SCIENCE PROGRESS
IN THE TWENTIETH CENTURY
A QUARTERLY JOURNAL OF SCIENTIFIC WORK & THOUGHT

EDITOR

SIR RONALD ROSS, K.C.B., F.R.S., N.L., D.Sc., LL.D., M.D., F.R.C.S.

VOL. IX

1914—1915

LONDON

JOHN MURRAY, ALBEMARLE STREET, W.

1915

PRINTED BY
HAZELL, WATSON AND VINEY, LD.,
LONDON AND AYLESBURY,
ENGLAND.

INDEX TO VOL. IX (1914—1915)

I. ARTICLES AND NOTES

II. AUTHORS OF ARTICLES

III. AUTHORS OF BOOKS REVIEWED

SCIENCE PROGRESS

IRRATIONALISM

One of the first things which strike the man of science when he studies natural objects of the same class, especially living objects, is the great variation which exists amongst them. Nature abhors not only the vacuum but the straight line; and if we arrange objects according to any single measurable quality which they possess, we generally find that they group themselves according to certain well-known laws: the majority of them are nearly but not quite equal in respect to the given quality, but, at one extremity of the curve, a few of the objects are very deficient in it, and, at the other extremity, a few of them greatly excel the rest. The study of such arrangements has now become a new and valuable branch of science; and we know exactly what to expect when we discuss, let us say, the tallness or the weight of men, or the frequency of blue eyes, or of certain deformities, and so on. We have scarcely yet attempted to apply the same analysis to certain high mental qualities, such as capacity for reasoning; but to judge from analogy even these lofty possessions of man are likely to be dominated by precisely the same law.

In an inarticulate manner, indeed, the world generally does recognise the principle—men are said to be of average intelligence, or of high intelligence, or are even called fools. But the principle is frequently disregarded in affairs of great public importance. Thus in philosophical discussions appeals are sometimes made to the opinion of the majority of mankind, which is thought to over-rule the opinions of exceptional individuals; and also in politics, especially in Britain, the suffrage is given regardless of intellectual ability, and the verdict of the nation is cited as being sufficient to overwhelm that of any individuals, however trained they may be in the art of reasoning, or however learned they may be in the details of the measure under consideration. Yet a study of nature will suggest to the man of science that such conclusions are not always justifiable.

We are very apt to wander here into the thickets of dogma. Men have become accustomed to consider themselves to be all equally heirs of heaven. They like to maintain that their faculty of reasoning is a part of the spirit which they all possess; and from this datum they assume that they can all reason equally well. But if we analyse the strands of which reasoning is woven we shall begin to doubt such a doctrine. We perceive that all the other faculties of the mind are subject to great variation in different individuals. Take the musical faculty, for example: most men and women are able to enjoy the pleasures of melody and harmony; but some have "no ear for music," while others are so eminently gifted with it that they become Mozarts and Wagners. The same holds with regard to the appreciation of poetry, painting, sculpture, and architecture. Science itself is a remarkable case in point, because, while some persons can scarcely endure even to think of a scientific problem, and others become Newtons and Kelvins, the mass of mankind can do no more than comprehend with difficulty what they are taught as to the great laws discovered by people more gifted than themselves. Similar variations are to be seen as regards the lower faculties of the mind—the hand-and-eye faculty which gives us success in sports, the dexterity necessary in many arts, the readiness of speech required in Parliament and the law courts, and even the cleverness which certainly so often leads to success in life. Yet we seem to think that we can all reason, if we choose, with equal exactitude, and are very much hurt if others doubt our capacity in this respect.

The extreme degrees of irrationalism constitute insanity; and lower degrees are found in a large proportion of persons who cannot be called insane, but are recognised as being stupid—slow in apprehension and inaccurate in judgment. But irrationalism covers more than insanity and stupidity. It is frequently found in men and women of very quick apprehension and of very good judgment in many affairs—especially in those which concern their every-day life. They are often agreeable, good, well-instructed, accomplished, successful, and even capable or distinguished; and in their own business or profession may excel better reasoners than themselves. But their mind fails at once when it is applied to any proposition outside the class of propositions with which they have been accustomed to deal. Their capacity for reasoning has been trained to a certain point by their education and by the necessity of making a livelihood—but not to the further point where it becomes as secure a guide as possible in all matters. When called upon to judge in affairs which concern themselves they are thoroughly capable; but they lose balance at once when they let go the rock of their own interests. In a moment they become either inflexibles or cranks—they stand stock-still or fall over. Such persons are certainly not insane, nor may they even be called stupid. Their mental vision is clear enough for a short distance from the soul-centre, but becomes out of focus for a longer one.

The defect really springs from that well-known weak spot in the intellectual machinery—want of apprehension of the fact that we must really not generalise upon too small a sample; that our little garden-plot of experience is, really, not the whole world. Starting from this point—judging classes from individuals, leaping from one single observation to another—we end by becoming generally unable to distinguish probability from certainty, and finally land in any quagmire of dogma which we may happen to reach; and there we stick. But of itself, this is little worse than that which often happens, from hurry or misfortune, to the best of minds. True irrationalism depends upon graver faults—first, the intellectual hebetude which will not trouble to study more than one or two samples; and secondly, the curious pride which strives to cover such laziness by the pretence that no further study is necessary. Arrived at this, the man becomes an incurable; for in thought, as in life, pride is the end of effort and the proof of its own falsity.

Take, for example, the case of scientific experiments on animals. The careful and honest reasoner would not dream of studying this particular case in isolation from the whole class of cases in which trouble or pain is caused by the stronger to the weaker. He sees that we are not angels in this world, but are dominated by the laws of Nature. We cannot live without jostling, nor move without treading on corns. Every man's and every animal's existence means some deprivation to others. If we warm our hands at a fire, we do so at the expense of the gloomy subterranean labours of those who have obtained the coal for us. Our fur coats are torn from the bleeding backs of poor creatures of the arctic by our fellow-men, who risk their lives for us in the work. The building of our houses, the laying of our water-pipes and drains, and the daily supply of our food, depend upon forced labour—forced by natural laws—of thousands of others, perhaps less happy than ourselves. Our journeys are made luxurious by those who swink at roaring furnaces, or toil all day in a thousand factories; and our banquets are won at the expense of untold miseries to other living creatures. Our simple daily food implies an enormous butcher's bill, which can be roughly computed if we remember that each of the thousand millions of human beings in the world destroys so many lives a week. Nor can the meekest vegetarian escape in this matter, because he slays populations of beautiful little creatures in a mouthful of lettuce or celery! Even if he lives merely on cereals and vegetables, still the crops must be protected by the slaughter of innumerable small birds and beasts; and by what right does he snatch the milk from the cow or her egg from the hen? By what right do we go forth, gun on shoulder, to shoot creatures for the mere amusement of our idle hours; or, rod in hand, to drag fish by means of cruel hooks from the water? Who has given us a charter to flog weary horses in order to save us the labour of our own overfed bodies; or to whip dogs in order to train them to perform on their hind legs; or to keep wild animals and small birds in cages? If it comes to that, by what right do we slay the tiger, which follows his own nature in taking toll of our flocks; or the murderer who attacks us; or the innocent germs which live in our own blood? Still further; if all these things are to be forbidden, how shall we deal with our humble relations of the animal kingdom, who themselves do to others just as we do to them? Yet, in face of this immense complex of fact, comes the irrationalist, who, ignoring all the unnecessary cruelties of the world, tries to argue that science may not do a few experiments in order to lessen suffering due to disease!

Of course, the fact that Nature is "red in tooth and claw" does not debar us from efforts to mitigate the sufferings of animals; but those efforts must be logically directed towards reducing unnecessary pain. Even the killing of animals for food may partly be brought into this category, since we all eat a great deal too much meat. But of all the sufferings of animals caused by men, those only are ethically justifiable which are caused, not for the sake of gluttony, sloth, fashion, or amusement, but for the high object of reducing suffering among men and other animals—that is to say, the very scientific experiments which the irrationalist attacks!

His argument is therefore of no consequence, but it is of interest to inquire how he reaches it. He does so merely by considering the single sample. His mind is fixed on the experiments alone, and all the innumerable other species and instances of contact between men and animals become blurred in his myopic vision. The only thing which he does see occupies the whole of his mind, which has no room for more than one idea at once. He becomes confused, and his judgment fails him—like a hare's in the blaze of a motor-lamp. Indeed, so feeble does his judgment become that, though he professes abhorrence of all acts which cause pain, he himself generally continues to eat meat, wear furs and feathers, and even enjoy sport; and we once heard a sportsman loudly condemning vivisectors at a moment when a number of antelope and birds, just shot by him for mere amusement, were lying dead in his verandah. Yet he was quite a sane man—who indeed held a high post obtained by competitive examination!

But this kind of person rarely stops at the mere innocent irrationalism of inadvertence. He generally possesses enough intelligence to feel the weight of the arguments which, later in life, are brought against him; but then his pride forbids him to yield, and he has recourse to the invention of falsities to support his credit as a reasoner. Thus the anti-vivisectionist invents the utter untruths that experiments on animals have served no useful purpose, and even that those who perform them do so in order to gratify a supposed lust of cruelty. At this stage he is past praying for; he is no longer a reasoner, but merely one who endeavours to escape conviction by the fabrication of evidence, and nothing that others can say will ever persuade him to retract a single one of his absurdities.

Irrationalism is, generally, the enemy of humanity. In the form of crankism it clings shrieking to the hands of science just when she is engaged upon her most difficult but beneficent labours, and, in the form of political party, it paralyses the efforts of the wisest legislators. It brings wars by encouraging race-antagonism, and it lowers philosophy by false ideals. It will never cease because it is due to incomplete mental development; but the best way to reduce it is to give young minds that education which allows them the widest purview—not the education which forces them to burrow for ever in the dark pits of a single knowledge, but that which leads them to look out early from the summit of things upon the whole universe. In that way only shall they learn how to avoid the life of the frog in the well, and rather to view, like eagles, the true width of the world in which they live.

In the meantime we should clearly understand that irrationalism is due to a natural defect in the mind, amounting sometimes almost to insanity. It is a defect of the reason comparable to that defect of the vision which we call colour-blindness; but while colour-blindness is admitted by those who suffer from it, because it does not affect their reasoning powers (as, for example, in the distinguished case of Dalton), those who suffer from reason-blindness are unable, from the very deformity which afflicts them, to recognise their deficiency. They therefore pursue their fad at all costs, whatever mischief they may inflict by their efforts upon humanity or upon individuals. And we see innumerable examples of this in our present state of civilisation—anti-vaccination, anti-vivisection, militant suffragism, anarchism, and nihilism are some of them. It is a difficult question to know what to do with these forms of semi-insanity. There is one way in which the press could help towards disarming them—simply by placing their propagandisms on the same level as personalities, indecencies, and libels, and by refusing to publish them. We think, however, that the time has come when a more organised campaign should be conducted against them by bringing certain forms of them within the action of well-considered laws.

THE TEMPERATURE OF MARS

By PHILIP H. LING, M.Sc., Brist., B.Sc., Lond.

It is only comparatively recently that it has been found possible to examine, with any approach to scientific method, the question of the habitability of the planets. The ingenious theories of Prof. Lowell have drawn attention to the possibilities presented by Mars, and though the problem is still far from being settled, considerable advance has been made.

In discussing the question we have, of course, to limit our inquiries entirely to life as we know it. Thus the planets Jupiter, Saturn, Uranus, and Neptune appear to be in a semi-molten state, which quite precludes any possibility of their being inhabited. In fact, Mars and Venus seem to be the only planets which are not ruled out by some unfavourable physical condition, though the suggestion has been made that the satellites of Jupiter may receive sufficient heat from their primary to render some of them habitable.

Now the problem can be definitely solved only by the appearance of some phenomenon which can be due to no conceivable cause other than living intelligent beings. This is what Prof. Lowell claims to have discovered in the case of Mars. But while in view of the extreme diversity of opinion concerning them in the astronomical world his theories cannot be considered decisive, they may be supported or opposed by a different type of argument. Such is supplied by an examination of the physical conditions on the surface of Mars. Unfortunately there is, even here, very considerable difference of opinion. It is clear that the non-existence of oxygen in the Martian atmosphere, if proved, would settle the matter at once; and the same would occur if the temperature were not somewhere in the neighbourhood of that prevailing on the earth.

But the question of temperature is even more intimately connected with the subject. Prof. Lowell's "canal" theory depends essentially on the idea that water is conveyed by artificial means from the polar regions. The theory is therefore quite untenable, unless the maximum temperature on Mars is well above the melting-point of ice. There are two outstanding determinations of the mean temperature, and the great difficulty is that they appear to be absolutely contradictory. The first is due to the late Prof. Poynting,[1] who, from a discussion of the general properties of solar radiation, finds the mean temperature of Mars to be $-38°$ C.; the second is that of Prof. Lowell[2] himself, and leads to the value $+8°$ C. The discrepancy, though less than 50° C., affects the whole question, for if Prof. Poynting's value is correct, it is quite certain that on Mars ice will never melt.

It is easy enough, under certain assumptions, to calculate the mean temperature of Mars from that of the earth. Assuming the two planets to be similar in their behaviour towards solar radiation, and ignoring the central heat, we have that the energy received from the sun is inversely proportional to the square of the distance, while the energy given out is, by Stefan's Law, directly proportional to the fourth power of the absolute temperature. But since the temperature does not vary much, the energy received must balance that radiated out, so that the fourth power of the absolute temperature is inversely proportional to the square of the distance from the sun; or, in other words, the absolute temperature is inversely proportional to the square root of the distance. Taking the ratio of the distances as 1·5237, and the mean temperature of the earth as 15° C., we get that of Mars as $-39°$ C., practically that obtained by Prof. Poynting.

Now we have here made the assumption that Mars and the earth are similar in their behaviour towards solar radiation. This is what Prof. Lowell denies.

The solar radiation on a planet may be either reflected or absorbed. The reflected radiation plays no part in raising the temperature. "Strange to say," remarks Prof. Lowell,[3] this important fact had never been taken into account till the present investigation of the subject, which led to a completely different outcome from what had previously been supposed."

[1] "Radiation in the Solar System: its Effect on Temperature and its Pressure on Small Bodies," *Phil. Trans.* A, **202** (1903), p. 525.

[2] *Mars as the Abode of Life* (New York, 1909), pp. 240 *et seq.*

[3] *Ibid.* p. 83.

I do not think that this statement is quite just to Prof. Poynting, in whose paper, as we shall see, the idea certainly occurs. It is in the relative importance which they attach to it that the two investigators differ so completely.

Prof. Poynting's paper not only deals with the temperatures of different planets compared with that of the earth; it also gives a method by which the mean temperature of the earth may be obtained absolutely. Strictly speaking, his results apply only to an ideal planet, for which certain assumptions are rigorously true. Most of these assumptions present no difficulty as a basis for an approximate result. But with regard to the absorbing power of the planet, Prof. Poynting assumes that the reflection at each point is one-tenth of the radiation received, justifying the assumption by the following remarks: "This is probably of the order of the actual reflection from the earth. According to Langley the moon reflects about one-eighth of the radiation received. The earth certainly reflects less. The temperatures determined hereafter are proportional to the fourth root of the coefficient of absorption. Even if this coefficient is as low as 0·9, its fourth root is 0·974. Hence if the actual value is anywhere between 0·9 and 1, the assumed value of 0·9 will not make an error of more than 2½ per cent. in the value of the temperature."

But is it between 0·9 and 1? The albedo (or fraction of incident light reflected) varies enormously for different planets. For Venus it is 0·92 and for Mars 0·27. In these cases the proportion of incident light absorbed is therefore only 8 per cent. for Venus, and actually 73 per cent. for Mars. It is of course quite true that this applies only to the visible part of the spectrum, and that there is a much greater proportion of absorption in the infra-red; but on the face of it it seems very unlikely that in the case of Venus the absorption is anywhere near the value 90 per cent.

