, and associated with similar terraces in other valleys, all leading agreeably to the conclusion that after the first glacial epoch the region was generally elevated and the streams thereby given increased power of erosion. The *Deckenschotter*, where preserved on the ridges between the adjacent valleys, together with the terraces on the valley slopes, are bent backwards across a belt six or eight kilometers broad, so as to slope *towards* instead of *from* the Alps; and the deformation of the *Deckenschotter* and of the earlier higher terraces is greater than that of the lower and younger terraces, thus proving the progressive action of the deforming forces. Associated with this change, there was a general depression of the *Molasse* belt, between the Jura and the Alps, and in the depressed part of the valley of the Limmat, thus generally outlined by the latter process and locally deepened by the former process, the lake had its birth. The belt in which the terraces are deformed crosses the valley somewhat obliquely, but runs parallel to the strike of the general Alpine deformations of the region. Into the lake thus formed, the glaciers of the second and third epochs advanced; but they exercised so little destructive power that they did not obliterate the terraces on the valley sides. The lateral moraines of the last epoch are distinctly discordant with the terraces; the moraines reaching successively higher and higher terraces up-stream, and crossing the belt of deformation without indication of disturbance. Outside of the several terminal moraines, the former lower end of the lake received the valley gravels that were washed from the ice. Hence while the later glaciers may have acted to some degree in altering the form of the lake, their chief effect was to diminish its size by supplying plentiful gravels from the inner Alps, with which a part of the lake basin that they entered was filled.

TARNS OF THE ENGLISH LAKE DISTRICT.

J. E. MARR has examined the tarns or smaller lakes of the mountainous district of northwest England, and finds that their reputed dependence on rock basins is not justified by local study. They appear to result from drift obstructions, by which their outlets have been turned to one side of the former valley troughs and detained in discharging the lakes by settling on rock ledges. In many cases lakes of similar origin have been converted into meadows when their outlets did not depart greatly from the former valley line, and hence encountered only drift in trenching new discharging channels (Quart. Jour. [London] Geol. Soc., li., 1895, 35–48). This does not bear so much on the general question of glacial erosion as on the particular question of the ability of glaciers to form basins by local erosion in excess of their general action along their floor.

THE REGION ABOUT MUNICH.

IN celebration of its twenty-fifth anniversary, the Geographical Society of Munich has issued a handsome volume of 440 pages, containing a number of essays by Günther, Ratzel, Penck and others. Ratzel makes the coast line of Maine 4,300 miles in actual length; though a direct line from Eastport to Kittery measures only about 200 miles. The essay most likely to interest American readers is on the geology of the region about Munich by Ammon, illustrated with a geological map, plate and cuts. It may serve as a guide to excursions from this attractive center; from few other points can so many phases of piedmont glacial geology and geography be seen to so good advantage. Würm and Ammer lakes lie twenty odd miles to the southwest, enclosed by the younger morainic belt. Older moraines stretch farther out from on the plain, especially to the east of the city; and beyond them are spread the flat confluent gravel fans that are associated with various epochs of ice advance. On the sloping plain stands Munich, and across it the Isar and the Amper have trenched their new valleys. The illustrations of morainic topography are very characteristic. A good bibliography accompanies the article; while on an earlier page, Simonsfeld contributes a thirty-page *Bibliotheca geographica bavarica*.

HARVARD UNIVERSITY. W. M. DAVIS.

SCIENCE IN CANADA.

THE CANADIAN ROYAL SOCIETY'S ANNUAL MEETING.

THE annual meeting of the Royal Society of Canada opened at Ottawa on the 14th ult. and closed on the 17th. Mr. J. M. Le Moine, the well known historian and friend of Parkman, presided. This institution, which was founded in 1882 by the Marquis of Lorne, then Governor-General of Canada, consists of four sections, of which two are entirely scientific—the third being devoted to the mathematical, physical and chemical sciences; the fourth to the geological and biological sciences. The first and second have also a scientific element, for though nominally set apart for English and French literature, respectively, they admit, under the head of archæology a class of subjects that are not unrelated to important branches of science. During the recent session, for instance, three papers of a scientific character were presented in the second section: 'The present position of American Anthropology,' by Professor John Campbell, LL. D., of Montreal; 'Religion and Aerolites,' by Mr. Arthur Harvey; and 'An Iroquois Condoling Council,' by the venerable ethnologist, Mr. Horatio Hale. Those who are acquainted with Mr. Hale's excellent monograph, 'The Iroquois Book of Rites,' in Dr. Brinton's 'Library of Aboriginal American Literature,' will not be entire strangers to the subject of Mr. Hale's paper.

Of the papers of the scientific sections, the first read in Section 3 was the presidential address of Dr. Harrington (McGill College), which dealt with a subject of considerable interest. Professor Harrington urged the necessity of using absolutely pure materials in chemical operations where the object was to establish formulæ. The address, which was illustrated by abundant examples, gave rise to an interesting discussion, in which Mr. T. Macfarlane, chief analyst of the Dominion; Professor Goodwin, of Kingston, Ont., and Dr. Ells, of Toronto, took part. Professor Harrington also read a paper on 'The Chemical Composition of Andradite from two localities in Ontario.' It gave the results of the examination of a black garnet, occurring in association with the magnetic iron ore of the Paxton mine, Lutterworth, Ont., and of a brown andradite present in the nepheline syenite of Dungannon, Ont. Of these andradites the former was found to be free from titanium, the latter to be titaniferous.

Other papers read in Section 3 were the following: 'A short essay on an attempt to measure the relative easterly and westerly transmission lines through an Atlantic cable,' by Professor C. H. McLeod (McGill College); 'On the estimation of Starch,' by Mr. Thos. Macfarlane, Ottawa; 'Viscosity in Liquids and Instruments for its Measurment,' by Mr. Anthony McGill; 'Periodicity of Aerolites,' by Mr. Arthur Harvey, Toronto; 'On Some Applications of De Moivre's Formulæ,' by Professor F. N. Dupuis, Queen's College, Kingston, Ont.; 'On the Hypotheses of Dynamics,' by Professor McGregor, of Dalhousie College, Halifax. In a former paper Professor McGregor had tried to express the ordinary hypotheses employed in dynamics in a form suited to the conception of bodies as consisting of particles acting upon one another at a distance. In the later paper he endeavors to express those hypotheses in a form suited to the conception of bodies and intervening media, as consisting of parts which act directly on one another only across surfaces of contact.

In the 4th Section Sir J. William Dawson, of Montreal, read a 'Note on Tertiary Fossil Plants from the vicinity of the City of Vancouver, B. C.' This important paper related to a series of beds holding lignite and vegetable fossils and estimated at 3,000 feet or more in thickness and occurring in the southern part of British Columbia, between Burrard Inlet and the United States boundary. These beds, which have been noticed in the Reports of the Geological Survey by Dr. G. M. Dawson and the late Mr. Richardson, are believed to be newer than the Cretaceous coal-measures of Nanaimo and Comox, and probably equivalent to the 'Puget group' of the United States geologists in the State of Washington. Collections of the fossil plants have been made at various times by officers of the Geological Survey, and more recently by Mr. G. F. Monckton, of Vancouver, who placed his material in the hands of the author, along with that previously entrusted to him by the Geological Survey. The species contained in the several collections are mentioned in the paper, and are compared with those of the Puget group, as described by Newberry and Lesquereux, and with those of other localities in British Columbia and the United States. The conclusion as to the age of the flora is similar to that arrived at by Newberry for the Puget flora, making it equivalent to the Upper Laramie or Fort Union group. It thus intervenes in date between the Upper Cretaceous of Nanaimo and the Oligocene or Lower Miocene of the Similkameen district, and is therefore of Eocene age, filling a gap hitherto existing in the mesozoic flora of the West coast. Much, according to Sir W. Dawson, still remains to be known of this interesting flora, and as the formation containing it, which seems to be estuarine in character, extends over a wide area in British Columbia and Washington, and is of considerable thickness, more especially in its extension south of the Canadian boundary, it may ultimately be shown to include several sub-divisions representing the long interval between the Cretaceous and the Middle Tertiary.

Mr. J. F. Whiteaves, paleontologist of the Geological Survey, Ottawa, read an interesting 'Note on the occurrence of Primnoa Reseda on the coast of British Columbia.' The main value of Mr. Whiteaves' paper lies in the fact that *Primnoa Reseda* (a tree-like Alcyonarian coral), though known for over a century as occurring in the Atlantic, has not hitherto been with certainty assigned a Pacific habitat. Dr. R. W. Ells and Mr. A. E. Barlow presented a joint paper on 'The Geology of the proposed Ottawa Ship Canal,' the route of which is of unusual interest from a geological, as well as historical and commercial, point of view. A contribution to the history of botanical research on this continent was offered by Prof. (Mgr.) Laflamme, of Laval University, who seeks an answer to the question, 'Where did J. Cornut, who published his *Canadensium Plantarum Historia* in 1635, obtain the specimens from which he wrote his descriptions, and by whom were they transported to Europe?' Mr. G. U. Hay discussed 'some variations in *Epigœa repens.*' Dr. G. F. Matthew, of St. John, New Brunswick, continued a series of studies on the organic remains of the Little River Group in that province. Dr. Wesley Mills (McGill College) presented a series of papers embracing results of investigations into the psychology of the dog, the cat, the rabbit, the guinea pig and certain birds, with corresponding physical indications. The papers also compared the mongrel with the pure-bred dog; the dog with the cat, the rabbit with the guinea pig, etc. These inquiries were conducted with extreme care with the aid of the best equipment for observation and experiment.

One of most important of the scientific papers contributed to the Society was presented, not in section, but before a public audience. Prof. John Cox, M. A. (Cantab), late fellow of Trinity College, Cambridge, and William C. McDonald Professor of Experimental Physics in McGill University, Montreal, had been asked to give a public lecture on Thursday evening the 16th inst., in connection with the Royal Society's meeting. Prof. Cox has a gift too rare with men of science, and most precious to him whose chosen path of research leads him into *selve oscure* of abstruse problems where for the many no light shines—the gift of clear and eloquent exposition. His subject was 'Unsolved Problems in The Manufacture of Light,' and, in order to illustrate it worthily, he had brought with him from Montreal the admirable apparatus of his laboratory necessary for a series of experiments and lantern views. He was assisted by Messrs Barnes and Pitcher, and the large and cultivated audience gathered in the hall of the Normal School listened enraptured as he made plain mysteries that most of them had regarded as impenetrable. After referring to the time-honored sources of light—the candle, oil lamp, gas, Auer light, and the lime light—and showing that each consisted in heating something till it was incandescent, the lecturer pointed out that none of these gave an efficiency of more than one per cent., the only scientific systems of combustion being the Auer light and lime light. The modern method of electric lighting dated from Sir Humphrey Davy's first production of the arc, with a battery of 2,000 cells. The current thus produced was still ample to heat refractory substances to incandescence, but as zinc and acid were many times as expensive as coal and air the light could not come into practical use until the invention of the dynamo forty years afterward. With the dynamo the modern system was completed, and consisted of three stages—the steam-engine, the dynamo and the lamp. The purpose of the lecture was to show that in the steam-engine and the lamp there is still an enormous waste. After pointing out that light was not created but was produced by the conversion of energy, and explaining the nature of energy as stored up in coal, Prof. Cox dealt with the three stages in detail. "The conversion of the coal-energy into the mechanical energy," he said, "is of course effected by the steam-engine, but in practice not more than from 7 to 16 per cent. of the energy stored in the coal can be extracted by the steam-engine, and theoretical considerations fix an absolute limit to the perfection of the steam-engine, showing that we can never hope to convert so much as 30 per cent. of the energy of coal by any form of heat-engine. This is one unsolved problem in the manufacture of light—unsolved but still capable of solution if some means of extracting energy from coal otherwise than by heat, and more like the methods used in burning zinc in a battery, can be discovered. At present we are recklessly wasting our coal supplies, and posterity will have a serious grievance against us for squandering these priceless stores."

In the second stage of the process, the dynamo, though so recently invented, is already nearly perfect, and scarcely any energy is lost in its conversion by the dynamo into an electrical current. We reach the third stage, that is, the lamp, with some 7 per cent. of the original energy still available. The lecturer here showed a number of interesting experiments to prove that the only form of energy useful in producing vision consisted of a short series of very minute waves, ranging from the forty-thousandth to the sixty-thousandth of an inch in length, and that to produce these our only means at present was to heat the molecules of some substance, whereby we were compelled to waste the greater part of our efforts in producing heat, which was worse than useless, before we obtained the luminous rays. "Here then," said Professor Cox, "is the second unsolved problem, since even in the incandescent lamp and the arc lamp not more than from three to five per cent. of the energy supplied is converted into light. Thus, of the original store in the coal less than three parts in a thousand ultimately become useful. In the last six years, however, some hint of means to overcome this difficulty has been obtained from the proof by Maxwell and Hertz that light is only an electric radiation. Could we produce electric oscillations of a sufficient rapidity we might discard the molecules of matter and directly manufacture light without their intervention. To effect this we must be able to produce oscillations at the rate of five hundred billions per second. Tesla has produced them in thousands and millions per second, and Crookes has shown how by means of high vacua to raise many bodies to brilliant fluorescence at a small expense of energy." Illustrations of these processes having been given, the lecturer concluded: "These are hints toward a solution of the problem, but give no solution as yet. Professor Langley states that the Cuban firefly spends the whole of its energy upon the visual rays without wasting any upon heat, and is some four hundred times more efficient as a light producer than the electric arc, and even ten times more efficient than the sun in this respect. Thus while at present we have no solution of these important problems we have reason to hope that in the not distant future one may be obtained, and the human inventor may not be put to shame by his insect rival."

At the general final meeting on Friday (17th inst) it was moved by Dr. Sandford Fleming, C. M. G., of Ottawa, and seconded by Sir William Dawson, F. R. S., that the Royal Society of Canada was of opinion that it is in the interests of science and seamen in all parts of the world that a final determination be speedily reached regarding the unification of the nautical, astronomical and civil days, so that all may begin everywhere at midnight, and that as the proposal can with least difficulty be carried into effect on January 1st, 1901, the Council be requested in the name and on behalf of the Society to adopt such measures as may be considered expedient to bring about the desired result. This is a subject to which Dr. Sandford Fleming has devoted much and fruitful attention.

The following officers were elected for the ensuing year: President, Dr. R. S. C. Selwyn, C. M. G., F. R. S., ex-Director of the Geological and Natural History Survey; Vice-President, the Archbishop of Halifax, Dr. O'Brien; Secretary, Dr. J. G. Bourinot, C. M. G.; Treasurer, Prof. J. Fletcher. Prof. Bovey, Dean of the Faculty of Applied Science, McGill University, was chosen president of the third section; Prof. Dupuis, of Queen's College, Kingston, Ont., Vice-President, and Capt. E. Deville, Surveyor-General of the Dominion, Secretary. In the fourth section, the following choice was made: President, Prof. Wesley Mills, M. A., M. D., McGill University; Vice-President, Prof. Penhallow, B. Sc., of the same institution; Secretary, Dr. Burgess, Superintendent of the Protestant Insane Asylum, Verdun, near Montreal. J. T. C.

CORRESPONDENCE.

VOLCANIC DUST IN UTAH AND COLORADO.

Science of April 26th contains an article by H. W. Turner, of Washington, D. C., upon '*Volcanic Dust in Texas*.' It may perhaps be of interest to some of the readers of Science to learn that large deposits of volcanic dust occur in Utah, and also in the extreme northwestern portion of Colorado. In the year 1890, while I was a professor in the University of Utah, my attention was called to an extensive deposit of a grayish-white substance near Stockton in the Oquirrh range of mountains, some sixty miles southwest of Salt Lake City, by Mr. Ben Johnson of that place. Upon examination I found it to consist almost wholly of microscopic, transparent, siliceous flakes of various, irregular forms, one of the most common being curved and nearly triangular. When put into pure water, it invariably showed a slight acidity, reddening blue litmus paper. It can be taken from the deposit in lumps; but they readily fall to powder, the particles or flakes becoming separated by the pressure of one's hand. During a tour through southern Utah in the year 1893 I found another large deposit of the same kind of volcanic product on the east side of the Wasatch Mountains in the vicinity of Monroe village, in Sevier county. I could find no difference between this latter and that which occurs near Stockton. Both give a slight acid reaction, which, I suspect to be due to a sulphur compound.

In the same year, 1893, there was brought to me a good sample of grayish white, stratified mineral substance, said to be kaolinite and to have been taken from an immense deposit of a similar character east of Green River and in northwestern Colorado. This so-called 'kaolinite' proved upon examination to be similar volcanic dust, which had been subjected to the action of water mixed with clay, deposited in layers under the water, and afterwards hardened.

HENRY MONTGOMERY.
TRINITY UNIVERSITY, TORONTO.

VOLCANIC DUST IN TEXAS.

UNDER the above title Mr. H. W. Turner contributes an article to SCIENCE of April 26, 1895, briefly describing a specimen from the Llano Estacado region. Some of the previous notices of this or similar material are noted below.

The first specimen of the material which came under my notice was received by the Texas Survey in February, 1890, with other material forwarded by Professor W. F. Cummins. It was collected from the beds to which he gave the name 'Blanco Canyon' from the place of their most characteristic development, and in his first description of them* he calls it chalk. Later, microscopic slides of this material were prepared in the Survey laboratory, by Mr. J. S. Stone, under the direction of Professor R. T. Hill, and these exhibited a large number of very finely preserved diatoms.

These diatoms were partially identified by Mr. C. H. Kain and published by Prof. Cope in his first notice of the probable Pliocene age of the Blanco Canyon beds.†

The diatomaceous character of this material was further noted by Messrs. Lewis Woolman and C. Henry Kain, and list of species given in *The American Naturalist* for 1892, p. 505, under the title 'Fresh-Water Diatomaceous Deposit from Staked Plains, Texas.'

In 1892 an examination of this material by the writer showed the presence of volcanic dust, but the diatoms constituted by far the greater part of the mass examined, and it was therefore classed with other materials of a similar kind from the coast region as diatomaceous earth, and only those siliceous deposits of like character which failed to reveal diatoms were classed as volcanic dust and briefly described in the Transactions of the Texas Academy of Science.‡ Further reference to these siliceous deposits are also made by Kennedy in the Fourth Annual Report Geol. Sur. Texas, pp. 20, etc.

The stratigraphic position of the deposit referred to by Mr. Turner has been accurately determined, as will be seen by reference to the different reports of Professor Cummins on northwest Texas and the Llano Estacado. The hill mentioned, on Duck Creek, in Dickens county, is in the type locality of the Blanco Canyon beds, and sections are given of it in the first three

*First Ann. Rep. Geol. Sur. Texas, p. 190.

† Proc. Amer. Phil. Soc., 1892, p. 123.

‡ Vol. I., Part I., 1892. P. 33. 'Volcanic Dust in Texas.'

annual reports of the survey. The fossils of these beds (one of them, a turtle, from the hill in question) were sent Professor Cope, and are described by him in the fourth annual report of the survey. He says: "Its position is between the Loup Fork and Equus terranes. The fauna is intermediate and peculiar, as not a single species occurs in it which has been found in terranes prior or subsequent to it in time. The horizon is more nearly and strictly Pliocene than any of the lacustrine terranes hitherto found in the interior of the continent."

E. T. DUMBLE.

ON THE CLASSIFICATION OF SKULLS.

TO THE EDITOR OF SCIENCE: I learn from an article by Dr. Harrison Allen (SCIENCE April 5, 1895) that, in a paper entitled 'Observations on the Cranial Forms of the American Aborigines,' Proceedings of the Academy of Natural Sciences of Philadelphia, 1866, 232, J. Aitkin Meigs classified various types of crania, using nomenclature which in some part coincides with that proposed by me in my new 'Method of Classification of Skulls.'

I am very glad to learn that Meigs distinguished the various forms of human skulls as early as 1866, as I have done twenty-six years later. When two men, at so great a distance in time and space, have conceived a similar idea it is a strong argument that this idea is not a fantastic one.

I first tested my new method in the summer of 1891, examining a large collection of Malanesian skulls, and published my first memoir in the spring of 1892, which was translated into German (*Die Menschen Varietäten in Malanesian*. Archiv. für Anthropologie, XXI., 1892). In the same year, 1892, I had fortunately the opportunity of examining more than 2,200 skulls of the Mediterranean and Russian races, ancient and modern. I then systematized my classification, which was imperfect, and distinguished varieties and subvarieties of human skulls in a systematic catalogue of ancient Russian skulls.

This method has the approval of many Italian anthropologists, a notable exception being Mantegazza, a strange type of man, and of some German anthropologists, as Ranke and Benedict. The French anthropologists are indifferent, but they find the method useful as an analysis of forms.

The memior of Meigs is not known in Europe. The only work of this author that I possess is the Catalogue of the Specimens contained in the collection of the Academy of Natural Science of Philadelphia, 1857. In view of the notice published by Dr. Allen in SCIENCE, I am anxious to read the work referred to, and I should be much obliged if some American friend will procure a copy for me. I shall be glad to refer to the work of Meigs in a special note.

G. SERGI.

UNIVERSITY OF ROME, April 23, 1895.

SCIENTIFIC LITERATURE.

The Geological and Natural History Survey of Minnesota, Volume III., Part I. Palæontology. 4to, 1895, Pp. lxxv., 474. Plates xxxiv.

Considerable activity has been manifested of late in a more careful and systematic study of the invertebrate faunas of the various geological horizons of this country, and several works upon the subject have already been published or are now under preparation. The value of a thorough examination and proper illustration of the faunas of many of our geological divisions will be very great to the stratigraphical geologist, for many problems are now obscure on account of the lack of knowledge of the very criteria most important for correlative purposes.

What is most required in this field is not so much the increase in number of species, although many horizons even in the eastern portion of the country have as yet been but partially explored, as the thorough revision of the synonomy and geological distribution of the well-known forms described by the earlier paleontologists. At present the confusion is so great in many faunas that it becomes almost a hopeless task for the geologist to use the evidence with any hope of satisfactory results.

It is therefore very gratifying to find that the elaborate volumes upon the geology of Minnesota are to be accompanied by exhaustive reports upon the paleontology of the State. The first of these monographs, constituting Part 1., of Volume III., of the Final Reports, has just appeared and treats chiefly of the Lower Silurian faunas of the southeastern portion of the State.

The introductory chapter consists of an 'Historical sketch of investigation of the Lower Silurian in the Upper Mississippi Valley' and contains a chronological catalogue of the palæontological writings upon this subject, including lists of the species described.

Although the introduction deals only with the Lower Silurian, the first chapter is devoted to the Cretaceous fossil plants, a posthumous publication of Leo Lesquereux. Some twenty-eight species, six of them new, are described, the majority of the forms being also figured. More than half of the determinable species have been found in the States to the west, and the flora as a whole indicates the Dakota group as the geological horizon.

The second chapter deals with the microscopical fauna of the Cretaceous and is chiefly given up to a description of the Foraminifera, most of which are from boulder clay, although regarded as derived originally from the Cretaceous. Thirty species, representing eighteen genera, are determined. The authors are Woodward and Thomas.

The three remaining chapters of the volume are devoted to the fauna of the Lower Silurian, the third and fifth chapters being by Winchell and Schuchert and dealing with the 'Sponges, Graptolites and Corals' and the 'Brachiopoda.' The authors follow Hinde in placing Receptaculites and Ischadites among the Hexactinellid sponges, and Ulrich in regarding Cylindrocoelia and Heterospongia as Calcispongiæ. If the latter reference should prove correct it is of interest as the earliest occurrence of representatives of that order. Among the corals a new genus, Lichenaria, regarded as related to Columnaria, but without septa, is established.

The rich Brachiopod fauna receives very exhaustive treatment, as might be anticipated from so thorough a student of the subject as Mr. Schuchert. Altogether eighty-two species with many varieties are recognized, of which several are new.

The longest chapter in the volume is that upon the Bryozoa by E. O. Ulrich. As a class the fossil Bryozoa are most difficult, and the different attempts at their systematic classification have not been attended hitherto with the most satisfactory results. To the author of the present chapter we are indebted more than to any one else for our knowledge of the Paleozoic representatives of this group. In the classification adopted, however, the reference of the Monticuliporoid forms to the Bryozoa is not in accordance with the more recent conclusions in this line.

The report as a whole is a most valuable contribution to the paleontology of the Upper Mississippi basin, and will supply a distinct want to the invertebrate palæontologist. The State Geologist is deserving of much credit for the admirable manner in which the volume has been brought out, and it is to be hoped that other State Surveys, which pay little attention to the paleontology of their States, may be induced to pursue the same course.

William B. Clark.

Johns Hopkins University.

Fossil Mammals of the Puerco Beds. By HENRY FAIRFIELD OSBORN and CHARLES EARLE. Bull. American Museum of Natural History. Vol. viii., Art. I. Pp. 1–70.

The Puerco Eocene (or Post-Cretaceous) was discovered and named by Cope in 1880, and up to the present time our knowledge of its very remarkable and interesting fauna has been due almost entirely to his labors. It is a fauna which in many ways is very puzzling and raises many exceedingly difficult problems. To the solution of these problems the admirable work of Osborn and Earle is an important contribution. While adding but few new names to the long list of genera and species already established by Cope, the authors have accomplished what is of far greater value, namely, materially increased our knowledge concerning the structure and systematic relationships of many mammals which had previously been known only from fragmentary remains. In this way the character of the fauna as a whole is set in much clearer light than ever before.

Of the more significant results of this investigation, the following deserve particular mention: (1) The determination of the complete dentition of *Polymastodon*, a representative of the Multituberculata, which was one of the dominant types of Mesozoic mammals. (2) The description of parts of the skeleton of *Indrodon*, showing that it was a true lemuroid, as had been doubtfully surmised before, and the reference of the *Chriacidæ* to the same group. Cope had referred the genera of this family to the creodonts, an example which I had followed, though expressing the opinion that Chriacus and its allies might eventually prove to be lemuroids. (3) A very welcome contribution to our knowledge of the Puerco creodonts is the description of an excellent skeleton of *Dissacus*, the ancestral form of the *Mesonychidæ*. What renders this particularly valuable is the fact that the Bridger genus *Mesonyx* is already very completely known, and the comparison of the two forms is very instructive for discerning some of the modes of mammalian development. (4) A nearly complete skull of the primitive tillodont *Onychodectes* is described and has an important bearing upon the early morphology of the mammalian skull. (5) The skull of *Pantolambda*, the forerunner of the coryphodonts, which became so abundant and varied in the succeeding Wasatch time, is for the first time made known. This is one of the most valuable results of the whole investigation. (6) The suggestion originally made by Schlosser, that *Mioclænus* and its allies are ungulates rather than creodonts, is confirmed, and a new family of Condylarthra is established for their reception. (7) The upper teeth of *Protogonodon* are determined and the likeness of its dentition to that of the primitive artiodactyls pointed out.

Of the greatest general interest to both geologists and biologists are the conclusions reached regarding the character of the Puerco fauna as a whole, which is shown to be of a prevailingly Mesozoic type. Only a small fraction of this fauna is ancestral to Wasatch and Bridger types, and of these most do not persist beyond the Eocene, while by far the greater number of Puerco genera die out without leaving any successors behind them. This generalization is of much importance in clearing away certain stratigraphical difficulties. It is hardly an exaggeration to say that the Puerco mammalian fauna differs more from that of the Wasatch than does the latter from the recent fauna. If the Wasatch mammals, as a whole, were derived from those of the Puerco, then we must assume the existence of a long, unrecorded gap between the two formations, an assumption which geological data do not support. When, however, we examine the Wasatch genera which clearly were derived from Puerco ancestors, such as *Coryphodon*, *Pachyæna*, *Didymictis*, *Ana-*

codon, etc., we find that the degree of advance displayed by these forms is not so very great and that it does not involve any very long lapse of time. The radical difference between the two faunas consists in the ordinal groups which are present in one and not in the other. Thus the Puerco has neither artiodactyls, perissodactyls nor rodents, while the Wasatch has no Multituberculata and relatively few Condylarthra, and the creodonts of the two formations belong, for the most part, to quite different types. The obvious significance of these facts is that at some time between the Puerco and the Wasatch a great migration of mammals from some other region took place and revolutionized the character of the North American fauna.