Unfortunately the value of the earth's albedo is highly uncertain. The latest value, that of Prof. Very,[1] obtained from the "earth-shine" on the moon, is 0·89—*i.e.* nearly that of Venus; but there are enormous difficulties in the determination. Prof. Lowell's value, estimated from the reflecting powers of air, cloud, and the earth's surface, is 0·75. This has to be

[1] *Astronomische Nachrichten*, 4696.

largely modified for the invisible rays, and the ratio of the absorbing coefficients comes out:

$$\frac{\text{Earth}}{\text{Mars}} = \frac{60}{99}$$

These figures give a value for the mean temperature of Mars equal to 22° C.; this, however, has to be reduced owing to difference in the rate of loss of heat, and the final result is 8° C.

The question now arises: if Prof. Poynting's value of the absorption coefficient is incorrect, how is it that his theory gives an accurate value for the earth's mean temperature? Prof. Poynting's theory gives the result:

$$\theta = 0{\cdot}93\theta_E = 0{\cdot}93 \times (\epsilon S/\pi\sigma)^{\frac{1}{4}}$$

where θ and θ_E are respectively the average and equatorial temperatures of the earth (in absolute Centigrade degrees), ϵ is the coefficient of absorption, S is the solar constant (in ergs per sq. cm. per sec.), and σ is the radiation constant (*i.e.* the constant of Stefan's Law). Now Prof. Poynting gets three different values for θ, owing to uncertainty in the value of S. These are 52° C, 29° C., and 17° C., corresponding to the values of S obtained by Ångström, Langley, and Rosetti respectively. Expressed in ergs per square centimetre per second, these values of S are respectively $0{\cdot}28 \times 10^7$, $0{\cdot}21 \times 10^7$, and $0{\cdot}175 \times 10^7$. Prof. Poynting takes the third value of the temperature as being that most in accordance with the fact, under the assumption that ϵ is 0·9. But if we put $\epsilon = 0{\cdot}6$, we get the values of θ as 22° C, 1° C., and − 11° C., and from this it is clear that it is merely a question of choosing the solar constant suitably. But now a serious difficulty arises.

Since Prof. Poynting's paper appeared, a long series of determinations of the solar constant has been carried out by Prof. Abbot. Of a number of values obtained in 1902–4, and given in the *Encyclopædia Britannica*,[1] the mean is 2·12 cal. per sq. cm. per min., or $0{\cdot}148 \times 10^7$ ergs per sq. cm. per sec. A later value given in a paper read before the American Philosophical Society[2] in 1911 is 1·93 cal. per sq. cm. per min. The latest of all[3] is 1·933 cal. per sq. cm. per min., or 0·135 ergs per

[1] Art. "Meteorology."

[2] See *Nature*, lxxxvi. p. 534 (June 15, 1911).

[3] See *Nature*, xciii. p. 198 (April 23, 1914).

sq. cm. per sec. It is true that the value is not quite constant (appearing to indicate that the sun is a variable star), but this cannot have any great effect on the planetary temperatures. If we substitute this value in Prof. Poynting's expression, then, giving ϵ its maximum possible value of unity, we shall not get θ higher than 6° C., and since ϵ must be considerably less than 1, the theoretical temperature must be still less. There is a certain discrepancy here which still awaits explanation.

This, however, will not influence the temperature of Mars, for S will not appear in the ratio of the temperatures of the two planets. If the absorbing powers of the two planets are sufficiently different—and the difference between the albedoes of Mars and Venus seems to indicate the possibility—the temperature of Mars need not vary much from that on the earth.

The difference in the absorbing powers seems to be due, as Prof. Lowell remarks, to a lack of clouds in the Martian atmosphere. We know that clouds reflect 72 per cent. of the incident light, and their permanent absence will cause the greater absorption. The nights, however, owing to the rarity of the atmosphere, may be very cold.

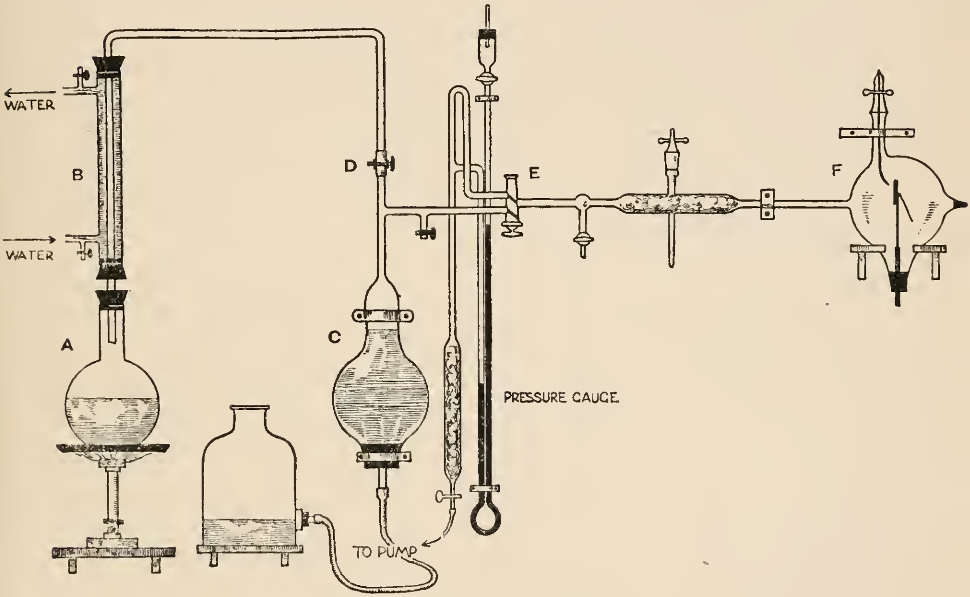
THE TERRESTRIAL DISTRIBUTION OF RADIUM

By ARTHUR HOLMES, B.Sc., A.R.C.S., F.G.S.

Imperial College, London

Discussing the origin and duration of the sun's heat in 1862, Lord Kelvin concluded that "the inhabitants of the earth cannot continue to enjoy the light and heat essential to their life for many million years longer," but he prudently added, "unless sources now unknown to us are prepared in the great storehouses of creation." To the geologist the extreme importance of the radioactive elements lies in their fulfilment of Kelvin's cautious qualification. Atomic disintegration is, in all known cases, accompanied by a spontaneous evolution of energy which ultimately appears in the form of heat. This unexpected discovery was first announced in 1903 by Curie and Laborde, who showed that radium was capable of maintaining a temperature slightly above that of its immediate environment. Already Elster and Geitel had begun to investigate the radioactivity of the atmosphere. It was found that the air in caverns and cellars was unusually high in its content of active matter, and this led naturally to the view that the latter had escaped as radium emanation from the soil. Impressed with the significance of these observations, implying as they did a widespread distribution of radioactive matter in the surface materials of the earth, Professor (now Sir Ernest) Rutherford suggested in 1905 that the heat constantly evolved in virtue of the disintegration of the earth's supply of radium might be sufficient to maintain the observed temperature gradient. On the assumption that 100 calories per hour represented the heat output of one gram of radium, he calculated that if each gram of the substance of the earth contained $4{\cdot}6 \times 10^{-14}$ grams of radium, the heat so produced would be equivalent to that brought to the earth's surface by conduction and lost by radiation into space.

In the following year Rutherford's suggestion was quantitatively put to the test by Professor Strutt, who devised a method for determining minute quantities of radium, and applied



Apparatus for the estimation of Radium.

The rock is brought into solution by fusion, treatment with water (alkaline solution) and treatment of the resione with acid (acid solution). Each solution is stored up for a few weeks in a closed flask until the equilibrium amount of emanation has accumulated. To estimate the radium in one of the solutions the flask A containing it is attached to the water condenser B. Emanation is expelled by vigorous boiling, the steam condensing in B and falling back into A. At the end of an hour the cooling water is run out of B and the steam then drives the emanation into C, after which the connection at D is closed. Meanwhile the electroscope F has been exhausted and the air of the gasholder C, charged with emanation, is passed into the electroscope through the tap at E. A measurement of the rate of fall of the leaf then suffices to determine the amount of radium emanation present.

it successfully to a large number of representative rocks.[1] The apparatus employed is figured in the adjoining illustration, to

[1] See R. J. Strutt, *Proc. Roy. Soc.*, A., vol. lxxvii. p. 475, 1906, and A. Holmes, *The Age of the Earth*, London, 1913, p. 105.

which a brief account of the mode of precedure is appended. The results obtained were very surprising in the light of Rutherford's calculation. The average of twenty-eight igneous rocks gave $1{\cdot}6 \times 10^{-12}$ grams of radium per gram of rock, about thirty-five times as much as Rutherford had demanded for thermal equilibrium of the earth.

Strutt's figures have been confirmed by several other observers, and rocks from every continent have now been examined. Prof. Joly, in his later measurements, employed a method of extracting the radium emanation which avoids the labour of preparing clear solutions of rock material. A mixture of finely powdered rock and fusion mixture is heated in an electric furnace, and the emanation is driven off with the expelled gases. Carbon dioxide is absorbed in a soda-lime tube, and the remaining gases, containing the emanation, are finally passed into the electroscope, after which the rate of fall of the leaf is measured just as in the solution method.[1] Joly's fusion method gives consistently higher results than those obtained by the solution method, but the reason for the discrepancy is not altogether clear. There is no doubt that suspended particles in the solution may bring about a partial occlusion of the emanation, which would prevent a complete expulsion of the latter by boiling, and would therefore lead to a low determination.[2]

With careful chemical treatment, however, this source of error may be avoided, and Mache has recently developed a method of extracting emanation by aspirating air through the solution, which renews our confidence in the accuracy of the method devised by Strutt.[3] Mache has standardised his apparatus directly with the Hönigschmid radium standard, and he finds that his results conform with surprising closeness to those obtained from the same material when treated according to Strutt's method of boiling out the emanation.

All the results obtained for igneous rocks up to the present are summarised and analysed in the adjoining table.

[1] Joly, *Phil. Mag.* vol. xix. p. 695, 1912.

[2] Eve and McIntosh, *Phil. Mag.* vol. xiv. p. 237, 1907; *Trans. Roy. Soc. Canada*, series iii. p. 67, 1910; Joly, *Phil. Mag.* vol. xix. p. 695, 1912.

[3] Mache (in the press, 1914).

RADIUM PER GRAM OF IGNEOUS ROCKS IN BILLIONTHS (10^{-12}) OF A GRAM

The number of rocks included in the averages is given in the first of each pair of columns

Observer.	Acid.		Intermediate.		Basic.		Ultrabasic.	
Strutt[1]	11	2·59	4	2·25	9	0·52	4	0·46
Eve and McIntosh[2]	—	—	3*	2·80	—	—	—	—
Farr and Florance[3]	3	1·83	4	1·68	6	0·54	—	—
Schlundt and Moore[4]	7	1·95	—	—	—	—	—	—
Buchner[5]	8	2·61	15	1·64	4	0·73	—	—
Fletcher[6]	4	0·85	20	0·85	5	0·71	—	—
Holmes[7]	8	2·80	24*†	2·45	4	0·85	10†	0·51
Mean	41	2·51	70	1·74	28	0·66	14	0·50
Joly[8] †	86	3·01	48	2·57	31	1·28	—	—

* Alkaline rocks. † Composite analyses.

Prof. Joly's results may also be expressed in the following form, which brings out the connection between the mode of occurrence of igneous rocks and their radium contents. As before, the latter are stated in billionths (10^{-12}) of a gram per gram of rock.

Type of Rock.	Acid.		Intermediate.		Basic.	
Volcanic, Hypabyssal	23	3·9				
Volcanic			18	3·0	43	1·4
Hypabyssal			10	2·8	8	1·0
Plutonic	63	2·7	20	2·1	5	1·3

My own composite analyses were made by applying the solution method to the three following sets of well-known rocks, and the results obtained are given here because they appear to fill important gaps in the data hitherto published.

[1] Strutt, *Proc. Roy. Soc.*, A., vol. lxxvii. p. 472, 1906.

[2] Eve and McIntosh, *Trans. Roy. Soc. Canada*, series iii. p. 69, 1910.

[3] Farr and Florance, *Phil. Mag.* vol. xviii. p. 812, November, 1909.

[4] Schlundt and Moore, *Bull. U.S.G.S.* 395, 1909.

[5] Buchner, *Proc. Kon. Akad. v. Weten*, Amsterdam, vol. xiii. p. 359, 1910; vol. xiii. p. 818, 1911; vol. xiii. p. 1063, 1912.

[6] Fletcher, *Phil. Mag.* vol. xx. p. 36, July 1910; vol. xxi. p. 102, January 1911; vol. xxi. p. 770, June 1911.

[7] Holmes, *The Age of the Earth*, London, 1913, pp. 130 and 182.

[8] Joly, *Phil. Mag.* vol. xxiv. p. 694, October 1912.

I. Volcanic and Hypabyssal Alkaline Rocks

1. Phonolite, Sokoto Hill, Mozambique.
2. Phonolite, Sanhuti River, Mozambique.
3. Solvsbergite, Sanhuti River, Mozambique.
4. Phonolite, Athi Plains, British E. Africa.
5. Phonolite, Black Hills, Wyoming, U.S.A.
6. Phonolite, Montreal, Canada.
7. Phonolite, Bohemia.
8. Phonolite, Wolf Rock, Cornwall.
9. Phonolite, Fernando Nironha Is., Brazil.
10. Leucitophyre, Reiden, Eifel.
11. Paisanite, Ailsa Craig, Firth of Clyde.
12. Tinguaite, Serra de Tingua, Brazil.
13. Solvsbergite, Gran, Hadeland, Sweden.

Average radium content of 13 rocks = $2{\cdot}94 \times 10^{-12}$ grams per gram.

II. Plutonic Alkaline Rocks

1. Foyaite, Serra de Monchique, Portugal.
2. Ditroite, Ditro, Transylvania, Hungary.
3. Laurdalite, Laurdal, Norway.
4. Laurvikite, Laurvik, Norway.
5. Miaskite, Miask, Urals, Siberia.
6. Pulaskite, Fourche Mts., Arkansas, U.S.A.
7. Leucite Syenite, Serra de Caldus, Brazil.
8. Elæolite Syenite, Serra de Tingua, Brazil.
9. Nepheline Syenite, Renfrew Co., Ontario, Canada.
10. Elæolite Syenite, Langesund Fjord, Norway.
11. Elæolite Syenite, Magnet Cove, Arkansas, U.S.A.

Average radium content of 11 rocks = $1{\cdot}87 \times 10^{-12}$ grams per gram.

III. Plutonic Ultrabasic Rocks

1. Serpentine, Shetland Is.
2. Serpentine, Ballantrae, N.B.
3. Hornblende Peridotite, Portsoy, Banff.
4. Enstalite Peridotite, Assynt, Sutherland.
5. Serpentine, Knockdhu, Ayrshire.
6. Serpentine, Rhode Is., U.S.A.
7. Dunite, Lake Superior, Canada.
8. Dunite, Dun Mt., New Zealand.
9. Kimberlite, Transvaal.
10. Peridotite, East Griqualand.

Average radium content of 10 rocks = $0{\cdot}51 \times 10^{-12}$ grams per gram.