A distinction that is likely to be fruitful of important results is Osborn's division of the placental mammals into the Mesoplacentalia, of early and more or less Mesozoic type, and the Cenoplacentalia, characteristic of later Tertiary and recent time. "The difference between these two groups consists mainly in the lower state of evolution and apparent incapacity for higher development exhibited by the Mesoplacentals, in contrast with the capacity for rapid development shown by the Cenoplacentals." It can hardly be right, however, to include the creodonts in the lower group, since they not only underwent a great expansion in the Puerco, but in later times they also gave rise, by independent development along at least three lines, to the true Carnivora. Such a group cannot be fairly charged with 'incapacity for higher development.'

This necessarily brief review cannot do more than indicate the many points of unusual interest in this paper, and must refer to the original those who would learn more of it.

W. B. SCOTT.

PRINCETON COLLEGE.

The Ornithology of Illinois; Descriptive Catalogue. By ROBERT RIDGWAY. Published by authority of the State Legislature. Vol. II. May, 1895. Large 8°, pp. 282, pls. 33.

Ridgway's Ornithology of Illinois has a curious history. It was conceived by the able Director of the Illinois State Laboratory of Natural History, Prof. S. A. Forbes, who twelve years ago asked the leading American ornithologist to undertake its preparation. Mr. Ridgway finished the manuscript early in July, 1885. The first volume was finally printed, but the entire edition, together with the plates and cuts, was destroyed by fire. This was in February, 1887. It was reprinted from proof sheets, and proof of the reprint was not submitted to the author. It was issued in November, 1889.

By a singular fatality, the manuscript of the second volume was consumed in the same fire; and, excepting proof of the first 90 pages, which was preserved, the entire book had to be rewritten. This formidable and disheartening task was accomplished in 1891, and the printed book has just been received (May 7, 1895).

The original plan contemplated two distinct parts: Part I., Descriptive Catalogue, by Robert Ridgway; Part II., Economic Ornithology, by S. A. Forbes. The present volume completes the Descriptive Catalogue, and it is earnestly hoped that the volume on Economic Ornithology will follow; though the labor of preparing such a work is too great to be accomplished in a single lifetime or by a single man.

The first volume is prefaced by an introduction of 35 pages, treating of the physical features of the State, the climate, and characteristic features of the avifauna, and ending with a bibliography. The systematic part begins with a key to the higher groups, which are arranged in the old style, the Thrushes coming first. The orders, families and genera are defined, as well as the species. Some of the descriptions are original, but most of them are quoted from 'Baird, Brewer and Ridgway's History of North American Birds', and its continuation, the 'Water Birds of North America,' for which work, as everyone knows, they were originally written by Mr. Ridgway. The general matter is not very full and is frequently quoted from the same work. Unfortunately about two-thirds of the biographical part was omitted because of the necessity of limiting the number of pages. There are numerous quotations from Mr. E. W. Nelson's papers on the birds of Illinois, and a few personal observations by the author, chiefly relating to the Austroriparian fauna of the extreme southern part of the State, where he has done much field work, extending over a long period of years. A novel feature is a synonomy of popular names, given under each species.

The first volume covers 520 pages and is illustrated by 32 plates; the second volume covers 282 pages and has 33 plates. Nearly all the plates in both volumes are from Baird, Brewer and Ridgway, and Ridgway's Manual. Most of those in the second volume were made originally for this work, but owing to delay in publication were first used in the 'Manual.' The great majority are outline figures of heads, wings and feet; but some are shaded cuts of birds. Owing to the destruction of the electros, part of these are process reproductions made from proofs and are poorly printed. The frontispiece is a beautiful colored picture of a Meadowlark in full song, drawn by the author, and of unusual excellence.

In faunal works relating to particular areas it is customary to record somewhat in detail the manner of occurrence of each species, to indicate breeding ranges, time of nesting, dates of migration and so on. Very little information of this kind is to be found in the Ornithology of Illinois. The work consists mainly of technical descriptions and synonymy, to which is added, under each species, a paragraph or two of general matter which as a rule, excepting the quotations from Nelson, is hardly more pertinent to the State of Illinois than to any other part of America where the bird occurs.

Of 49 species classed by Mr. Ridgway as rare, detailed records of occurrence within the State are given for 36.

Mr. Ridgway states that the intent of the book was "to supply the people of Illinois with an inexpensive work which would enable them to identify the birds they desired to learn the names of, and to acquaint them with their leading characteristics." These primary aims the work certainly has fulfilled.

C. H. M.

Tests of Glow-Lamps: W. E. Ayrton and E. A. Medley. The Philosophical Magazine, May, 1895.

Readers of Science who are interested in the matter of electric lighting from a practical standpoint will find much that is instructive in this paper recently printed in the Philosophical Magazine and published as a separate. For several years Professor Ayrton has been investigating the question of the economy of incandescent lighting and and especially the behaviour of the glow-lamp under continuous use. Some of the earlier results of this investigation have been announced from time to time in the English journals, having been communicated by Professor Ayrton to the Physical Society of London. The present pamphlet contains some additions made in January, 1895, and from these additions it appears that the results previously obtained have not been entirely supported by subsequent tests. The principal result reached in these tests was the rather unexpected fact that the glow-lamps examined appeared to increase in effectiveness during the first 80 or

100 hours of their use. It had been very generally assumed that a glow-lamp was at its best, under fixed conditions of pressure, at the very beginning of its life and that it would deteriorate from that time on. The authors of this paper appear to have found, however, that this is not the case and that, on the contrary, the light is increased from the beginning through a certain considerable part of the life of the lamp, after which it slowly fails. One form in which this conclusion is put is that if a group of glow-lamps, such as were examined in this case, being the Edison-Swan Lamps, marked 100–8 and run at a pressure of 100 volts, be kept continuously in operation by putting in a new lamp of the same character whenever a filament breaks, and never replacing the lamps by new except for a broken filament, the light given out by the group will never be as small as at the beginning. Some reference is made to the probable cause of the rise in candle power by use, and the explanation given a year or two ago by Mr. Howell, at a meeting of the American Institute of Electrical Engineers, *i. e.*, that such a rise in candle power is due to an improvement of the vacuum of the lamp during the early part of its life, is commented upon. Some of the earlier examinations of the increase in candle power and improvement in vacuum by the authors of this paper seem decidedly to confirm this explanation by Mr. Howell; but subsequent tests, referred to in the addition to the paper made in January, 1895, are not so favorable to that hypothesis. The authors suggest that the rise in candle power may possibly have been due to a change in the surface of the filament causing the emissivity for heat to decrease, since that would raise the light emitted, as well as the number of candles per watt; but they declare that they have not yet discovered whether such change in heat-emissivity takes place. The methods of carrying on the investigation, both electric and photometric, are explained in sufficient detail, and the whole is a valuable contribution to the subject.

T. C. M.

NOTES AND NEWS.

ENTOMOLOGY.

Dr. T. A. CHAPMAN has been publishing in the Entomologists' Record of London, and has now completed, a paper of no great length but of much importance, on the classification of butterflies, based on the structure of the pupæ, and a comparison of the same with the pupæ of the lower lepidopterous families. He places special emphasis on two points hitherto entirely neglected: The relative freedom of motion of the middle joints of the abdomen, and the relation of the parts on the head on dehiscence. His conclusions are that the Papilionidæ (excluding the Pierinæ) are the nearest relatives of the Hesperidæ (which agrees with all latest researches), but further that the Lycænids "should no longer be regarded as in any way intermediate between the Papilionids and Nymphalids; rather should the Lemoniidæ and Lycænidæ be regarded as a branch which developed from the primæval butterfly (above the Hesperids) in one direction, whilst the Papilionids arose and branched to the Pierids and Nymphalids quite independently. Another point is that the Pierid separated from the Papilionid at a very early stage of the evolution of the latter, and that the Nymphalid almost immediately thereafter separated from the Pierid." These conclusions are borne out by many facts in the structure of the other stages and especially render the position of the Libytheinæ less anomalous.

BRUNNER VON WATTENWYL has just published his Monographie der Pseudophylliden, the last large group of Orthoptera that has specially needed monographic treatment. The group is essentially a tropical one, unknown in Europe and with only one species (as recognized by Brunner) in the United States—our true Katydid. Others will doubtless be found upon our southern borders, for in Mexico, Central America and the Antilles Brunner recognizes 34 genera and 73 species, the larger part of them new. The work, which is published in Vienna in 8°, contains descriptions of 434 species, divided among 122 genera, and is accompanied by a quarto atlas of ten plates.

A NEW QUADRUPLE EXPANSION ENGINE.

Messrs. Hall and Treat announce, in the *Sibley Journal of Engineering* for April, 'A New Quadruple Expansion Engine.' This machine, built for regular working at 500 pounds pressure, and with its boiler, tested to 1300 pounds, has now been in operation in Sibley College, at Cornell University, for many months. It was designed by the authors of the paper, built by them in the shops of the College, and has since been tested under a great variety of conditions. The design was entirely original, although, of course, embodying the principles taught them in their college course, the one being a graduate of '93 and the other of '94, and both now candidates for advanced degrees, the one for a doctor's, the other for the master's, degree in engineering. The valve-gear is new and the invention of the builders of the engine. The proportions of the multiple-cylinder system are those derived by application of their text-book and lecture-room work; and the engine as a whole is a success. The boiler has worked well and economically up to above 600 pounds per square-inch, and its waste heat is utilized in the re-heating apparatus of the engine and so thoroughly as to make the temperature of the chimney very low. The steel for 'running parts' was obtained from the Bethlehem Iron Company and proves to be of very fine quality. Special devices have been required, in every direction, to make the operation of the machine with such high-pressure steam satisfactory and safe. Even the injector was necessarily reconstructed, as no ordinary instrument would force water into the boiler against 600 pounds pressure. The figures reported for economy are something under *ten pounds of steam* per h. p. per hour, and the best conditions of operation are not yet fully identified, though unquestionably corresponding closely with the preliminary computations of the designers. This figure is the lowest yet reported, even for engines of many times the size of that here described. It will require authoritative revision and corroboration; but there seems no reason to doubt its substantial accuracy, as the result of many engine-trials under a great variety of conditions. If thus corroborated, it will stand as the 'record of the world' for the nineteenth century. The thermodynamic consumption of this engine should be about 7 pounds of steam per h. p. per hour, exclusive of all thermal wastes, and this should be approximated much more closely in engines of similar type built on a large scale. The figure attained is extraordinary, and almost incredible, for a model engine such as is described; yet it indicates a waste, by conduction and radiation, after all, of no less than twenty-five per cent. of all heat sent to the machine from its boiler.

PAPERS FOR THE MATHEMATICAL CONGRESS AT KAZÁN.

On the occasion of the dedication of the Lobachévski monument at Kazán will be held a mathematical congress of a week's duration.

It is very much desired by the management that some papers may be contributed by Americans. As a complete program of the scientific communications to be made in the session will be issued this coming February, it is not too early to solicit American scientists to think of preparing something for this memorable occasion. Dr. G. B. Halsted has been asked by President Vasiliev to act for him in this matter, to correspond on questions of detail with any who hope to attend the Congress in person, to take charge of the communications of those who do not anticipate being present and to guarantee their proper presentation.

THE ROYAL GEOGRAPHICAL SOCIETY.

The annual award of the honours of the Royal Geographical Society was made on May 14th, as follows: The Founders' medal to Dr. John Murray for his services to physical geography, and especially to oceanography during the last 23 years, and for his work on board the Challenger and as director of the Challenger Commission and editor of the Challenger publications since the death of Sir Wyville Thomson in 1882; the Patrons' medal to the Hon. George Curzon, M. P., (1) for his work on the history, geography, archæology, and politics of Persia, (2) for his subsequent journeys in French Indo-China, which have resulted in further publications of geographical as well as political and general value, and (3) for his journeys in 1894 to the Hindu Kush, the Pamirs and the Oxus, together with his visit to the Ameer of Afghanistan in Kabul; the Murchison grant was awarded to Mr. Eivind Astrup for his remarkable journey with Lieutenant Peary across the interior glacier to the northern shores of Greenland, and for his independent journey along the shores of Melville Bay, during which he laid down a portion of the northern part only previously seen at a great distance; the Back grant was awarded to Captain C. A. Larsen for the geographical and meteorological observations made by him during his Antarctic voyage in 1894; the Gill memorial was awarded to Captain J. W. Pringle, R. E., for his share in the railway survey operations carried on under the direction of Captain Macdonald, R. E., in the country between the coast from Mombasa to the Victoria Lake; the Cuthbert Peek grant was awarded to Mr. G. F. Scott-Elliot for his explorations of Mount Ruwenzori and the region to the west of the Victoria Nyanza.—*London Times.*

THE NATIONAL GEOGRAPHIC SOCIETY.

The National Geographic Society of Washington held its annual business meeting on May 31. Reports from the various officers bore witness to the increasing usefulness of the Society. When it was first organized, in 1888, there were but 205 members. Since then there has been a steady increase, the membership now numbering 1,193. A similar increase may be noticed in the number of public lectures delivered; sixty-two lectures having been given during the past winter, while in the winter of 1890 there were but eighteen.

Mr. Gardner G. Hubbard was reëlected President and Lieut. Everett Hayden Recording Secretary, and the following were elected Vice-Presidents: C. W. Dabney, Jr., Assistant Secretary of Agriculture; H. G. Ogden, Coast and Geodetic Survey; Gen. A. W. Greely, Chief Signal Service; C. Hart Merriam, Agricultural Department; W. W. Rockhill, Assistant Secretary of State, and Henry Gannet, Chief Topographer United States Geological Survey; Board of Managers, Marcus Baker, United States Geological Survey; G. K. Gilbert, Chief Geographer, United States Geological Survey; John Hyde, Statistical Expert, Agricultural Department; Prof. W J McGee, Bureau of Ethnology; F. H. Newell, Chief Hydrographer, United States Geological Survey; Prof. W. B. Powell and John R. Proctor; Treasurer, C. J. Bell; Recording Secretary, Everett Hayden; Corresponding Secretary, Miss E. R. Scidmore.

BOTANICAL BOOKS AT AUCTION.

Among the botanical books in the library of William B. Rudkin sold at auction in New York by Bangs & Co. were the following: H. Baillon's 'Natural History of Plants,' 7 vols., 8vo, brought $15.87; Bentham and Hooker, 'Genera Plantarum,' London, 1862–83, $17.25; Bentley and Trimen, 'Medicinal Plants,' 306 colored plates, London, 1880, $34; Botanical Gazette, 13 vols., Madison, Wis., v. b., $19.50; Charles Darwin's Works, a rare 'set' of 15 vols., 8vo, uniform green morocco, full gilt, $41.25; D. C. Eaton, 'Ferns of North America,' colored plates by Emerton and Faxon, Salem, 1879, $27; Elwes, J. H., 'Genus Lilium,' grand 4to, London, 1880, $12.50; Emerson, 'Trees of Massachusetts,' 1878, $8.50; John Gerarde, 'The Herball,' enlarged by Thomas Johnson, London, 1636, $14.75; Goodale, 'Wild-flowers of America,' Boston, 1882, $8.25; Lesquereux, 'Coal-Flora of Pennsylvania,' Harrisburg, 1880, $10; J. C. Loudon, 'Arboretum Britannicum,' London, 1854, $17; M. T. Masters, 'Vegetable Teratology,' London, 1869, $8.25; Michaux and Nuttall, 'N. A. Sylva,' 277 colored engravings, 5 v., 8vo, embossed morocco, Philada., 1871, $51.25; Parkinson, 'Theatrum Botanicum,' 4to, panelled calf, London, 1640, $16.40; Ch. Pickering, 'Chronological History of Plants,' Boston, 1879, $6; Powell, 'A Compleat History of Druggs,' London, 1725, $5.50; Seeman, Berthold, 'Plants of the Fiji Islands,' 100 fine colored plates, London, 1865–73, $20.50; Sowerby, 'English Botany,' colored figures by Sowerby, Fitch and others, 12 vols., 8vo, $63; Torrey Botanical Club, various Bulletins, etc., 17 vols., $26.35.

GENERAL.

At the monthly meeting of the trustees of the University of Pennsylvania the acting provost, Charles C. Harrison, made a donation of $500,000 to the University, in honor of his father, the late George L. Harrison, LL. D. Mr. Harrison stipulates that the fund shall be known as 'The George L. Harrison Foundation for the Encouragement of Liberal Studies and the Advancement of Knowledge.' The principal of this fund must be retained intact, the income alone to be used for the purposes of foundation. The following suggestions as to the use of the fund were made by the donor: 1 The establishment of scholarships and fellowships intended solely for men of exceptional ability. 2 The increasing the library of the University, particularly by the acquisition of works of permanent use and of lasting reference, to and by the scholar. 3 The temporary relief from routine work of professors of ability, in order that they may devote themselves to special work. 4 The securing men of distinction to lecture and, for a term, to reside at the University.

According to an announcement from Macmillan & Co., the University Press of Columbia College will issue an *Atlas of Fertilization and Karyokinesis*, by Professor Edmund B. Wilson, with the coöperation of Dr. Edward Leaming. The work will contain forty figures, photographed from nature by Dr. Leaming from the preparations of Professor Wilson at an enlargement of one thousand diameters, and reproduced, without retouching or other alterations, by the gelatine process by Bierstadt, of New York. The photographs are very perfect and convey a good idea of the actual object. They illustrate nearly every important step in fertilization, from the first entrance of the spermatozoön onwards to the cleavage-stages, and not only present a very clear picture of the more familiar outlines of the subject, but embody many original discoveries as well. They are accompanied by an explanatory text, comprising a general elementary introduction, a critical description of the plates and a large number of text-cuts.

THE death is announced of Theodor Brorsen, at the age of seventy-six. He discovered at Kiel, on February 26, 1846, the small comet called by his name, which was found to have a period of about 5½ years, and was observed at returns in 1857, 1868, 1873 and 1879, but has not since been seen, though a conjecture has been thrown out that it had some connexion with one discovered by Mr. Denning last year. Brorsen discovered another comet in 1846, a third in 1847, and two more in 1851.

DR. HUGH FRANCIS CLARKE CLEGHORN died at Strabithie, in Fife, Scotland, on May 19th. He was appointed Professor of Botany in Madras University in 1852, and was an authority on Indian botany and arboriculture. While in Madras he organized a forest department, having for its object the preservation of tree life, and established an admirable system of management. Dr. Cleghorn returned to Scotland in 1869, filling temporarily the chair of Botany in Glasgow University. He was also president of the Royal Arboricultural Society and an active member of the Edinburgh Botanical Society.

AT the commencement exercises of Stanford University, President Jordan stated that Mrs. Stanford had been spending $1,000 a day of her private fortune to maintain the University. In case Mrs. Stanford's fortune should be exhausted before the decision of the Courts in regard to the Stanford estate had been reached, it would be necessary to close the University.

JOHN PAUL PAULISON died at Tenafly, New Jersey, on May 30th. Mr. Paulison was interested in astronomy and owned a private observatory.

PROFESSOR J. J. STEVENSON, of the University of the City of New York, will spend the summer in the coal fields of Arkansas, Indian Territory and Texas, with incidental studies in New Mexico and Colorado.

DR. ADOLF ELSASS, Professor of Physics in the University of Marburg, died on May 12th, at the age of forty years.

THE June issue of the Amherst Literary Monthly will be a special memorial number devoted to President Seelye.

THE Royal Natural History, edited by Richard Lydekker (reviewed in SCIENCE, April 5, p. 387) is being published in America by Frederick Warne & Co. It will be issued in thirty-six fortnightly numbers and will be completed at the same time as the English edition.

DR. D. K. PEARSON has offered $50,000 to Mount Holyoke College if an additional $150,000 can be raised. It is said that Dr. Pearson has already given $2,000,000 to various colleges.

HAROLD WHITING, Professor of Physics in the University of California, was among those lost in the submergence of the steamship Colima.

AT the May meeting of the Victoria Institute, London, the subject of 'Early Man' was considered. In dealing with it the evidence for the existence of a 'missing link' was first examined, the subject being introduced in a paper by Professor E. Hull, late Director-General of the Geological Survey of Ireland. In dealing with it he reviewed all the known instances of so-called 'missing links,' including that discovered by Dr. Dubois in Java, and concluded that none could be regarded as in fact 'a missing link.' After this the question of the earliest man was discussed in a paper by Sir William Dawson, in which he described the physical character and affinities of the Gaunches, an extinct race in the Canary Islands.

MR. W. W. ROCKHILL, Assistant Secretary of State, who has been appointed by the State Department a delegate to the International Geographical Congress, meeting in London this summer, will join with a delegation from the National Geographic Society in an effort to persuade the Congress to hold its next meeting in Washington.

The death is announced of Dr. Franz Neumann, the oldest active teacher in Germany. In 1826 he was called to the Professorship of Physics and Mineralogy in the University of Königsberg, and for sixty-nine years has been teaching and working in the same institution. Dr. Neumann was the first man in Germany to teach Mathematical Physics.

It is stated that Professor E. E. Barnard and Professor Burnham have accepted positions in the Yerkes Observatory, Chicago.

Principal Peterson, who has accepted the Principalship of McGill University, in succession to Sir William Dawson, graduated at Edinburgh University in 1875, and afterwards gained an open scholarship at Corpus Christi College, Oxford. For two and a half years he acted as assistant to the Professor of Humanity in Edinburgh University. On the inauguration of University College, Dundee, in 1882, Mr. Peterson was unanimously appointed Principal and Professor of Classics and Ancient History.

Major William A. Shepard, for twenty-five years Professor of Chemistry in Randolph Macon College, died in Ashland, Va., on June 3d.

A statue of the late Professor Billroth was unveiled in the Hospital Rudolfinerhaus on April 25th.

SOCIETIES AND ACADEMIES.

GEOLOGICAL SOCIETY OF WASHINGTON.

The following are abstracts of the communications presented at the thirty-fourth meeting, May 8, 1895 :

G. F. Becker. 'Gold Fields of the Southern Appalachians.' This communication presented a summary of a report upon these gold fields, based upon field work of the last season, which will appear in the Sixteenth Annual Report of the Director of the U. S. Geological Survey, and will be issued in separate form very soon.

The geographical position, history and statistics of the known deposits were first given, followed by a discussion of the rock formations and the structural features of the regions in which the deposits occur. The gold-bearing veins and impregnations were then described, and a long list of the observed gangue minerals was given, with comments upon their significance. The secondary, or placer deposits, were also considered.

C. Willard Hayes. 'Notes on the Geology of the Cartersville Sheet, Georgia.' The region covered by the Cartersville sheet is in northwest Georgia, its northern and western borders being about thirty miles respectively from the Tennessee and Alabama lines. Its topography is dominated by two peneplains, the older preserved by the harder metamorphic and crystalline rocks on the eastern side of the sheet, and the younger developed on comparatively soft limestones and shales. The older peneplain shows a decided southward inclination from an altitude of 1,400 feet at the north edge of the sheet to 1,000 at the south edge. Above the peneplain rise a few monadnocks from 800 to 1,000 feet, while the larger streams have cut their channels several hundred feet deep within it. The lower peneplain has an altitude of between 800 and 900 feet, and a slight inclination toward the west. The two plains probably coincide a short distance east of this region, in the vicinity of Atlanta.

Two distinct groups of rocks are found in this sheet, separated by a profound fault. The rocks west of the fault are unaltered Cambrian and Silurian, while those to the east are crystalline and metamorphic, probably Archean and Algonkian. The most striking structural feature on the sheet is the Cartersville fault by which the metamorphic rocks are superposed upon the unaltered Paleozoics. In the northern portion of the sheet the fault plane dips eastward at a low angle, in general less than 15°, the Cambrian limestone and shale passing under the black Algonkian slate and conglomerate which lie in open folds to the eastward.

In the vicinity of Cartersville the fault plane dips eastward much more steeply, probably not less than 75°. A short distance east of this portion of the fault is a large mass of granite, probably Archean, to which the change in the character of the fault is doubtless due. While to the north and south of this granite mass the sedimentary rocks were readily moved upon their bedding planes, so that they transgressed a long distance upon the Paleozoics, the absence of planes in the granite retarded movement at this point, causing a deep reentrant angle in the course of the fault. A further effect of the anchoring of the strata by this granite mass is seen in the abnormal strikes at its northern end. The sedimentary rocks have been carried past it toward the west, so that for a distance of fifteen miles they strike northwest, at right angles to the normal axes of this region.

ALFRED H. BROOKS. 'Notes on the Crystalline Rocks of the Cartersville Sheet, Georgia.' In this paper Mr. Brooks gave petrographical descriptions of the granites, diorite, gabbro and hornblende schist of the Cartersville district.

LESTER F. WARD. 'The Red Hills and Sand Hills of South Carolina.' The speaker considered these well known topographic features of a broad band crossing South Carolina, concerning which various opinions have been held, to be remnants of the Lafayette formation. He described localities where the red and white sands were observed to grade into, or alternate with, each other as parts of one formation. As this formation overlaps various older beds to the granite, the discovery of Eocene fossils by Tuomey at the base of certain hills may be understood.

The red and white sands are associated with shales and clays, and Professor Ward believed that they were to be considered as a northeastern extension of the 'Red loam' (Lafayette) formation of the Gulf States.

WHITMAN CROSS,
Secretary.

NEW YORK ACADEMY OF SCIENCES.

THE section of geology met on May 20, and listened to the following papers:

J. F. Kemp, 'The Iron-ore Bodies at Mineville, Essex County, N. Y.' The history of iron mining in this district was briefly outlined by the speaker, and the early development of the enormous orebodies at Mineville was sketched. Their geological relations were then shown by means of a series of sections, about twenty-five in number, which had been prepared by the engineer of the companies operating the mines, Mr. S. B. McKee, assisted by the speaker. These sections had been drawn under the guidance of Prof. Kemp on panes of thin crystal plate glass about one-eighth inch in thickness and 21x33 inches. The glass is of such transparency that the entire series of sections came out very clearly and showed the relations of the ore-bodies with great vividness. The scale was one inch to the hundred feet, making thus twenty-five vertical sections, one hundred feet apart and extending nearly half a mile. It was at once apparent that Miller Pit, Old Bed, '21,' the Bonanza and the Joker ore-bodies were all really parts of one enormous mass which lies on a pitching anticline. Miller Pit and Old Bed are faulted from each other and from '21.' A trap dike intersects Miller Pit. In the field the relations are very confusing, and it can not be stated that the model clears them all up, but it shows the broader features admirably and will be later described in greater fullness.

The speaker gave some further details of the geological relations of the ore and the character of the rocks as shown by drill cores. The presence of intruded sheets of gabbro in the gneisses was especially emphasized, and in particular their existence as proved by the cores, immediately beneath some thin beds or veins of ore. The paper was further illustrated by a large series of lantern slides of the mines.

The second paper, by G. van Ingen, on 'The significance of the recent studies of Mr. G. F. Matthew on Cambrian Faunas as published by the Academy,' covered practically the same ground as did Mr. Matthew's abstract printed in SCIENCE April 26, p. 452. Mr. van Ingen added many additional particulars based on his field experience in collecting the fossils, and also exhibited comparative sections of the Cambrian in both Europe and America.

The third paper, by W. D. Matthew, 'The Effusive and Dike Rocks, near St. John, N. B.,' was postponed on account of the lateness of the hour. It appears, however, in full in the Transactions of the Academy, and adds much to our knowledge of the Pre-Cambrian volcanic rocks of New Brunswick.

J. F. KEMP,
Recording Secretary.

SCIENTIFIC JOURNALS.

THE PHYSICAL REVIEW.

Vol. II., No. 6. May–June, 1895.

The Capacity of Electrolytic Condensers: By SAMUEL SHELDON, H. W. LEITCH and A. N. SHAW.

This paper contains a description of experiments performed upon two types of Platinum—H_2SO_4 cells, which, when charged to potentials less than the E. M. F. of polarization, are found to act as condensers. The capacity of such condensers is dependent upon the impressed E. M. F. as well as upon the surface and character of the electrodes. By a method quite analogous to the 'ballistic method' of testing iron the authors have shown the presence of a very considerable hysteresis in the relation between potential and charge. The curves showing this relation present in fact a striking resemblance to the ordinary hysteresis loop. Considerable difficulty was met with in reducing the electrodes to an unpolarized condition, even with new specimens of platinum. Here also an application of magnetic methods was found useful, the cells being conveniently depolarized by reversals. The paper contains also an investigation of the effect of temperature and concentration upon the capacity. In spite of the large capacity of electrolytic condensers, the authors are of the opinion that the high temperature coefficient and low efficiency of such cells are prohibitive to practical usage.

Thermal Conductivity of Copper, I. By R. W. QUICK, C. D. CHILD and B. S. LANPHEAR.

In this article is begun the description of observations made to determine the thermal conductivity of a bar of copper intended for use as a standard of length. The method used was that of Forbes. The measurement of the temperature at different points of the bar was made by a method different from that usually employed, and depended upon the variation in the resistance of a coil of fine copper wire, which could be shifted from point to point throughout the length of the bar. Results were obtained for the conductivity through a range of temperatures extending from 74° to 167°, the extreme values being 0.914 at the lower of these two temperatures and 1.024 at the higher. Observations at temperatures below 0° will appear in a subsequent article.

On the Absorption of Certain Crystals in the Infra-red as Dependent Upon the Direction of the Plane of Polarization. By ERNEST MERRITT.

By means of a spectro-bolometer the writer has determined the transmission curves for Quartz, Iceland Spar, and Turmalin out to a wave length of 5.5 μ. In order to detect the differences between the absorption of the ordinary and extraordinary rays the radiation used (that of a Zirconium lamp) was polarized by reflection before passing through the crystal specimen. On account of diffuse rays from the surface of the fluorite prism considerable difficulty was met with in obtaining a pure spectrum; a difficulty which was finally met by using two spectrometers 'in series;' *i. e.*, the spectrum formed by one spectrometer was thrown upon the slit of another. The results show that the transmission curves of the ordinary and extraordinary rays are entirely independent in all three cases. In the case of Iceland Spar the differences between the two curves is especially marked, sharp absorption bands being present in the one curve which are entirely absent in the other. At $\lambda = 3.3\ \mu$ Iceland Spar is found to behave as turmalin, *i. e.*, the ordinary ray is suppressed, while the extraordinary ray is transmitted in considerable amount. The difference between the two curves is less marked in the case of Quartz, but is very considerable with Turmalin. In the latter case the two curves are found to intersect, and in the region lying between the points of intersection the dechroism of turmalin is reversed.

Resonance in Transformer Circuits. By F. BEDELL and A. C. CREHORE.

In this article the writers discuss the action of a condenser in either circuit of a transformer, and develop by purely graphical methods the conditions necessary for primary resonance due to a secondary condenser, a phenomenon to which Dr. Pupin has given the name electrical consonance. A primary circuit alone, and with no condenser, would have no natural period of oscillation; but it may have such a period when a neighboring secondary circuit contains a condenser. The elastic influence of the condenser is transferred from one circuit to the other, on account of their mutual relationship; and the natural period of the primary circuit depends not only upon the value of its own constants, but those of the secondary as well. There is a surging of energy back and forth between the primary circuit and the secondary condenser by intervention of their common magnetic field; the period of these surgings determines the period of the system. In addition to the graphical analysis, Drs. Bedell and Crehore subject the problem to a brief analytical treatment leading to identical results. It is shown that there are two values of the capacity of the secondary condenser which will give rise to consonance. It is pointed out that a condenser in the secondary of the transformer may compensate for the drop due to magnetic leakage; in fact, this drop may be over-compensated for, so that the secondary potential will actually rise as the transformer is loaded down.

Aside from the particular conclusions reached, the paper is of interest for the methods employed, the problem in hand illustrating well the writer's method of reciprocal points in constructing admittance and current diagrams from diagrams of impedance and electromotive forces.

On the Secular Motion of a Free Magnetic Needle, I. By L. A. BAUER.

This article forms the introduction to an important paper on the secular variation of terrestrial magnetism which will be concluded in the next number. The present article is devoted to a description of the methods of accumulating and discussing the available data. An abstract is postponed until the appearance of the remainder of the paper.

New method of Testing the Magnetic Properties of Iron. By W. S. Franklin.

In determining the curve of magnetization, the sample, in the form of a long narrow ∩, is suspended from the arm of a balance, the legs of the ∩ being surrounded by fixed magnetizing coils. The induction may then be calculated from the weight necessary to hold the specimen in equilibrium. A novel method of determining hysteresis loss is also described. In this case the sample was in the form of a long rod, and was magnetized by a rather short coil. The rod was suspended from one part of a balance, and was weighed first while the coil was moved slowly upward and afterwards during a slow downward motion of the coil. A method is developed by which the hysteresis loss may be computed from the difference of these weights. Experimental data accompany the paper.

New Books. The following books are reviewed: Rayleigh. *Theory of Sound, Vol. I.* Poincaré. *Les Oscillations Electriques.* Carhart. *University Physics.*

THE JOURNAL OF COMPARATIVE NEUROLOGY.

The Journal of Comparative Neurology for March contains three original papers. The first, 'Modern Algedonic Theories,' by C. L. Herrick, is a critique based primarily upon Marshall's Pain, Pleasure and Æsthetics, though most of the other recent literature is reviewed in the same connection. The physiological theory of emotion finally adopted by the writer is in the main a composite drawn chiefly from the nutrition theory of Meynert, the discharge theory of Lange and James, and the theory of habit of Gilman. In brief, it is a resistance theory. When we have agreed upon the nature of the simplest sense, pain and gratification, the foundation will have been laid for the more complex æsthetic phenomena. This foundation is believed to consist in the recognition of a special kind of neurosis for the feelings due to two classes of stimuli of a very similar but not identical kind. Given an excessive stimulus which for whatever reason freely irradiates, and pleasure is felt; given another stimulus, or the same excessive stimulus with other neural conditions which prevent irradiation and produce a summation and overflow, and pain is felt. Emotion consists (1) of general sensations of total, organic or irradiating varieties which have in common a lack of localization and, as a result of associational laws, are amalgamated more or less closely with the empirical ego; (2) of more or less explicate or implicate cognitions (perceptions, intuitions) of the relation between the cause of the sensation and our well-being; (3) the emotion is more or less closely attached to various impulsive expressions which tend in various ways to intensify the two preceding. The psychical element of emotion is essentially intellectual, and the attempt to secure a serial relation of the 'faculties' must be abandoned.

The second paper by M. A. Raffalovich deals with 'Uranism, or Congenital Sexual Inversion.' It is a plea for the early recognition of congenital inversion in children and the proper education of such children. Inversion is no excuse for debauchery and Krafft-Ebbing's pity for the race of inverted persons is largely misplaced. The psychological history of a superior uranist is traced and commented upon at length.

In a brief paper entitled 'The Histogenesis of the Cerebellum,' C. L. Herrick notices the recent work of Dr. Shaper upon the cerebellum of teleosts and calls attention to the gratifying harmony between these results and his own studies published in 1891.

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Subscriptions and advertisements should be sent to SCIENCE, 41 N. Queen St., Lancaster, Pa., or 41 East 49th St., New York.

ON THE DISTRIBUTION AND THE SECULAR VARIATION OF TERRESTRIAL MAGNETISM.

IN two papers* read before the Philosophical Society of Washington, May 25th, the following main results were obtained:

The minimum change in declination along a parallel of latitude at any particular time, and the minimum average secular change along a parallel of latitude during a given interval of time occur near the equator; both quantities generally increase on leaving the equator.

Exactly the reverse is the case with regard to the inclination, viz.:—

The maximum change in inclination along a parallel of latitude at any particular time, and the maximum average secular change along a parallel of latitude during a given interval of time occur near the equator; both quantities generally diminish on leaving the equator.

These laws were established with the aid of data scaled from magnetic charts from 1780 to 1885 at points 20° distant in longitude and in latitudes 60°N, 40°N, 20°N, equator, 20°S, 40°S and 60°S. They again point to the same conclusion reached previously by the writer in a somewhat different way, namely, *that the distribution and the secular variation of terrestrial magnetism appear to be closely related; they are subject to similar laws. It is hence probable that they are both to be referred primarily to the same cause. This common cause seems to be connected in some way with the earth's rotation.*

If we regard the earth as uniformly magnetized, having its magnetic poles coincident with the geographical poles, and take the X axis of a system of coördinates whose origin is in the center of the earth, parallel to the magnetic axis, we shall get the fol-

* 'On the Secular Variation of Terrestrial Magnetism' and 'A Preliminary Analysis of the Problem of Terrestrial Magnetism and its Variations.'

lowing expression for the potential function at any external point, viz.:

$$\psi = \tfrac{4}{3}\,\pi\,\mu\,a^3.\,\frac{x}{r^3}$$

a is the mean radius of the earth, r the distance of the point from the origin, and μ the intensity of magnetization per unit of volume.

For points on the earth's surface, this reduces to:

$$\frac{\psi}{a} = \psi_a = \tfrac{4}{3}\,\pi\,\mu.\,\sin\phi = c.\,\sin\phi \qquad (1)$$

ϕ is the geographical latitude and $c = \frac{4}{3}\pi\mu$.

This formula is doubly interesting just now, as it has been recently deduced empirically by Professor W. von Bezold.* This eminent investigator, when considering the mean values of the geomagnetic potential along parallels of latitude, found them subject to the simple law $\psi_a = c.\,\sin\phi = 0.330\,\sin\phi$. Since $c = \frac{4}{3}\pi\mu$, and the magnetic moment, M, of the earth is equal to $\frac{4}{3}\pi\mu.\,a^3$, we find that von Bezold's empirical coefficient implies a value of the magnetic moment equal to 8.52×10^{25} against 8.55×10^{25} as determined by Gauss. We thus see the theoretical significance of von Bezold's factor.

Since for the case supposed the horizontal component of the intensity, H, is directed meridionally, it follows from (1) that:

$$H = \frac{\delta\psi}{a\,\delta\phi} = c.\,\cos\phi \qquad (2)$$

Furthermore, with the aid of simple transformations:

$$V = 2\,c.\,\sin\phi \qquad (3)$$
$$F = c.\,\sqrt{3\sin^2\phi + 1} \qquad (4)$$
$$\tan I = 2\,\tan\phi \qquad (5)$$

V being the vertical force, F, the total and I, the inclination. Formulæ (2), (3), (4) and (5) are familiar to every nautical geomagnetician.

* See his admirable paper 'Über Isanomalen des erdmagnetischen Potentials,' Sitz. berichte d. Kgl. Preuss. Akad. d. Wiss. zu Berlin. Phys.-math. Classe, April 4, 1895.

Now the writer finds that these formulæ give the mean values of the magnetic elements along parallels of latitude with a high degree of precision. As this paper will be printed in full in the *American Journal of Science* beginning with the August number, I will select but one typical example.

1885.

Latitude	I obs'd*	I Comp'd	O.–C.
60° N	74°.9 N	73°.9 N	+ 1°.0
40°	59 .7 N	59 .2 N	+ 0 .5
20°	34 .3 N	36 .1 N	− 1 .8
Equator	3 .2 S	0 .0	− 3 .2
20°	36 .8 S	36 .1 S	− 0 .7
40°	57 .2 S	59 .2 S	+ 2 .0
60° S	70 .2 S	73 .9 S	+ 3 .7

Since, according to equation (2).

$$c = \tfrac{4}{3}\,\pi\,\mu = \frac{M}{a^3} = H.\,\sec\phi \qquad (6).$$

we can get a fair value of the magnetic moment of the earth without the aid of the laborious Gaussian computation by simply scaling the value of H for equidistant points along a parallel of latitude from isodynamic charts and substituting the mean of the values thus found in (6).

Thus I get for 1885 as the mean result of the scalings along 40° N, 20° N, Equator, 20° S and 40° S, the value of 0.325 a^3 for M, against 0.322 a^3 resulting from the 1885 Neumayer-Petersen re-computation of the Gaussian co-efficients.

But why should the values obtained on the assumption that the earth is uniformly magnetized, and its magnetic axis coincident with the geographical axis, so nearly agree with those based upon observed quantities? It seems to me that this opens the question whether the asymmetrical distribution of land and water is the primary cause of the asymmetrical distribution of telluric magnetism, as generally supposed. Why do the 'anomalies' in the distribution so nearly cancel each other in going along a parallel of lati-

* These quantities are the results of the scalings of Neumayer's charts for the points mentioned.

tude? Does this again imply that the rotation of the magnetic earth is an important factor?

If we connect by lines all the places on the earth's surface having the same departure (with due regard to sign) from the values as computed from above formulæ we get a series of curves that converge around two foci of maximum and minimum departures. I have carried out this idea with the aid of my collected data in the case of the inclination for three epochs, 1780, 1880 and 1885. I call the curves thus obtained lines of equal departing inclination, or, briefly, 'isapoclinics.' It is especially remarkable that these lines close around two points not on *opposite* sides of the equator, but on the *same* side.* Their preliminary positions are:

	Latitude.	Longitude.
For 1885.		
North end attracting focus,	20°S.	40°W of Gr.
South end attracting focus,	5°S.	40°E of Gr.
For 1780.		
N. F.	0°	50°W.
S. F.	0°	60°E.

These positions are subject to a slight revision. The main part, however, is brought out very clearly in both cases, viz.: *that the chief cause of distortion of the primary symmetrical field can be represented as due to a secondary polarization approximately equatorial in direction.*

I then showed that the isapoclinics obey in a remarkable degree the laws governing a magnetic system. They do not run at random. Thus, for example, the foci or poles of this secondary system fall nearly on the agonic lines of the actually observed field, and the secondary magnetic equator running roughly north and south marks out approximately the places where occurs the maximum declination. *In a word, the magnetic field which we actually observe can be nearly obtained by super-imposing a secondary equatorial field upon a primary polar one.*

By comparing the maximum horizontal intensities of the the two systems, as found in the respective magnetic equators, I find that the *polar field is about five to six times stronger than the secondary, and that the axis of the resultant system would make an angle of about 10° with the rotation axis.*

Furthermore, the secular variation phenomena can be qualitatively explained by the shifting of just two such poles as belong to the secondary system. It cannot be explained by the disturbance of poles on *opposite* sides of the equator.

We should thus have to refer both the distribution and the secular variation to apparently the same kind of a polarization.

This harmonizes with the empirical conclusions at the beginning of this paper.

Since the intersection of the agonic lines with the equator fall so nearly together with the positions of the isapoclinic foci, a fair idea, perhaps, can be obtained of the shifting of these foci from the motion of the agonic lines along the equator. I find that both agonic lines have been moving westwardly along the equator for the last 300 years at the average rate of about 0.°2 per annum. If the motion continues around the equator at this rate the resulting period would be about 2000 years, but I do not wish to be understood as asserting that this is the secular variation period.

A possible third field, which has been made probable by Dr. A. Schmidt's beautiful researches, was also pointed out. Schmidt found, namely, that not the entire observed magnetic effect on the earth can be referred to a potential; currents that *pierce* the earth's surface seem to make themselves felt. Perhaps his currents can be explained thus: If an arbitrarily magnetized sphere rotates in a conducting fluid,

*Similar results have been obtained by von Bezold in the paper cited, and by A. von Tillo as seen in his preliminary paper in Comptes Rendus, Oct. 8, '94, pp. 597–599. It is very much to be hoped that von Tillo's charts will soon be published.

the surface of contact of sphere and fluid being conducting, currents will be incited in the fluid that will pass into the sphere and out again.

In the case of the earth there is no fluid with reference to which the solid earth performs a *total* differential rotation; still there are *partial* differential rotations due to moving streams, ocean currents, tidal waves and air currents. Such a field, if it exist, can be differentiated with the aid of the potential theory.

Purely local disturbances would constitute a fourth—the 'anomalous field.'

We as yet have no satisfactory answer as to the *origin* of the earth's primary magnetic field, neither has the astronomer an answer to the query 'Whence the moon.' He, however, accepts the moon's existence and computes its disturbing effects upon the earth's motions. Just so it is with the earth's magnetism. We do not know whence it has come, but we know it is there. We know that to-day the magnetic earth is rotating about an eccentric axis, and so let us ask ourselves *What is the effect of the self-inductive action of the rotating magnetic earth? How is the principle of the conservation of energy when applied to the motions of the magnetic earth to be fulfilled?*

L. A. Bauer.

ON A DEVONIAN LIMESTONE-BRECCIA IN SOUTHWESTERN MISSOURI.

The brecciated limestone which it is proposed to describe in this paper outcrops near the base of Eagle Ridge, on the west side of the valley of Dry Creek, five miles west of the town of Galena, county seat of Stone County, Missouri. The several members of the Devonian strata in this portion of the State are, in their normal condition, very regular and evenly bedded, and are perfectly conformable, from their base, to and with the overlying Kinderhook Group. They rest, with slight local unconformity, on the magnesian limestones of the Ozark Series, and then out toward the east, at the expense of the lower members, each stratum overlapping that which is under it. In the vicinity of the limestone breccia they present the following sections: 1. Green Shale, 7 feet. 2. Shaley Limestone, 10 feet. 3. Speckled Crinoidal Limestone, 3 feet. 4. Basal Conglomeratic Sandstone, 4 inches.

Proceeding south along the west side of the valley we find the first indication of a disturbance in the form of a gentle undulation of the upper portion of the shaley limestone, No. 2 of the section. A few hundred yards further we encounter the first of a series of huge masses of breccia, consisting of the light gray, amorphous limestone and thin shale of No. 2, broken into angular fragments of various sizes, and recemented, partly by a similar substance, and partly by the subsequent infiltration of calcareous matter occurring now in the form of calcite. The original bedding planes have been mostly obliterated, and the breccia weathers out along the hillside in boulder-like masses, 10 to 20 feet thick, and 50 to 100 feet in width. A stratum of shaley limestone at the base of these masses partially retains its original appearance, and from its relation to the more massive breccia overlying it the whole is seen to have been subjected to violent contortion and fracture, such that boulders of hard limestone have been forced into the midst of calcareous shale. There are about half a dozen of these masses exposed along the valley side, in a distance of about 1000 feet; then the undulations decrease, and at one-half mile from where the first disturbance in the strata was noticed they entirely cease, and from thence down the valley the strata are in their normal condition.

There is no indication of the action of water in the formation of the breccia. All the fragments are sharply angular, and frequently a fossil has been broken through and the positions of the pieces slightly changed, but not widely separated as they would inevitably have been had the brecciated masses been accumulated by wave action on a seashore. The hypothesis that the brecciation and contortion were produced by undermining of the strata and by subsequent crushing from the weight of the superincumbent rock is inconsistent with the facts. The lower members of the Devonian strata are undisturbed, and in the central portion even the whole of No. 2 seems to be present and perfectly horizontal and the breccia rests on it increasing the thickness of the Devonian strata from its normal 20 feet to 40 feet in the central portion of the disturbance.

In short, the only theory which will explain all the phenomena is that which has been applied, in explanation of the manner of formation of similar but vastly more extended Devonian limestone breccias in Iowa, viz., by lateral pressure produced by the 'creep' or sliding on a sloping sea bottom of the displaced strata immediately after their deposition.

From a study of the strike of the undulations, displacements and other attendant phenomena, it becomes evident that the pressure was applied from the northeast. The Devonian strata at present rise in that direction at a rate not exceeding 8 or 10 feet per mile, and during the Devonian age were doubtless still more nearly horizontal. It is remarkable that so slight a slope could have given rise to a sliding of a portion of the sea bottom, but it is undoubtedly the fact that, while the deposition of the Devonian strata had proceeded without interruption to the top of the shaley limestone No. 2, the upper 2 or 3 feet began to slide on the underlying stratum. About the western line of Stone county the resistance overcame the weight of the 'creeping' strata, and the tension becoming too strong, at one place certainly and perhaps at others not yet discovered, that they suddenly gave way, were contorted, brecciated, forced forward and hurled in boulder-like masses on to other undisturbed strata.

Considering the intensity of the force and the conditions under which it was applied, it is surprising that the area of the disturbance should be so small; on the opposite side of the valley, one-eighth of a mile distant, there is not the slightest sign of it, and in the next valley, one-fourth of a mile southwest from it, the Devonian strata are undisturbed. Its areal extent cannot be greater than one-fourth square mile.

The lithification of the shaley limestone was practically complete at the time of the displacement, for the fragments are all sharply angular and must then have been very hard. And as the relation of the overlying strata shows that the period of the disturbance immediately succeeded that of deposition of No. 2, deposition and lithification must have proceeded contemporaneously.

The green shale, which is the upper member of the Devonian in this region, thins out in the hollows between the dome-shaped prominences of the surface of the breccia, and totally disappears over the higher portions of the disturbed area. The points where it is absent are not now and never were more than twenty feet higher than the surrounding sea bottom, where the green shale was deposited in very regular laminæ, without wave action. The areal distribution of the green shale is such as to show that it was deposited in a comparatively small and shallow esturine basin, connecting with the sea toward the south, and supplied with fine sediment from the land on the east and north. The limited extent of this body of water accounts for the feebleness of its waves, which did not affect the green shale at the depth of only twenty feet around the elevated area formed by the breccia. The higher prominences of the breccia were slightly eroded by wave action during the deposition of the green shale in the surrounding water, but the leveling had not proceeded far when the Devonian age came to a close; the entire region was depressed, and the Louisiana limestone (formerly known as the Lithographic limestone), or basal member of the Kinderhook Group, was laid down over the breccia. It is usually a regularly bedded, dark gray limestone, everywhere perfectly conformable to the green shale, but over the distributed area it is irregularly bedded and slightly arched, but soon succeeded, by thickening in the hollows and thinning over the prominences, in leveling off the ancient sea bottom. The Lower Carboniferous strata are here locally unconformable with the Devonian. We have thus seen that the thinning of the green shale over the area of disturbance fixes the time of said disturbance at the period between the deposition of Nos. 1 and 2 or the shaley limestone and the green shale. From a general resemblance between the shaley limestone of this region and portions of the Cedar valley limestone of Iowa, and from the fact that this peculiar mode of brecciation obtained in both regions, I wish to suggest that the light brown or gray, amorphous, shaley limestone of southwestern Missouri may be the equivalent of the Cedar valley limestone of central Iowa.

Oscar H. Hershey.

Galena, Mo.

CURRENT NOTES ON PHYSIOGRAPHY (X.)

LEY'S CLOUDLAND.

This long expected work (Stanford, London, 1894. 208 p.) is an effort to establish a classification and terminology of clouds on a genetic basis. While such a plan has much to commend it, and must eventually be adopted in fully developed form, its presentation now is perhaps premature; for there is yet much to learn regarding the origin of certain cloud forms, and much difference of opinion still prevails on the subject. Four chief classes are recognized in Ley's scheme: clouds of radiation, such as ground fogs; of inversion, such as cumulus, dependent on overturnings in an unstable atmosphere; of interfret, such as waving stratiform clouds formed at the contact of layers of different temperature; and of inclination, such as pendent cirrus wisps, caused by the settlement of particles from one atmospheric stratum into another. The illustrations, reproduced from photographs by Clayden, are for the most part excellent. The chief deficiency of the work is the absence of comparative tables, by which the terms proposed by Ley may be translated into those adopted by the International Meteorological Congress. In a number of passages exceptions must be taken to the manner of physical explanation of cloud formation, especially to statements concerning the relation of water and ice particles in cumulus and cirrus clouds, and to the repeated implication that the liberation of latent heat in the condensation of vapor actually warms the air. The chapters on the theory of atmospheric currents and on the prevailing winds of the globe are hardly relevant to the rest of the book and add little value to it. Remembering that the author has devoted years of observation to cloud study, and that latterly his work has been much interrupted by ill health, it is doubly a regret that his book cannot be more highly commended.

BUREAU CENTRAL MÉTÉOROLOGIQUE.

The latest series of *Annales* of this important Bureau contain as usual a volume of memoirs in which, besides the statistical studies of thunder storms in France by Fron and several reports of magnetism, there are essays by Angot on the advance of vegetation and the migration of birds in France for ten years, 1881–1890, and on the meteorological observations on the Eiffel tower during 1892; and by Durand-Gréville on squalls and thunderstorms. Nearly all the features of the advance of vegetation exhibit the accelerating influence of the Mediterranean and the retarding influence of the Bay of Biscay. The records of the Eiffel tower are chiefly interesting in showing inversions of nocturnal temperature in the means of all the months, and consequently in proving a distinct variation in the diurnal values of the vertical temperature gradient in the lower atmosphere; as well as a change of the time of maximum wind velocity from afternoon at surface stations to night at the top of the tower. Durand-Gréville's essay is illustrated by an excellent chart of the distribution of pressure during an extended squall that occurred on August 27, 1890; the isobars being drawn for every millimeter, and showing a sharp N-like double bend at the place of the squall.

WINTER STORMS IN THE NORTH SEA.

THE famous Christmas storm of 1821, which led Brandes and Dove to their early statements concerning the system of storm winds, finds a modern parallel in a storm of December 22–23, 1894, described by Köppen in the *Annalen der Hydrographie*, edited by the Naval Observatory at Hamburg, and published in Berlin. On the morning of December 22 the storm center, with a pressure of 715 mm., lay just east of Scotland; on the evening, with a pressure of 725, the center lay just west of Denmark. The whirling courses of the winds are well illustrated; a southerly gale crossed the Baltic, while a northerly gale raged on the North sea; violent east winds blew off the coast of Norway, and westerly gales were recorded in northern Germany. Disastrous storm floods were felt at many points on the coast, and salty rain fell at many points in England. Other storms were felt a week earlier and later; but, apropos of this apparent periodicity, Köppen remarks that thus far all efforts to establish weekly, monthly or longer weather cycles have, without exception, failed, and that, while the faint and easily obliterated traces of such periods have a certain scientific interest, they have not yet a practical value. The *Annalen der Hydrographie* is a characteristic German journal, in which a serious and scientific style of work is carried into the accounts of foreign coasts and harbors, as reported by officers of the marine. It frequently contains articles and reviews of interest on winds, tides and currents.

ELEVATION AS A CAUSE OF GLACIATION.

IT is probable that no one questions the sufficiency of elevation to account for glaciation, if other things, such as external controls of climate, remain unchanged; but there are serious difficulties in the way of accepting the thesis maintained by Upham (latest expressed in Bull. Geol. Soc. Amer., vi., 1895, 343–352) to the effect that the glacial sheets of northeastern America and northwestern Europe were caused by and hence were coincident in time with the elevation that permitted the erosion of the deep marginal valleys of the continents. Upham cites the case of the Sogne fiord, on the west coast of Norway with a maximum sounding of 4,080 feet, as a measure of the epirogenic uplift which at its culmination caused the glaciation of northern Europe. The difficulty here is that while a comparatively long period of elevation must be postulated for the excavation of the valley of Sogne fiord, and while climatic change would respond immediately to elevation, yet glacial conditions are not known to have occured until the erosive effects of elevation were practically completed. The steepness of the fiord walls indicates that the elevation was not slowly progressive, but was rather promptly completed and steadily maintained; being in this unlike the elevation by which the erosion of the flaring and benched valleys of the northern Alps has been allowed. The problem involved in the relation of elevation and glaciation would therefore seem to be not the simple one of immediate cause and effect, but on the other hand the difficult one of why the apparently competent cause should not have at once had its expected effect; why glaciation should have waited so long after elevation, not attaining its maximum until a time of depression.

FORESTS AND TORRENTS.

The much-debated problem of the influence of forests on rainfall remains unproved, after all that has been said and done; but the influence of forests on torrents admits of no question. The soil is washed from the deforested slopes and the torrents spread it over the valleys, greatly to the injury of both high and low land. The Shenandoah Valley, for example, one of the most beautiful and productive farming districts in our country, is suffering along its margin from the encroachments of gravels and sands washed from the enclosing deforested ridges. Those who wish to present this matter to forestry meetings in popular and impressive form will find an abundance of illustrative material with references to European literature on the subject in an essay by Toula: *Ueber Wildbach-Verheerungen and die Mittel ihnen vorzubeugen* (Schr. Vereins zur Verbreitung naturw. Kenntnisse in Wien, xxxii., 1892, 499–622, with forty-one views from photographs). W. M. Davis.