As yet the data are rather too scanty to justify more than a few wide generalisations. Strutt's results indicated that on the average the acid rocks were richer in radium than those of more basic composition. Later determinations did not at first altogether support this view, chiefly because of the remarkably high values obtained by Joly. However, Joly's recent work has replaced his earlier determinations by results more in accordance with those of other analysts. Taking the averages of the results obtained by the solution method, it will now be seen, that with one exception, they indicate an unbroken proportionality between the average amount of radium and the average amount of silica in igneous rocks. The one exception is afforded by the three determinations of Eve and McIntosh, which are as follows:

Canada	Tinguaite	$4{\cdot}3 \times 10^{-12}$ grams per gram.
	Tinguaite	$3{\cdot}0 \times 10^{-12}$ " " "
	Nepheline Syenite . .	$1{\cdot}1 \times 10^{-12}$ " " "
	Average	$2{\cdot}8 \times 10^{-12}$ " " "

A similar exception is illustrated by three radium analyses of alkaline rocks made by the present writer:

Mozambique	Phonolite	$2{\cdot}83 \times 10^{-12}$ grams per gram.
	Phonolite	$4{\cdot}10 \times 10^{-12}$ " " "
	Solvsbergite	$2{\cdot}26 \times 10^{-12}$ " " "

and by the first of the following averages for alkaline rocks of intermediate composition:

13 Volcanic (and Hypabyssal) Rocks . .	$2{\cdot}94 \times 10^{-12}$ grams per gram.
11 Plutonic Rocks	$1{\cdot}87 \times 10^{-12}$ " " "
Average	$2{\cdot}45 \times 10^{-12}$ " " "

These results lead on to a further generalisation: that alkaline rocks tend to be richer in radium than normal or calc-alkaline rocks of similar acidity. Indeed, the extent to which alkalies are present, and in particular, the extent to which sodium is present, would appear to be the predominating factor in determining the quantity of radium in a rock magma. The association of radium with alkalies is more fundamental than its association with silica, for it is the combination of silica with a high proportion of alkali that favours the relative abundance of radium.

Joly's results, which bring out very clearly the sympathetic relation between the radium and silica contents of a rock, also indicate the probability that volcanic, and, to a less extent, hypabyssal rocks, are on the average more richly charged with radium than their plutonic equivalents. My own determinations,

2

and those of some other workers, point also to the same significant conclusion. In the case of basic rocks, however, the applicability of this generalisation is doubtful, and more analyses are required before it can be extended to that class of rocks. It is interesting to observe, in this connection, that volcanic rocks contain more soda and more silica than the corresponding plutonic rocks. With regard to potash, the volcanic rocks are, if anything, poorer rather than richer, but the differences are in this case variable and alternating. The following table is compiled from the average analyses of rock types collected by R. Daly[1] and A. N. Winchell.[2] The distinction to which attention is drawn appears to hold too consistently to be assignable to the particular choice of material; rather is it an expression of a real difference due to differentiation.

Comparison of Volcanic and Plutonic Rock Types

No. of Analyses.	Types of Rocks.	Average percentages of Silica, SiO_2.	Average percentages of Soda, Na_2O.
64	Rhyolite . . .	72·60	3·54
236	Granite . . .	69·92	3·28
13	Alkali Rhyolite . .	75·45	5·88
10	Alkali Granite . .	72·70	5·42
48	Trachyte . . .	60·68	4·43
13	Syenite . . .	58·06	3·67
17	Alkali Trachyte . .	62·46	6·30
23	Alkali Syenite . .	61·99	5·54
25	Phonolite . . .	57·45	8·84
43	Nepheline Syenite .	54·63	8·26
30	Dacite	66·91	4·13
20	Tonalite . . .	59·47	2·98
16	Rhyo-dacite . .	67·67	4·10
12	Grano-diorite . .	55·10	3·82
57	Andesite . . .	61·30	3·99
70	Diorite . . .	56·77	3·39
18	Latite	57·93	4·19
10	Monzonite . . .	55·30	3·73
198	Basalt . . .	49·06	3·11
41	Gabbro . . .	48·25	2·55

[1] R. Daly, *Proc. Am. Ac. Arts. and Sci.*, vol. xlv. p. 211, 1910. See also *Igneous Rocks and their Origin*, pp. 13–39, New York, 1914.

[2] A. N. Winchell, *Journ. Geol.*, vol. xxi. p. 208, 1913.

Summing up, we may conclude that in general radium shows a marked preference for alkaline and acid rocks, and to a less extent for volcanic as compared with plutonic rocks. That is to say, the processes of differentiation which are responsible for the production of rocks rich in alkalies (particularly soda) and silica, and incidentally for the difference in composition between volcanic and corresponding plutonic rocks, are also responsible for the relative concentration of uranium, and therefore of radium. Although the evidence in the case of thorium is less abundant than that for radium, it tends to show that similar statements are equally true of that element. This is indicated very forcibly by the well-known association of uranium-and-thorium-bearing minerals with pegmatites, which, in turn, are genetically related to granites and syenites of an alkaline character. Occurrences in Norway, Finland, Greenland, Connecticut, and Ceylon sufficiently illustrate the statement.

The problem now arises whether there is any common factor in the conditions governing the formation of pegmatites and alkaline rocks, which is also capable of extracting uranium and concentrating it. The writer believes that such a common factor may be found in the so-called "mineralising agents." Among the minerals which are unable to crystallise except under the influence of magmatic gases and vapours (*e.g.* water, chlorine, fluorine, boron, sulphur oxides, etc.), are quartz, albite, orthoclase, sodalite, haüyne, amphiboles, micas, tourmaline, topaz, zircon, sphene, beryl, and cassiterite. It is significant that these are among the most characteristic minerals of pegmatites and alkaline rocks, and of acid rocks in general. Further, we may observe, that among the "mineralising agents" are just those gases which would combine with uranium and thorium to form volatile and mobile compounds. It is therefore suggested that, in a normal magma, selective differentiation proceeds in such a way that the radioactive parent elements are concentrated in those subsidiary portions of the magma which ultimately give rise to pegmatites or to alkaline rocks, the process of differentiation being largely controlled by magmatic gases and vapours. For the same reason, the gaseous emanations of active volcanoes may be the agents originally responsible for the relatively higher percentages of soda and silica carried by the lavas from which they escape.

How far similar principles may help to explain the origin of the granites which constitute so large a part of the outermost layers of the earth's crust, it is as yet impossible to say. If, however, the hypothesis is true that the atmosphere, the oceans, and the carbon dioxide represented by carbonaceous deposits and limestones were all originally "juvenile" magmatic gases, then it seems justifiable to assume that their escape did not take place without profoundly affecting the nature and distribution of the magmas which they helped to carry upwards and through which they passed. It may be that the evolution of the earth's crust and its peculiar chemical composition is to be correlated with that of the oceans and atmosphere, the latter representing part of the "mineralising agents" which helped to enrich the outermost, lighter, and more mobile magmas in silica and alkalies, and their associates, at the expense of the deeper-seated, heavier, and more viscous magmas.

Leaving these far-reaching speculations in petrogenesis, let us return to the thermal significance of the radio-elements. In order to calculate the total heating effect of the radio-elements it is necessary to take into consideration the complete families of uranium and thorium. Expressing the former in terms of the equilibrium amount of radium, we have for the respective heat outputs of the two families:

Radium per gram . . .	226 calories per hour.
Thorium	270×10^{-7} ,, ,,

If we take $2{\cdot}5 \times 10^{-12}$ grams as the average radium content in a gram of the crystal rocks, and $2{\cdot}0 \times 10^{-5}$ grams as the corresponding average for thorium, then each gram of the earth's crust is a source of heat emitting 565×10^{-12} calories per hour on account of its radium content, and 540×10^{-12} calories per hour on account of its thorium content. The total heat emission per gram of the known crustal rocks is therefore of the order $1{,}105 \times 10^{-12}$ calories per hour. It is important to notice here that radium and its congeners are responsible only for approximately half of the earth's radiothermal energy, for thorium, in virtue of its greater abundance, is equally potent as a generator of heat.

The total amount of heat, Q, which escapes from the earth by conduction to its surface and radiation into space, is given by the formula:

$$Q = 4\pi r^2.k.d\theta/dr \text{ calories per second,}$$

where

$4\lambda r^2$, the area of the earth's surface $= 51 \times 10^{17}$ sq. cms.
k, the average conductivity of rock $= 0{\cdot}004$,
and, $d\theta/dr$, the average observed temperature gradient
$= 1°$ C. for 32 metres
or $0{\cdot}00031°$ C. per cm.

Substituting the given data in the above formula, Q is found to be nearly 228×10^{14} calories per hour.

Thus, if each gram of rock generates $1{,}105 \times 10^{-12}$ calories per hour on account of its radioactive contents, it is clear that 2×10^{25} grams of rock would suffice to make good the earth's loss. But the total mass of the earth is 600×10^{25} grams. Are we therefore to suppose that the earth gains from radioactive sources 300 times as much heat as it loses by conduction and radiation? Clearly we are in the face of a serious embarrassment. It is impossible to believe that the earth is growing hotter, not only for geological reasons, but also because our planet could never have cooled beyond a state in which the gain of radiothermal energy would just balance the loss of heat by conduction. Equilibrium being once established, the earth would continue to cool at the exceedingly low rate dictated by the atomic decay of the parent elements, uranium and thorium.

Since the earth is not growing hotter, a remarkable discrepancy has to be explained. There are two ways of escaping the difficulty, both of which were originally put forward by Strutt.[1] It is possible that the average radium content of the surface rocks is far above the average for the materials of the earth when taken as a whole. The earth's store of radioactive elements would then be concentrated in, and confined to, a mere superficial shell, and distributed in such a way that the observed temperature gradient would be maintained solely by their output of thermal energy. On the other hand, can it be granted that in the deep interior of the earth the radio-elements would continue to disintegrate and generate heat just as they do at the earth's surface? The parent elements may be present, but, being subjected to high pressure and temperature, it is conceivable that their decay may be inhibited. There would then be within the earth an irregularly bounded zone extending to

[1] *Proc. Roy. Soc.*, A., vol. lxxix., p. 476, 1906.

such a depth that at its base pressure and temperature would attain certain critical values. Below that zone radioactive processes would be inhibited by the excessive physical conditions. Only in the outer shell would radioactive matter be allowed to decay, and consequently only the rocks within that shell could be appealed to as an active source of radiothermal energy.

It is clearly of the greatest importance to geologists to decide between these alternative but not mutually exclusive views. Unfortunately it is impossible to come to a securely founded conclusion, but such evidence as is now available may profitably be reviewed. Let us take first the possibility of radioactive inhibition. The suggestion is based on the well-known reaction law of Le Chatelier, which states that the internal reactions within a material system are such as will tend to diminish the effect of any external influences by which its equilibrium may be disturbed. Thus, under a rising temperature, elements change their state or form new compounds in such a way as to absorb energy, and so oppose the tendency of the temperature to increase further. Similarly, under high pressure, the atoms of a compound rearrange themselves so that the molecular volume is reduced, and the ultimate stresses by which the system would have been constrained are therefore also reduced.

Dr. F. C. S. Schiller[1] suggests that uranium does not disintegrate in the earth's deep interior, or does so more slowly than near the surface, and he thinks that radioactivity may be an acquired habit of the substances that exhibit it. Dr. Leigh Fermor[2] points out that the change from uranium to radium, resulting as it does in an emission of energy, and, presumably, an increase in atomic volume, is the kind of action which would be inhibited by high pressure and temperature. Mr. H. S. Shelton[3] is not content with inhibition only; he postulates complete reversal. He thinks that "radioactive substances, particularly uranium compounds, are synthesised from other elements as a result of the conditions of great temperature and pressure found in the earth's interior." This idea was originally due to Dr. Barrell,[4] and has also been held by no less an

[1] *Nature*, June 26, 1913, p. 424. [2] *Nature*, July 10, 1913, p. 476.
[3] *Science Progress*, No. 31, p. 456, 1914.
[4] Rutherford, *Radioactive Transformations*, p. 194, 1904.

authority than Arrhenius.[1] Rutherford,[2] however, has suggested that at the enormous temperature of the sun it is possible that a process of transformation may take place in ordinary elements analogous to that observed in the radioactive elements. This implies inhibition under conditions not of high but of low temperatures.

Mr. Shelton himself, in an article on "The Age of the Sun's Heat"[3] says, "Elements which are absolutely stable under conditions which we can produce in our laboratories would, in the colossal furnaces of stellar heat, change, decompose, and gradually assume other and stabler forms." He then suggests that such a transformation would make available sufficient energy to maintain the sun's heat for thousands of millions of years. What Mr. Shelton states, then, is this: that stable terrestrial elements when subjected to high temperatures, such as that of the sun, would assume stabler forms with emission of energy. This is inconsistent with Le Chatelier's law of reaction, for obviously at high temperatures the terrestrial elements would, according to that law, change into stabler forms with *absorption* of energy. Apparently what is meant, however, is that elements, stable under terrestrial conditions, would, if present in a star or nebula, and subject to a high and rising temperature, disintegrate into stabler forms with absorption of energy. Later, when the temperature had begun to fall, this process would be reversed, and the nebular or stellar elements (*e.g.* helium and hydrogen) would re-unite with emission of energy to build up the terrestrial types of elements, and so to sustain the falling temperature. According to Mr. Shelton, "Uranium and thorium are compounded beyond the limits of stability." This implies that he believes that the radioactive parent elements are formed with absorption of energy under conditions of high and falling temperature, but that when cooling has progressed sufficiently and the temperature is lower, they again disintegrate with evolution of the energy previously stored up. That is to say, that, under conditions of falling temperature, energy is first absorbed and then emitted.

However, quite apart from Mr. Shelton's self-made difficulty,

[1] *The Life of the Universe*, vol. ii. p. 237, 1908.

[2] *Radioactive Substances and their Transformations*, p. 656, 1913.

[3] *Contemp. Review*, p. 846, June 1913.

there is a real discrepancy between the atomic disintegration exemplified by radioactivity, the only type of which we have any definite knowledge, and that which is implied by the spectroscopic classification of stars and nebulæ. In the latter, the evolution of the elements would seem to proceed from simple elements of low atomic weights, to more complex elements of high atomic weights, the transformation being attended by an emission of energy. In the case of radioactive disintegration, on the contrary, complex elements of high atomic weights break down with emission of energy into simpler elements with lower atomic weights. Applying the law of reaction to radioactive processes, it would be deduced that the parent elements, uranium and thorium, should be stable at sufficiently high temperatures, thus implying that they ought to be characteristic elements in certain classes of stars, and in any case, that they would there be less unstable than under terrestrial conditions. Spectroscopic analysis indicates that in stars of the hottest class, hydrogen and helium appear to be the chief components. In stars of a less high temperature, oxygen, nitrogen, and carbon appear. Silicon comes next, and finally, at a much lower temperature, iron, manganese, calcium, and other metals, including uranium, are introduced. Either the spectroscopic evidence of the temperature and constitution of the stars is insufficient and misleading, or Le Chatelier's law, in so far as it refers to temperature changes alone, is not applicable to the radioactive transformations. As a third alternative by which the dilemma may be avoided, it may be suggested that radioactivity is not primarily controlled either by temperature or pressure, but by other physical conditions, possibly electromagnetic, in some way of which we are as yet entirely ignorant. It is conceivable, for example, that the forms of hydrogen and helium which exist in the hottest stars, may be very different from the familiar terrestrial forms, so that they may represent, either intrinsically or in virtue of their environment, an even higher concentration of energy than the radioactive atoms. In such a case it would be possible for the high temperature elements to condense, with evolution of energy, to form the radioactive elements as well as the more stable elements of terrestrial conditions. Any changes of this kind, however, could not be referable to temperature conditions alone, and it seems impossible that they could take place in the interior of the earth, for, involving as they would the emission of energy, they would aggravate the very difficulty we are attempting to avoid. We may therefore reasonably conclude that the radioactive elements are not formed in the earth's interior from other elements, although, if they are already present, their decay may, nevertheless, be more or less inhibited.