Harvard University.

NOTES ON AGRICULTURE (III.)

THE EXPERIMENT STATION RECORD.

The Experiment Station Record, a monthly (practically) published from the office of Experiment Stations of the U. S. Department of Agriculture gives under the heads of Chemistry, Botany, Zoölogy, Meteorology, Soils, Fertilizers, Field crops, Horticulture, Forestry, Seeds, Weeds, Diseases of Plants, Entomology, Foods, Veterinary Science, Dairying, Technology, Statistics and Miscellaneous, the progress made in these various branches in the Experiment Stations of our country. The recent work in Agricultural Science in foreign countries is also briefly summarized.

From the last issue of the Record, just received, the reader is first of all informed as to the amounts of the appropriations made by Congress for the U. S. Department of Agriculture for the year ending June 30, 1896. The total amount is $2,578,750, which includes $720,000 for the Experiment Stations established under the act of Congress of March 2, 1887. There will be two new divisions in the U. S. Division of Agriculture, namely, that of Agrostology, which contemplates 'field and laboratory investigation relating to the natural history, geographical distribution and use of the various grasses and forage plants,' and that of Soils.

Among reports of agricultural science in foreign lands is a paper upon 'Agricultural Investigations in Switzerland,' by Dr. Grete, director of the Swiss Station at Zurich. In 1878 a Station for control of fertilizers and feeding stuffs was established, and recently its work has been extended to include culture tests of soils. There is a Seed Control Station which at the present time has eight workers besides the director, and tests by germination thousands of samples of seeds.

Under the head of chemistry the Record gives the new methods of obtaining solutions in soil analyses and the determination of phosphoric acid. The department of Botany contains a review of Professor Scribner's 'Grasses' of Tennessee, which is a valuable contribution to the Agrostology of the whole country. 'Notes on Maize,' by Dr. Sturtevant, contains generalizations upon the effect of climate upon corn, the view being maintained that northern grown varieties are not necessarily earlier than southern sorts. The popping of corn is due to the starch lying within a tough layer which bursts upon the application of heat.

Under meteorology winds injurious to crops are considered at length in a digest of Mr. Curtis' bulletin. Three classes of destructive winds are considered, namely, violent, cold and drying winds. Of the cold winds there are two classes, the mountain and valley, and those associated with cyclones, the so-called blizzards and 'northers,' chiefly destructive to orchard crops. The extent of the latter has increased with the progress of deforestation, and the Michigan peach belt, with its failures in late years, is given as an example. Under 'Variations in the Character of the Seasons,' Mr. Gawthrop shows cause and makes an appeal for the exploration of the upper atmosphere. Mr. Clayton, under 'Rhythm in the Weather,' claims that 'there is good reason to believe that through all this seeming irregularity there runs a web of harmony and rhythm,' and expects that meteorology will in time become an exact science. It is certainly gratifying to note how much attention is being given to the weather and the progress that is made from year to year in its study.

While the air is being investigated the soils are not neglected. In addition to analyses in relation to fertilizers the action of organic acids is reported upon by H. Snyder, of the Minnesota Station. Soil temperatures are taken at many Stations and facts are rapidly accumulated upon soil meteorology as well as the movements of liquids and gases in the soil.

Naturally, a large part of the chemical work of the Experiment Stations is with fertilizers and the record before us gives a full share of its space to this branch of the Station service. The New Jersey Station issues a large bulletin giving the results of analyses, while the Maine Station reports upon the foraging powers of some agricultural plants for phosphoric acid, as tested by box experiments. The Louisiana Station issues a large bulletin upon the 'Results of five years' experiments with fertilizers.' This is not the place to give conclusions, the point here being for the readers of SCIENCE to realize that experiment work in this country is widespread in the broad sense, and that we are entering an age that has for its watchword, 'Prove all things,' while we may hope that we hold fast to that which is good.

Mr. Crazier, of Michigan, takes up a single somewhat obscure crop, the millet, and with sixty-four pages of text and six figures gives results obtained from seventy-three samples grown under varying conditions. In like manner Mr. Hilgard, of California, brings out the facts concerning the new tannin-producing plant carnaigre. From the same Station is a bulletin upon the Australian salt bush, which grown upon 'some of the most alkali spots yielded at the rate of five tons of dry matter per acre,' and is eaten with relish by live stock. Experiments upon wheat, tobacco, potatoes and several other standard crops receive notice in the Record.

Under Horticulture Mr. Heideman, of the Minnesota Station, gives a 'classification of the sexual affinities of Prunus Americana vars. Numerous crosses were made between the various forms of flowers, most of which were not hermaphroditic, and out of forty-nine possible combinations of pollination only 13 were legitimate. Mr. Lodeman, of Cornell, has issued a bulletin upon 'grafting of grapes,' illustrating directions for the various methods and remarking upon the physiology of the process. In his annual report, Mr. Munson, of Maine, gives notes upon various crops. Thus in a cross between ignotum and peach tomatoes there was 'a marked falling off in the second generation over the advantage indicated by the first.'

Mr. Buckhout after 'five years' experience in planting forest trees' concludes in the Pennsylvania Station Report "that considering the time, expense and work involved, artificial forest planting cannot be recommended, at least in the way pursued in the experiment and that natural methods of reforesting supplemented by some seed sowing, thinning and planting will suffice for the present." Mr. McCarthy, of the North Carolina Station, has prepared a bulletin upon seed testing and fully describes its uses and methods. Weeds receive attention from Mr. Wooton, of the New Mexico Station, who figures several of the worst in his Territory.

Under diseases of plants some grape troubles in New York are reported upon by Mr. Lodeman of Cornell. Thus the so-called 'shelling' is ascribed to one or more of four causes, namely, parasites, conditions of vine, of soil, or of atmosphere. An English experimenter shows that finely ground lime 700 pounds per acre will check the club root in turnips. Resin is found by Mr. Webber to be effective in preventing the sooty mould of the orange.

Economic entomology receives consideration under many heads as the damage caused by American locusts, chinch bugs, codling moth, etc. A new saw-fly and pear insect are mentioned and many species are named under beneficial insects. Gas treatment for destroying scale insects is reported upon from California and 'Entomology and Quarantine' is considered.

Much space is given to the consideration of foodstuffs, their analyses, digestibility, etc., the Maine Station perhaps taking the lead in these matters in the copy of the Record in hand, while Utah and Minnesota come in for a share in 'dairy herd records' and 'relative value of corn and oats for horses.' Several papers are mentioned by title or at length under dairying.

Surely enough has been here given to show that the Experiment Stations of the United States are pushing on along many lines, and that through the facts accumulated principles cannot but be laid bare.

THE HORTICULTURALISTS' RULE-BOOK.

THE first edition of this 'compendium of useful information for fruit-growers, truck-growers, florists and others' by Professor L. H. Bailey, of Cornell University, was published in 1889 and a second in 1892. The great advances made in methods of combating insect and fungous enemies during the past few years led the author to revise and extend his work. A chapter upon greenhouse heating has been added and another upon the current literature of horticulture.

The following are some of the leading subjects considered: insecticides and injurious insects, plant diseases with preventives and remedies; injuries from mice, rabbits and other animals; weeds, seed-tables, etc. There is a chapter upon *Rules* in which are given rules for naming fruit, codes of various societies, etc. Within the flexible covers of this little book the publishers (Macmillan & Co.) have neatly packed together a surprising amount of valuable information. Here the horticulturist may learn how much seed to sow per acre, how many plants to set in his orchard, how to keep off the enemies to his crop, and when to harvest and market it. Not the least is a list of the leading books that have been published upon horticultural subjects and within easy reach of crop growers.

BYRON D. HALSTED.

CORRESPONDENCE.

THE ILLUSTRATIONS IN THE STANDARD NATURAL HISTORY.

TO THE EDITOR OF SCIENCE—*Sir:* Referring to the statement in SCIENCE of April 5, 1895, page 387, top of second column, that certain illustrations of Brehm's *Thierleben* 'were pirated by the *Standard Natural History,*' I beg to say that it is incorrect and libelous. The matter concerns me, as one of the authors of the *Standard Natural History*, and also as the author of the *Key to North American Birds*, in several later editions of which many of the same illustrations were used by my publishers, Messrs. Estes & Lauriat, of Boston. As 'piracy,' like plagiarism, implies dishonesty, the allegation thus made by Dr. C. Hart Merriam, who signs the article, is too serious to be overlooked.

Nevertheless, being ready to believe that Dr. Merriam erred through inadvertence, I am prepared to accept an apology, in so far as I am personally concerned; but I am not authorized to state that this will be considered satisfactory by the other parties who have been thus libeled.

Very truly yours,
ELLIOTT COUES.

WASHINGTON, D. C., June 5, 1895.

[The word piracy may be used in two senses—moral and commercial. When I wrote the article in which it was stated incidentally that the Brehm plates in the *Standard Natural History* were pirated, I believed that they were in both senses. Among the reasons for this belief may be mentioned the following:

1. The book itself contains no statement of the fact that the illustrations are taken from Brehm.

2. The anatomist Fürbringer states that he searched in vain for a copy of the *Standard Natural History* in Germany (Journal für Ornithologie, Apr., 1892, 138).

3. It is stated in the *Naturæ Novitates*, Berlin (Vol XV., No. 1, Jan., 1893, p. 18, nr. 326), that the work 'may not be imported into Europe on account of the reproduction of the Brehm woodcuts.' ['Darf in Europa wegen Nachdruck der Brehmschen Holzschnitte nicht eingeführt werden.']

4. The name of the artist, Mützel, was erased from many of the copied plates. When the attention of the editor was called to this injustice, he replied: "The cutting out of Mützel's name was a business necessity."!

If, in spite of the above facts, the cuts in question were sold to the publishers of the *Standard Natural History* by the publishers of Brehm's *Thierleben*, I withdraw so much of my original charge as may be inferred to imply commercial piracy; but I by no means retract the charge of moral piracy—the greater offense of the two, because it has no legal redress.

Is the deliberate reproduction of another's pictures without credit less censurable than the reproduction of another's words or ideas? And what shall one say when the sin of plagiarism is darkened by the erasure of the artist's name, so that neither artist nor author may be known?

Just why Dr. Coues mentioned his *Key to North American Birds*, and his publishers, Estes & Lauriat, who by the way were not the publishers of the *Standard Natural History,* is hard to understand, inasmuch as neither were mentioned in the review to which he takes exception.

Since the above note was sent to SCIENCE I have received a letter from the publishers of Brehm's *Thierleben* in Leipsic. They state that they sold to Estes & Lauriat certain electrotypes from Brehm, to be used by Estes & Lauriat only, 'under an agreement according to which it was forbidden to Messrs. Estes & Lauriat to resell these electrotypes.' They state further: "As we had been informed that notwithstanding this settlement our electrotypes had been resold, we called Messrs. Estes & Lauriat to account, and they were forced to confess that they had resold the electrotypes" to three different firms!

In reply to my question: "Were the electrotypes sold by you to S. E. Cassino & Co., and published in the *Standard Natural*

History with your knowledge and consent," they state: "We answer No! These electrotypes had *not* been sold by us to Messrs. S. E. Cassino & Co., and were used without our permission in the said works. Besides, we are still at issue with Messrs. Estes & Lauriat, Boston, on account of this affair."

C. Hart Merriam.]

SCIENTIFIC LITERATURE.

Report on Water Supply; Geological Survey of New Jersey. By Cornelius Clarkson Vermeule, Consulting Engineer. Vol. III. of the Final Report of the State Geologist. 1894.

The Geological Survey of New Jersey has just issued a report bearing the above title, the interest and value of which are not limited by State lines. Its author, under whose direction the topographic map of the State was made, has had the best of opportunities for studying the questions involved, and has not failed to avail himself of them. The results of his study have been put in as simple and available form as possible, considering the complex nature of the problems.

The range of interests touched by the report is great. It will be of inestimable value to cities and communities which draw or may draw their supply of water from the streams of the State, and to manufacturers who use or may use the power afforded by them. Less directly, but not less certainly, the report will be of great value in the same lines outside the State, since many of the principles developed are of general and some of them of universal application. The report also contains discussions and suggestions which have a bearing on agriculture and forestry, the latter of which is just now attracting wide attention in this and other States. The educational value of the report is great, not only to those whose financial and sanitary interest are touched by it, but also to students of hydrography and geology, and to intelligent citizens in general. From this standpoint, its value lies not only in what it proves and affirms, but also in what it disproves and denies. It is scarcely too much to say that there is not a community or a class in the State which may not be benefited by the intelligent study of the volume before us.

The study of the water resources of the State was begun by Professor Cook long ago. As early as 1868 the subject was discussed by him, and the annual reports of the State Geologist have since made frequent reference to the subject, and have reported the progress of the work, the results of which are now embodied in this volume. Interest in the questions of which it treats has been stimulated by the rapid growth in population, especially in the vicinity of New York and Philadelphia. In 1882, 587,760 people in New Jersey were dependent for water upon systems of public supply. In 1894 this number had nearly doubled, while the amount of daily consumption had increased from about 49,000,000 gallons to about 108,000,000. Of this amount, 100,000,000 gallons were drawn from streams. If the population of the State continues to increase at the present rate for another half century, and if the demand for water keeps pace with the increase in population, as is sure to be the case, it is evident that another half century will make heavy demands upon the available supply of water which the State affords. On the basis of the recent rate of increase in population, it is estimated that by 1950 that part of New Jersey adjacent to New York City will need 547,000,000 gallons of water daily; and the author remarks that "since fifty years cannot be considered a long time in the future for which to make provision, it is evident that the time has come for us to know what our resources are and to provide for their preservation and wise development" (p. 6).

The investigation of the water resources of the State has involved a careful study of the relation between precipitation and stream flow. This study has led to some very important conclusions, the data for which are drawn not merely from within the State of New Jersey, but from all available sources. The analysis of the facts has led Mr. Vermeule to the conclusion that a formula may be adopted which shall express with approximate accuracy the relation between rainfall and evaporation, within the basins of the streams studied. This formula is $E=15.50+.16\ R$, in which $E=$ total annual evaporation, $R=$ annual precipitation, and 15.50 stand for inches of water. R minus E will equal the annual flow of the river in question. A modification of the formula for mean annual temperature is suggested, and in this modified form it becomes universal. In this connection it is stated that a careful study of the annual precipitation and flow of variously widely separated streams "has practically demonstrated that the difference in amount discharged (by streams) for given rainfalls is due almost entirely to increase or decrease of evaporation owing to increased or decreased annual temperature" (p. 75); and that temperature is 'a much more potent factor than forests, topography, or the other causes usually assigned' (p. 77) to account for the variations in the discharges of streams. So thoroughly is evaporation believed to be dependent on temperature that "the (river) gaugings (representing the rainfall which does not evaporate) actually indicate the mean temperature of the water sheds more closely than we can obtain it from available temperature observations" (p. 334). It will be readily seen that the formulæ noted above, and the principles which go along with them, greatly simplify the whole question of the relation of rainfall and stream flow, and are of the greatest importance to all interests dependent on streams, or effected by them. Formulæ are deduced for calculating the proportion of rainfall which disappears by evaporation for each month, and for determining the flow of a stream for any given month, the rainfall and temperature of its basin being known.

Of immediate practical value to the citizens of the State are the detailed data concerning the streams of New Jersey. These data include the total, the average and the minimum flow of each stream of the state, the available and the utilized power, etc., etc. The data are combined in various ways with a view to making them useful in various directions.

Popular ideas to the contrary notwithstanding, statistics show that there has been a slow but steady increase in the use of water power within the State. While many small powers have been abandoned, this loss has been made more than good by the establishment of larger ones. The total amount now in use is about 31,000 horse power. Pertinent suggestions are offered as to the further utilization of the power afforded by the streams.

Forests are thought not to influence the annual evaporation or stream flow to any marked extent, nor to influence particularly extreme floods. With deforesting, however, comes increased irregularity of stream flow, including more frequent moderate floods, lower flow of streams during periods of drought, and more protracted periods of low flow (page 344). Care is taken to emphasize the beneficial effects of forests in preserving soil on slopes, in creating absorbent matter (humus, etc.), which holds the water and helps to equalize its flow.

Cultivation is thought not to greatly affect the total stream flow, though it affects its regularity. It increases the absorbent capacity of the soil, and so the total flow from underground water, while under drainage tends to produce irregularity of flow. "As between cultivated and barren watersheds, * * the cultivated will show the steadiest conditions and the best-sustained dry-season flows, but as between cultivated and forested water sheds the forested will produce the best results. * * It follows also that floods will be most severe upon barren areas." Hence there exists * * 'the urgent necessity of preserving forests upon slopes, and all areas which are not adapted to agriculture' (p. 348).

Enough has been said to indicate the scope of the volume; which can hardly fail to become a hand-book on the question of water supply. It is probably not too much to say that this report alone is worth more to the State of New Jersey than its geological survey has ever cost. Other States of dense population would do well to follow the example of New Jersey, not only in studying their water resources, but in putting the work under the direction of their geological surveys; for the relation between the geology of a region and the availability of its water supply is so intimate that no other organization is better qualified to direct the work. The U. S. Geological Survey has work of this sort in progress in some parts of the semi-arid regions of the West, from which good results are sure to come.

Rollin D. Salisbury.

University of Chicago.

John Dalton and the Rise of Modern Chemistry. By Sir Henry E. Roscoe. New York and London, Macmillan & Co. 8vo. Pp. 216. Price, $1.25.

It is one of the greatest achievements of modern chemistry to have shown that for each chemical element there is a measurable quantity which, throughout all the transformations that the element undergoes, remains unchanged, and is, therefore, to be regarded as a constant. The laws of definite, multiple and reciprocal proportions of gas volumes and of specific heats, of mass action and of the periodicity of properties, all give converging evidence that for each element there is a definite constant quantity which, in all the changes that the element undergoes, acts like a unit. This constant is the one unchanging, and, therefore, the most characteristic property of the element. The chemical and physical properties of an element, its behavior under different conditions, its possibility of undergoing change under given circumstances, in short its whole character, is dependent upon the magnitude of this constant. A large part of theoretical chemistry is taken up with a consideration of the general methods that are available for the determination of this important quantity, and it is customary to express it by means of a number which indicates its magnitude in terms of the characteristic quantity of some one element, usually hydrogen, taken as a unit. To this number the name Atomic Weight has been given, and to John Dalton, indisputably, belongs the great credit of having first introduced into chemistry the idea of atomic weights. He transformed the Newtonian corpuscular theory of the constitution of bodies into a workable chemical hypothesis, and the subsequent development of his idea, that the atoms of different elements have different constant masses, has given us our present system of atomic weights. But, whether we associate with this term the conception of an atomic constitution of matter or not, the fact remains that these constants stand to-day independent of any hypothesis, and are to be regarded as mathematical quantities that can be deduced from the general laws and principles of the science.

In this book Sir Henry Roscoe has given us a most interesting account of the life and work of the great Manchester chemist. Dalton's life, like that of many scientific workers, was not an eventful one, but he was a man of marked personality, of positive traits of character, and our author has interwoven a description of the personal characteristics of the man with an account of his scientific work and the incidents of his life in such a way as to make a most attractive and entertaining biography.

From his early years Dalton was accustomed to looking at things from the standpoint of the atomic theory, and throughout his life he remained a firm supporter of this doctrine. Like Newton, he conceived of atoms as 'hard impenetrable, movable particles,' 'incomparably harder than any porous bodies compounded of them, even so very hard as never to wear or break in pieces.' These atoms were supposed to be surrounded with an atmosphere of heat. He has left some drawings which show how he pictured to his mind the structure of the smallest particles of compounds, and in these he foreshadowed the modern constitutional and stereo-chemical formulas. In gases and elastic fluids he considered matter to be in an extreme state of division, and nearly all of his important discoveries resulted from experiments upon gases. It was by considering the constitution of gases that he came to the idea of atomic weights.

Dalton was not as skillful an experimenter as some of his contemporaries; most of his apparatus was made by himself and was often of a very primitive kind. It is remarkable that he should have been able to get the results with it that he did; results that were in most cases confirmed by other workers who used more accurate instruments and more exact methods. Some of the important facts that he discovered were the equal expansibility of different gases under the influence of heat; the practical constancy of the composition of the air, a fact which he established by means of a large number of analyses of air collected at different places and at different altitudes; the law of partial pressures, or that the total pressure of a gas mixture is equal to the sum of the partial pressures of the components, and that in a mixture of gases each component acts like a vacuum to the other components and behaves as though it alone were present. He also investigated the solubility of gases in liquids; but his greatest discovery was the law of multiple proportions. Upon this discovery and upon the fact that he introduced the atomic theory with the idea of atoms of different weights his great fame as a scientific man rests.

Of especial interest in this book is the account here published for the first time of how Dalton arrived at his important conclusion. Among the Dalton papers belonging to the Manchester Literary and Philosophicial Society, Sir Henry Roscoe has found some manuscript notes prepared by Dalton for a course of lectures that he delivered at the Royal Institution in the winter of 1809–10. These notes are printed in full and give an account by Dalton himself how his ideas regarding the atomic theory came to him.

Mentally he was vigorous, independent and self-reliant; he gave little attention to the results obtained by others. Like Newton he reached his conclusions by quiet, steady, continuous thinking. His long life was spent in experimenting and reflecting. It is pleasant to know that in his later years many honors and tokens of esteem came to him from his countrymen and from abroad.

After Dalton the atomic theory was developed and put upon a much broader foundation by Berzelius, and through his work and that of a long line of illustrious successors it has become the central dominant feature of theoretical chemistry.

It is noteworthy that Joule, who did so much to establish the law of the conservation of energy, was a pupil of Dalton, and that the names of both master and pupil are so intimately associated with our two great intellectual instruments of investigating nature, the atomic hypothesis and the theory of energy. The deductions of the former have the advantage of being readily apprehended, those of the latter of being mathematically exact.

Sir Henry Roscoe deserves the thanks of all workers in chemistry for having provided them with an unusually interesting biography of one of the founders of the science.

EDWARD H. KEISER.

BRYN MAWR COLLEGE.

Elasticität und Festigkeit. By C. BACH, Professor in the Technical High School at Stuttgart. Second Edition. Berlin, Julius Springer. 1894. Octavo, 432 pages and xiv plates.

In this work the author lays down the guiding principle that the student of mechanics of materials should first of all become acquainted with the actual phenomena of stress. To this end photographic illustrations are given exhibiting the deformations of bars under tension, of blocks under compression, of beams and plates under flexure and of shafts under torsion. These illustrations are most useful and show the typical changes of form in a beautiful manner. Nevertheless their value is probably not so great as the author assumes, for nearly all the theories and computations of the mechanics of materials are confined to the case where the elastic strength is not exceeded and where changes of form are not perceptible to the eye.

The modulus or coefficient of elasticity, usually represented by the letter E, is not employed in this book. Instead its reciprocal is used and called the extension coefficent, which may be defined as the stretch of a bar per unit of length due to a stress of unity on each square unit of cross section. There can be no doubt but that the term coefficient of elasticity is a most unfortunate one, as it has no relation to elasticity in the ordinary sense of the word, but is a measure of stiffness or rigidity. The improvement desired would be a change of name rather than the introduction of a new term and symbol. Even the author, who uses the new constant consistently in all his formulas, rarely gives numerical values for it, but expresses these in terms of its reciprocal, which is, of course, the coefficient of elasticity as universally employed.

The scope of the work is that of a text-book on the mechanics of materials and of beams, columns and shafts, suitable for technical schools which desire to avoid extended mathematical discussions. The usual theoretic formulas are demonstrated in a neat manner, and many results of tests are presented; those on circular, elliptical and rectangular plates may in particular be noted as novel and valuable. The subject of internal work or resilience is discussed more fully than in British or American books. True internal stresses resulting from the change of shape are properly used in the treatment of cylinders, spheres and plates; owing to the neglect of this precaution, formulas based upon apparent stresses, like those of Rankine, are liable to give values often deviating twenty-five per cent. from the truth.

The formula for the design of columns, long used in the United States under the name of Gordon's formula or Rankine's formula, has not been employed in Germany to the extent that its value demands. The author, however, emphasizes it as an important rule, and gives empirical constants for its use. He also states that the formula was first deduced by Navier; on referring to Navier's works this statement is not found to be justified, it being only mentioned that the stress on the concave side of the column is the sum of the stresses due to direct compression and to lateral flexure, while no formula similar to Gordon's is established.

On the whole, the perusal of the book leaves the impression that the author has done his work with much painstaking care, and that both the theoretical and the practical part are set forth in a manner which cannot fail to give students an excellent foundation in the science of the elasticity and strength of materials.

MANSFIELD MERRIMAN.

LEHIGH UNIVERSITY.

The Pocket Gophers of the United States. Bulletin No. 5, U. S. Department of Agriculture, Division of Ornithology and Mammalogy. Prepared under the direction of DR. C. HART MERRIAM, Chief of division, by VERNON BAILEY, Chief Field Agent, Central Park, New York. Published by authority of the Secretary of Agriculture. Washington, Government Printing Office. 1895. 8vo., pp. 47. Frontispiece, 6 cuts in the text, and colored map.

In a former number of SCIENCE (N. S. Vol. I., No. 9, March 1, 1895) attention was called to a monograph by Dr. Merriam on the Pocket Gophers (family Geomyidæ), in which was presented the scientific results of his extended and detailed studies of the group. The present 'Bulletin' is a fitting sequel to the technical monograph already noticed, dealing, as it does, with the economic relations to agriculture of these destructive rodents. This paper was prepared by Mr. Vernon Bailey, under the direction of Dr. Merriam, Chief of the Division of Ornithology and Mammalogy of the U. S. Department of Agriculture. Mr. Bailey is one of the most experienced and expert of the many expert field naturalists now connected with this branch of government service, and is therefore eminently fitted by personal experience in the field for the preparation of a report like the one under notice.

The first ten pages relate to the general habits of these animals, which live almost wholly under ground, and make known their presence chiefly by the mounds of earth thrown out from their burrows, or by their troublesome depredations upon farm and garden products. Even where so numerous as to be exceedingly troublesome they are rarely seen, and little is known of their life habits by even the people who suffer from their depredations. Hence the detailed account of their habits and methods of working here given is a welcome contribution toward a fuller knowledge of their life histories. Although deficient in vision, their senses of taste, touch and smell seem to be compensatingly acute, and their ample external cheek-pouches serve an important function in the transportation of food, for which they seem exclusively used. The Gophers, says Mr. Bailey, "are industrious workers, and whatever food is found and not needed at once is carried to chambers in some part of the tunnel and stored. * * * Sometimes a peck of small potatoes, roots of coco grass, wild parsnip, wild sunflower and other fleshy or bulbous roots are found in a single chamber." They are especially fond of potatoes, turnips, carrots, beets, onions, parsnips, corn, barley, rye and alfalfa, and even squashes and melons do not escape their ravages. They are also very destructive to fruit and ornamental trees by eating off their roots, which are sometimes so thoroughly cut away that the trees fall from lack of support. Their burrows are also often a source of injury over comparatively large areas, through the large amount of earth thrown up as mounds, thus burying crops, and sometimes they cause breaks in irrigating ditches and induce serious washing of hillside lands.

The Gophers have few natural enemies, and seem to flourish and increase through the fruits of man's industry. Hence the question of artificial means of destruction becomes a matter for careful consideration. They can be trapped readily by those who know how to do it, but generally the art is unknown, and it is a widespread belief among farmers subject to their inroads that they cannot be caught in traps. Mr. Bailey especially commends the use of bisulphide of carbon for their destruction, which is readily accomplished by placing an ounce or two of this volatile fluid on cotton or rags in their burrows. Instructions are also given for the use of poison and traps. In consequence of the harm done by Gophers, bounties have been offered in many parts of the West, but the system is condemned as a means of depleting the county treasuries without effecting the extirpation of the Gophers. Thus it is stated that Benton county, Iowa, paid out $18,000 in three years in Gopher bounties, "but the Gophers, though greatly reduced in numbers, were not exterminated."

Gophers of one species or another occupy practically the whole of the United States west of the Mississippi River, and also the greater parts of the States of Illinois, Georgia, Alabama and Florida. Detailed accounts are given of the habits of the various species found east of the Rocky Mountains. Aside from its important economic bearings, the Gopher Bulletin is a most interesting contribution to the life history of a group of animals hitherto little known. Four of the six illustrations in the text are from Dr. Merriam's monograph, as are the frontispiece (Georgia Gopher), and the colored map of the distribution of the species of the genera *Geomys* and *Craterogeomys*. The two colored plates (of the Prairie Gopher and Gray Gopher), called for in the list of illustrations, and prepared especially for this Bulletin, are lacking, in consequence, as we are privately informed, of their having been 'mislaid' at the Government Printing Office after their production and delivery by the Department of Agriculture.

J. A. Allen.

[*The Norway Lemming*] *Myodes lemmus, its Habits and Migrations in Norway*, by R. Collett. Christiania. 1895. 8°. pp. 62.

The distinguished naturalist of Christiania, Dr. R. Collett, has just published a treatise on the Norwegian Lemming that at once becomes a classic on the subject. He tells us that, in a manuscript believed to have been written in the latter half of the 13th century, the Lemmings are supposed to have been the same as the 'locusts' mentioned in the Bible in connection with the plagues in Egypt. In a book published by Jacob Ziegler in 1532 the theory of their descent from the clouds is proposed, based on statements of two bishops from Trondhjem. In 1555 Olaus Magnus, Archbishop of Upsala, published a figure showing the Lemmings (with tails like house mice) falling from the clouds and being preyed upon by Ermines.

Dr. Collet states that normally the Lemming inhabits all of the mountain plateaus of Norway above the zone of coniferous trees, descending in Finmark to sea level, thus occupying about one-third of the total land area. Besides the mainland they inhabit the large rocky islands off the coast, especially to the northward.

In normal years they are rarely seen, even by explorers. In prolific years they suddenly increase and overflow vast areas. In such years according to Dr. Collet, "The litters produced during the course of the summer follow so closely one upon the other that the one set is barely allowed time to leave the nest ere the next lot arrives. Futhermore, the litters are unusually large, as they constantly contain up to 10 younglings in each set (although possibly 6 or 7 on the whole is the rule); and all these young ones appear to be possessed of greater powers of attaining maturity than those produced during a normal year."

This excessive reproduction results in overcrowding the breeding grounds, from which vast numbers move away in different directions. Descending the mountains and following the valleys they continue blindly on, proceeding hopelessly to certain death. The direction of the march is dependent on the valleys, and the exodus may "radiate in quite opposite directions from one and the same mountain plateau. * * * * Thus during migratory years the southern ramifications of the Lang Fjeld will emit swarms which may advance eastward as far as the Christiania Fjord; southward, down to the coastal regions of Christiania Stift; and westward, to the fjords in the counties of Stavanger and Söndre Bergenhus. * * * During the entire course of the summer and autumn, they continue to pour forth from the mountains. * * * * In the valleys they invariably meet with lakes or rivers, and large numbers constantly endeavor to cross them. If the mountains are high on both sides, the valley will, as a rule, receive contributions from each slope, and individuals may be observed crossing the river in both directions."

"During the migrations they do not allow themselves to be stopped by rivers, or even by the arms of a fjord, but trust themselves, without hesitation, to the mercy of the waves, in order to reach the opposite shore. It would almost seem as if no stretch of water were too wide for them to cross if they but see land on the other side. During the great migration in the district of Trondhjem in 1868, which has previously been mentioned, a steamer on the Trondhjem Fjord steamed into a crowd of swimming Lemmings of such vast extent that she took over a quarter of an hour to pass through it, and as far as one could see from the vessel down the fjord its waters were covered everywhere with these animals. During the great migratory years similar accounts are received from all the great lakes (Mjösen, Randsfjord, Kröderen, etc., etc.)."

Great havoc is wrought in meadows and grain fields by the hungry hordes, particularly in mountain pastures and farms situated on the higher slopes.

It is stated that no rule can be laid down concerning the frequency of the migratory years. The greatest migrations, which extend down to the most distant lowlands, take place but seldom and rarely occur in the southern districts oftener than once in ten years. The number Dr. Collett has collected data for is surprising. He gives the dates and areas invaded for seven great migrations from 1739 to 1790, and for no less than 24 in the present century.

As to the extent of the areas invaded, Dr. Collett says: "On the whole it may be assumed that scarcely any accessible point of Norway (except the outermost islets) has not been invaded by their hordes during one or other prolific year."

"It has hardly ever happened that a prolific year (and the consequent migration) has simultaneously embraced the entire land. The rule is that the increase takes place in great or small districts independent of each other, but the area which may be involved thereby may be of very considerable extent. Occasionally the increase will take place simultaneously in two separate districts, divided from each other by an area of greater or lesser extent, in which the production is normal. In Norway there may be recognized, on the whole, at least five great groups of mountains within which most of the migrations have their radiating centre. One migration may embrace either the entire group or small portions of it."

The regular enemies of the Lemming are numerous and many of them increase with the Lemmings; as the birds of prey, the large gulls and skuas, and weasels and foxes. In prolific years certain birds which follow the Lemmings change their breeding grounds and nest in localities where they are never seen at other times. To these may be added certain irregular enemies; for Dr. Collett tells us that reindeer (both wild and domesticated), cows, goats and pigs kill and eat them in great numbers.

But the destruction of the Lemmings after reaching the lowlands is only in small part due to these enemies. "The most active factor in their extermination," says Dr. Collett, "appears to be infectious diseases, which invariably occur whenever a species of animal has multiplied in excess of its natural numbers."

Not only do the Lemmings themselves die of disease; but they are believed to cause serious disease among the human population. This belief has been current in Norway from time immemorial and was published by Ziegler more than 350 years ago. Dr. Collett states that during Lemming years all running water is contaminated by the decaying excrement. "To this may be added the dead animals, which will be found lying scattered about in great numbers, and which, during hot summers, become quickly decomposed. The rain carries the putrid matter on to the nearest watercourse, whence it makes its way to wells, and becomes mixed with the drinking water of the inhabitants.

"During some great prolific years, definite forms of sickness have appeared in certain of the overrun districts, and the people have given these the name of 'Lemming Fever,' as they presumed that they were connected with the appearance of these animals."

After citing medical testimony and describing the disease, Dr. Collett concludes: "Lemming fever is thus a disease which, in its phenomena, is related to scarlet fever. Its origin is regarded, both by medical men and the populace, as having a certain connection with the appearance of the swarms of Lemmings and the pollution of water by their putrifying carcasses and dung during dry summers."

Dr. Collett's treatise on the *Habits and Migrations of the Lemming in Norway* is replete with interest from beginning to end and must long remain the standard authority on the subject. C. H. M.

NOTES AND NEWS.

ASTRONOMY.

The London *Times* gives the following accounts of recent lectures before the Royal Institution and of the last meeting of the British Astronomical Association:

Dr. W. Huggins, F. R. S., gave the second of his course of lectures on the instruments and methods of spectroscopic astronomy, at the Royal Institution, on May 30th. He dealt with the more complex instrument which is placed at the eye-end of the telescope so that the images of the stars fall upon its slit. The important question of its efficiency was connected, the lecturer said, with its power to break up the spectrum into as many parts as possible. This power of separation was fixed by certain conditions—the linear length of the spectrum, its dispersion, and the resolving power of the prism. The latter, which was independent of dispensive power, was governed by the size of the prism, hence larger prisms have greater resolving power. But the use of larger prisms in astronomical work entailed certain disadvantages, such as increased weight and cost, and difficulty of obtaining glass of uniform quality. It was therefore fortunately possible to get the results of large prisms by passing the beam through several smaller ones, though the loss of light by absorption and reflection from the faces of the prisms was very serious. An alternative way of obtaining a spectrum was to use a diffraction grating, which we owed to the experiments Fraunhofer made to discover whether the lines of the spectrum were due to interference of light. His original gratings were made by winding wire in a screw-thread round a piece of glass; ultimately he adopted the plan of ruling the lines on glass with a diamond point. Great advances were made by Rutherfurd, whose machine cut lines to the number of 17,000 to an inch, and by Rowland. There is, however, but little to choose between a prism and a grating with 14,000 lines to the inch.

The Friday evening discourse at the Royal Institution on May 31st was given by the Earl of Rosse, who took as his subject the 'Radiant Heat from the Moon during the progress of an Eclipse.' Sir Frederick Abel was in the chair, and among those present were Lord Kelvin, Sir James Crichton-Browne, Sir Frederick Bramwell, Professor Dewar, Mr. C. V. Boys, Dr. Frankland, Mr. Ludwig Mond and Mr. Crookes. Lord Rosse began by showing the results of his observations on the variations in the amount of heat radiation from the moon during the lunar month. Speaking of the heat given off during an eclipse, he said that in the total eclipse of January, 1888, he had found there was a great decrease in its amount some time before the first contact. During the total phase the heat radiated was a mere trifle, and it had not regained more than 80 per cent. at full moon —an hour and a half after the last contact. Lord Rosse then described the apparatus he had used, and also the apparatus and some of the results of other investigators.

The usual monthly meeting of the British Astronomical Association was held at University College on May 28th, Mr. E. W. Maunder, the president, being in the chair. A paper was read from Professor H. H. Turner, Savilian Professor of Astronomy at Oxford, on 'Simple Apparatus for Measuring Stellar Photographs.' Mr. Holmes read a paper on on 'The Reproductions of Astronomical Drawings,' etc., in which the value of photographic processes was commented on as being more accurate. He also read a paper on the apparent roundness of small spot markings on planets. A paper from Mr. Monck on the 'Spectra and Colours of Stars' was read. The report of the Lunar Section, by Mr. T. Gwyn Elger, F. R. A. S., the director, was read, and attention was called to the progress made recently in lunar photography.

GENERAL.

Professor C. Lloyd Morgan, author of *Animal Life and Intelligence* and other works upon comparative psychology, is coming to this country next winter to deliver one of the Lowell Institute courses in Boston. He will also deliver four lectures upon Instinct in the Columbia Biological Course.

Field exploration in vertebrate palæontology is increasing very rapidly, and this summer a large number of parties will be in the field. The American Museum expedition to the Uinta Basin entered the field in March, accompanied by Mr. J. B. Hatcher, representing the Princeton Museum. On June 1st Dr. J. L. Wortman takes charge of the American Museum party, which will include four collectors. The University of Kansas will send three parties into the fossil beds of Kansas, Dakota and Wyoming. The University of Nebraska will also send a party under the direction of Prof. Barbour. Prof. Baur, of the University of Chicago, announces a field expedition as a regular part of the University curriculum.

The Royal Academy of Sciences of the Institute of Bologna offers a gold medal of the value of 1,000 francs for a memoir which either from the chemical, physical or mechanical point of view will indicate a practical system or new apparatus for the prevention or extinction of fire. The essays may be written in Italian, French or Latin. Those in other languages must be accompanied by an Italian translation. The essays are to be signed with a *nom de plume* and to be accompanied by an envelope containing the author's real name. All essays must be in before May 29, 1896, and should be addressed: "*Al Segretario della R. Academia delle Scienze dell' Instituto di Bologna.*"

The Trustees of the British Museum have issued a Catalogue of Additions to the Manuscripts in the years 1888–1893. The catalogue is provided with a serviceable index. They have also published a translation of the Papyrus of Ani which contains the most complete text of the famous Egyptian Book of the Dead. The translation, which is accompanied by a valuable introduction, is from the pen of Mr. E. A. Wallis Budge.

Another Egyptian publication of importance is from the press of Brill, at Leiden, and contains fac similes and descriptions of a papyrus (F. T. 71 So-am-tra) devoted to mortuary customs.

Mr. M. A. Mackenzie, of Trinity University, Toronto, has been appointed professor of mathematics in place of the Rev. Dr. Jones, who has accepted the position of bursar in the same institution.

Professor Frankland has been elected a foreign associate of the *Académie des Sciences*. The vacancy was caused by the death of M. van Beneden.

Applications for the position of lecturer in Chemistry in the university of Toronto should be sent to the Canadian Minister of Education before August 15th. The initial salary will be $1,000, increasing by annual increments of $100 until it reaches $1,800. The duties of the lecturer will be to assist the demonstrator in the superintendence of the laboratories under the direction of the professor of chemistry, and also to deliver such lectures on physiological, organic and inorganic chemistry as may be assigned to him by the professor.

The Lancet announces the following foreign medical appointments : At Erlangen—Dr. G. Hauser has been promoted to the chair of general and anatomical pathology, vacant by the retirement of Dr. von Zenker. At Gratz—Drs. Drasch and Jarisch have been promoted to professorships of histology and dermatology, respectively. At Oporto—Dr. I. do Valle, Professor of General Pathology, has been appointed to succeed Dr. Carlos Lopez in the chair of materia medica, Dr. Maximiano de Lemos taking the chair of general pathology.

At Berlin, Dr. Ferdinand Karsch and Dr. Anton Reichenow have been made professors in the Zoölogical Museum, Dr. Victor Kremser in the Meteorological Institute, and Dr. A. Börsch in the Geodetic Institute.

At the anniversary meeting of the Royal Geographical Society of London, Mr. Clements R. Markham was elected President for 1895–6. Mr. W. T. Blanford, the Hon. G. C. Brodrick, the Hon. George Curzon, Sir George Taubman Goldie, General R. Strachy and Rear-Admiral W. J. L. Wharton were elected Vice-Presidents.

Daniel Kirkwood, professor of mathematics in Indiana State University, died at Riverside, Cal., on June 11th, at the age of eighty-one. He retired from the active duties of the professorship in 1856.

The chair of physics in the University of California, recently filled by the late Professor Harold Whiting, has been offered to Mr. Exum Percival Lewis, Ph. D., of Johns Hopkins University.

At a meeting of the Royal Botanical Society on May 31st Professor George Henslow delivered a lecture on 'A Century of Progress in Floriculture.' He exibited specimens of the original wild plants from which some of our most admired garden flowers have been developed, illustrating with numerous diagrams the various stages in the way of cultivation and hybridization through which they passed before reaching the perfection of to-day.

Flood & Vincent (Chautauqua Press), of Meadville, Penna., announce the appearance of 'Thinking, Feeling, Doing,' a popular exposition of experimental psychology by E. W. Scripture, of Yale University. The book contains one colored plate and over 200 illustrations; it has a voluminous index.

ACCORDING to the *Evening Post* Professor Fabian Franklin has resigned his Professorship of Mathematics in Johns Hopkins University in order to become editor of the *Baltimore Evening News*.

THE American Medical College Association in Baltimore has decided by a vote of of 29 to 5 that a four years' course of study shall be demanded of all students henceforth matriculating in institutions belonging to this organization.

AT the graduating exercises of Johns Hopkins University on June 13th the degree of Ph. D. was conferred on 46 candidates, distributed among the different departments as follows: History and economics 12, chemistry 12, geology 3, German 2, English 3, physics 4, Romance 3, Latin and Greek 5, biology, mathematics and astronomy, each 1.

BARNARD COLLEGE has purchased for $160,000 a site on Cathedral Heights, adjacent to that of Columbia College. The sum of $200,000 has been subscribed towards the new buildings.

ON January 18th the great seismometrograph at the Osservatorio del Collegio Romano at Rome registered five complete pulsations of slow period characteristic of earthquakes originating at a great distance. They commenced at 4h. 37m. 30s. p. m. (Greenwich mean time), and lasted 1m. 22s., giving an average duration of 16.4 seconds for each pulsation. On the same day a severe earthquake was felt along the east coast of Japan, and was recorded at Tokio at 3h. 48m. 24s. The distance between this place and Rome being about 9,500 km., the pulsations must have traveled with an average velocity of 3.2 km. per second (see *Nature*, vol. l, pp. 450–51; vol. li., p. 462). At Nicolaiew and Charkow, in the south of Russia, the horizontal pendulums were disturbed for nearly an hour, the epoch of maximum amplitude occurring a few minutes earlier than at Rome.—*Nature.*

MESSRS. MACMILLAN & Co. will shortly publish an *Introduction to the Study of Seaweeds*, with illustrations, by Mr. George Murray, the newly appointed Keeper of Botany in the Natural History Department of the British Museum.

IT is announced that Professor Albert S. Bickmore, of the Museum of Natural History, New York, will deliver the address at the laying of the corner-stone of Butterfield Museum, Dartmouth College. It is hoped that the museum, which will cost about $60,000, will be ready for occupancy in the latter part of 1896.

ARRANGEMENTS for an accurate map of Africa will be made at the International Geographical Congress which is about to meet in London. It is expected that Great Britian, France, Germany, Belgium, Italy and Portugal, being the powers chiefly interested, will divide the expenses of the map.

THE Naturalists' Directory published by S. E. Cassino, Boston, for 1895, contains the names of 5,747 naturalists of the United States and Canada arranged in alphabetical order, giving under each name the specialty studied and the address. The names are also arranged by subjects and geographically by States. The directory contains 382 pages, and is neatly bound in cloth. The price is $2.50.

THE following appointments have been made in Cornell University: Virgil Snyder Ph. D. (Göttingen) has been appointed instructer in mathematics; Darwin A. Mortant, assistant in chemistry; W. K. Hatt (assistant professor at Purdue University) and John Hayfold, instructors in civil engineering; Elias J. Durand, assistant in cryptogamic botany, and H. H. Denham, instructor in chemistry.

THE Cambridge Scientific Instrument Company (Limited) has been formed with a capital of £10,000, in £5 shares. Its objects are to acquire the business carried on at Cambridge by Mr. Horace Darwin as 'The Cambridge Scientific Instrument Company,' and to adopt an agreement for the purpose, and to carry on the business of mechanical and electrical engineers, and scientific instrument and apparatus manufacturers. The first directors are Mr. Horace Darwin (chairman and managing director), Major Leonard Darwin, Mr. Hugh F. Newall and Mr. William N. Shaw. The remuneration of the directors will be fixed by the company.

DR. ALBERT MANN has been appointed professor of biology in Ohio Wesleyan University.

IN Syracuse University Dr. E. C. Quereau has been appointed professor of geology and mineralogy, and Dr. W. H. Metzler associate professor of mathematics.

DR. W. L. ABBOTT has sent to the U. S. National Museum the collections made during his travels in Pamir, Central Asia. Among these are the skins of 228 birds and more than 100 mammals, many of which are said to be new to science.

AN editorial article in *Garden and Forest* for May 29th contains on appeal for a fitting memorial to Andrew Jackson Downing. From it we may quote the following facts:

"Mr. Downing was an authoritative writer on the art of landscape-gardening. His treatise on the *Theory and Practice of Landscape-Gardening*, published in 1841, became at once the accepted text-book of the subject. In 1849 he wrote a series of articles in *The Horticulturalist* on public parks which had a marked influence in creating and molding public sentiment in this direction. The actual work of constructing Central Park was not begun until six years after Downing's untimely death, but it was his stirring appeals that aroused the city to feel its need, and provision to meet it quickly followed. It is not too much to say that Downing takes rank among the greatest benefactors to his country which this century has produced. It is now more than forty years since his death, and it is surely time that some memorial of him should be erected in the park which his genius secured for the city of New York."

THE last number Vol. VII., No. 4, of the Journal of the College of Science of the Imperial University of Japan bears witness, as the preceding numbers have done, to the aptitudes of the Japanese for exact research. The number contains eight short contributions to chemistry and an account of the earthquake of June 20th, 1894. This was the most violent earthquake that has occurred in Tokyo since 1855.

A WORK on electricity and magnetism by Professor Francis E. Nipher, Washington University, St. Louis, will be published during the summer.

THE State Agricultural College at Corvallis, Ore., has begun the publication of a series of laboratory studies in zoölogy edited by Prof. F. L. Washburn.

THE paper on the Proto-historic Ethnography of Western Asia, read by Dr. D. G. Brinton before the American Philosophical Society on April 19th, has been reprinted from the Proceedings of the American Philosophical Society and is published by MacCalla & Co., Philadelphia.

DR. J. DÖRFLER, I. Burgring 7, Vienna, is compiling a *Directory of Living Botanists*, together with botanical gardens, societies, journals, etc. The coöperation of botanists throughout the world is requested.

AT the annual meeting of the Linnaean Society, held on May 24th, the gold medal founded in 1888 on the occasion of the centenary of the Society, and awarded alternately to a biologist and zoölogist, was presented to Dr. Ferdinand Cohn, professor of botany in Breslau. Last year the medal was awarded to Professor Haeckel, of Jena, in recognition of his researches in the science of marine invertebrate zoölogy.

THE third International Congress of Physiology will meet in Berne from Sept. 9th to Sept. 13th, 1895.

ACCORDING to the *Revue Scientifique* M. Tocchini, the director of the Central Bureau of Meteorology in Rome, has founded a Seismological Society, having for its object the study of earthquakes and volcanic phenomena, and the publication of short accounts of the results obtained and of the apparatus used.

The *Revue Scientifique* also reports the formation of an Astronomical Society in Bruxelles, with the object of bringing into closer communication all those interested in astronomy and related sciences.

Two hundred unprinted letters of Pestalozzi have been found in Switzerland. They will be published by Seyffarth, whose biography of Pestalozzi has already reached its sixth edition.—*N. Y. Evening Post.*

HENRY PHILIPS, JR., died in Philadelphia on June 6th, at the age of 57. Mr. Philips was well known as an archæologist, numismatist and philologist.

THE University of Glasgow has received an anonymous gift of £10,000 for the purpose of founding a chair of political economy to be named after Adam Smith, who was once professor in the University.

THE honorary degree of Doctor of Science has been conferred by the University of Cambridge on Dr. John Murray, editor of the 'Challenger' publications.

THE following recent appointments to assistant professorships are announced from Johns Hopkins University: Dr. Charles Lane Poor, astronomy; Dr. Sidney Sherwood, political economy; Dr. Alexander S. Chessin, mathematics and mechanics; Dr. John M. Vincent, history; Dr. Simon Flexner, Pathology. Dr. Edward B. Matthews and Herbert G. Geer have been appointed associates in mineralogy and mechanical engineering respectively.

AN International Horticultural Congress was opened at Paris on May 24th.

THE *Institut* of France has opened an international subscription for a moument to Lavoisier, to be erected in Paris.

MR. RALPH SWINBURNE, said to have been the oldest engineer in the country, died recently, aged ninety years.

MR. L. L. PRICE's paper on 'The Colleges of Oxford and Agricultural Depression' contains, according to the *Academy*, "a detailed analysis of the expenditure of the colleges in 1883 and 1893. During this period the amount received by the heads (excluding Christ Church) has fallen from £22,811 to £20,905, or by more than 8 per cent.: in some cases, of course, the decrease is much more, while in a few there is an increase. The amount received by fellows (apparently including professor-fellows) has fallen from £70,980 to £59,715, or by more than 15 per cent. Here, again, there are wide variations, though only two examples of actual increase. In the case of one college, which shall be nameless, eight fellows in 1893 had only £400 to divide among them. On the other hand, the amount appropriated to scholarships and exhibitions has risen during the same period from £44-776 to £48,378, or by nearly 10 per cent. In hardly any case is there a decline; while at the unnamed college referred to above the scholars now receive nearly four times as much as the fellows. The number of scholars and exhibitors has risen from 570 to 658, while the number of fellows seems to have remained stationary. In addition, the colleges in 1893 paid over an assessment of £4,334 to the common university fund, a heading which practically did not exist in 1883; while during these ten years contributions to the salaries of the professoriate have increased from £12,840 to £15,034. It seems pretty clear that the results of agricultural depression have fallen almost solely upon the fellows, and upon some of them hardly."

Professor Bunsen celebrated his eighty-fifth birthday on March 31st.

At the last meeting of the Geological Society, Prof. Judd drew attention to an interesting series of photographs sent for exhibition by Prof. Liversidge, of Sydney, who has found that sections of gold nuggets, when etched with chlorine-water, exhibit lines like the Widman-Stetten figures of meteorites, showing that the gold has a crystalline structure, octahedral and cubic forms being displayed.—*The Academy.*

SOCIETIES AND ACADEMIES.

BIOLOGICAL SOCIETY OF WASHINGTON.

At the meeting held May 18th Dr. Merriam spoke of the Mammals of the Pribilof Islands in Bering Sea. Excluding Cetaceans, eight mammals are known from the Islands. Four of these are land mammals and four amphibious or marine, as follows: One, Arctic fox; two, brown lemming; three, shrew; four, house mouse; five, harbor seal; six, fur seal; seven, sea-lion; eight, walrus. To these the sea-otter might be added, though it is not a resident and visits the islands very rarely. The house mouse was introduced by the Russians and has run wild. The fox also is said to have been introduced. The shrew has been found on St. Paul only; the lemming on St. George only.

A paper entitled 'The Hares (genus *Lepus*) of the Mexican Border' was read by Dr. Edgar A. Mearns, who stated that it was written in the course of preparation of a report on the collections made by the biological section of the recent re-survey of the Mexican boundary line, of which expedition Dr. Mearns was the surgeon and naturalist from January, 1892, to September, 1894, with one intermission of a few months. The doctor's field experience in that general region covers in all a period of seven years. The specimens of *Lepus* accumulated during that time amount to 288, representing 15 species and subspecies, to which material were added the collections of the United States National Museum and a portion of those of the American Museum of Natural History in New York, making a total of about 400 specimens examined. The species of the Mexican border were shown to represent three sections of the genus *Lepus*, which might with advantage be recognized as subgenera. These were Hydrolagus Gray (Water Hares, represented by a single species, *Lepus aquaticus* Bachman); Sylvilagus Gray (comprising (1) the Cottontails, 3 species and 3 additional subspecies, and (2) the Cactus Hare, *Lepus cinerascens* Allen); and Macrotolagus (a new subgenus created for the Mexican group of Jackrabbits, of which 6 species and 3 additional subspecies were found on the Mexican border). In all, 17 forms were recognized as occurring on the strip of the United States which borders on Mexico, of which number seven were treated as subspecies and the remainder as species, of which latter there are eleven, *Lepus sylvaticus* being represented by (3) subspecies. Two species and four subspecies were described as new. Of these, Holzner's Cottontail inhabits wooded mountains from New Mexico and Arizona southward, and the Lesser Desert Cottontail the region from the upper Rio Grande of Texas westward to the continental divide. The black-naped Jackrabbit of the Lower Rio Grande was named in honor of Dr. C. Hart Merriam; and another species of Jackrabbit from the plains east of the continental divide was dedicated to Lieutenant D. D. Gaillard, U. S. A., a member of the International Boundary Commission. The Gray Jackrabbit of the Upper Rio Grande region, and the Desert Jackrabbit of the Colorado Desert, were described as superficially distinct from the *Lepus texianus* Waterhouse. The Mexican Jackrabbit (*Lepus callotis* Wagler), with which several species inhabiting the United States have hitherto been confounded, was shown, principally on the authority of Dr. C. Hart Merriam, as the result of explorations lately conducted in Mexico by his Division of the U. S. Department of Agriculture, to be wholly extralimital to the United States, and not to occur near our southern border.

Diagnoses of the new Hares discovered by Dr. Mearns will soon appear in the proceedings of the U. S. National Museum, the complete article to form a part of the biological report of the International Boundary Commission.

Dr. Erwin F. Smith read a paper on *The Biology of Bacillus tracheiphilus* n. sp., *the cause of wilt in various Cucurbits.* The organism has been isolated and numerous infections secured from pure cultures—more than fifty—in the greenhouse under strict control. The disease has also been induced by spraying the bacillus on insects (Diabrotica vittata and Coreus tristis) and turning these loose on the plants, thus confirming a belief expressed in 1893, and due to field observations, that the disease is ordinarily transmitted by leaf eating beetles and squash bugs. During the nine months in which experiments have been conducted under glass, the only cases have been those due to artificial infections, none of the numerous control plants having developed the disease. The paper described the morphology of the organism, its behavior in various media—agar, gelatine, potato and sweet potato, beef broth, vegetable infusions, milk and various saccharine fluids in fermentation tubes; resistance to heat and dry air; behavior with stains; growth in acid and alkaline media, in hydrogen; parts of plants attacked, lesions, symptoms, time of appearance after inoculation, etc. Numerous repeated inoculations into potato and tomato vines failed to induce any disease, and the positive and negative evidence are both conclusive that this disease is entirely different from the southern potato and tomato blight. Inoculations into pears and hyacinths also gave negative results. The organism used for infections was isolated from the cucumber, and most of the inoculations were performed on the cucumber and muskmelon by pricking the germs into the blade of a leaf. Experiments on pumpkins and squashes are still in progress. The prompt destruction of leaf-eating and leaf-puncturing insects appears to be the only satisfactory way of combating this disease. How this shall be done to best advantage is a problem belonging to the province of economic entomology.