The experimental evidence indicates on the part of the radioactive elements an utter disregard for all the changes in physical environment to which we can subject them. The effect of temperature has been investigated by several observers,[1] and their results lead to the final conclusion that atomic transformation proceeds at the same rate at all temperatures between −186° C. (liquid air) and 1,500° C. Quite recently Giebeler[2] found lines in the spectrum of Nova Geminorum (2), which have been identified with those of uranium, radium, and radium-emanation, and Dyson[3] has shown that similar lines can be observed in the spectrum of the sun's chromosphere. It would therefore appear that radioactivity is not inhibited by a temperature of 6,000° C. or 7,000° C. It has been found that pressures ranging to 2,000 atmospheres are without influence on the activity of radium. Radium emanation introduced into a high-pressure bomb was unaffected by a temperature of 2,500° C. and a pressure of 1,000 atmospheres.

Two other types of phenomena lead to apparently different conclusions. All elements are known to emit electrons when under the influence of ultra-violet light, which indicates that the atom is not altogether indifferent to external influences. Further, the vibrations which give rise to spectrum lines are damped slightly by increase of pressure, so that the lines are displaced towards the red. Strictly, however, this phenomenon is due less to pressure as such than to increase of density (*i.e.* closer packing of the molecules), for, as Larmor has pointed out, mechanical pressure arises from the translatory motions of molecules, which are too slow to have any detectable influence upon radiation periods. As the density is increased, however, the electrons to which the radiations are due are brought into closer association with surrounding electrons, and the vibrations

[1] Rutherford, *Radioactive Substances*, p. 502, 1913. Russell, *Proc. Roy. Soc.*, A., vol. lxxxvi. p. 240, 1912.

[2] *Astr. Nachr.*, 191, no. 4,582, June 1912.

[3] *Ibid.*, 192, no. 4,589, July 1912.

then emitted differ slightly from those corresponding to the natural periods of the electrons.

According to the model now favoured by the leading physicists the atom consists essentially of two strongly contrasted portions. There is a central system or nucleus surrounded by an intense electric field, and it is in the nucleus that the mass of the atom is mainly concentrated. Here, also, the positive charge of the atom is situated. Only a few negative electrons are present in the nucleus, and it is they which determine the column occupied by an element in the Periodic Table.[1] Around the nucleus is an outer shell of electrons, and it is to the latter that chemical and all the common physical phenomena are to be referred. Such phenomena are in general reversible, because the atom may lose those electrons and regain them from external sources. The above-mentioned experimental results, from which it is deduced that the atom is affected by increase of density and by the impact of ultra-violet waves, are concerned only with the outer ring of electrons. They therefore leave the question at issue untouched, for radioactivity is primarily controlled by changes in the central nucleus.

In the uranium atom the nucleus has, according to the theory developed by Rutherford, a diameter of the order of 1/10,000th of that of the whole atom.[2] For some reason this central system becomes actively unstable and a component α-particle or positively charged helium atom escapes. In passing through the electric field, it rapidly gains kinetic energy, and finally is violently expelled from the parent atom with a velocity of about 2×10^9 cms. (12,000 miles) per second. The temperature equivalent of this velocity in the case of helium is easily calculated to be about 65,000,000,000° C.[3] This result, of course, is meaningless, but it serves a useful purpose in suggesting the intense concentration of energy which is involved, and further, it indicates that radioactive processes are controlled more by electric than by thermal phenomena. Moreover, if pressure is due to the translatory motions of molecules, it seems impossible that the impulsive forces thus set up should ever prove equal to the task of imposing inactivity upon a radioactive atom.

It has been suggested as a possible effect of high pressure

[1] Soddy, *Chemistry of the Radio Elements*, part ii. p. 41, 1914.

[2] Rutherford, *Phil. Mag.*, vol. xxi. p. 669, 1911.

[3] Ramsay, *Elements and Electrons*, p. 149, 1912.

that owing to the closer packing of the atoms, intense repulsive forces between adjacent nuclei, or between neighbouring groups of electrons, might be set up, and that this would tend to prevent escape of the α- and β-particles. This seems to be plausible, but until the dimensions of atoms are known with sufficient accuracy to investigate their individual average densities and their mutual spatial relations, it is impossible to decide whether an increase of molar density due to pressure could, within the earth, suffice to induce inhibition. At present it would appear that unless the density of a solid were increased many times by the application of pressure—a condition which is certainly unrepresented in any part of our planet—then the suggested cause would be inadequate to explain the hypothetical phenomenon attributed to it.

Experimental evidence, as far as it goes, supports these theoretical considerations, for, although experiments have been made in the hope of detecting some change when radioactive substances are vigorously bombarded with α-, β-, and γ-rays, no effect has been observed. It would, for example, be reasonable to suppose that when radium emanation is disintegrating, a small amount of radium, the immediate parent of the emanation, might be formed by the re-introduction into the latter of newly liberated α-particles. Rutherford has experimented on these lines with a number of substances, but in no case has evidence been obtained that the process of transformation is reversible. It is, unfortunately, impossible to arrive at a satisfactory conclusion, but (*a*) experimental results, (*b*) theoretical considerations based on the constitution of the atom, the violence of atomic decay, and the incompressibility of solids, and (*c*) spectroscopic evidence of stellar and atomic evolution, all tend to the final suggestion that atomic disintegration is mainly controlled by conditions other than those of pressure and temperature.

This necessarily vague conclusion can scarcely form a sound basis on which to build a theory of the terrestrial distribution of radium. It certainly points to the probability that the radio-elements, if they were distributed throughout the substance of the earth, would disintegrate much as they are known to do at the surface, and that, since the earth is not growing hotter, the radio-elements are therefore limited in their occurrence to a comparatively thin superficial shell. In justice to the evidence, no more definite statement than this can be made. We must therefore consider the constitution of the earth itself, and, from what little knowledge we have, attempt to deduce the probable distribution of radium. It is no more absurd for the geologist to speculate on the interior of the earth than it is for the physicist to postulate the architecture of an atom. Both earth and atom have an outer shell with which we are to some extent familiar. Both have a central core or nucleus which is not amenable to direct observation; but the one is not more inaccessible than the other. The atom is penetrated by the Becquerel rays; the earth is inwardly explored by earthquake waves.

In 1900, Dr. R. D. Oldham[1] showed that the disturbances due to an earthquake may be analysed into three distinct forms of wave motion, which, after passing around or through the earth, give rise to three different phases in their distant record. The third phase is attributable to surface waves, and with these we need not here concern ourselves. The first and second phases are due respectively to waves of compression and waves of distortion, the former travelling much more rapidly than the latter. Traversing the heterogeneous rocks of the earth's crust, the two types of waves cannot be readily distinguished. However, as soon as they sink (at a depth of say 20 miles) into the homogeneous material that lies below, they are sorted out in virtue of their different velocities, and at a distance of 700 miles from their source two distinct records may be detected, each referable to the same original shock.

If the velocity of waves transmitted through a medium remains constant, then the wave motion is propagated in all directions in straight lines. If, however, the velocity varies owing to internal constitutional changes, the wave path is refracted. Now, since refraction of earthquake waves actually occurs within the earth, we are able to learn something about the variation of the physical conditions of the interior. When massive waves are propagated through the body of the earth, they travel faster as they penetrate to greater depths, provided that the maximum depth does not exceed 2,000 miles. There appears to be, according to Oldham,[2] a well-marked surface of physical discontinuity at a depth of between 2,000 and 2,400 miles, for, when the waves penetrate still more deeply, there

[1] *Phil. Trans. Roy. Soc.*, A., vol. cxciv. p. 135, 1900.
[2] *Nature*, August 21, 1913, p. 635.

is a remarkable decrease in their rates of propagation, a fact which indicates a high degree of resistance to compression, and therefore a marked change in constitution.

Oldham[1] originally suggested a thickness of about 20 miles for the outer shell of heterogeneous and fractured rocks which surrounds the homogeneous zones of the interior, and which we may conveniently describe as the earth's crust. He has now[2] halved his former estimate and gives 10 miles as a probable value. The late Prof. Milne[3] in 1906 deduced a thickness of 30 miles from the data then at his disposal. These varying figures may mean that the thickness itself varies from place to place, but in any case they are certainly of the right order. Some recent experiments by Adams[4] have demonstrated that under the conditions of pressure and temperature believed to obtain in the earth's crust, empty cavities may exist to a depth of at least 11 miles. King[5] has similarly shown that small cavities will remain open at all depths up to about 20 miles, provided that the temperature is not excessive. We may therefore conclude that the earth's crust as defined above is at least 10 miles thick and may be more than 20.

According to Wiechert,[6] the earth is built up essentially of two strongly contrasted zones separated somewhat sharply by a surface of discontinuity at a depth of about 950 miles. An inner core of mean density 7·8–8·0 is postulated, this being surrounded by a rocky mantle having a mean density of about 3·4. Oldham finds a surface of discontinuity beneath the Pacific at a depth of 1,000 miles,[7] which is in close agreement with Wiechert's results, but for the main body of the earth he gives 2,000–2,400 miles as the probable depth. The whole subject, however, is still in its infancy, and the discrepancies suggest that the problem is less simple than these pioneer solutions would indicate. That this is undoubtedly the case is shown by more recent work. K. Zoeppritz, L. Geyer, and B. Gutenberg have made an exhaustive study of the earth-

[1] *Q.J.G.S.*, vol. lxii. p. 456, 1906.
[2] *Nature*, August 21, 1913, p. 635.
[3] Milne, Bakerian Lecture, *Proc. Roy. Soc.*, A., vol. lxxvii. p. 365, 1906.
[4] *Journ. Geol.*, vol. xx. p. 97, 1912.
[5] *Ibid.* vol. xx. p. 119, 1912.
[6] *Deutsche Rundschau*, pp. 376–94, 1907.
[7] *Q.J.G.S.*, vol. lxiii. p. 344, 1907.

quake records received at Gottingen during the years 1904–1911, and as a consequence they have been able to distinguish, not two zones alone, but four, separated by marked physical discontinuity at depths of approximately 750, 1,060, and 1,530 miles.[1] The Wiechert law of density must be correspondingly modified. Beneath the outer crust, which has a mean density of about 2·8, are four zones, of which the outer one has probably a density of 3·4; the inner being as before about 7·8–8·0. The two intermediate zones, which are of less bulk than the others, are presumably characterised by intermediate densities. This conclusion is more likely to be in accordance with the facts than Wiechert's, for it implies a gradual downward increase of density in place of a relatively sudden change.

It is manifestly impossible ever to know directly the chemical constitution of the earth's interior. However, we may study in the laboratory the disrupted fragments of some other world,[2] for it is now believed that meteorites were once arranged according to their densities as parts of a cosmic body. If this be true it seems highly probable that the constitution of the meteoritic parent body thus determinable was essentially similar to that of the earth. Let us briefly review the evidence in favour of these suggestions.

Meteorites may be divided into five well-marked classes according to the relative proportions of their metallic and stony constituents. The iron meteorites, which are known as holosiderites, consist almost entirely of a coarsely crystalline nickel-iron alloy (average density 7·8). The dimensions and uniformity of structure of the giant metallic crystals of many iron meteorites indicate that they crystallised very slowly from a magma which remained for a long period at a nearly uniform temperature not far below the fusion point. This in turn suggests that most of the holosiderites were formed in the deep interior of the parent body, where the pressure would be high.

It has been conjectured that the well-known Widmanstätten figures represent an internal structure which may have been due either to (*a*) very slow crystallisation, (*b*) sudden chilling

[1] *Gessellwiss Gottengen Nachr. Nath. Phys. Klasse*, **2**, pp. 121–206, 1912; **6**, pp. 625–75, 1912.

[2] Chamberlin, *Journ. Geol.*, vol. ix. p. 369, 1901. See also a valuable series of papers by Farrington in the same volume.

subsequent to solidification, or to (*c*) the effects of high pressure. The second view supports the hypothesis that meteorites are the scattered fragments of a suddenly disrupted and therefore suddenly chilled cosmic body; the first and last that holosiderites came from the central core of such a parent body, *i.e.* where cooling would be long delayed, and mechanical pressure would be at its maximum.

The octohedral form of many of the crystals betrays in part their past thermal history, for it proves that the temperature which conditioned their growth must have exceeded 860° C. One effect of high pressure would be, of course, to raise this lower limit very considerably.

Lithosiderites are meteorites which consist of a nickel-iron matrix containing granules of basic silicates such as olivine and bronzite. When the silicate minerals preponderate over the metallic alloy so that the latter occurs in grains embedded in stone, the meteorite is known as a siderolite. The nickel-iron in these two types is also generally of octohedral form, and exhibits the Widmanstätten figures. Equally significant is the presence of tridymite, a crystalline form of silica which is stable between temperature limits of 800° C. and 1,625° C.

The stone meteorites proper are divided into chondrites and achondrites according to the presence or absence respectively of peculiar rounded masses of olivine or pyroxene, which are known as chondri or chondrules. The origin of these puzzling structures as yet is not understood, for they have no known terrestrial analogues.[1] Some of the meteoric stones have certainly crystallised from a molten magma, and may be paralleled with the ultra-basic rocks. Others, however, have a clastic or fragmental structure, and seem to be of the nature of volcanic tuffs and breccias, to which they bear a close resemblance. The common presence of a crypto-crystalline matrix, or a dark basic glass, affords clear evidence of rapid cooling. These features point to the superficial conditions under which some of the stones were originally formed.

Dr. Prior[2] has recently shown that there is a striking similarity both in the chemical and mineralogical compositions of

[1] For an interesting suggestion see Fermor, *Rec. Geol. Surv. India*, vol. xliii. 1913, pp. 41-47.

[2] *Min. Mag.* vol. xvii. p. 33, 1914.

chondritic stones, all of which approximate to the following type:

Nickeliferous Iron (Fe/Ni = 10/1)	9
Troilite (FeS)	6
Olivine (Mg/Fe = 3/1)	44
Bronzite (Mg/Fe = 4/1)	30
Oligoclase	10
Chromite	1
	100

If such a close correspondence were found in a series of terrestrial rocks, they would be said to present a high degree of consanguinity. The evidence points directly to community of origin, both as regards the chemical constitution of the magma from which they crystallised, and the physical conditions which determined their structures.

It is a noteworthy feature in the fragmental meteorites that no trace of stratification, foliation, or weathering has ever been observed. Moreover, the minerals which require the presence of "mineralising agents" in order to crystallise successfully are one and all absent from meteorites. Olivine is never altered to serpentine, nor felspar to kaolin, or sericite, or epidote. The minerals are throughout fresh and unaltered. These facts may be interpreted as showing that the parent body was lacking in the gases and vapours which would have promoted mobility of the magmas and selective differentiation, and that consequently it was devoid of a "crust" such as that of the earth, and was also without an appreciable atmosphere or ocean.

That the parent body had already cooled well into its interior at the time of disruption is proved in a most striking way by the occurrence of combustible hydrocarbons in certain meteoric stones, and also in a limited but well-defined group of carbonaceous meteorites. The latter, having a very low density and an unusual composition, may possibly represent whatever "crust" the parent body possessed. The presence of hydrocarbons implies that ever since their formation the meteorites which carry them can never have been subjected to any but low temperatures. During the swift flight through the atmosphere the superficial skin of a meteorite is fused, but its interior, already chilled by its passage through space, remains cold, and so continues to preserve the volatile compounds from destruction.

The mean densities of the chief groups of meteorites are classified in the following table:

Type of Meteorite.		Mean density.	
Achondrites	Stone	3·2	3·4
Chondrites		3·5	
Siderolites	Iron-stone	4·8	5·5
Lithosiderites		6·5	
Holosiderites	Iron	7·8	

The achondrites do not widely differ in chemical composition from terrestrial ultra-basic rocks. The chondrites are also very similar, except as regards the small proportion of free metal which is usually present, and even this can be matched in certain terrestrial rocks which carry native iron. The field relations of the ultra-basic rocks, as well as their superior density, indicate that they are genetically connected with a deep-seated zone which underlies the more acid rocks of the earth's crust. On the meteoritic analogy, which is based as much on structures as on densities, it may be assumed that they extend to the first surface of discontinuity, so constituting not only the deeper portions of the crust itself, but also the first great zone which lies beneath it. The second zone would presumably be formed of material like that of the siderolites; the third would compare closely with the lithosiderites, and, finally, corresponding to the holosiderites, would come the central metallic core. On this distribution the mean density of the earth would be 5·1. Although the earth's actual mean density is 5·53, the discrepancy is not without significance, for it is highly probable that under the pressures obtaining in the ultra-basic zone minerals would form of higher density than those in the stone meteorites. It has been suggested by Fermor that the reason why such minerals (*e.g.* garnet) do not occur in meteorites is that a general recrystallisation would accompany the disruption of, and consequent relief of pressure in, the parent body.