An interesting paper on the *Means of Intercommunication among Wolves*, by Mr. Ernest Thompson, was read. Mr. Thompson gave first place to the sense of smell as a means of obtaining information.

M. B. WAITE,
Recording Secretary.

THE NEW JERSEY STATE MICROSCOPICAL SOCIETY.

THE Society held its 26th annual meeting on Monday, May 27th, and elected the following officers for 1895–96:

President, Byron D. Halsted, Sc. D.
Vice-President, Julius Nelson, Ph. D.
Recording Secretary, Frederick H. Blodgett.
Corresponding Secretary, John Helm, M. D.
Treasurer, A. C. Hutton, M. D.
Curator, A. H. Chester, Ph. D.
Librarian, Frederick H. Blodgett.
Trustee (two years), Fred. B. Kilmer.

The Secretary's report showed an increase in general interest on the part of the members and an increase also in the attendance of visitors at the regular meetings.

The quarter-centennial was celebrated by a well attended public meeting. The program of this meeting included the projection of micro-slides of rock sections, marine algæ, living animalculæ and wood sections, and table exhibits from the three natural kingdoms under thirty-five instruments.

About a year ago the Society was sectionalized, and the following sections created:

(1) Agriculture, (2) Bacteriology, (3) Biology (Zoölogy), (4) Botany, (5) Chemistry, (6) Entomology, (7) Geology, (8) Histology, (9) Mineralogy, (10) Pathology, (11) Physics, (12) Technique, (13) Literature.

Of these the sections on Bacteriology, Botany and Mineralogy have had charge of one meeting each, and reports of less length have been made by the sections on Technique and Literature.

The membership includes 40 active, 19 corresponding and 1 honorary member.

After the business session A. H. Chester, Ph. D., read a paper on 'Crystals,' describing the means used in the preparation of crystals for micro-mounts; slow crystallization from fusion, or solution, sublimation, precipitation and electrolysis. The paper described the systems of crystals to some extent, mentioning more especially those of gold, silver and copper. With the aid of ten microscopes the minute beauties of the crystals were shown with appreciation to a goodly number of members and friends.

SCIENTIFIC JOURNALS.

AMERICAN JOURNAL OF SCIENCE, JUNE, 1895.

The June number of the American Journal of Science opens with an article by Prof. Frank Waldo discussing the daily march of the wind velocities in the United States. This is based upon the published data furnished by the Chief Signal Officer's Report for 1890, giving the average wind movement for each hour of each day in this year, and also the daily averages for the seven years 1883–89. These are discussed for the different portions of the country and the results presented in a series of curves; they show distinct maxima for many stations in January, which are still more developed in July. D. A. Kreider describes the preparation of perchloric acid and its application to the determination of potassium; also W. H. Hobbs, the crystal form of borneol and isoborneol. R. Ruedemann gives an abstract of a paper (to appear in full in the Report of the New York State Geologist) on the mode of growth and development of the graptolitic genus Diplograptus; a series of figures illustrates the subject. N. H. Darton gives an account of the recent discovery of a dike penetrating the Salina formation at DeWitt near Syracuse, N. Y.; this occurrence is of especial interest because doubtless connected with the Syracuse dike described by Dr. G. H. Williams in 1887. The petrography of the DeWitt dike is fully given by J. F. Kemp. Another article is by G. M. Dawson, giving a general discussion of the amount of elevation that has taken place along the Rocky Mountain Range in British America since the close of the Cretaceous; the minimum estimate obtained of greatest uplift for the region (about latitude 50°) is 32,000 to 35,000 feet. Three analyses of sodalite are given by L. McI. Luquer and G. J. Volckening. The number closes with a series of abstracts and reviews, and finally the volume index. Under the Geological Notes, R. T. Hill mentions the discovery of a dicotyledonous flora in the Cheyenne sandstone at the base of the beds belonging to the Comanche series in Comanche and Barber counties, of southern Kansas.

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Subscriptions and advertisements should be sent to Science, 41 N. Queen St., Lancaster, Pa., or 41 East 49th St., New York.

*ARGON.**

It is some three or four years since I had the honour of lecturing here one Friday evening upon the densities of oxygen and hydrogen gases, and upon the conclusions that might be drawn from the results. It is not necessary, therefore, that I should trouble you to-night with any detail as to the method by which gases can be accurately weighed. I must take that as known, merely mentioning that it is substantially the same as is used by all investigators nowadays, and introduced more than fifty years ago by Regnault. It was not until after that lecture that I turned my attention to nitrogen; and in the first instance I employed a method of preparing the gas which originated with Mr. Vernon Harcourt, of Oxford. In this method the oxygen of ordinary atmospheric air is got rid of with the aid of ammonia. Air is bubbled through liquid ammonia, and then passed through a red-hot tube. In its passage the oxygen of the air combines with the hydrogen of the ammonia, all the oxygen being in that way burnt up and converted into water. The excess of ammonia is subsequently absorbed with acid, and the water by ordinary desiccating agents. That method is very convenient; and, when I had obtained a few concordant results by means of it, I thought that the work was complete, and that the weight of nitrogen was satisfactorily determined. But then I reflected that it is always advisable to employ more than one method, and that the method that I had used—Mr. Vernon Harcourt's method—was not that which had been used by any of those who had preceded me in weighing nitrogen. The usual method consists in absorbing the oxygen of air by means of

*A Lecture given by Lord Rayleigh before the Royal Institution of Great Britain, on Friday, April 5, 1895. Reprinted from the official report.

red-hot copper; and I thought that I ought at least to give that method a trial, fully expecting to obtain forthwith a value in harmony with that already afforded by the ammonia method. The result, however, proved otherwise. The gas obtained by the copper method, as I may call it, proved to be one-thousandth part heavier than that obtained by the ammonia method; and, on repetition, that difference was only brought out more clearly. This was about three years ago. Then, in order, if possible, to get further light upon a discrepancy which puzzled me very much, and which, at that time, I regarded only with disgust and impatience, I published a letter in *Nature* inviting criticisms from chemists who might be interested in such questions. I obtained various useful suggestions, but none going to the root of the matter. Several persons who wrote to me privately were inclined to think that the explanation was to be sought in a partial dissociation of the nitrogen derived from ammonia. For, before going further, I ought to explain that, in the nitrogen obtained by the ammonia method, some—about a seventh part—is derived from the ammonia, the larger part, however, being derived as usual from the atmosphere. If the chemically derived nitrogen were partly dissociated into its component atoms, then the lightness of the gas so prepared would be explained.

The next step in the enquiry was, if possible, to exaggerate the discrepancy. One's instinct at first is to try to get rid of a discrepancy, but I believe that experience shows such an endeavor to be a mistake. What one ought to do is to magnify a small discrepancy with a view to finding out the explanation; and, as it appeared in the present case that the root of the discrepancy lay in the fact that part of the nitrogen prepared by the ammonia method was nitrogen out of ammonia, although the greater part remained of common origin in both cases, the application of the principal suggested a trial of the weight of nitrogen obtained wholly from ammonia. This could easily be done by substituting pure oxygen for atmospheric air in the ammonia method, so that the whole, instead of only a part, of the nitrogen collected should be derived from the ammonia itself. The discrepancy was at once magnified some five times. The nitrogen so obtained from ammonia proved to be about one-half per cent. lighter than nitrogen obtained in the ordinary way from the atmosphere, and which I may call for brevity 'atmospheric' nitrogen.

That result stood out pretty sharply from the first; but it was necessary to confirm it by comparison with nitrogen chemically derived in other ways. The table before you gives a summary of such results, the numbers being the weights in grams actually contained under standard conditions in the globe employed.

ATMOSPHERIC NITROGEN.

By hot copper (1892)	2.3103
By hot iron (1893)	2.3100
By ferrous hydrate (1894)	2.3102
Mean	2.3102

CHEMICAL NITROGEN.

From nitric oxide	2.3001
From nitrous oxide	2.2990
From ammonium nitrite purified at a red heat	2.2987
From urea	2.2985
From ammonium nitrite purified in the cold	2.2987
Mean	2.2990

The difference is about 11 milligrams, or about one-half per cent.; and it was sufficient to prove conclusively that the two kinds of nitrogen—the chemically derived nitrogen and the atmospheric nitrogen—differed in weight, and therefore, of course, in quality, for some reason hitherto unknown.

I need not spend time in explaining the various precautions that were necessary in order to establish surely that conclusion. One had to be on one's guard against impurities, especially against the presence of hydrogen, which might seriously lighten any gas in which it was contained. I believe, however, that the precautions taken were sufficient to exclude all questions of that sort, and the result, which I published about this time last year, stood sharply out, that the nitrogen obtained from chemical sources was different from the nitrogen obtained from the air.

Well, that difference, admitting it to be established, was sufficient to show that some hitherto unknown gas is involved in the matter. It might be that the new gas was dissociated nitrogen, contained in that which was too light, the chemical nitrogen —and at first that was the explanation to which I leaned; but certain experiments went a long way to discourage such a supposition. In the first place, chemical evidence—and in this matter I am greatly dependent upon the kindness of chemical friends—tends to show that, even if ordinary nitrogen could be dissociated at all into its component atoms, such atoms would not be likely to enjoy any very long continued existence. Even ozone goes slowly back to the more normal state of oxygen; and it was thought that dissociated nitrogen would have even a greater tendency to revert to the normal condition. The experiment suggested by that remark was as follows—to keep chemical nitrogen—the too light nitrogen which might be supposed to contain dissociated molecules—for a good while, and to examine whether it changed in density. Of course it would be useless to shut up gas in a globe and weigh it, and then, after an interval, to weigh it again, for there would be no opportunity for any change of weight to occur, even although the gas within the globe had undergone some chemical alteration. It is necessary to re-establish the standard conditions of temperature and pressure which are always understood when we speak of filling a globe with gas, for I need hardly say that filling a globe with gas is but a figure of speech. Everything depends upon the temperature and pressure at which you work. However, that obvious point being borne in mind, it was proved by experiment that the gas did not change in weight by standing for eight months—a result tending to show that the abnormal lightness was not the consequence of dissociation.

Further experiments were tried upon the action of the silent electric discharge—both upon the atmospheric nitrogen and upon the chemically derived nitrogen—but neither of them seemed to be sensibly affected by such treatment; so that, altogether, the balance of evidence seemed to incline against the hypothesis of abnormal lightness in the chemically derived nitrogen being due to dissociation, and to suggest strongly, as almost the only possible alternative, that there must be in atmospheric nitrogen some constituent heavier than true nitrogen.

At that point the question arose, What was the evidence that all the so-called nitrogen of the atmosphere was of one quality? And I remember—I think it was about this time last year, or a little earlier —putting the question to my colleague, Professor Dewar. His answer was that he doubted whether anything material had been done upon the matter since the time of Cavendish, and that I had better refer to Cavendish's original paper. The advice I quickly followed, and I was rather surprised to find that Cavendish had himself put this question quite as sharply as I could put it. Translated from the old-fashioned phraseology connected with the theory of phlogiston, his question was whether the inert ingredient of the air is really all of one kind, whether all the nitrogen of the air is really the same as the nitrogen of nitre. Cavendish not only asked himself this question, but he endeavoured to answer it by an appeal to experiment.

I should like to show you Cavendish's experiment in something like its original form. He inverted a U tube filled with mercury, the legs standing in two separate mercury cups. He then passed up, so as to stand above the mercury, a mixture of nitrogen, or of air, and oxygen; and he caused an electric current from a frictional electrical machine like the one I have before me to pass from the mercury in the one leg to the mercury in the other, giving sparks across the intervening column of air. I do not propose to use a frictional machine to-night, but I will substitute for it one giving electricity of the same quality of the construction introduced by Mr. Wimshurst, of which we have a fine specimen in the Institution. It stands just outside the door of the theatre, and will supply an electric current along insulated wires, leading to the mercury cups; and, if we are successful, we shall cause sparks to pass through the small length of air included above the columns of mercury. There they are; and after a little time you will notice that the mercury rises, indicating that the gas is sensibly absorbed under the influence of the sparks and of a piece of potash floating on the mercury. It was by that means that Cavendish established his great discovery of the nature of the inert ingredient in the atmosphere, which we now call nitrogen; and, as I have said, Cavendish himself proposed the question, as distinctly as we can do, Is this inert ingredient all of one kind? and he proceeded to test that question. He found, after days and weeks of protracted experiment, that, for the most part, the nitrogen of the atmosphere was absorbed in this manner, and converted into nitrous acid; but that there was a small residue remaining after prolonged treatment with sparks, and a final absorption of the residual oxygen. That residue amounted to about $\frac{1}{120}$ part of the nitrogen taken; and Cavendish draws the conclusion that, if there be more than one inert ingredient in the atmosphere, at any rate the second ingredient is not contained to a greater extent than $\frac{1}{120}$ part.

I must not wait too long over the experiment. Mr. Gordon tells me that a certain amount of contraction has already occurred; and if we project the U upon the screen, we shall be able to verify the fact. It is only a question of time for the greater part of the gas to be taken up, as we have proved by preliminary experiments.

In what I have to say from this point onwards, I must be understood as speaking as much on behalf of Professor Ramsay as for myself. At the first, the work which we did was to a certain extent independent. Afterwards we worked in concert, and all that we have published in our joint names must be regarded as being equally the work of both of us. But, of course, Professor Ramsay must not be held responsible for any chemical blunder into which I may stumble to-night.

By his work and by mine the heavier ingredient in atmospheric nitrogen which was the origin of the discrepancy in the densities has been isolated, and we have given it the name of 'argon.' For this purpose we may use the original method of Cavendish, with the advantages of modern appliances. We can procure more powerful electric sparks than any which Cavendish could command by the use of the ordinary Ruhmkorff coil stimulated by a battery of Grove cells; and it is possible so to obtain evidence of the existence of argon. The oxidation of nitrogen by that method goes on pretty quickly. If you put some ordinary air, or, better still, a mixture of air and oxygen, in a tube in which electric sparks are made to pass for a certain time, then, in looking through the tube, you observe the well-known reddish-orange fumes of the oxides of nitrogen. I will not take up time in going through the experiment, but will merely exhibit a tube already prepared (image on screen).

One can work more efficiently by employing the alternate currents from dynamo machines which are now at our command. In this institution we have the advantage of a public supply; and if I pass alternate currents originating in Deptford through this Ruhmkorff coil, which acts as what is now called a 'high potential transformer,' and allow sparks from the secondary to pass in an inverted test tube between platinum points, we shall be able to show in a comparatively short time a pretty rapid absorption of the gases. The electric current is led into the working chamber through bent glass tubes containing mercury, and provided at their inner extremities with platinum points. In this arrangement we avoid the risk, which would otherwise be serious, of a fracture just when we least desired it. I now start the sparks by switching on the Ruhmkorff to the alternate current supply; and, if you will take note of the level of the liquid representing the quantity of mixed gases included, I think you will see after, perhaps, a quarter of an hour that the liquid has very appreciably risen, owing to the union of the nitrogen and the oxygen gases under the influence of the electrical discharge, and subsequent absorption of the resulting compound by the alkaline liquid with which the gas space is enclosed.

By means of this little apparatus, which is very convenient for operations upon a moderate scale, such as for analysis of 'nitrogen' for the amount of argon that it may contain, we are able to get an absorption of about 80 cubic centimetres per hour, or about 4 inches along this test tube, when all is going well. In order, however, to obtain the isolation of argon on any considerable scale by means of the oxygen method, we must employ an apparatus still more enlarged. The isolation of argon requires the removal of nitrogen, and, indeed, of very large quantities of nitrogen, for, as it appears, the proportion of argon contained in atmospheric nitrogen is only about 1 per cent., so that for every litre of argon that you wish to get you must eat up some hundred litres of nitrogen. That, however, can be done upon an adequate scale by calling to our aid the powerful electric discharge now obtainable by means of the alternate current supply and high potential transformers.

In what I have done upon this subject I have had the advantage of the advice of Mr. Crookes, who some years ago drew special attention to the electric discharge or flame, and showed that many of its properties depended upon the fact that it had the power of causing, upon a considerable scale, a combination of the nitrogen and the oxygen of the air in which it was made.

I had first thought of showing in the lecture room the actual apparatus which I have employed for the concentration of argon; but the difficulty is that, as the apparatus has to be used, the working parts are almost invisible, and I came to the conclusion that it would really be more instructive as well as more convenient to show the parts isolated, a very little effort of imagination being then all that is required in order to reconstruct in the mind the actual arrangements employed.

First, as to the electric arc or flame itself. We have here a transformer made by Pike and Harris. It is not the one that I have used in practice; but it is convenient for certain purposes, and it can be connected by means of a switch with the alternate currents of 100 volts furnished by the Supply Company. The platinum terminals that you see here are modelled exactly upon the plan of those which have been employed in practice. I may say a word or two on the question of mounting. The terminals require to be very massive on account of the the heat evolved. In this case they consist of platinum wire doubled upon itself six times. The platinums are continued by iron wires going through glass tubes, and attached at the ends to the copper leads. For better security, the tubes themselves are stopped at the lower ends with corks and charged with water, the advantage being that, when the whole arrangement is fitted by means of an indiarubber stopper into a closed vessel, you have a witness that, as long as the water remains in position, no leak can have occurred through the insulating tubes conveying the electrodes.

Now, if we switch on the current and approximate the points sufficiently, we get the electric flame. There you have it. It is, at present, showing a certain amount of soda. That in time would burn off. After the arc has once been struck, the platinums can be separated; and then you have two tongues of fire ascending almost independently of one another, but meeting above. Under the influence of such a flame the oxygen and the nitrogen of the air combine at a reasonable rate, and in this way the nitrogen is got rid of. It is now only a question of boxing up the gas in a closed space, where the argon concentrated by the combustion of the nitrogen can be collected. But there are difficulties to be encountered here. One cannot well use anything but a glass vessel. There is hardly any metal available that will withstand the action of strong caustic alkali and of the nitrous fumes resulting from the flame. One is practically limited to glass. The glass vessel employed is a large flask with a single neck, about half full of caustic alkali. The electrodes are carried through the neck by means of an indiarubber bung provided also with tubes for leading in the gas. The electric flame is situated at a distance of only about half an inch above the caustic alkali. In that way an efficient circulation is established; the hot gases as they rise from the flame strike the top, and then as they come around again in the course of the circulation they pass sufficiently close to the caustic alkali to insure an adequate removal of the nitrous fumes.

There is another point to be mentioned. It is necessary to keep the vessel cool; otherwise the heat would soon rise to such a point that there would be excessive generation of steam, and then the operation would come to a standstill. In order to meet this difficulty the upper part of the vessel is provided with a water-jacket, in which a circulation can be established. No doubt the glass is severely treated, but it seems to stand it it in a fairly amiable manner.

By means of an arrangement of this kind, taking nearly three-horse power from the electric supply, it is possible to consume nitrogen at a reasonable rate. The transformers actually used are the 'Hedgehog' transformers of Mr. Swinburne, intended to transform from 100 to 2400 volts. By Mr. Swinburne's advice I have used two such, the fine wires being in series so as to accumulate the electrical potential and the thick wires in parallel. The rate at which the mixed gases are absorbed is about seven litres per hour; and the apparatus, when once fairly started, works very well as a rule, going for many hours without attention. At times the arc has a trick of going out, and it then requires to be restarted by approximating the platinums. We have already worked 14 hours on end, and by the aid of one or two automatic appliances it would, I think, be possible, to continue operations day and night.

The gases, air and oxygen in about equal proportions, are mixed in a large gasholder, and are fed in automatically as required. The argon gradually accumulates; and when it is desired to stop operations the supply of nitrogen is cut off, and only pure oxygen allowed admittance. In this way the remaining nitrogen is consumed, so that, finally, the working vessel is charged with a mixture of argon and oxygen only, from which the oxygen is removed by ordinary well-known chemical methods. I may mention that at the close of the operation, when the nitrogen is all gone, the arc changes its appearance and becomes of a brilliant blue colour.

I have said enough about this method, and I must now pass on to the alternative method which has been very successful in Professor Ramsay's hands—that of absorbing nitrogen by means of red-hot magnesium. By the kindness of Professor Ramsay and Mr. Matthews, his assistant, we have here the full scale apparatus before us almost exactly as they use it. On the left there is a reservoir of nitrogen derived from air by the simple removal of oxygen. The gas is then dried. Here it is bubbled through sulphuric acid. It then passes through a long tube made of hard glass and charged with magnesium in the form of thin turnings. During the passage of the gas over the magnesium at a bright red heat, the nitrogen is absorbed in a great degree, and the gas which finally passes through is immensely richer in argon than that which first enters the hot tube. At the present time you see a tolerably rapid bubbling on the left, indicative of the flow of atmospheric nitrogen into the combustion furnace; whereas, on the right, the outflow is very much slower. Care must be taken to prevent the heat rising to such a point as to soften the glass. The concentrated argon is collected in a second gasholder, and afterwards submitted to further treatment. The apparatus employed by Professor Ramsay in the subsequent treatment is exhibited in the diagram, and is very effective for its purpose; but I am afraid that the details of it would not readily be followed from any explanation that I could give in the time at my disposal. The principle consists in the circulation of the mixture of nitrogen and argon over hot magnesium, the gas being made to pass round and round until the nitrogen is effectively removed from it. At the end that operation, as in the case of the oxygen method, proceeds somewhat slowly. When the greater part of the nitrogen is gone, the remainder seems to be unwilling to follow, and it requires somewhat protracted treatment in order to be sure that the nitogen has wholly disappeared. When I say 'wholly disappeared,' that, perhaps, would be too much to say in any case. What we can say is that the spectrum test is adequate to show the presence, or at any rate to show the addition, of about one-and-a-half per cent. of nitrogen to argon as pure as we can get it; so that it is fair to argue that any nitrogen at that stage remaining in the argon is only a small fraction of one-and-a-half per cent.

I should have liked at this point to be able to give advice as to which of the two methods—the oxygen method or the magnesium method—is the easier and the more to be recommended; but I confess that I am quite at a loss to do so. One difficulty in the comparison arises from the fact that they have been in different hands. As far as I can estimate, the quantities of nitrogen eaten up in a given time are not very different. In that respect, perhaps, the magnesium method has some advantage; but, on the other hand, it may be said that the magnesium process requires a much closer supervision, so that, perhaps, fourteen hours of the oxygen method may not unfairly compare with eight hours or so of the magnesium method. In practice a great deal would depend upon whether in any particular laboratory alternate currents are available from a public supply. If the alternate currents are at hand, I think it may probably be the case that the oxygen method is the easier; but otherwise, the magnesium method would, probably, be preferred, especially by chemists who are familiar with operations conducted in red-hot tubes.

I have here another experiment illustrative of the reaction between magnesium and nitrogen. Two rods of that metal are suitably mounted in an atmosphere of nitrogen, so arranged that we can bring them into contact and cause an electric arc to form between them. Under the action of the heat of the electric arc the nitrogen will combine with the magnesium; and if we had time to carry out the experiment we could demonstrate a rapid absorption of nitrogen by this method. When the experiment was first tried, I had hoped that it might be possible, by the aid of electricity, to start the action so effectively that the magnesium would continue to burn independently under its own developed heat in the atmosphere of nitrogen. Possibly, on a larger scale, something of this sort might succeed, but I bring it forward here only as an illustration. We turn on the electric current and bring the magnesiums together. You see a brilliant green light, indicating the vaporisation of the magnesium. Under the influence of the heat the magnesium burns, and there is collected in the glass vessel a certain amount of brownish-looking powder which consists mainly of the nitride of magnesium. Of course, if there is any oxygen present it has the preference, and the ordinary white oxide of magnesium is formed.

The gas thus isolated is proved to be inert by the very fact of its isolation. It refuses to combine under circumstances in which nitrogen, itself always considered very inert, does combine—both in the case of the oxygen treatment and in the case of the magnesium treatment; and these facts are, perhaps, almost enough to justify the name which we have suggested for it. But, in addition to this, it has been proved to be inert under a considerable variety of other conditions such as might have been expected to tempt it into combination. I will not recapitulate all the experiments which have been tried, almost entirely by Professor Ramsay, to induce the gas to combine. Hitherto, in our hands, it has not done so; and I may mention that recently, since the publication of the abstract of our paper read before the Royal Society, argon has been submitted to the action of titanium at a red heat, titanium being a metal having a great affinity for nitrogen, and that argon has resisted the temptation to which nitrogen succumbs. We never have asserted, and we do not now assert, that argon can under no circumstances be got to combine. That would, indeed, be a rash assertion for any one to venture upon; and only within the last few weeks there has been a most interesting announcement by M. Berthelot, of Paris, that, under the action of the silent electric discharge, argon can be absorbed when treated in contact with the vapor of benzine. Such a statement, coming from so great an authority, commands our attention; and if we accept the conclusion, as I suppose we must do, it will follow that argon has, under those circumstances, combined.

Argon is rather freely soluble in water. That is a thing that troubled us at first in trying to isolate the gas; because, when one was dealing with very small quantities, it seemed to be always disappearing. In trying to accumulate it we made no progress. After a sufficient quantity had been prepared, special experiments were made on solubility of argon in water. It has been found that argon, prepared both by the magnesium method and by the oxygen method, has about the same solubility in water as oxygen—some two-and-a-half times the solubility of nitrogen. This suggests, what has been verified by experiment, that the dissolved gases of water should contain a larger proportion of argon than does atmospheric nitrogen. I have here an apparatus of a somewhat rough description, which I have employed in experiments of this kind. The boiler employed consists of an old oil-can. The water is applied to it and drawn from it by coaxial tubes of metal. The incoming cold water flows through the outer annulus between the two tubes. The outgoing hot water passes through the inner tube, which ends in the interior of the vessel at a higher level. By means of this arrangement the heat of the water which has done its work is passed on to the incoming water not yet in operation, and in that way a limited amount of heat is made to bring up to the boil a very much larger quantity of water than would otherwise be possible, the greater part of the dissolved gases being liberated at the same time. These are collected in the ordinary way. What you see in this flask is dissolved air collected out of water in the course of the last three or four hours. Such gas, when treated as if it were atmospheric nitrogen, that is to say after removal of the oxygen and minor impurities, is found to be decidedly heavier than atmospheric nitrogen to such an extent as to indicate that the proportion of argon contained is about double. It is obvious, therefore, that the dissolved gases of water form a convenient source of argon, by which some of the labor of separation from air is obviated. During the last few weeks I have been supplied from Manchester by Mr. Macdougall, who has interested himself in this matter, with a quantity of dissolved gases obtained from the condensing water of his steam engine.

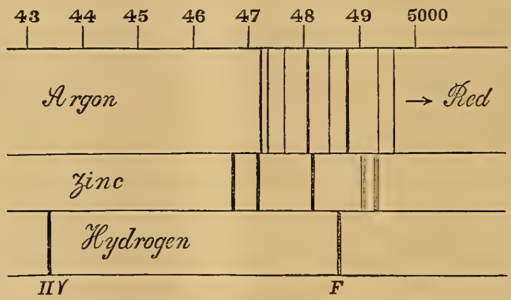
As to the spectrum, we have been indebted from the first to Mr. Crookes, and he has been good enough to-night to bring some tubes which he will operate, and which will show you at all events the light of the electric discharge in argon. I cannot show you the spectrum of argon, for unfortunately the amount of light from a vacuum tube is not sufficient for the projection of its spectrum. Under some circumstances the light is red, and under other circumstances it is blue. Of course when these lights are examined with the spectroscope—and they have been examined by Mr. Crookes with great care—the differences in the color of the light translate themselves into different groups of spectrum lines. We have before us Mr. Crookes' map, showing the two spectra upon a very large scale. The upper is the spectrum of the blue light; the lower is the spectrum of the red light; and it will be seen that they differ very greatly. Some lines are common to both; but a great many lines are seen only in the red, and others are seen only in the blue. It is astonishing to notice what trifling changes in the conditions of the discharge bring about such extensive alterations in the spectrum.

One question of great importance, upon which the spectrum throws light is: Is the argon derived by the oxygen method really the same as the argon derived by the magnesium method? By Mr. Crookes' kindness I have had an opportunity of examining the spectra of the two gases side by side, and such examination as I could make revealed no difference whatever in the two spectra, from which, I suppose, we may conclude either that the gases are absolutely the same, or, if they are not the same, that at any rate the ingredients by which they differ cannot be present in more than a small proportion in either of them.

My own observations upon the spectrum have been made principally at atmospheric pressure. In the ordinary process of sparking, the pressure is atmospheric, and if we wish to look at the spectrum we have nothing more to do than to include a jar in the circuit and to put a direct vision prism to the eye. At my request, Professor Schuster examined some tubes containing argon at atmospheric pressure prepared by the oxygen method, and I have here a diagram of a characteristic group. He also placed upon the sketch some of the lines of zinc, which were very convenient as directing one exactly where to look. (See Fig.)

Within the last few days Mr. Crookes has charged a radiometer with argon. When held in the light from the electric lamp the vanes revolve rapidly. Argon is anomalous in many respects, but not, you see, in this.

Next, as to the density of argon. Professor Ramsay has made numerous and careful observations upon the density of the gas prepared by the magnesium method, and he finds a density of about 19.9 as compared with hydrogen. Equally satisfactory observations upon the gas derived by the oxygen method have not yet been made, but there is no reason to suppose that the density is different, such numbers as 19.7 having been obtained.



One of the most interesting matters in connection with argon, however, is what is known as the ratio of the specific heats. I must not stay to elaborate the questions involved, but it will be known to many who hear me that the velocity of sound in a gas depends upon the ratio of two specific heats—the specific heat of the gas measured at constant pressure, and the specific heat measured at constant volume. If we know the density of a gas, and also the velocity of sound in it, we are in a position to infer this ratio of specific heats; and by means of this method, Professor Ramsay has determined the ratio in the case of argon, arriving at the very remarkable result that the ratio of specific heats is represented by the number 1.65, approaching very closely to the theoretical limit, 1.67. The number 1.67 would indicate that the gas has no energy except energy of translation of its molecules. If there is any other energy than that, it would show itself by this number dropping below 1.67. Ordinary gases, oxygen, nitrogen, hydrogen, etc., do drop below, giving the number 1.4. Other gases drop lower still. If the ratio of specific heats is 1.65, practically 1.67, we may infer then that the whole energy of motion is translational; and from that it would seem to follow by arguments which, however, I must not stop to elaborate, that the gas must be of the kind called by chemists monatomic.

I had intended to say something of the operation of determining the ratio of specific heats, but time will not allow. The result is, no doubt, very awkward. Indeed, I have seen some indications that the anomalous properties of argon are brought as a kind of accusation against us. But we had the very best intentions in the matter. The facts were too much for us; and all we can do now is to apologise for ourselves and for the gas.

Several questions may be asked, upon which I should like to say a word or two, if you will allow me to detain you a little longer. The first question (I do not know whether I need ask it) is, have we got hold of a new gas at all? I had thought that that might be passed over, but only this morning I read in a technical journal the suggestion that argon was our old friend nitrous oxide. Nitrous oxide has roughly the density of argon; but that, as far as I can see, is the only point of resemblance between them.

Well, supposing that there is a new gas, which I will not stop to discuss, because I think the spectrum alone would be enough to prove it, the next question that may be asked is, is it in the atmosphere? This matter naturally engaged our earnest attention at an early stage of the enquiry. I will only indicate in a few words the arguments which seem to us to show that the answer must be in the affirmative.

In the first place, if argon be not in the atmosphere, the original discrepancy of densities which formed the starting point of the investigation remains unexplained, and the discovery of the new gas has been made upon a false clue. Passing over that, we have the evidence from the blank experiments, in which nitrogen originally derived from chemical sources is treated either with oxygen or with magnesium, exactly as atmospheric nitrogen is treated. If we use atmospheric nitrogen we get a certain proportion of argon, about 1 per cent. If we treat chemical nitrogen in the same way we get, I will not say absolutely nothing, but a mere fraction of what we should get had atmospheric nitrogen been the subject. You may ask, why do we get any fraction at all from chemical nitrogen? It is not difficult to explain the small residue, because in the manipulation of the gases large quantities of water are used; and, as I have already explained, water dissolves argon somewhat freely. In the processes of manipulation some of the argon will come out of solution, and it remains after all the nitrogen has been consumed.

Another wholly distinct argument is founded upon the method of diffusion introduced by Graham. Graham showed that if you pass gas along porous tubes you alter the composition, if the gas is a mixture. The lighter constituents go more readily through the pores than do the heavier ones. The experiment takes this form. A number of tobacco pipes—eight in the actual arrangement—are joined together in series with india rubber junctions, and they are put in a space in which a vacuum can be made, so that the space outside the porous pipes is vacuous or approximately so. Through the pipes ordinary air is led. One end may be regarded as open to the atmosphere. The other end is connected with an aspirator so arranged that the gas collected is only some 2 per cent. of that which leaks through the porosities. The case is like that of an Australian river drying up almost to nothing in the course of its flow. Well, if we treat air in that way, collecting only the small residue which is less willing than the remainder to penetrate the porous walls, and then prepare 'nitrogen' from it by removal of oxygen and moisture, we obtain a gas heavier than atmospheric nitrogen, a result which proves that the ordinary nitrogen of the atmosphere is not a simple body, but is capable of being divided into parts by so simple an agent as the tobacco pipe.

If it be admitted that the gas is in the atmosphere, the further question arises as to its nature.

At this point I would wish to say a word of explanation. Neither in our original announcement at Oxford, nor at any time since, until the 31st of January, did we utter a word suggesting that argon was an element; and it was only after the experiments upon the specific heats that we thought that we had sufficient to go upon in order to make any such suggestion in public. I will not insist that that observation is absolutely conclusive. It is certainly strong evidence. But the subject is difficult, and one that has given rise to some difference of opinion among physicists. At any rate, this property distinguishes argon very sharply from all the ordinary gases.

One question which occurred to us at the earliest stage of the enquiry, as soon as we knew that the density was not very different from 21, was the question of whether, possibly, argon could be a more condensed form of nitrogen, denoted chemically by the symbol N_3. There seem to be several difficulties in the way of this supposition. Would such a constitution be consistent with the ratio of specific heats (1.65)? That seems extremely doubtful. Another question is, Can the density be really as high as 21, the number required on the supposition of N_3? As to this matter, Professor Ramsay has repeated his measurements of density, and he finds that he cannot get even so high as 20. To suppose that the density of argon is really 21, and that it appears to be 20 in consequence of nitrogen still mixed with it, would be to suppose a contamination with nitrogen out of all proportion to what is probable. It would mean some 14 per cent. of nitrogen, whereas it seems that from one-and-a-half to two per cent. is easily enough detected by the spectroscope. Another question that may be asked is, Would N_3 require so much cooling to condense it as argon requires?

There is one other matter on which I would like to say a word—the question as to what N_3 would be like if we had it. There seems to be a great discrepancy of opinions. Some high authorities, among whom must be included, I see, the celebrated Mendeleef, consider that N_3 would be an exceptionally stable body; but most of the chemists with whom I have consulted are of opinion that N_3 would be explosive, or, at any rate, absolutely unstable. That is a question which may be left for the future to decide. We must not attempt to put these matters too positively. The balance of evidence still seems to be against the supposition that argon is N_3, but for my part I do not wish to dogmatise.

A few weeks ago we had an eloquent lecture from Professor Rücker on the life and work of the illustrious Helmholtz. It will be known to many that during the last few months of his life Helmholtz lay prostrate in a semi-paralyzed condition, forgetful of many things, but still retaining a keen interest in science. Some little while after his death we had a letter from his widow, in which she described how interested he had been in our preliminary announcement at Oxford upon this subject, and how he desired the account of it to be read to him over again. He added the remark: "I always thought that there must be something more in the atmosphere."

LLOYD MORGAN UPON INSTINCT.

In the last number of *Natural Science* Professor C. Lloyd Morgan gives a valuable synopsis of the various definitions of instinct which have been proposed by Darwin, Wallace, Romanes, James, Spencer and other writers upon this subject. He shows that surprisingly wide differences of opinion prevail and concludes that, "Since the question of origin is still *sub judice*, the definition should be purely descriptive, so as not to prejudge this question. And since the phenomena of instinct can only be rightly understood in their relation to automatism connate and acquired, to impulse, to imitation and to intelligence, our definition of instinctive activities should find a place in a scheme of terminology." He sets forth such a scheme sending us in MSS. a number of additions and modifications which are embodied in the following table and abstract:

"It may be premised:

1. That the terms *congenital* and *acquired* are to be regarded as mutually exclusive. What is congenital is, as prior to individual experience, not acquired. What is acquired is, as the result of individual experience, not congenital.

2. That these terms apply to the individual, whether what is acquired by one individual may become congenital through inheritance in another individual, is a question of fact which is not to be settled by implications of terminology.

3. That the term *acquired* does not exclude an inherited potentiality of acquisition under the appropriate conditions, such inherited potentiality may be termed *innate*. What is acquired is a specialization of a vague and general innate potentiality.

4. That what is congenital and innate is inherent in the germ plasm of the fertilized ovum.

Congenital Movements and Activities: Those the performance of which is antecedent to individual experience; they may be performed either (a) at or very shortly after birth (*connate*) or (b) when the organism has undergone further development (*deferred*).

Congenital Automatism: The congenital physiological basis of those movements or activities which are antecedent to individual experience.

Physiological Rhythms: Congenital (or connate) rhythmic movements essential to the continuance of organic life.

Reflex Movements: Congenital, adaptive and coördinated responses of limbs or parts of the body; evoked by stimuli.

Random Movements: Congenital, more or less definite, but not specially adaptive movements of limbs or parts of the body; either centrally initiated or evoked by stimuli.

Instinctive Activities: Congenital, adaptive and coördinated activities of the organism as a whole; specific in character, but subject to variation analogous to that found in organic structures; similarly performed by all the members of the same more or less restricted group, in adaptation to special circumstances frequently recurring or essential to the continuance of the race; often periodic in development and serial in character.

Mimetic Movements and Activities: Due to individual imitation or similar movements or activities performed by others.

Impulse (Trieb): The affective or emotional condition, connate or acquired, under the influence of which a conscious organism is prompted to movement or activity, without reference to a conceived end or ideal.

Instinct: The congenital psychological impulse concerned in instinctive activities.

Control: The conscious inhibition or augmentation of movement or activity.

Intelligent Activities: Those due to individual control or guidance in the light of experience through association.

Motive: The affective or emotional condition under the influence of which a rational being is guided in the performance of deliberate acts.

Deliberate Acts: Those performed in distinct reference to a conceived end or ideal.

Habits: Organized groups of activities, stereotyped by repetition, and characteristic of a conscious organism at any particular stage of its existence.

Acquired Movements, Activities or Acts: Those the performance of which is the result of individual experience. Any modifications of congenital activities which result from experience are so far acquired.

Acquired Automatism: The individually modified physiological basis of the performance of acquired movements or activities which have been stereotyped by repetition."

Professor Morgan points out that there is some overlap in these definitions, but it is difficult to see how such overlaps are to be avoided. H. F. O.

SOME MEANDERING RIVERS OF WISCONSIN.

Two years ago Professor Davis* called attention to the wide meanders of the Osage river of Missouri. He said: "The meanders of the river are peculiar in not being like those of the Mississippi, spread upon a flat flood-plain. High spurs of the upland occupy the neck of land between every turn of the stream. Evidently the meanders are not of the ordinary kind." He explained the peculiar tortuous course of the river as an inheritance from an earlier cycle, during which the river had worn the land down to a surface of faint relief. The stream at that time swung to and fro in broad meanders developed on a wide flood-plain. The whole region was then somewhat elevated, and the stream again set to work to cut down its channel to the new baselevel. But the meandering course which it had acquired late in the preceding cycle was carried over into the new cycle of its life.

A recent visit to a part of the driftless area of Wisconsin, Lafayette and Grant counties, gave me an opportunity of observing a similar habit of some of the rivers of that region. The general surface of the country is that of a gently rolling plain, at an elevation of from 850 to 1000 feet, A. T. The interstream surfaces are broad and slightly undulating, but well drained. The surface rock, except in the immediate vicinity of the streams, is the Galena limestone. Occasionally the general level of the top of the country is broken by hills, which rise 200 to 300 feet above the general level. The highest of these are capped by the hard Niagara limestone; the lower by beds of the Cincinnati group. These hills form the so-called 'mounds,' of which, in the area visited, the Platte Mounds—1250–1300 feet, A. T.—are the highest. The hard Niagara limestone caps of these mounds are the remnants of beds which formerly stretched over all this region, and which has since been removed by denudation. To hills of this type Prof. Davis has given the name, Monadnocks.

The rocks of this region are nearly horizontal, and in general there is not a sharp contrast between the slant of the beds and the general slope of the upland surface. It seems, therefore, as if the upland might be a structural plain due to a resistant stratum, the Galena limestone, at the level of the upland—a stratum which had been revealed by denudation of the overlying beds. If this were the case, the upland level would be independent of any former baselevel. But such a conclusion does not seem to be admissible; although nearly horizontal, the limestone has been bent into gentle flexures, some of which are sufficient to bring the underlying Trenton limestone and St. Peter's sandstone up to the level of the upland surface. The plain is continuous across these low arches and bevels the edges of the gently inclined beds. Moreover, to the north of the outcrop of the Galena limestone, the upland plain bevels the gently inclined edges of the underlying formation, which there come to the surface. In that region, however, the plain is now more completely dissected than further south. Whatever correspondence exists between the inclination of the beds and the slope of the plain is fortuitous and not due to structure primarily. It is believed that this plain is a surface of denudation, the result of long continued erosion on a greater land mass when the land stood lower

*Science, April 28, 1893, vol. xxi., p. 225 et seq. Science, November 17, 1893, vol. xxii., p. 276 et seq.

than at present. The upland surface is believed to be an elevated peneplain.

It is now moderately dissected by valleys which along the larger rivers are from 100 to 200 feet deep. In comparison with the width of the gently undulating interstream surfaces these valleys are not very wide. The slopes are quite steep and locally form bluffs, but towards the top they pass by a graceful curve into the almost level upland. The present flood-plains along the bottoms of the valleys are generally from an eighth to a quarter of a mile in width. In terms of development the present valleys are well on towards maturity. The sharp narrow valleys of extreme youth are entirely absent. The rivers have made considerable progress in the present cycle in reducing the land mass to the level dependent on the grade of their channels, but the amount of work still to be done is vastly in excess of what has already been accomplished.

The three topographic features mentioned, namely, the broad undulating upland, with an elevation of from 850 to 1,000 feet; the few monadnocks rising above it, and the valleys cut into it, give a clue to the stages of geographic development of this region. The upland peneplain is a surface of denudation produced by long continued erosion, when the land mass stood lower than at present. This cycle of erosion lasted a long time and the baseleveling was almost completed. Very few monadnocks rose above the general plain. The cycle was ended by an uplift, which quickened the streams, restored to them their cutting powers, and compelled them to erode new valleys in the old peneplain. They have now cut down their channels until their ability to transport material is just about equal to the material which they have to carry. Rivers, the profiles of whose stream-channels are in this condition of equilibrium, have been called by Davis (SCIENCE, N. S., Vol. I., p. 176) graded rivers. The difference in the slope of the valley sides and the upland plain indicates a change of level before the excavation of the valleys and after the formation of the upland plain. The process by which the valleys are being formed is not a direct continuation of the process by which the gentle upland slopes were fashioned. The valleys were cut in the upland surface after it was elevated from the low position which it had during its formation.

Confirmatory evidence for this hypothesis is found in the winding courses of the valleys which now dissect this upland. Fever river was studied in the field, and the topographical atlas of the Wisconsin Geological Survey shows that the Platte, Little Platte, Grant and Pecatonica rivers have this same habit. If the geographical development of this region was as outlined above, the streams at the close of the earlier cycle must have possessed wide, flat valleys, with broad flood-plains, in which they meandered freely. The elevation of the land would have caused the streams to degrade their channels rapidly. In many cases the meanders on the flood-plain would have been superimposed upon the rock below, as the river bed was lowered. The valleys cut in the elevated peneplain would thus come to preserve, and, as pointed out by Winslow, also increase the meanders of the earlier cycle.

Such seems to have been the case with the Fever river. Its meanders have an average radius of a little less than half a mile, but they are by no means constant. Rock spurs of the upland project into each curve. The slopes on these spurs are generally gentler than on the outside of the curves, where the stream is often undercutting the base of the slope and increasing the meanders. Both open and close oxbows occur. The most marked of the close type of meanders was noted near Benton, where the river makes an almost closed sigmoid curve, the halves of which are from one-half to three-fourths of a mile in diameter. The rock neck of land between the two ends of the closer curve is less than a hundred yards in width and rises about seventy feet above the stream.

Along Platte, Little Platte, Grant and Pecatonica rivers, larger streams than Fever river, the meanders are slightly larger on the average than along the smaller streams. Both open and close curves occur. Rock salients between 100 and 200 feet high project into the bow of each meander. Almost as complete a series of meander types can be found among the curves of the rock valleys of these rivers as along the broad flood-plains of other streams. Indeed, the small meanders of these rivers in their present flood-plains can readily be duplicated by the wider curves of the rock valley. There can be no reasonable doubt but that the meanders of these valleys are an inheritance from meanders developed on broad flood-plains in a previous cycle of erosion. So far as could be made out, these meanders are not due to difference in hardness or structure of the rocks of the region. The limestone does not present sufficiently marked differences of structure to account for these curves upon a theory of readjustment of courses due to the contrasts between hard and soft beds. Whatever differences exist are not distinctly such as to modify the courses of rivers, particularly in a manner such as to resemble so closely flood-plain meanders. Nor does it seem to be admissible to suppose that these curves are the perpetuation of meandering courses taken when the land first emerged from the sea bottom. Such a supposition presupposes too constant and stable a relationship, through an enormous lapse of time between all the forces which control erosion and determine the position of streams.

The sinuosities of these meanders may have been somewhat changed since the elevation of the peneplain. In places the increased velocity may have straightened the curves to some extent. In other instances the meanders have been somewhat increased. Such seems to have been the case near Benton, where the stream is now undercutting the narrow strip of land separating two parts of the curve. If this process continues, a cut-off will result.

In comparison with the Osage river, these streams are small and their meanders insignificant. But apart from size, the analogy between them is complete. They must be added to the growing list of streams known to be persisting in habits acquired under conditions which have long since disappeared.

Henry B. Kümmel.
The University of Chicago.

CORRESPONDENCE.

MISSOURI BOTANICAL GARDEN.

The attention of botanists is called to the facilities afforded for research at the Missouri Botanical Garden. In establishing and endowing the Garden, its founder, Henry Shaw, desired not only to afford the general public pleasure, and information concerning decorative plants and their best use, and to provide for beginners the means of obtaining good training in botany and horticulture, but also to provide facilities for advanced research in botany and cognate sciences. For this purpose, additions are being made constantly to the number of species cultivated in the grounds and plant houses, and to the library and herbarium, and, as rapidly as it can be utilized, it is proposed to secure apparatus for work in vegetable physiology, etc., the policy being to secure a good general equipment in all lines of pure and applied botany, and to make this equipment as complete as possible for any special subject on which original work is undertaken by competent students.

A very large number of species, both

native and exotic, and of horticulturists' varieties, are cultivated in the Garden and Arboretum and the adjoining park, and the native flora easily accessible from St. Louis is large and varied. The herbarium, which includes nearly 250,000 specimens, is fairly representative of the vegetable life of Europe and the United States, and also contains a great many specimens from less accessible regions. It is especially rich in material illustrative of Cuscuta, Quercus, Coniferae, Vitis, Juncus, Agave, Yucca, Sagittaria, Epilobium, Rumex, Rhamnaceæ and other groups monographed by the late Dr. Engelmann or by attachés of the Garden. The herbarium is supplemented by a large collection of woods, including veneer transparencies and slides for the microscope. The library, containing about 8,000 volumes and 10,000 pamphlets, includes most of the standard periodicals and proceedings of learned bodies, a good collection of morphological and physiological works, nearly 500 carefully selected botanical volumes published before the period of Linnæus, an unusually large number of monographs of groups of cryptogams and flowering plants, and the entire manuscript notes and sketches representing the painstaking work of Engelmann.

The great variety of living plants represented in the Garden, and the large herbarium, including the collections of Bernhardi and Engelmann, render the Garden facilities exceptionally good for research in systematic botany, in which direction the library also is especially strong. The living collections and library likewise afford unusual opportunity for morphological, anatomical and physiological studies, while the plant house facilities for experimental work are steadily increasing. The E. Lewis Sturtevant Prelinnean library, in connection with the opportunity afforded for the cultivation of vegetables and other useful plants, is favorable also for the study of cultivated plants and the modifications they have undergone.

These facilities are freely placed at the disposal of professors of botany and other persons competent to carry on research work of value in botany or horticulture, subject only to such simple restrictions as are necessary to protect the property of the Garden from injury or loss. Persons who wish to make use of them are invited to correspond with the undersigned, outlining with as much detail as possible the work they desire to do at the Garden, and giving timely notice so that provision may be made for the study of special subjects. Those who have not published the results of original work are requested to state their preparation for the investigation they propose to undertake.

Under the rules of Washington University, persons entitled to candidacy in that institution for the Master's or Doctor's degree may elect botanical research work as a principal study for such degrees, if they can devote the requisite time to resident study.

WILLIAM TRELEASE,
Director.

ST. LOUIS, MO.

SCIENTIFIC LITERATURE.

THE GEOLOGY OF THE SIERRA NEVADA.

Geologic Atlas of the United States. U. S. Geological Survey; J. W. POWELL, Director. *Sacramento Folio*, Geology by W. LINDGREN. *Placerville Folio*, Geology by W. LINDGREN and H. W. TURNER. *Jackson Folio*, Geology by H. W. TURNER. Washington, D. C. 1894.

These three sheets are the first installment of a series covering the gold belt of California which has been in course of preparation for several years by the officers of the Geological Survey. It is needless to say that they form a very important and welcome contribution to our knowledge of the geology of California. Since the collapse of the old State Survey under Whitney, but little effort has been made by California to elucidate her economic geology, notwithstanding the liberal appropriations which the State Legislature makes regularly for the maintenance of a so-called 'Mining Bureau.' In the knowledge of her geologic resources, California is far behind many minor States of the Union. It is therefore fortunate that the Federal authorities have so steadily prosecuted the inquiry into the geology of the gold belt of the Sierra Nevada and of other portions of northern California. The sheets under review are the results of this work. They form part of the geologic atlas of the United States and they are among the first dozen of the entire series. The mechanical execution of the folios challenges the admiration of all familiar with such work. In the opinion of the writer they compare very advantageously with the best European efforts of a similar kind. It is gratifying to American pride to see the beginnings of so vast a scientific project as a geologic atlas of the United States realized in a manner so eminently satisfactory. If there exists a doubt in the minds of the geologists of the country, and in this case the geologists speak for the people, as to the ultimate success of the project, it is based on the fear that there may not be in the future, as there certainly has not been in the past, a proper coördination of the topographic and the geologic branches of the survey. A correct topographic base is the *sine qua non* of a good geologic map; and unfortunately the topographer's conception of a correct map, in the present state of his professional education, is not what it ought to be. Thorough and conscientiously executed topographic surveys are expected of the geological survey. The ambitious extension of the topographic surveys *far* in advance of geologic investigation, at a rate which not only absolutely precludes the possibility of thorough work but demoralizes the topographer, can only bring serious discomfiture to the Geological Survey as a government institution.

The Sacramento, Placerville and Jackson folios bring out clearly the salient features of a section which may be taken as typical for the western slope of the Sierra Nevada. The Sierra slope rises from the eastern edge of the Great Valley of California to the crest of the range, some 60 miles distant at an inclination of less than 2°. It presents the characters of a gently tilted plain which has been incisively dissected by the streams which traverse it. This slope is underlain by two very different assemblages of rocks. The first of these is composed of sedimentary and eruptive formations which have been intensely disturbed, metamorphosed and invaded by vast intrusions of granitic magma, forming a complex whose eroded surface serves as the basement upon which the second assemblage reposes in little disturbed attitudes. The older assemblage is designated in the folios the 'Bed-rock' series, and the newer, the 'Superjacent' series. Neither of these terms is felicitous, although the first is based on popular usage and will appeal to the mining community. The Bed-rock series comprises the rocks which are known popularly as the *auriferous slates*, together with their associated eruptives and irruptives, and also the granitic rocks which invaded the series as a whole at the close of the Jurassic. It would be better if these granitic intrusions were not classed in the same category with the auriferous slates as part of a 'series.' The auriferous slates comprise the *Calaveras* formation (Carboniferous, with possibly some older Paleozoic) and the *Mariposa* formation. In the earlier Sacramento and Placerville folios, which are chiefly Lindgren's work, the Mariposa formation is colored as Cretaceous, while in the later Jackson folio by Turner the same formation is colored as Jurassic. The reference of this formation to two different horizons can scarcely be taken as indicative of decided difference of opinion between these two geologists, but rather of a rapid change of opinion on the part of the officers of the survey in consequence of the recent paleontological determinations of Hyatt, whose results were probably not available at the time the earlier folios went to press. The Mariposa formation is of economic importance as that in which occurs the zone of auriferous veining which constitutes the famous 'Mother Lode.'

In a field so overburdened with igneous rocks, contemporaneous and intrusive, geologists will readily understand that many problems arise which are not easily answered by the most earnest efforts of the field geologist. The lack of definite statements as to the structural relations of the various sedimentary and igneous formations indicates that these relations are obscure and difficult to determine. Still, a brief statement from Messrs. Turner and Lindgren as to the interpretation of their structural sections would have been a desirable addition to the letter press, which is limited strictly to historical, petrographic and economic geology. For example: Are the two belts of the Mariposa slates on the Jackson sheet essentially synclinal troughs with an anticline bringing up a belt of the lower Calaveras between them? If so, the structure is comparatively simple, and the great body of amphibolite schist, diabase and porphyrite probably represents volcanic accumulations chiefly intermediate in age between the Calaveras and the Mariposa, but perhaps passing up into the latter. Or is the region traversed by a great system of longitudinal faults? A discussion of these and other tectonic questions we may doubtless expect in more detailed reports upon the geology of the region. But something of the tectonic should find a place in the folios to help out the sections. While alluding to the igneous rocks it may be well to mention that the user of the geological map is handicapped by not having the effusive rocks discriminated from the intrusive on the color scale. From the text it is apparent that many of the igneous rocks are clearly intrusive, while others are effusive. This discrimination should be expressed graphically, as it is impossible to understand the structure without keeping it in mind. The doubtful rocks should be grouped apart from those which are clearly effusive or intrusive. An extra convention or two to express doubt or ignorance on particular points would greatly enhance the scientific value of our geological maps.

One of the most important features of the Sierra Nevada slope is the invasion of the Calaveras and Mariposa formations by the Sierra Nevada batholite. The relations of the older rocks to this invading magma are beautifully brought out by the careful mapping of Messrs. Turner and Lindgren. Petrographically, the rocks of this batholite are chiefly of a type intermediate between granite and diorite, and are therefore designated as *granodiorite*. Other important facies of the same magma are granite, gabbro and gabbro-diorite. These rocks appear as great intrusive areas in the midst of the auriferous slates and establish pronounced zones of contact metamorphism in the latter. Putting the three geologic sheets together, and bearing in mind the distribution of these same granitic rocks to the eastward and southeastward of the area mapped, it is difficult to resist the suggestion that these rocks underlie practically the whole of the Sierra slope beneath the rocks through which they project as isolated masses. In other words, the mapping suggests strongly that if the plane of truncation effected by erosion had been lower a much larger proportion of granite would have been exposed, and if higher less. If this suggestion be accepted it follows that the Calaveras and Mariposa formations must have reposed upon the granodiorite magma as a crust, up into which the magma advanced, not only by displacement, but absorption. For we have no trace apparently of the original basement upon which the Calaveras formation was deposited. In these relations of batholite to disturbed and metamorphic crustal rocks we have a striking analogy with the relations which obtain between the Laurentian granites and the metamorphic rocks of the Ontarian system in the Lake Superior region. The amphibolites and other schists of 'auriferous slates' are petrographically the same as many of the schists of the Ontarian system.