The view that meteorites allow us to read at our leisure many of the secrets which are otherwise locked up in the earth's interior was originally held by Boisse,[1] Meunier,[2] and Daubrée,[3] who arranged meteorites according to their densities, and founded an analogy solely on that arrangement. The same idea has more

[1] *Mem. de la. Soc. des Lets. Sci. et Arts de l'Aveyron*, vol. vii. p. 168.

[2] *Cours. de Geol. Comparée.*

[3] Suess, *The Face of the Earth* (Eng. trans.), vol. iv. p. 543.

recently been developed by Farrington,[1] who, however, was led to his hypothesis from a study of the structural characters of meteorites. Suess[2] has further supported the parallel on account of the remarkable and detailed correspondence between the qualitative chemical composition of the ultra-basic rocks of the earth's crust and that of the stony material in meteorites. He proposes three zones as determining the structure of the earth: *Nife* (Ni, Fe), the metallic barysphere; *Sima* (Si, Mg), the intermediate zone; and *Sal* (Si, Al), the outer crust. Merrill[3] has also investigated and discussed the chemical characters of meteorites, and his results show that the elements which he was unable to detect are chiefly those which are least characteristic of terrestrial ultra-basic rocks, and which are, in fact, never abundant on the earth except in acid or alkaline rocks.

As we have seen above, the parallel can now be carried further than ever before, for to each type of meteorite there corresponds a terrestrial zone of such dimensions that the density requirements are satisfied as closely as could reasonably be expected. In view of this analogy between meteoritic and terrestrial materials, it appeared to the author that it would be of great interest to investigate in detail the radioactive characters of meteorites. Only a few preliminary results have so far been obtained, but they seem to be particularly significant. Radium estimations have been made of the following composites,* this method of analysis having been rendered necessary because the available quantity of each individual specimen was too small to allow of separate treatment. The specimens were detached from a small collection which belongs to the Geological Museum of the Imperial College of Science and Technology, by permission of the authorities of the College, to whom I wish here to express my thanks. The results may be summarised, together with those for average rock types, in the table opposite.

In 1906 Prof. Strutt[4] estimated the radium in the Dhurmsala stony meteorite, and found it to contain $0{\cdot}56 \times 10^{-12}$ grams per gram. In the case of three iron meteorites, however, he was

[1] *Journ. Geol.*, vol. ix. p. 623, 1901.

[2] *Loc. cit.*, p. 544.

[3] *Am. Journ. Sci.*, vol. xxvii. p. 469, 1909; vol. xxxv. p. 509, 1913. See also Wahl, *Zeit. anorg. Chem.*, vol. lxix. p. 52, 1910.

[4] *Proc. Roy. Soc.*, A., vol. lxvi. p. 480, 1906.

Type of Material.	Density.	Radium.	Silica.	Alkalis.
PLUTONIC ROCKS:				
Acid	2·65	3	70	8
Intermediate . . .	2·80	2	60	6
Basic	2·95	1	50	5
Ultra-basic	3·20	0·5	40	1
METEORITES: *				
Stone	3·4	0·25	40	1
Iron-stone	5·5	0·1	15	0·2
Iron	7·8	—	—	—

Silica and alkalis are given in percentages. Radium is stated in units of billionths (10^{-12}) of a gram per gram of material. For a useful collection of analyses of meteorites, see Farrington, Field Columbian Museum Publications, Chicago, Geological Series, vol. iii. nos. 5 and 9. For details of the classification of meteorites, see Brezina, *Proc. Am. Phil. Soc.*, vol. xliii. p. 211, 1904.

* The composites were made up of the following groups of meteorites:

(8) *Stone Meteorites:* [Radium averages $0·25 \times 10^{-12}$ grams per gram.]
 (*a*) Achondrites: Stannern, Nagy-Borové, and Bluff.
 (*b*) Chondrites: L'Aigle, Charsonville, Dhurmsala, Pultusk, and Knyahinya.

(4) *Iron-Stone Meteorites:* [Radium averages $0·10 \times 10^{-12}$ grams per gram.]
 Lithosiderites and Siderolites: Estherville, Pallas Iron, Vaca Muerta and Doña Inez.

(3) *Iron Meteorites:* [No radium detectable.]
 Holosiderites: Youndegin, Staunton, and Toluca.

unable to detect a measurable quantity. Native iron from Disco, Greenland, contained $0·21 \times 10^{-12}$ grams per gram, this being probably associated with the silicate minerals also present in the specimen.

In their natural occurrence there would thus appear to be a strong chemical antipathy between uranium and iron, and it will now be clear that if the meteoritic analogy is true, or if for any other reason it is believed that metallic iron is the chief constituent of the earth's internal core, then there is considerable evidence that the latter is entirely devoid of uranium. It may be objected that if the stony material of the earth were charged throughout with the same amount of uranium (or radium) as is the stony material of meteorites, there would still be an *embarras des richesses*, as regards the thermal output of the whole. The analogy, however, must not be pushed too far, and quantitative equivalence is not suggested. It is impossible, for example, that meteorites (or planetesimals of an identical average composition) could have built up the earth, because in many respects (*e.g.* lack of minerals which require the presence of mineralisers; deficiency in potash, etc.) the crustal rocks derived from them would have been somewhat different in composition from those with which we are familiar. In so far as the crust is the result of a long and cumulative process of differentiation, it represents in a highly magnified or exaggerated way, the detailed chemical peculiarities of the materials from which the earth was formed.

Summing up, we have seen that in the earth itself radium and its congeners are undoubtedly more abundant in the upper parts of the crust, and that in successive layers the radium content rapidly decreases with depth. In meteorites, radium is found in small quantities in the silicate minerals, but is absent from the nickel-iron alloy. That is to say, the percentage of radium in each successive zone of the parent body gradually decreased with depth, until ultimately it died out altogether. It is suggested that, in the case of the earth, the decrease of radium with depth does not stop at the point where our means of observation come to an end, but that it continues until at last the radium content is reduced to zero. If within the depth to which the radio-elements extend, pressure and temperature are ineffective in preventing or inhibiting atomic decay, then the total quantity of the earth's store of the radio-elements is calculable with some accuracy.

The problems that are suggested by the general conclusions of this inquiry are of supreme geological importance. The evolution of the earth, of its zonal structure, and particularly of its crust are all questions which remain to be solved. The thermal history of the earth must be investigated afresh. Volcanic phenomena, and the differentiation and movements of molten magmas receive a new significance. To enter into these wider problems[1] would lead us farther afield than the title of this paper would justify, but its purpose will have been served if it affords a basis for future discussion, and indicates the special subjects concerning which the geologist urgently desires extended knowledge and more securely founded conclusions.

[1] In this connection the following may be referred to: J. Joly, *Radioactivity and Geology*, pp. 154–82, 1909; T. C. Chamberlin, *Journ. Geol.*, p. 673, 1911; A. Holmes, *Nature*, p. 398, June 19, 1913, *The Age of the Earth*, p. 30, 1913; L. L. Fermor, *Geol. Mag.*, p. 65, 1914.

THE BIRTH-TIME OF THE WORLD[1]

By J. JOLY, Sc.D., F.R.S.

Professor of Geology and Mineralogy, Trinity College, Dublin

Long ago Lucretius wrote: "For lack of power to solve the question troubles the mind with doubts, whether there was ever a birth-time of the world and whether likewise there is to be any end." "And if" (he says in answer) "there was no birth-time of earth and heaven and they have been from everlasting, why before the Theban war and the destruction of Troy have not other poets as well sung other themes? Whither have so many deeds of men so often passed away, why live they nowhere embodied in lasting records of fame? The truth methinks is that the sum has but a recent date, and the nature of the world is new and has but lately had its commencement."[2]

Thus spake Lucretius nearly 2,000 years ago. Since then we have attained another standpoint and found very different limitations. To Lucretius the world commenced with man, and the answer he would give to his questions was in accord with his philosophy: he would date the birth-time of the world from the time when poets first sung upon the earth. Modern Science has swept utterly away this beautiful imagining, along with the theory that the earth dated its beginning with the advent of man. We can, indeed, find no beginning of the world. We trace back events and come to barriers which close our vista—barriers which, for all we know, may for ever close it. They stand like the gates of ivory and of horn; portals from which only dreams proceed; and Science cannot as yet say of this or that dream if it proceeds from the gate of horn or from that of ivory.

In short, of the earth's origin we have no certain knowledge;

[1] A lecture delivered before the Royal Dublin Society, February 6, 1914.

[2] H. A. J. Munro, *De Rerum Natura* (Cambridge, 1886).

nor can we assign any date to it. Possibly its formation was an event so gradual that the beginning was spread over immense periods. We can only trace the history back to certain events which may with considerable certainty be regarded as ushering in our geological era.

Notwithstanding our limitations the date of the birth-time of our geological era is the most important date in Science. For in taking into our minds the spacious history of the universe, it must play the part of time-unit upon which all our conceptions depend. If we date the geological history of the earth by thousands of years, as did our forerunners, we must shape our ideas of planetary time accordingly; and the duration of our solar system, and of the heavens, becomes comparable with that of the dynasties of ancient nations. If in millions of years the sun and stars are proportionately venerable. If in hundreds or thousands of millions of years the human mind must consent to correspondingly vast epochs for the duration of material changes. The geological age plays the same part in our views of the duration of the universe as the earth's orbital radius does in our views of the immensity of space. Lucretius knew nothing of our time-unit: his unit was the life of a man. So also he knew nothing of our space-unit, and he marvels that so small a body as the sun can shed so much heat and light upon the earth.

A study of the rocks shows us that the world was not always what it now is and long has been. We live in an epoch of denudation. The rains and frosts disintegrate the hills; and the rivers roll to the sea the finely divided particles into which they have been resolved; as well as the salts which have been leached from them. The sediments collect near the coasts of the continents; the dissolved matter mingles with the general ocean. The geologist has measured and mapped these deposits and traced them back into the past, layer by layer. He finds them ever the same: sandstones, slates, limestones, etc. But one thing is not the same. *Life* grows ever less diversified in character as the sediments are traced downwards. Mammals and birds, reptiles, amphibians, fishes, die out successively in the past; and barren sediments ultimately succeed, leaving the first beginnings of life undecipherable by him. Beneath these barren sediments lie rocks collectively differing in character from those above: mainly volcanic or poured out from fissures in the early crust of the earth. Sediments are scarce among these materials.[1]

There can be little doubt that in this underlying floor of igneous and metamorphic rocks we have reached those surface materials of the earth which existed before the long epoch of sedimentation began, and before the seas came into being. They formed the floor of a vapourised ocean upon which the waters condensed here and there from the hot and heavy atmosphere. Such were the probable conditions which preceded the birth-time of the ocean and of our era of life and its evolution.

It is from this epoch we date our geological age. Our next purpose is to consider how long ago, measured in years, that birth-time was.

That the geological age of the earth is very great appears from what we have already reviewed. The sediments of the past are many miles in collective thickness: yet the feeble silt of the rivers built them all from base to summit. They have been lifted from the seas and piled into mountains by movements so slow that during all the time man has been upon the earth but little change would have been visible. The mountains have again been worn down into the ocean by denudation and again younger mountains built out of their redeposited materials. The contemplation of such vast events prepares our minds to accept many scores of millions of years or hundreds of millions of years, if such be yielded by our calculations.

The Age by the Thickness of the Sediments

The earliest recognised method of arriving at an estimate of the earth's geological age is based upon the measurement of the collective sediments of geological periods. The method has undergone much revision from time to time. Let us briefly review it on the latest data.

The method consists in measuring the depths of all the successive sedimentary deposits where these are best developed. We go all over the explored world, recognising the successive deposits by their fossils and by their stratigraphical relations;

[1] For a description of these early rocks, see especially the monograph of Van Hise and Leith on the Pre-Cambrian Geology of North America (Bulletin 360, U.S. Geol. Survey).

measuring their thickness and selecting as part of the data required those beds which we believe to most completely represent each formation. The total of these measurements would tell us the age of the earth if their tale was indeed complete, and if we knew the average rate at which they have been deposited. We soon, however, find difficulties in arriving at the quantities we require. Thus it is not easy to measure the real thickness of a deposit. It may be folded back upon itself, and so we may measure it twice over. We may exaggerate its thickness by measuring it not quite straight across the bedding or by unwittingly including volcanic materials. On the other hand, there may be deposits which are inaccessible to us; or, again, an entire absence of deposits; either because not laid down in the areas we examine, or, if laid down, again washed into the sea. These sources of error in part neutralise one another. Some make our resulting age too long, others make it out too short. But we do not know if a balance of error does not still remain. Here, however, is a table of deposits which summarises a great deal of our knowledge of the thickness of the stratigraphical accumulations. It is due to Prof. Sollas.[1]

	Feet.	
Recent and Pleistocene	4,000	
Pliocene	13,000	
Miocene	14,000	
Oligocene	12,000	
Eocene	20,000	
		63,000
Upper Cretaceous	24,000	
Lower "	20,000	
Jurassic	8,000	
Trias	17,000	
		69,000
Permian	12,000	
Carboniferous	29,000	
Devonian	22,000	
		63,000
Silurian	15,000	
Ordovician	17,000	
Cambrian	26,000	
		58,000
Keweenawan (Algonkian)	50,000	
Animikian (Algonkian)	14,000	
Huronian (Algonkian)	18,000	
		82,000
Archæan	?	
Total		335,000 feet.

[1] Address to the Geol. Soc. of London, 1909.

In the next place we require to know the average rate at which these rocks were laid down. This is really the weakest link in the chain. The most diverse results have been arrived at, which space does not permit us to consider. The value required is most difficult to determine, for it is different for the different classes of material, and varies from river to river according to the conditions of discharge to the sea. We may probably take it as between two and six inches in a century.

Now the total depth of the sediments as we see is about 335,000 feet (or 64 miles), and if we take the rate of collecting as 3 inches in a hundred years we get the time for all to collect as 134 millions of years. If the rate be 4 inches, the time is 100 millions of years, which is the figure Geikie favoured, although his result was based on somewhat different data. Sollas most recently finds 80 millions of years.[1]

The Age by the Mass of the Sediments

In the above method we obtain our result by the measurement of the *linear* dimensions of the sediments. These measurements, as we have seen, are difficult to arrive at. We may, however, proceed by measurements of the *mass* of the sediments, and then the method becomes more definite. The new method is pursued as follows:

The total mass of the sediments formed since denudation began may be ascertained with comparative accuracy by a study of the chemical composition of the waters of the ocean. The salts in the ocean are undoubtedly derived from the rocks; increasing age by age as the latter are degraded from their original character under the action of the weather, etc., and converted to the sedimentary form. By comparing the average chemical composition of these two classes of material—the primary or igneous rocks and the sedimentary—it is easy to arrive at a knowledge of how much of this or that constituent was given to the ocean by each ton of primary rock which was denuded to the sedimentary form. This, however, will not assist us to our object unless the ocean has retained the salts shed into it. It has not generally done

[1] Geikie, *Text Book of Geology* (Macmillan, 1903), vol. i. p. 73 *et seq.* Sollas, *loc. cit.* Joly, *Radioactivity and Geology* (Constable, 1909), *Phil. Mag.* Sept, 1911.

so. In the case of every substance but one only, the ocean continually gives up again more or less of the salts supplied to it by the rivers. The one exception is the element sodium. The great solubility of its salts has protected it from abstraction, and it has gone on collecting during geological time, practically in its entirety. This gives us the clue to the denudative history of the earth.[1] It is the secret of the sea.

The process is now simple. We estimate by chemical examination of igneous and sedimentary rocks the amount of sodium which has been supplied to the ocean per ton of sediment produced by denudation. We also calculate the amount of sodium contained in the ocean. We divide the one into the other (stated, of course, in the same units of mass), and the quotient gives us the number of tons of sediment. The most recent estimate of the sediments made in this manner affords 56×10^{16} tonnes.[2]

Now we are assured that all this sediment was transported by the rivers to the sea during geological time. Thus it follows that if we can estimate the average annual rate of the river supply of sediments to the ocean over the past we can calculate the required age. Now the land surface is at present largely covered with the sedimentary rocks themselves. Sediment derived from these rocks must be regarded as, for the most part, purely cyclical; that is, circulating from the sea to the land and back again. It does not go to increase the great body of detrital deposits. We cannot, therefore, take the present river supply of sediment as representing that obtaining over the long past. If the land was all covered still with primary rocks we might do so. It has been estimated that about 25 per cent of the existing continental area is covered with archæan and igneous rocks, the remainder being sediments.[3] On this estimate we may find valuable major and minor limits to the geological age. If we take 25 per cent. only of the present river supply of sediment, we evidently fix a major limit to the age, for it is certain that over the past there must have been

[1] *Trans. R.D.S.*, May 1899.

[2] Clarke, *A Preliminary Study of Chemical Denudation* (Washington 1910). My own estimate in 1899 (*loc. cit.*) made as a test of yet another method of finding the age, showed that the sediments may be taken as sufficient to form a layer 1·1 mile deep if spread uniformly over the continents; and would amount to 64×10^{16} tons.

[3] Van Tillo, *Comptes Rendues* (Paris), vol. cxiv. 1892.

on the average a faster supply. If we take the entire river supply, on similar reasoning we have what is undoubtedly a minor limit to the age.

The river supply of detrital sediment has not been very extensively investigated, although the quantities involved may be found with comparative ease and accuracy. The following table embodies the results obtained for some of the leading rivers.[1]

	Mean annual discharge in cubic feet per second.	Total annual sediment in thousands of tons.	Ratio of sediment to water by weight.
Potomac	20,160	5,557	1 : 3,575
Mississippi	610,000	406,250	1 : 1,500
Rio Grande	1,700	3,830	1 : 291
Uruguay	150,000	14,782	1 : 10,000
Rhone	65,850	36,000	1 : 1,775
Po	62,200	67,000	1 : 900
Danube	315,200	108,000	1 : 2,880
Nile	113,000	54,000	1 : 2,050
Irrawaddy	475,000	291,430	1 : 1,610
Mean	201,468	109,650	1 : 2,731

We see that the ratio of the weight of water to the weight of transported sediment in six out of the nine rivers does not vary widely. The mean is 2,730 to 1. But this is not the required average. The water-discharge of each river has to be taken into account. If we ascribe to the ratio given for each river the weight proper to the amount of water it discharges, the proportion of weight of water to weight of sediment, for the whole quantity of water involved, comes out as 2,520 to 1.

Now if this proportion holds for all the rivers of the world—which collectively discharge about 27×10^{12} tonnes of water per annum—the river-born detritus is $1{\cdot}07 \times 10^{10}$ tonnes. To this an addition of 11 per cent has to be made for silt pushed along the river-bed.[2] On these figures the minor limit to the age comes out as 47 millions of years, and the major limit as 188 millions. We are here going on rather deficient estimates, the rivers involved representing only some 6 per cent of the total river supply of water to the ocean. But the result is probably not very far out.

[1] Russell, *River Development* (John Murray, 1898).

[2] According to observations made on the Mississippi (Russell, *loc. cit.*).

We may arrive at a probable age lying between the major and minor limits. If, first, we take the arithmetic mean of these limits, we get 117 millions of years. Now this is almost certainly excessive, for we here assume that the rate of covering of the primary rocks by sediments was uniform. It would not be so, however, for the rate of supply of sediment must have been continually diminishing during geological time, and hence we may take it the rate of advance of the sediments on the primary rocks has also been diminishing. The average rate of supply has therefore been greater than the mean rate. Now we may probably take, as a fair assumption, that the sediment-covered area was at any instant increasing at a rate proportionate to the rate of supply of sediment; that is, to the area of primary rocks then exposed. On this assumption the age is found to be 87 millions of years.

The Age by the Sodium of the Ocean

I have next to lay before you a quite different method. I have already touched upon the chemistry of the ocean, and on the remarkable fact that the sodium contained in it has been preserved, practically, in its entirety from the beginning of geological time.

That the sea is one of the most beautiful and magnificent sights in Nature all admit. But, I think, to those who know its story its beauty and magnificence are ten-fold increased. Its saltness is due to no magic mill. It is the dissolved rocks of the earth which give it at once its brine, its strength, and its buoyancy. The rivers which we say flow with "fresh" water to the sea nevertheless contain those traces of salt which, collected over the long ages, occasion the saltness of the ocean. Each gallon of river water contributes to the final result; and this has been going on since the beginning of our era. Consider the mighty total of the rivers: 6,500 cubic miles of water in the year! Yet vast as it is, how little in the overwhelming magnitude of the ocean!

There is little doubt that the primeval ocean was in the condition of a fresh-water lake. It can be shown that a primitive and more rapid solution of the original crust of the earth by the slowly cooling ocean would have given rise to relatively small salinity. The fact is the quantity of salts in the ocean is enormous. We are only now concerned with the sodium;

but if we could extract all the rock-salt (the chloride of sodium) from the ocean we would have enough to cover the entire dry land of the earth to a depth of 400 feet. It is this gigantic quantity which is going to enter into our estimate of the earth's age. The calculated mass of sodium contained in this rock-salt is 14,130 million million tonnes.

If now we can determine the rate at which the rivers supply sodium to the ocean, we can determine the age.[1] As the result of many thousands of river analyses, the total amount of sodium annually discharged to the ocean by all the rivers of the world is found to be probably not far from 175 million tonnes.[2] Dividing this into the mass of oceanic sodium we get the age as 80·7 millions of years. Certain corrections have to be applied to this figure which result in raising it to a little over 90 millions of years. By this method Sollas gets the age as between 80 and 150 millions of years. My own result[3] was between 80 and 90 millions of years; but I subsequently found that upon certain extreme assumptions a maximum age might be arrived at of 105 millions of years.[4] Clarke regards the 80·7 millions of years as certainly a maximum in the light of certain calculations by Becker.[5]

The order of magnitude of these results cannot be shaken

[1] *Trans. R. D. S.* 1899. A paper by Edmund Halley, the astronomer, in the *Philosophical Transactions of the Royal Society* for 1715, contains a suggestion for finding the age of the world on somewhat similar lines. He proposes to make observations on the saltness of the seas and ocean at intervals of one or more centuries, and from the increment of saltness arrive at their age. The measurements, as a matter of fact, are impracticable. The salinity would only gain (if all remained in solution) one millionth part in 100 years; and, of course, the continuous rejection of salts by the ocean would invalidate the method. The last objection also invalidates the calculation by T. Mellard Reade (*Proc. Liverpool Geol. Soc.* 1876) of a minor limit to the age by the calcium sulphate in the ocean. Both papers were quite unknown to me when working out my method. Halley's paper was, I think, only brought to light in 1908.

[2] J. W. Clarke, *A Preliminary Study of Chemical Denudation* (Smithsonian Miscellaneous Collections, 1910).

[3] *Loc. cit.*

[4] "The Circulation of Salt and Geological Time" (*Geol. Mag.* 1901, p. 350).

[5] Becker (*loc. cit.*), assuming that the exposed igneous and archæan rocks alone are responsible for the supply of sodium to the ocean, arrives at 74 millions of years as the geological age. This matter was discussed by me formerly (*Trans. R. D. S.* 1899, pp. 54 *et seq.*). The assumption made is, I believe, quite inadmissible. It is not supported by river analyses, or by the chemical character of residual soils from sedimentary rocks. There may be some convergence in the rate of solvent denudation, but—as I think on the evidence—in our time unimportant.

unless on the assumption that there is something entirely misleading in the existing rate of solvent denudation. On the strength of the results of another and entirely different method of approaching the question of the earth's age (which shall be presently referred to), it has been contended that it is too low. It is even asserted that it is from nine to fourteen times too low. We have then to consider whether such an enormous error can enter into the method. The measurements involved cannot be seriously impugned. Corrections for possible errors applied to the quantities entering into this method have been considered by various writers. My own original corrections have been generally confirmed. I think the only point left open for discussion is the principle of uniformitarianism involved in this method and in the methods previously discussed.

In order to appreciate the force of the evidence for uniformity in the geological history of the earth, it is, of course, necessary to possess an acquaintance with that history. Some of the most eminent geologists, among whom Lyle and Geikie[1] may be mentioned, have upheld the doctrine of uniformity. It must here suffice to dwell upon a few points having special reference to the matter under discussion.

The mere extent of the land surface does not, within limits, affect the question of the rate of denudation. This arises from the fact that the rain supply is quite insufficient to denude the whole existing land surface. About 30 per cent of it does not, in fact, drain to the ocean. If the continents become invaded by a great transgression of the ocean, this "rainless" area diminishes: and the denuded area advances inwards without diminution. If the ocean recedes from the present strand lines, the "rainless" area advances outwards, but, the rain supply being sensibly constant, no change in the river supply of salts is to be expected.

Age-long submergence of the entire land, or of any very large proportion of what now exists, is negatived by the continuous sequence of vast areas of sediment in every geologic age from the earliest times. Now sediment-receiving areas always are but a small fraction of those exposed areas whence the sediments are supplied.[2] Hence in the continuous records of the

[1] See especially Geikie's Address to Sect. C., Brit. Assoc. Rep. 1899.

[2] On the strength of the Mississippi measurements about 1 to 18 (Magee, *Am. Jour. of Sc.* 1892, p. 188).

sediments we have assurance of the continuous exposure of the continents above the ocean surface. The doctrine of the permanency of the continents has in its main features been accepted by the most eminent authorities. As to the actual amount of land which was exposed during past times to denudative effects, no data exist to show it was very different from what is now exposed. It has been estimated that the average area of the North American continent over geologic time was about eight-tenths of its existing area.[1] Restorations of other continents, so far as they have been attempted, would not suggest any more serious divergency one way or the other.

That climate in the oceans and upon the land was throughout much as it is now, the continuous chain of teeming life and the sensitive temperature limits of protoplasmic existence are sufficient evidence.[2] The influence at once of climate and of elevation of the land may be appraised at their true value by the ascertained facts of solvent denudation, as the following table shows.

	Tonnes removed in solution per square mile per annum.	Mean elevation. Metres.
North America	79	700
South America	50	650
Europe	100	300
Asia	84	950
Africa	44	650

In this table the estimated number of tonnes of matter in solution, which for every square mile of area the rivers convey to the ocean in one year, is given in the first column. These results are compiled by Clarke from a very large number of analyses of river waters. The second column of the table gives the mean heights in metres above sea level of the several continents, as cited by Arrhenius.[3]

Of all the denudation results given in the table, those relating to North America and to Europe are far the most reliable. Indeed these may be described as highly reliable, being founded on some hundreds or thousands of analyses, many of which have been systematically pursued through every season of the year. These show that Europe with a mean altitude of less than half that of North America sheds to the ocean 25 per cent. more

[1] C. Schuchert, *Bull. Geol. Soc. Am.*, vol. xx. 1910.

[2] See also Poulton, Address to Sect. D., Brit. Assoc. Rep. 1896.

[3] *Lehrbuch der Kosmischen Physik*, vol. i. p. 347.

salts. Hence if it is true, as has been stated, that we now live in a period of exceptionally high continental elevation, we must infer that the average supply of salts to the ocean by the rivers of the world is less than over the long past, and that, therefore, our estimate of the age of the earth as already given is excessive.

There is, however, one condition which will operate to unduly diminish our estimate of geologic time, and it is a condition which may possibly obtain at the present time. If the land is, on the whole, now sinking relatively to the ocean level, the denudation area tends, as we have seen, to move inwards. It will thus encroach upon regions which have not for long periods drained to the ocean. On such areas there is an accumulation of soluble salts which the deficient rivers have not been able to carry to the ocean. Thus the salt content of certain of the rivers draining to the ocean will be influenced not only by present denudative effects, but also by the stored results of past effects. Certain rivers appear to reveal this unduly increased salt supply: those which flow through comparatively arid areas. However, the flow-off of such tributaries is relatively small and the final effects on the great rivers apparently unimportant—a result which might have been anticipated when the extremely slow rate of the land movements is taken into account.

The difficulty of effecting any reconciliation of the methods already described and that now to be given increases the interest both of the former and the latter.

The Age by Radioactive Transformations

Rutherford suggested in 1905 that as helium was continually being evolved at a uniform rate by radioactive substances (in the form of the alpha rays) a determination of the age of minerals containing the radioactive elements might be made by measurements of the amount of the stored helium and of the radioactive elements giving rise to it. The parent radioactive substance is—according to present knowledge—uranium or thorium. An estimate of the amounts of these elements present enables the rate of production of the helium to be calculated. Rutherford shortly afterwards found by this method an age of 240 millions of years for a radioactive mineral of presumably remote age. Strutt, who carried his measurements to a wonderful degree of refinement, found the following ages for mineral substances originating in different geological ages:

Oligocene	8·4 millions of years.
Eocene	31 " " "
Lower Carboniferous . . .	150 " " "
Archæan	710 " " "

Periods of time much less than, and very inconsistent with, these were also found. The lower results are, however, easily explained if we assume that the helium—which is a gas under prevailing conditions—escapes in many cases slowly from the mineral.

Another product of radioactive origin is lead. The suggestion that this substance might be made available to determine the age of the earth also originated with Rutherford. We are at least assured that this element cannot escape by gaseous diffusion from the minerals. Boltwood's results on the amounts of lead contained in minerals of various ages, taken in conjunction with the amount of uranium or parent substance present, afforded ages rising to 1,640 millions of years for Archæan and 1,200 millions for Algonkian time. Becker, applying the same method, obtained results rising to quite incredible periods: from 1,671 to 11,470 millions of years. Becker maintained that original lead rendered the determinations indefinite. The more recent results of Mr. A. Holmes support the conclusion that "original" lead may be present and may completely falsify results derived from minerals of low radioactivity in which the derived lead would be small in amount. By rejecting such results as appeared to be of this character, he arrives at 370 millions of years as the age of the Devonian.

I must now describe a very recent method of estimating the age of the earth. There are, in certain rock-forming minerals, colour-changes set up by radioactive effects. The minute and curious marks so produced are known as haloes; for they surround, in ring-like forms, minute particles of included substances which contain radioactive elements. It is now well known how these haloes are formed. The particle in the centre of the halo contains uranium or thorium, and, necessarily, along with the parent substance, the various elements derived from it. In the process of transformation giving rise to these several derived substances, atoms of helium, projected with great

4

velocity into the surrounding mineral—the alpha rays—occasion the colour changes referred to. These changes are limited to the distance to which the alpha rays penetrate; hence the halo is a spherical volume surrounding the central substance.[1]

The time required to form a halo can be found if on the one hand we could ascertain the number of alpha rays ejected in, say, one year from the nucleus of the halo, and, on the other, if we determined by experiment just how many alpha rays were required to produce the same amount of colour alteration as we perceive to extend around the nucleus.