The invasion of the Jurassic and earlier rocks by the Sierra Nevada batholite seems to have been accompanied, or perhaps preceded, by uplift and the development of mountain structure. During early Cretaceous time these mountains were profoundly eroded, for on the edge of the valley of California we find the Chico Cretaceous, the earliest of the 'Superjacent' series, reposing upon the worn surface of the granodiorite. The Chico is followed by the Ione and later Tertiary formations. In part contemporaneously with the Ione, but chiefly at a later period, there were spread over portions of the region important sheets of gravel. Associated with these are flows of rhyolite and andesite. The rhyolite flows serve as a means of separating the 'older' from the 'later' gravels. The andesitic flows were contemporaneous chiefly with the first of the later gravels. These gravels constitute the once famous placers of California. Since they were spread over the Sierra slope, the latter has been tilted so as to accentuate the grade and intensify the downward corrasion of the streams. As a consequence of this corrasion, we now find only remnants of the gravels and volcanic flows reposing on the tops of nearly flat ridges between the river gorges.

Andrew C. Lawson.

University of California.

On the [*Harvest Mice*] *Species of the Genus Reithrodontomys.* By J. A. Allen. 8° May 21, 1895. From Bull. American Museum of Natural History, New York (pp. 107–143).

Dr. Allen has just published a much needed revision of the Harvest Mice—a group of small mammals differing from other murine rodents in having the upper incisors deeply grooved. Since Dr. Allen's study is based on upwards of 900 specimens (two-thirds of which belong to the rich collection of the U. S. Department of Agriculture) it is probable that future researches will add little to the results here published, so far as the United States forms are concerned. The name of the common species of the Carolinas is changed from *humilis* to *lecontei.* Fifteen species and subspecies are recognized, 12 of which inhabit the southern and western parts of the United States. Seven of the United States forms are accorded full specific rank. One of these, *R. montanus* of Baird, is known from the type specimen only, which was collected in Colorado more than 40 years ago and is in very poor condition. When additional specimens are obtained from the type locality it will probably displace one of the other species. Another, *R. arizonensis* from the Chiricahua Mountains, is separated from *R. longicauda* of California, chiefly on geographic grounds. In the case of one of the subspecies admitted—*R. longicaudus pallidus*—it is not likely that Dr. Allen will be followed by other mammalogists. Respecting this form he says: "I find myself greatly embarrassed as to which of three courses to pursue in the matter, namely: (1) To refer *R. pallidus* to *R. longicauda* as a pure synonym of the latter; (2) to treat *R. pallidus* as one of several local phases of *R. longicauda;* (3) to let the name stand in a subspecific sense for a generally dispersed paler southern form of *R. longicauda*, as opposed to true *longicauda* of the region from about Monterey and Merced counties northward. Through lack of material for properly working out the problem I have provisionally adopted the latter course."

Since he has 175 specimens that he regarded as typical *longicauda*, and 157 that he referred to subspecies *pallidus*, or 332 in all, and since these 332 specimens came from no less than 70 localities scattered over the single State of California, it is a little difficult to understand what he meant by 'lack of material for properly working out the problem.' Furthermore, an examination of the localities assigned to the two alleged forms shows them to be hopelessly mixed—both being recorded from the San Joaquin Valley, and both from the coast region north of Monterey !

One of the largest and most highly colored members of the group is a new form from Louisiana, collected by the field naturalists of the Department of Agriculture. It is a northern representative of *R. mexicanus* and is named, from its color, *R. mexicanus aurantius.*

The paper as a whole is a critical and painstaking study of an obscure group. It is based in the main on ample material and is particularly welcome as adding another genus to those recently revised by American mammalogists. C. H. M.

NOTES AND NEWS.

THE REMEDY FOR PEAR BLIGHT.

THE writer desires to announce that a satisfactory method of preventing pear blight has been discovered. After prolonged investigation the complete life history of the microbe (*Bacillus Amylovorous*) has been worked out. Most of the cases of blight either come to a definite termination in summer or else kill the tree. When this is the case the blight dies out completely, there being no source of supply for the germs the following spring. In certain cases where it is a sort of even battle between the host and parasite, or where late infections in the fall have not run their course before cold weather comes on, the blight keeps alive in the tree. When root pressure increases in the spring, such cases start into activity and serve as sources of infection for the new growth. The removal of these sources of infection is the preventive remedy for pear blight. The work is best performed in autumn after all late growth has ceased, but while the foliage is still on the trees. At this season the dead leaves which persist on the blighted branches serve admirably to attract attention to the points of danger. The work can be done at any time during the winter up to the time of the beginning of growth in spring. Cutting out the blight in summer is unsatisfactory on account of the continued appearance of new infections. The matter will be published in full in a bulletin from the Division of Vegetable Pathology.

M. B. WAITE,

DEPARTMENT OF AGRICULTURE.

THE NEW YORK BOTANIC GARDEN.

THE sum of $250,000 for the New York Botanic Garden has now been subscribed as follows :

J. P. Morgan	$25,000
Columbia College	25,000
Andrew Carnegie	25,000
C. Vanderbilt	25,000
J. D. Rockefeller	25,000
D. O. Mills	25,000
Judge A. Brown	25,000
Wm. E. Dodge	10,000
Jas. A. Scrymser	10,000
Wm. C. Schermerhorn	10,000
Ex-Judge C. P. Daly	5,000
O. Ottendorfer	5,000
Samuel Sloan	5,000
George J. Gould	5,000
Miss H. M. Gould	5,000
John S. Kennedy	5,000
Wm. Rockefeller	5,000
Jas. M. Constable	5,000

Morris K. Jesup	$2,500
Mrs. M. P. Dodge	1,000
Tiffany and Co.	1,000
Hugh N. Camp	500

The act of incorporation required that this amount be collected for an endowment. The city must now raise $500,000 by bonds for building purposes, and provide 250 acres of land in Bronx Park.

THE HELMHOLTZ MEMORIAL.

IN addition to the subscriptions to the Helmholtz Memorial acknowledged in the issue of SCIENCE of May 31, the sum of $97 has been collected by Prof. Rood from officers of Columbia College and forwarded to the committee.

Ogden N. Rood,	$10
William Hallock,	5
H. Cushman,	5
R. Gordon,	3
H. C. Parker,	5
H. S. Curtis,	2
Asa S. Iglehart,	3
C. C. Trowbridge,	1
H. T. Wade,	1
J. H. Van Amringe,	10
F. R. Hutton,	5
F. B. Crocker,	5
J. K. Rees,	2
C. F. Chandler,	10
H. C. Bowen,	3
J. W. Burgess,	5
R. Mayo-Smith,	10
Wm. R. Ware,	5
Thomas Price,	2
H. T. Peck,	2
Livingston Farrand,	1
N. M. Butler,	1
James H. Hyslop,	1
	$97

GENERAL.

THE thirty-fourth annual meeting of the National Educational Association of the United States will be held at Denver, Colorado, July 9th to 12th, 1895. The meeting promises to be the most important in the history of the Association. Among the large number of attractive addresses announced on the program are the address of the president, Professor Nicholas Murray Butler, on 'What Knowledge is of Most Worth,' and an address by Professor Joseph Le Conte on 'Effect of the Doctrine of Evolution upon Educational Theory and Practice.'

MR. ARCHIBALD, president of the trustees of Syracuse University, has offered to be one of six men to build a hall of science costing about $150,000. The University has also been offered $10,000 and $100,000 towards a new medical college.

THE University of Chicago announces that an *American Journal of Sociology* will be be issued bi-monthly from its press.

THERE are eleven candidates for the degree of Ph. D. at the University of Chicago —in Sociology and Geology each two, and in Philosophy, Greek, Latin, English History, Semetic and Chemistry each one.

MRS. L. P. BABBOTT, of Brooklyn, has endowed a fellowship for post-graduate study at Vassar College.

DURING the coming year lectures on experimental psychology will be given by Dr. Scripture to the entire Junior Class, 300 members, of Yale College. Fifty undergraduates have elected special courses in the laboratory.

COLORADO COLLEGE will hold the fourth annual session of its summer school of science, philosophy and languages from July 15th to August 16th. Among the lecturers from other universities are Prof. Bessey, of Kansas; Prof. Lounsbury, of Yale, and Prof. James, of Harvard.

PART of the collection of birds given to the Museum of Comparative Zoölogy of Harvard University by Mr. W. E. D. Scott was exhibited on June 18th. About 350 of the 3,200 birds have been mounted in 56 cases. Each case contains two or more birds of the same species, mounted in such a way that the character and ordinary habits and surroundings of the species are suggested without making the accessories of more apparent importance than the birds themselves.

THE death is announced of Dr. Eliseyeff, known for his explorations in Asia and Africa.

A PRIZE of $100 has been offered by a friend of Johns Hopkins University for the best essay by a student of the University upon the application of chemistry to the useful arts.

THE Ethical Seminary for graduates in Harvard University will be conducted by Professor G. T. Ladd, of Yale University, in the absence of Professor Palmer during the coming year.

ADDITIONAL courses of lectures will be given at Johns Hopkins University during the next academic year by Mr. G. K. Gilbert and Mr. Bailey Willis on geology, and by Dr. Frederick M. Warren, of Adelbert College on botany. The following appointments have also been made: Abraham Cohen, instructor in mathematics; Dr. Jacob H. Hollander, instructor in economics; Dr. Harry C. Jones, instructor in physical chemistry; Charles P. Singerfoos, an assistant in zoölogy and embryology.

DR. JOHN P. LOTZY has presented his herbarium of five thousand sheets to the Women's College of Baltimore.

THE death is announced of Heinrich Geisburg, an authority on Westphalian history and archæology, in his seventy-seventh year.

DR. THEOPHILUS A. WYLIE, Professor Emeritus in Indiana University, died recently at the age of eighty-five. He accepted the chair of natural philosophy and chemistry in Indiana University in 1837, in 1852 became professor of mathematics in Miami University, but returned to his former position after three years. He was transferred to the chair of languages in 1864, and withdrew from active work in 1886.

THE presidency of the Columbian University of Washington has been offered to the Rev. B. L. Whitman, President of Colby University in Maine.

PROFESSOR ALEXANDER GRAHAM BELL has presented the Volta Bureau Library of Georgetown with the Scientific Library of the late Joseph Henry of the Smithsonian Institution, numbering 1,500 volumes.

AT Harvard University Mr. G. A. Dorsey has been appointed instructor in anthropology, Mr. V. A. Wright instructor in descriptive geometry and stereotomy, and Dr. Alfred Schafer demonstrator of histology and embryology.

PROF. VALENTINE BALL, Director of the Museum of Science and Art of Dublin, died on June 17th, at the age of 52 years. He was elected a fellow of the Geological Society of London in 1874, fellow of the Royal Society in 1882, president of the Royal Geological Society of Ireland in 1882, and was professor of geology and mineralogy in the University of Dublin from 1881 to 1883. He was the author of works on the geology of India, and accounts of explorations in Afghanistan, Beloochistan, the Himalayas, etc.

JOHNS HOPKINS UNIVERSITY has received two gifts in memory of Prof. George H. Williams. His friends have given an oil portrait of Mr. Williams, and Mrs. Williams a sum of money sufficient to establish a lectureship in geology. Sir Archibald Geikie, Director of the Geological Survey of Ireland, has been invited to be first lecturer.

J. J. HOGAN, mechanic and electrician in the Yale Psychological Laboratory, has invented a practicable device whereby the high voltage city current is rendered readily available for low voltage instruments such as telegraph instruments, telephones, electric forks, bells, induction coils, etc. The General Electric Company has acquired patent rights. The details of the instrument will be made public as soon as the foreign patents are issued.

Dr. H. W. Williams, a distinguished opthalmological surgeon of Boston and author of several works on diseases of the eye, died at Boston on June 13th at the age of seventy-three years.

Prof. Michael Foster has now prepared an abridgement of his classical text-book of physiology, which in the sixth edition of five volumes had reached a size too large for the needs of the medical student. The abridged edition is published by Macmillan & Co. in an octave volume of about 1000 pages.

Mr. Erwin F. Smith, of the Agricultural Department, has become one of the associate editors of *The American Naturalist*, taking charge of the department of vegetable physiology.

Macmillan & Co. announce the third edition of *Graduate Courses*, edited by C. A. Duniway, Harvard Graduate Club, assisted by graduate students representing twenty leading American universities. The work gives the advanced courses of instruction to be offered for 1895–6 in Barnard, Brown, Bryn Mawr, California, Chicago, Clark, Columbia, Cornell, Harvard, Johns Hopkins, Michigan, Minnesota, Pennsylvania, Princeton, Radcliffe, Stanford, Vanderbilt, Western Reserve, Wisconsin and Yale. Much valuable information is included regarding the conditions of advanced work at these universities.

At the commencement of the University of Pennsylvania a bronze bust of the late Professor Joseph Leidy was presented by Dr. Harrison Allen.

Sir Archibald Geikie has been elected a corresponding member of the Vienna Academy of Sciences.

Professor Simon Newcomb was elected on June 16th an associate academician of the Académie des Sciences to fill the vacancy caused by the death of von Helmholtz.

Mrs. Cornelia Phillips Spencer has received the degree of LL. D. from North Carolina State University.

At the summer meeting of the University Extension Society of Philadelphia, July 1–26, Courses in literature and history, psychology, music, biology, mathematics, civics and politics will be offered. The courses in science are as follows:

Psychology of the Normal Mind, by William Romaine Newbold, Ph. D., Penna.; Physiological Psychology of Adult and Child, by Lightner Witmer, Ph. D., Penna.; Hypnotic and Kindred Abnormal States of Mind, by Willian Romaine Newbold, Ph. D.; Anatomy and Physiology of the Nervous System, by Lightner Witmer, Ph, D.; Experimental Methods of Child Study, by Lightner Witmer, Ph. D.; Botany, by W. P. Wilson, Sc. D., Penna.; Systematic Botany, by J. M. Macfarlane, Sc. D., Penna.; Vertebrate Zoölogy, by Edward D. Cope, Ph. D., Penna.; Invertebrate Zoölogy, by J. S. Kingsley, S. D., Tufts; The Lower Plants, by Byron D. Halsted, Sc. D., Rutgers; Biology in Elementary Schools, by L. L. W. Wilson, Philadelphia Normal School; How Garden Varieties Originate, by L. H. Bailey, M. S., Cornell; Relation of Certain Plants to Political Economy, by George L. Goodale, LL. D., Harvard; The New Evolution, by Charles O. Whitman, Ph. D., Chicago; Higher Mathematics, Algebra, Modern Geometry, Etc., by Isaac J. Schwatt, Ph. D., Penna.

The first number of a series of *Princeton Contributions to Psychology* has been issued from the University press, edited by J. Mark Baldwin and containing two articles reprinted from the Psychological Review: I. General Introduction—Psychology, past and present, by the editor; and II. Freedom and Psycho-genesis, by A. T. Ormond.

THE *Programme of the Department of Geology* of the University of Chicago for 1895–96 bears witness to the great strength of the department. Thirty-one courses are offered by the following officers of the department: Thomas C. Chamberlin, Head Professor of Geology; Rollin D. Salisbury, Professor of Geographic Geology; Joseph P. Iddings, Professor of Petrology; Richard A. F. Penrose, Jr., Professor of Economic Geology; William H. Holmes, Professor of Archæologic and Graphic Geology; Charles R. Van Hise, Non-resident Professor of Pre-Cambrian Geology; Oliver Cummings Farrington, Instructor in Determinative Mineralogy; Edmund C. Quereau, Tutor in Palæontologic Geology.

SOCIETIES AND ACADEMIES.

BIOLOGICAL SOCIETY OF WASHINGTON.

AT the meeting of June 1st Dr. C. Hart Merriam presented a paper on the Short-tailed Shrews of North America, stating that an examination of many specimens showed that the described species were only four, *Blarina brevicauda*, *B. carolinensis*, *B. parva* and *B. Berlandieri*. He discussed these and their distribution at some length, saying that each species was characteristic of one of the zoölogical divisions of North America.

Dr. G. Brown Goode made some remarks on the Location and Record of Natural Phenomena by a Method of Reference to Geographical Coördinates.

Dr. Gill presented a communication on The Relations of the Ancient and Modern *Ceratodontidæ*.

He commented on the unusual degree of interest connected with the Ceratodontids. The statement has been frequently made that *Ceratodus* is the oldest living generic type of fishes, and the identity of the living fishes so-called with the mesozoic species has been especially insisted on. The speaker, however, had denied such generic identity as early as 1878 on account of the difference in the form and plication of the dental plates, and had revived for the recent genus the name *Neoceratodus* given in mistake by Castelnau to a specimen of the genus. A new name, *Epiceratodus*, has recently been given by Teller to the same genus and must be abandoned. But Teller has given us useful data respecting the cranial characters of the mesozoic species, and we now have information sufficient at least to offer hints as to the relations of the ancient and modern forms. We can affirm positively that the recent Ceratodontids are very different from the mesozoic species; that consequently they should bear the name *Neoceratodus*, unless a still earlier one is applicable, and further that the differences between the living and long extinct species are enough to ever differentiate the two as distinct sub-families, the *Ceratodontinæ* including only extinct species and the *Neoceratodontinæ* being a recent type. The distinguishing characters of the two were given at length and derived from the dermal bones, the modification of the posterior region of the head, and the protrusion of the jaws. The ancient forms themselves belong to at least two genera: *Ceratodus*, typified by *C. Kaupii*, and *Anticeratodus*, typified by *C. Sturii*, of Teller. The latter is distinguished by the contiguity of the two palatine plates and their extended inner walls.

Professor Lester F. Ward exhibited specimens of the rhizomes of the Gama Grass, *Tripsacum dactyloides*, obtained at Great Falls, Md., on April 27th, which bore a striking resemblance to fossil forms described under the name of *Caulinites*, Brongn., and especially to *C. parisiensis*, Brongn., from the Eocene of the Paris Basin. He exhibited figures of that species to show this resemblance.

The genus *Caulinites* was first figured by Desmarest, who supposed it to be a polyp and named it *Amphitoites parisiensis* in Nov. Bull. de Sci., Société Philomathique, tom. II., pl. 2. This figure was reproduced by Cuvier and Alex. Brongniart in Essai sur la Geographie Minéralogique des Environs de Paris, pl. II., figs. 10 A. and 10 B., 1811, and has been repeated in all later editions. A large number of very fine specimens were collected subsequently, and Adolphe Brongniart had no doubt but that it represented the impression of a plant. In his 'Tableau,' 1849, p. 86, he placed it under a plant genus which he renamed *Caulinites*, from the genus *Caulinia*, of de Candolle, a name antedated by *Posidonia*, Kön., an aquatic plant related to the river-weeds, *Potamogeton*, and sea wracks, *Zostera*, in the *Naiadaceæ*. When Watelet, in 1866, undertook the elaboration of all the material in the Paris Museum from the Eocene of the Paris Basin he devoted several plates to illustrating this and other species of the same genus.

Prof. Ward stated that when he saw the rhizomes he was forcibly struck with their resemblance to the figures of Desmarest and Watelet. A comparison of them showed that in many respects they were not only similar but practically identical, although among Watelet's figures are some which deviate considerably from this type. A large number of similar forms have been found in various deposits, chiefly Tertiary, throughout the world, and more than 50 species of *Caulinites* have been named, many of which will, of course, prove to be synonyms, while others depart so widely from the normal type that they will require to be excluded.

Prof. Ward said further that in 1887, Prof. Lesquereux described a species collected by Mr. Geo. F. Becker at Clear Lake, Cal., under the name of *C. Beckeri*. Proc. U. S. Nat. Mus. Vol. X., p. 36, pl. I, fig. 3, pl. II, figs. 1–4. Mr. Becker stated that he had supposed these rhizomes to belong to the common Tule, *Phragmites phragmites*, (L.) Karst., the deposit being a very recent one in the bed of a dried-up pond where the Tule was supposed to have grown as it now grows in those regions.

Prof. Ward remarked in conclusion that he had found other, similar, rhizomes washed up along the Potomac, but was unable to say to what plant they belonged, but enough is now known to make it certain that a considerable number of grasses, and perhaps rushes and other monocotyledonous plants, possess rhizomes with short joints resembling or practically identical with those of the genus *Caulinites*.

The Society then adjourned until October.

F. A. Lucas, *Secretary*.

ENTOMOLOGICAL SOCIETY OF WASHINGTON.

The 109th meeting was held June 6. Mr. Wm. H. Ashmead read a paper on the discovery of Elasmosoma Ruthe in America. This remarkable monotypical Microgasterine genus, the type species of which (*E. berolinense*) was collected in Europe many years ago in company with an ant, is supposed to be parasitic upon ants. Mr. Ashmead has found three species in America, one collected at Washington in 1889 by E. A. Schwarz; one at Fort Collins, Col., by C. F. Baker, and the third near Washington by Th. Pergande. The last species was found flying about the nest of *Camponotus melleus*, and the genus may be parasitic either upon ants or upon myrmecophilous beetles.

A paper by F. M. Webster entitled 'Notes on the Distribution of some Injurious Insects,' was read by the corresponding secretary. In this paper Prof. Webster criticised some of the details brought out by Mr. Howard in his paper on the geographical distribution within the United States of certain insects injuring cultivated crops (Proc. Entom. Soc. Wash. III., No. 4), particularly in regard to the spread of injurious species into Ohio and their distribution in that State.

Mr. H. G. Hubbard exhibited specimens of the borings of Xyleborus and Platypus, Scolytid beetles, in orange wood. He described the habits of these beetles and showed that Platypus is capable of making extensive galleries of its own in hardwood trees. The nature of the food of these timber beetles was discussed. In addition to reviewing and confirming the observations of European writers, Mr. Hubbard described the so-called Ambrosia which nourishes the young, as welling up through the pores of the wood which are cut by the galleries, in the shape of minute white buttons, giving a tesselated appearance to the walls of the passages. The substance sometimes accumulates in the galleries, and when puddled by the larvæ resembles half-melted snow or slush. A growth of fungus forms upon the Ambrosia, and closing the mouth of the galleries causes them to fill up and suffocate the inmates. This method of treatment was found useful in Florida, to save from further injury the budded portion of trees killed back by the severe frost of February last. A piece of wire was pushed into the burrows as far as it would go and then cut off and left there.

As to the nature of Ambrosia, Mr. Hubbard made the conjecture that it is a ferment set up in the sap of the tree and augmented by the presence of the animals.

Mr. O. Heidemann exhibited specimens of *Coriscus flavomarginatus*, a brachypterus Nabid new to North America, which was collected at St. John's, New Brunswick, by the late Dr. Marx. Mr. Howard exhibited a female Scolia sent from Texas by Mr. E. A. Schwarz, and which had become, in some manner, impaled upon a sharp thorn, the thorn entering the middle of the face. It was a question whether the insect became so impaled by flying violently against the sharp point of the thorn, or whether it had been stuck there by a shrike. Mr. Frank Benton exhibited a comb of *Apis florea* which he had collected in Ceylon. This is the smallest species of Apis known. Curiously enough, the only two species of Apis which build in the open air, namely, *Apis florea* and *A. dorsata*, are the smallest and the largest species of the genus.

L. O. HOWARD,
Recording Secretary.

NEW YORK ACADEMY OF SCIENCE.

AT the meeting on May 27th Prof. Cattell described *Bodily and Mental Tests made on members of the Freshman Class of Columbia College* by him in conjunction with Dr. Farrand. About twenty-five observations and measurements were made on students entering college in 1894, and these will be repeated at the middle and end of the course. In describing the experiments especial attention was given to those of a more purely psychological nature, such as memory, accuracy of perception, sensitiveness to pain, reaction-time, rate of perception, imagery, etc., and some of the experiments were made on those present. Such experiments are of value to the individual student, as they give him information concerning his bodily and mental condition, and the effect of his college course upon these ; they are also of use in increasing our exact knowledge of mental processes and their relation to bodily conditions.

Professor Rees exhibited a *Geodetic Theodolite* made by Wanschaff, of Berlin, for use in the Summer Class of Practical Geodesy at Columbia College. The telescope was 19½ inches in focal length with 2½ inch objective. The horizontal circle was 8 inches in diameter and was read to single seconds of arc by two micrometer-microscopes. The graduations on the circle were microscopic and were seen easily in the reading microscopes. The telescope was provided with a small vertical circle 6¼ inches in diameter and reading by verniers to single minutes. The instrument was arranged for observations on Polaris for azimuth work.

J. F. Kemp, *Secretary*.

THE WISCONSIN ACADEMY OF SCIENCES, ARTS AND LETTERS.

The Wisconsin Academy of Sciences, Arts and Letters held its Summer meeting, on June 6th to 8th, 1895, at Milwaukee, Wis., under the auspices of the Natural History Society of Wisconsin, and the Presidency of Professor Charles R. Van Hise. In addition an address by President C. K. Adams and a number of other historical and sociological papers, the following were presented:

Address of Welcome: George W. Peckham, President of the Natural History Society of Wisconsin.

Opening address, 'Reforms in Germany after the Napoleonic Wars: C. K. Adams, President of the University of Wisconsin.

The relation of pooling to some phases of the transportation question: A. M. Simons.

The legal aspects of trusts: Edgar F. Strong. Read by title.

The forms spontaneously assumed by folk-songs: J. Comfort Fillmore.

Negro suffrage in Wisconsin: J. G. Gregory.

Some Observations on the Lateral Moraines at Devil's Lake: D. P. Nicholson.

Geology of Mts. Adam and Eve, Orange County, N. Y.; G. L. Collie.

Certain Uses of Topographical Maps: G. L. Collie.

The Production of Electrical Energy Directly From Carbon: A. J. Rogers.

A Contribution to the Mineralogy of Wisconsin: William H. Hobbs.

Some New Occurrences of Minerals in Michigan and Montana; William H. Hobbs.

On a Diamond from Kohlsville, Wisconsin: William H. Hobbs.

From Pinene to Carvacrol: Edward Kremers.

A Dredge for Collecting Crustacea at Different Depths: C. Dwight Marsh.

Method of Determining the Coefficient of a Plankton Net: E. A. Birge.

The Pelagic Crustacea of Lake Mendota During the Winter and Spring of 1894–1895: E. A. Birge.

The Biological History of Daphnia Hyalina, Leydig: E. A. Birge.

The Periodic System as a Didatic Basis: Edward Kremers. Read by title.

Observed and Computed Precession: D. P. Blackstone. Read by title.

THE TEXAS ACADEMY OF SCIENCE.

The Law of Hypnotism: Prof. R. S. Hyer.

County Roads: Charles Corner, C. E.

On the Glycerine Method of Preserving Specimens for the Anatomical Museum: Dr. Wm. Keiller, F. R. C. S.

Texas Soils; a Preliminary Statement and Classification: E. T. Dumble.

Simultaneous Quadratic Equations: I. H. Bryant.

NEW BOOKS.

Geological Survey of Michigan. Lucius L. Hubbard, State Geologist. Vol. v. 181, 1893. pp. x+179. xxiv+100.

The Theory of Light. Thomas Preston. 2nd Edition. London and New York, Macmillan & Co. 1895. Pp. xvii+566. $5.00.

A Monograph of the Order of Oligochæta. Frank Evers Beddard. Oxford. Clarendon Press, New York, Macmillan & Co. 1895. Pp. xii+769. $12.50.

Report of the International Meteorological Congress Held at Chicago, Ill. Part II. Edited by Oliver L. Fassig. Washington, Weather Bureau. 1895. Pp. xvi+583.