The latter estimate is fairly easily and surely made. But to know the number of rays leaving the central particle in unit time we require to know the quantity of radioactive material in the nucleus. This cannot be directly determined. We can only, from known results obtained with larger specimens of just such a mineral substance as composes the nucleus, guess at the amount of uranium, or it may be thorium, which may be present.

This method has been applied to the uranium haloes of the mica of County Carlow.[2] Results for the age of the halo of from 20 to 400 millions of years have been obtained. This mica was probably formed in the granite of Leinster in late Silurian or in Devonian times.

The higher results are probably the least in error, upon the data involved; for the assumption made as to the amount of uranium in the nuclei of the haloes was such as to render the higher results the more reliable.

This method is, of course, a radioactive method, and similar to the method by helium storage, save that it is free of the risk of error by escape of the helium, the effects of which are, as it were, registered at the moment of its production, so that its subsequent escape is of no moment.

Review of the Results

We shall now briefly review the results on the geological age of the earth.

By methods based on the approximate uniformity of denudative effects in the past, a period of the order of 100 millions of years has been obtained as the duration of our geological age; and consistently whether we accept for measurement the sedi-

[1] *Phil. Mag.*, March 1907 and February 1910; also *Bedrock*, January 1913.
[2] Joly and Rutherford, *Phil. Mag.*, April 1913.

ments or the dissolved sodium. We can give reasons why these measurements might afford too great an age, but we can find absolutely no good reason why they should give one much too low.

By the storage of radioactive products ages have been found which, while they vary widely among themselves, yet claim to possess accuracy in their superior limits, and exceed those derived from denudation from nine to fourteen times.

In this difficulty let us consider the claims of the radioactive method in any of its forms. In order to be trustworthy it must be true: (1) that the rate of transformation now shown by the parent substance has obtained throughout the entire past, and (2) that there were no other radioactive substances, either now or formerly existing, except uranium, which gave rise to lead. As regards methods based on the production of helium, what we have to say will largely apply to it also. If some unknown source of these elements exists we, of course, on our assumption over-estimate the age.

As regards the first point: In ascribing a constant rate of change to the parent substance—which Becker (*loc. cit.*) describes as "a simple though tremendous extrapolation"—we reason upon analogy with the constant rate of decay observed in the derived radioactive bodies. If uranium and thorium are really primary elements, however, the analogy relied on may be misleading; at least, it is obviously incomplete. It is incomplete in a particular which may be very important: the mode of origin of these parent bodies—whatever it may have been—is different to that of the secondary elements with which we compare them. A convergence in their rate of transformation is not impossible, or even improbable, so far as we know.

As regards the second point: It is assumed that uranium alone of the elements in radioactive minerals is ultimately transformed to lead by radioactive changes. We must consider this assumption.

Recent advances in the chemistry of the radioactive elements has brought out evidence that all three lines of radioactive descent known to us—*i.e.* those beginning with uranium, with thorium, and with actinium—alike converge to lead.[1] There are difficulties in the way of believing that all the lead-like atoms so produced ("isotopes" of lead, as Mr. Soddy proposes to call them) actually remain as stable lead in the minerals. For one thing there is

[1] See Soddy's *Chemistry of the Radioactive Elements* (Longmans, Green & Co.).

sometimes, along with very large amounts of thorium, an almost entire absence of lead in thorianites and thorites. And in some urano-thorites the lead may be noticed to follow the uranium in approximate proportionality, notwithstanding the presence of large amounts of thorium.[1] This is in favour of the assumption that all the lead present is derived from the uranium. The actinium is present in negligibly small amounts.

On the other hand, there is evidence arising from the atomic weight of lead which seems to involve some other parent than uranium. Mr. Soddy, in the work referred to, points this out. The atomic weight of radium is well known, and uranium in its descent has to change to this element. The loss of mass between radium and uranium-derived lead can be accurately estimated by the number of alpha rays given off. From this we get the atomic weight of uranium-derived lead as closely 206. Now the best determinations of the atomic weight of normal lead assign to this element an atomic weight of closely 207. By a somewhat similar calculation it is deduced that thorium-derived lead would possess the atomic weight of 208. Thus normal lead might be an admixture of uranium- and thorium-derived lead. However, as we have seen, the view that thorium gives rise to stable lead is beset with some difficulties.

If we are going upon reliable facts and figures, we must, then, assume: (*a*) That some other element than uranium, and genetically connected with it (probably as parent substance), gives rise, or formerly gave rise, to lead of heavier atomic weight than normal lead. It may be observed respecting this theory that there is some support for the view that a parent substance both to uranium and thorium has existed or possibly exists. The evidence is found in the proportionality frequently observed between the amounts of thorium and uranium in the primary rocks.[2] Or: (*b*) We may meet the

[1] It seems very difficult at present to suggest an end product for thorium, unless we assume that, by loss of electrons, thorium E, or thorium-lead, reverts to a substance chemically identical with thorium itself. Such a change—whether considered from the point of view of the periodic law or of the radioactive theory—would involve many interesting consequences. It is, of course, quite possible that the nature of the conditions attending the deposition of the uranium ores, many of which are comparatively recent, are responsible for the difficulties observed. The thorium and uranium ores are, again, specially prone to alteration.

[2] Compare results for the thorium content of such rocks (appearing in a paper by the author *Cong. Int. de Radiologie et d'Électricité*, vol. i. 1910, p. 373) and those for the radium content, as collected in *Phil. Mag.*, October 1912, p. 697.

difficulties in a simpler way, which may be stated as follows: If we assume that all lead is derived from uranium, and at the same time recognise that lead is not perfectly homogeneous in atomic weight, we must, of necessity, ascribe to uranium a similar want of homogeneity; heavy atoms of uranium giving rise to heavy atoms of lead and light atoms of uranium generating light atoms of lead. This assumption seems to be involved in the figures upon which we are going. Still relying on these figures, we find, however, that existing uranium cannot give rise to lead of normal atomic weight. We can only conclude that the heavier atoms of uranium have decayed more rapidly than the lighter ones. In this connection it is of interest to note the complexity of uranium as recently established by Geiger, although in this case it is assumed that the shorter-lived isotope is genetically connected with the longer-lived and largely preponderating constituent. There does not seem to be any direct proof of this as yet, however.

From these considerations it would seem that unless the atomic weight of lead in uraninites, etc, is sub-normal, the former complexity and more accelerated decay of uranium are involved in the data respecting the atomic weights of radium and lead and the radioactive events which occur in the transmutation of the one into the other. As an alternative view, we may assume, as in our first hypothesis, that some elementally different but genetically connected substance, decaying along branching lines of descent at a rate sufficient to practically remove the whole of it during geological time, formerly existed. Whichever hypothesis we adopt we are confronted by probabilities which invalidate time-measurements based on the lead and helium ratio in minerals. We have, in short, grave reason to question the measure of uniformitarianism postulated in finding the age by any of the known radioactive methods.

That we have much to learn respecting our assumptions, whether we pursue the geological or the radioactive methods of approaching the age of our era, is, indeed, probable. Whatever the issue it is certain that the reconciling facts will leave us with much more light than we at present possess either as respects the earth's history or the history of the radioactive elements. With this necessary admission we leave our study of the Birth-Time of the World.

It has led us a long way from Lucretius. We do not ask if other Iliads have perished; or if poets before Homer have vainly sung, becoming a prey to all-consuming time. We move in a greater history, the land-marks of which are not the birth and death of kings and poets, but of species, genera, orders. And we set out these organic events not according to the passing generations of man, but over hundreds or thousands of millions of years.

How much Lucretius has lost, and how much we have gained, is bound up with the question of the intrinsic value of knowledge and great ideas. Let us appraise knowledge as we would the Homeric poems, as something which ennobles life and makes it happier. Well, then, we are, as I think, in possession to-day of some of those lost Iliads and Odysseys for which Lucretius looked in vain.

SEA-SALT AND GEOLOGIC TIME

By H. S. SHELTON, B.Sc.

The present short article is a reversion to an aspect of the subject of geologic time which I had thought to be settled, and to require no further research or controversy. In my review of Mr. Holmes's book [1] I commented strongly on his ignorance of current literature. I now find that the same imperfect acquaintance with recent discussion and research is shared by the writer who is responsible for putting forward the amount of sodium in the sea as an index of geologic time. I assume, of course, that ignorance is the explanation, for I take it that no man of science of recognised position, when the errors of his research had been pointed out, would deliberately ignore the fact, and proceed as if his work was a valid contribution to the advancement of science. My excuse, therefore, for writing an article containing nothing material which I have not previously published is the following passage, for which Sir Ernest Rutherford and Prof. Joly are jointly responsible:

"But it is certain that, if the higher values so found are reliable, the discrepancy with estimates of the age of the ocean, based on the now well-ascertained facts of solent denudation, raises difficulties which at present seem inexplicable." [2]

The values of geologic time referred to, based on radioactive methods, especially the age of pleochroic haloes, I propose to criticise on a future occasion. There are good grounds, which cannot be stated here, for thinking that all attempts to assess exact times for particular geologic epochs by calculation either of the lead ratios of uranium minerals or otherwise are premature, and are based on an imperfect realisation of the complexity of the subject. The object of the present article, however, is to repeat [3] the arguments which show that the alternative method based on the salt-content of the ocean is of no value whatever.

[1] This journal, July 1913.

[2] *Philosophical Magazine*, May 1913, p. 657.

[3] The previous statements are: *Journal of Geology*, Feb.-March 1910; *Contemporary Review*, Feb. 1911.

Not only is the discrepancy not inexplicable, there is no discrepancy to explain. So much did I take this for granted that, in my last article on the subject,[1] I did not think it necessary to consider the sea-salt method. I therefore take this opportunity to repeat the arguments, and to remedy what is apparently a deficiency.

Prof. Joly's original paper[2] was based on the supposed facts (1) that, as roughly estimated by Sir John Murray, of the solid matter dissolved in river water which reaches the sea 3·47 per cent. is sodium ; (2) that nearly all this hypothetical sodium is obtained by erosion of the rocks ; (3) that when this hypothetical sodium reaches the sea, none of it returns to the rocks. On this supposition, dividing the amount of sodium in the sea by the amount which reaches it each year, an estimate of geologic time could be made. The objection is, briefly, that the three supposed facts are merely supposed facts. No single one of them is reliable.

For convenience we will take the second point first. Of the sodium which actually reaches the sea, a considerable proportion is associated with chlorine. None of the sodium chloride in the rivers can be attributed to erosion. This is so for two reasons. In the first place, it is well known that the proportion of chlorine in the rocks, igneous or sedimentary, is infinitesimal. In the second place, the sources of the chlorine have been thoroughly well determined. In the main, they are two, cyclic salt, carried by the wind from the sea, and salt due to human contamination. It has been found possible, particularly in New York State, to eliminate the cyclic salt, the amount of which is a function of the distance from the coast, and to show that the residual chlorine in river water is a direct function of density of population. Unless you take the sewage from town and country districts directly out to sea, the salt in it inevitably reaches the rivers. If you obtain an abnormally high chlorine ratio when the sewage is supposed to be carried out to sea, the inference is leakage. The source which would naturally occur to any one, brine-springs, has been shown to be negligible. Even in New

[1] This journal, Oct. 1913.

[2] *Trans. Royal Society, Dublin*, vol. 7, pp. 26f. Sir John Murray's paper, *Scottish Geographical Magazine*, 1887. The results are best tabulated for the purposes of this discussion in the "Data of Geochemistry," U.S.A. Geological Survey Bulletin No. 330, p. 88.

York State, where brine-springs are plentiful, there is no appreciable effect on the salt content of the rivers.[1] The only known means by which fresh chlorine reaches the sea is volcanic action, and it is a point open to dispute how much of the volcanic chlorine is not ultimately derived from the sea. It follows, therefore, that, of the sodium which actually reaches the sea, only that not associated with the chlorine can be counted.

This much Prof. Joly and those who agree with his earlier estimate have been willing to admit. But Prof. Joly maintains that, if the chlorine equivalent of the sodium be subtracted, there is still sufficient sodium to necessitate an estimate of geologic time less than 150 millions of years. His reason is merely Sir John Murray's rough tabulation of then current analyses and some more recent results. It does seem strange, however, that Prof. Joly never troubled to inquire whether there were any water analyses sufficiently accurate for his purpose. It is highly probable that Prof. Joly's original paper would never have been written if he had understood why water analyses are undertaken, and the manner in which they are actually performed. Had he been a water analyst, or even a chemist, the first thing that would have occurred to him would have been that these sodium determinations were decidedly hypothetical. Several chemists have expressed doubts as to the validity, but such discussions Prof. Joly has either ignored or failed to understand.[2] It may, therefore, surprise Prof. Joly to be informed that it is doubtful whether the sodium content of any single river water has ever been accurately determined. If any such cases have occurred, they are very few. Let us imagine that there is, in a given sample of river water, two parts of sodium per million. Such a proportion would be quite ordinary according to the usual tables. It would be a very interesting problem to try to separate this out and weigh it. To obtain a good weighable quantity (say ·05 gram of sodium giving about ·15 gram of sodium sulphate[3]) would require 25 litres of

[1] For further information on these points see Jackson's "Normal Distribution of Chlorine," U.S.A. Geological Survey, Water Supply Paper No. 144.

[2] See particularly discussion with Mr. Acroyd, *Chemical News*, 1901, and F. W. Clarke, *Data of Geochemistry*, p. 110.

[3] In ordinary accurate analysis sodium is usually weighed as sulphate. In water analysis, however, the quantity is so small that conversion to sulphate is not worth while. The residue is reckoned as chloride, though it need not necessarily be so.

water, the greater part of a carboy, and the difficulties in the way of isolating it are such as any chemist can understand.

As a matter of fact, the accurate determination of the sodium is a form of amusement in which the ordinary water analysts do not indulge. Sometimes the alkalis sodium and potassium are determined together by difference, that is, not determined at all. In a paper[1] that has been sent to me recently the analyst describes his methods. In this case, everything possible is got rid of by the usual methods of precipitation, and the remainder is evaporated and weighed as "Sodium and Potassium Chlorides." The amount dealt with is only that from 250 cc. of filtered water, and would, of course, be infinitesimal, and the fact that it amounts to not more than 2 or 3 per cent. of the total dissolved solid is a good indication of the general accuracy of the analysis. The residue includes, of course, everything that is not caught by the filter throughout the whole operation. It should be mentioned, also, that the samples usually stand for days in glass bottles. In such cases of water analysis when the sodium and the potassium are separated, the separation is, needless to say, a very approximate operation.[2]

It is no reflection on the accuracy of river water analysts to say that the results are of no value whatever for Prof. Joly's purpose. No one, except Prof. Joly and a few geologists, wants to know the proportion of sodium in river water. It is at the same time the constituent least important for the purposes of the water analyst and the constituent most difficult to determine. The assumption on which Prof. Joly proceeds, that 3·47 per cent. of the dissolved matter in river water is sodium, is absolutely unproven. For all the analyses prove, it might be less than half that amount. Indeed the principal evidence that there is an excess of sodium over and above its equivalent of chlorine is indirect rather than direct. The results of rock analyses are more reliable and it seems to be established that the sodium content of igneous rocks is greater than that of the aqueous. As a matter of fact it has been pointed out by Prof. Dubois that,

[1] "The Quality of the Surface Waters of Illinois," U.S.A. Geological Survey, Water Supply Paper No. 239, p. 16. The method is the one usually recommended in the text-books. See Wanklyn, 11th edition, p. 122.

[2] The potassium determination would be the more accurate of the two, as the potassium is weighed as Pt. + 2KCl. A further percentage of inaccuracy would thus be thrown on the sodium.

when there is any reason to ascribe special accuracy to river-water analysis, the excess of sodium diminishes and tends to vanish. Prof. Dubois[1] collected a number of good analyses, tabulated them, and inferred from them, according to Prof. Joly's method, a geologic time of 400 millions of years. The inference he made, as he was a believer in Lord Kelvin's methods, was that the original sea was salt. The true inference is that the method is of no value. Within the limits of experimental error you can deduce any value you please.

Though the previous discussion renders it unnecessary, it is as well to mention one other point. The assumption that no sodium returns from the sea to the rocks is unwarranted. Indeed one instance to the contrary can be mentioned. It is a recognised fact that much of the salt in the salt lakes, and inferentially in salt beds, is windborne and has its origin in the sea. But what would occur when strata containing salt beds are subject to metamorphosis or are absorbed by the magna? Is it not obvious that the sodium would be added to the content of the rocks and that the chlorine would be expelled as some volatile compound? Indeed, is it not probable that some portion of volcanic chlorine has this origin? Again, with regard to the ordinary processes of the formation of sedimentary rock, we do not know enough to say that no dissolved sodium is reabsorbed.

This speculation, however, is a side-issue, and is not necessary to the argument. Were the analyses of sufficient accuracy, were the method in general valid, such matters would require careful consideration. At present, without taking such remote speculations into account, we can still say that the sea-salt method is absolutely worthless. It is based on a misapprehension of the data on which it rests. It is an instance of the care that is required when results are transferred from one branch of science to another. With regard to geologic time, the value of radio-active methods is still to be determined. The value of the sea-salt method, like the still more famous ones of Kelvin and Tait, is nil.

[1] *Proceedings Amsterdam Academy*, 1904.

A REVIEW OF IGNEOUS ROCK CLASSIFICATION

By G. W. TYRRELL, A.R.C.Sc., F.G.S.
Lecturer in Mineralogy and Petrology, Glasgow University

The recent publication of the second volume of Prof. J. P. Iddings' great work on Igneous Rocks (Description and Occurrence) marks an epoch in the history of petrological science. It is a gallant attempt to infuse the quantitative spirit into the dry bones of the older and laxer system of igneous rock classification, and also to correlate the Quantitative Classification invented by Iddings, in collaboration with Cross, Pirsson, and Washington, with the older qualitative system. In the writer's opinion, it is a failure in the sense that it fails to reconcile irreconcilables, but it is a great failure which will do much to turn the mental outlook of petrographers from the comparatively barren qualitative past to the hopeful and fruitful quantitative future.

In view of the publication of this work, the present seems an appropriate time to re-examine the American Quantitative Classification and other classifications from a point of view frankly sympathetic to the quantitative idea. It may be taken that all petrologists are now more or less familiar with the main lines of the American Quantitative Classification, and it is therefore unnecessary to describe the system in detail. Many petrologists have used it as an auxiliary to older methods of classification, but none, so far as I am aware, have altogether dispensed with the latter. The persistence and vigour of the qualitative system at the present time, although the American Quantitative Classification has now had ten years for its trial, requires further explanation than the inherent conservatism of petrographers. In the writer's opinion, the Quantitative Classification has not displaced the older system because it does not provide a ready means of classification for the working petrographer. The chemical analysis is the unit of the system, and it is only possible for the petrographer to obtain analyses of a small proportion of the rocks he describes. The greater part of his work deals with the actual mineral composition or mode, and the classification immediate to his needs must therefore be based on the mode, and not on the norm (theoretical mineral composition) derivable only from a chemical analysis or from determinations equivalent to chemical analysis.

This view does not necessitate the abandonment of the American Quantitative Classification. The latter provides a final court of appeal for cases in which the mode is indecisive or indeterminate. It is, so to speak, a regularly-reticulated background on which we may trace the magmatic characters of the rocks; or, to change the figure, it is a more or less convenient system of pigeon-holing by which we may docket our rocks according to their magmatic characters.

The advantages conferred by the American Quantitative Classification and its authors on petrological science are many and various. They exposed the lax, unsystematic character of the older qualitative classification, and the chaotic condition of its nomenclature. They forced a quantitative view-point on petrographers and showed that the comparative method in petrology was almost impossible under the qualitative régime. Their criticisms have effected an immense improvement in the technique of rock-analysis, and it is safe to predict that in any new collection of rock-analyses the proportion of "superior" to "inferior" examples will be much greater than in those already published. They have insisted on the importance of the chemical analysis from a petrological point of view, and have raised its status from a mere ornamental but usually inaccurate adjunct to petrological work to that of an essential and indispensable part. The recognition of this changed status is the probable cause for the great increase in the number of analyses of igneous rocks now made. Furthermore, the exact, detailed, and systematic methods of the authors of the American Quantitative Classification have caused a great improvement in descriptive work. Only of late years has it become possible strictly to compare rocks from the ends of the earth simply from their published descriptions; and for this most desirable consummation the more exact and detailed mineralogical and textural description, and the publication of modal proportions, insisted upon by the authors of the Quantitative Classification, have undoubtedly been largely responsible. Finally, they have given petrographers an instrument of inestimable value in the conception of the norm.

The American Quantitative Classification starts from the hypothesis that there are no "natural" lines of division in igneous rocks on which a classification may be based; that igneous rocks constitute a continuous field in all directions, and are only capable of an arbitrary division into compartments of equal value, just as, for example, is the scale of temperature.[1] Dr. Cross's discussion of the adequacy of certain factors as a basis for classification on "natural lines" seems to me conclusive that there are no suitable factors save, perhaps, two.[2] The factors of geographic distribution, magmatic differentiation, eutectics, mineral composition and texture, are passed in review and dismissed as affording no suitable "natural" basis for rock-classification. The chemical composition, however, is treated as the most fundamental character of igneous rocks, and as the one most susceptible of arbitrary subdivision. The American Quantitative Classification has therefore been based on chemical composition so manipulated as to give a mineralogical expression. The authors of this classification have never, in the writer's opinion, given sufficient weight to the possibilities of a classification by actual mineral composition, or, in other words, a modal classification on quantitative lines. It is to be admitted that the presence of minutely-crystallised or uncrystallised matter, and the occultation of certain minerals in others, are grave difficulties; but these could probably be overcome by certain expedients of which an outline is given later. In any case, the difficulties so caused would probably not be so great as those caused by the omission of the alferric minerals, for example, from the norm on which the American Quantitative Classification is based.

From the standpoint of a utilitarian classification it is certainly better to accept the American view of igneous rocks as a "continuous series of chemical solutions and their solidified phases" rather than continue to apply "the misleading biological concepts of 'families' and 'descent'"; although it must be admitted that many of the solidified solutions are related by processes of differentiation from a common magma, and that there may be a concentration of rock-types in certain parts of the classificatory field.

[1] *Quantitative Classification of Igneous Rocks*, by W. Cross, J. P. Iddings, L. V. Pirsson, and H. S. Washington. Chicago, 1903.

[2] "The Natural Classification of Igneous Rocks," *Quart. Jour. Geol. Soc.* lxvi. 1910, pp. 470-506.

However manipulated, the chemical analysis is the unit dealt with in the American Quantitative Classification. The analysis of an igneous rock is first calculated into a set of standard minerals (the *norm*), under fixed rules which, it is generally admitted, follow and succinctly express most of the laws of mineral formation in igneous magmas as we know them. Certain important rock-forming minerals (the *alferric*—augite, hornblende, biotite, muscovite, etc.) are not utilised in the norm because of their complex chemical composition, although they may actually be present in the rock. They are split up and their components distributed to the normative minerals. The norm is therefore a possible mode of crystallisation of all magmas under certain conditions. Thereafter the classification proceeds by taking factors from the norm two at a time, and applying them consistently throughout (with one exception explained later). It follows that the American Quantitative Classification is a classification of chemical analyses or magmas, not of the actual rocks. It is in effect a normative classification, as contrasted with the modal classification which is forced on the working petrographer by the sheer impossibility of obtaining chemical analyses of all the rocks he wishes to describe.[1]

The norm is first divided into salic (quartz, felspars, felspathoids) and femic (pyroxenes, olivine, ores, etc.) groups, whose relative proportions furnish the first line of subdivision into Classes. Five Classes, bounded strictly by arithmetical ratios between the salic and femic groups, are thus formed, and express quantitatively Brogger's division of igneous rocks into leucocratic and melanocratic phases; not, as stated by Cross, the old subdivision into acid, subacid, sub-basic, basic, and ultrabasic. The first three Classes are then each divided into nine Orders on the basis of the ratios of normative quartz *or* lenads (felspathoids) to the felspars present. This really expresses the variations of rocks in respect to the ratio between alkalis and silica. Assuming the ferromagnesian minerals and anorthite to have their necessary quota of silica, the presence of quartz or felspathoid in a rock depends on whether the remaining silica does or does not exceed that necessary to the formation of alkali-felspar. The latter may be independent, or

[1] The utility of the norm is unquestionable. As an instrument for comparing rocks, especially those it is impossible to compare modally, it is of great value, as well as for other purposes.

in combination with anorthite as a mixed felspar. Classes IV. and V. are divided into Orders on the basis of certain ratios between the femic minerals, an arrangement criticised later in this paper.

The Orders in Classes I., II., and III. are subdivided into five arithmetically bounded compartments, called rangs, based on the molecular proportions of potash and soda as against lime in the salic minerals; that is, on the ratio between alkali-felspars plus lenads to anorthite. The rangs therefore quantitatively express the relations between alkali-felspars plus felspathoids to anorthite felspar in igneous rocks, less accurately the relation between alkali-felspars and felspathoids to plagioclase felspar. They give a quantitative expression to the conception of "alkalic" and "calcic" types amongst igneous rocks. The rangs are further subdivided, on the same five-fold basis, into compartments called subrangs, according to the ratio between the salic potash and soda. The subrangs therefore express the quantitative relations between orthoclase and leucite on the one hand, to albite and nepheline on the other—that is, between the potassic and sodic constituents of magmas. Subdivision of the subrangs is made into grads which depend on ratios subsisting between the subordinate femic minerals. The grads have been very seldom used.

In Classes IV. and V. the orders, sections of orders, rangs, and subrangs are based on certain ratios subsisting between the predominant femic constituents, which it is not necessary to particularise here. Further subdivision into grads is based upon the proportions obtaining among the subordinate salic minerals.

Other refinements of classification are also to be found in the system, as, for example, the formation of sub-classes in Classes I., II. and III. for the reception of rocks rich in corundum, zircon, etc.

Many criticisms of this classification have been made. The commonest, perhaps, is that it is "arbitrary," "unnatural," "a priori," and constructed without regard to the possible discovery of a "natural" mode of classification. This is the gist of Harker's unsparing criticism.[1] In answer C.I.P.W.[2] admit that

[1] *Natural History of Igneous Rocks*, 1909, pp. 362–6.

[2] A convenient contraction for the names of the authors of the Quantitative Classification—Cross, Iddings, Pirsson, and Washington.

the system is arbitrary, but that it is necessarily so since igneous rocks are insusceptible to a quantitative treatment on any "natural" or genetic factor as yet discovered, and constitute a uniform field in all directions, only capable of arbitrary subdivision into compartments of equal value. The discussion thus shifts to the question of the possibility of a "natural" quantitative classification expressing the genetic relations of igneous rocks. As the result of a rigorous discussion, Dr. Cross concludes that a natural basis for classification, whilst desirable, is impossible in view of the unsuitability of most of the factors proposed.[1] It must also be admitted that at present no suitable factors for quantitative treatment have emerged, save chemical and mineralogical composition, and that it is highly improbable any further "natural" basis for classification will be discovered in the future. But since the needs of petrographers are immediate, it appears necessary to fall back on an arbitrary mode of classification, even if only for purposes of reference and comparison.[2] Apart from the question as to whether there are any natural bases for classification, petrographers cannot afford to wait for their discovery. In the meantime, for want of an exact classification by which rocks may be compared, the science of comparative petrology, and the consideration of such important problems as that of petrographic provinces, may come to a standstill. It is of the utmost importance, therefore, to come to a decision on this question of classification, if only from the utilitarian point of view.

Another class of criticism of the American Quantitative Classification is that which centres round the norm and its use in the system. The norm is a set of standard minerals calculated from the analysis of a rock, and has been criticised as "hypothetical," "artificially selected," "ideal," even as "imaginary." To this Dr. Cross replies that the minerals of the norm are largely those which are believed by most petrographers to be present as definite compounds in the magma, and which singly, or variously combined, form the minerals actually present in the rock. There is, however, some point in the criticism, since petrographers have to deal with rocks composed of minerals, and not with magmatic molecules which are necessarily somewhat hypothetical. Dr. Cross also points out that the norm

[1] *Quart. Jour. Geol. Soc.* vol. 66, 1910, pp. 470–506.

[2] J. W. Evans, in discussion of Cross's paper, *ibid.* p. 504.

"is primarily a means of comparison, and has in itself nothing to do with system."[1] Many petrographers have found it extremely useful for this and other purposes, quite apart from classification.

The mode of calculation of the norm has received very little criticism save that of Evans. In the main it corresponds with the well-understood principles of the crystallisation of minerals from igneous magmas. Evans considers the operation of calculating the norm instructive but unreal, and as not comparing in educative value with that of calculating the mode. He also points out the undue influence of the amount of alumina and the state of oxidation of the iron on the result of the calculation. In fact the bulk of Evans's criticism is that the mode of calculation and subsequent subdivision of the norm causes certain constituents to have an undue effect on the classification, and therefore that "the lines of division of the Quantitative Classification do not stand in any logical relation to the chemical composition."[2]

Other criticism concerns the selection of certain minerals to form the "salic" and "femic" groups of the norm. Evans believes that "the salic group [is] in fact a collection of minerals which [have] nothing essential in common, and that the fundamental lines of the classification [are] accordingly practically meaningless." He instances especially the cases of corundum and anorthite. In the calculation of the norm the excess of alumina over that necessary for the formation of normative felspars and felspathoids is regarded as corundum and placed in the salic group. Generally, however, this excess enters pyroxenes, amphiboles, or micas, and should thus be regarded as femic. Anorthite, too, is considered as a basic silicate which should enter the femic group, in spite of the fact that it generally occurs in isomorphous admixture with albite. Nevertheless, the division of the norm into salic and femic groups expresses a common variation in igneous rocks which gives rise to what are known as leucocratic and melanocratic facies. It is doubtful, however, whether this factor should be given first place, as it is in the American Quantitative Classification.

A further line of criticism is that the Quantitative Classification does not fulfil its declared purpose of bringing like rocks

[1] *Quart. Jour. Geol. Soc.* vol. 66, 1910, p. 496.

[2] Science Progress, vol. i. 1907, p. 275.

together. It is pointed out that even the subrangs contain a great diversity of rocks, exhibiting wide variations in almost every constituent, and that the qualitative nomenclature is correspondingly varied. The latter is in part due to "the indefiniteness, confusion, and redundancy of modern nomenclature," as shown by C.I.P.W., and admitted by Evans; partly to the inclusion of all rocks chemically belonging to the subrang whatever their texture, which is an important factor in qualitative nomenclature. The wide variations in chemical composition, however, are considered by Evans to be due to defects in the classification brought about by the mode of calculation and the subdivision of the norm. Such variations are inherent and inevitable in any classification on a quantitative basis, even in the smaller compartments. The compartments of similar size in the qualitative classifications (such as Hatch's) probably contain an even wider range of rocks. It is to be remarked, however, that the method of the Quantitative Classification does not admit of overlapping between the various compartments, and that each compartment contains rocks differing from those of any other.

In the investigation of an abnormal manganiferous series of igneous rocks from India, Dr. L. L. Fermor found the Quantitative Classification to lack the elasticity necessary to accommodate the series, whilst he found it quite easy to fit it into Hatch's classification. This, however, is very natural, since Hatch's classes are based on silica percentage and are extremely comprehensive. The wider the compartments of a system, the easier it is to place a given rock.

The type of Fermor's kodurite series is a phaneric rock consisting of orthoclase, a manganese garnet (spandite), and apatite, and is the basic member of a series of differentiated igneous rocks ranging in acidity from quartz-orthoclase rocks, through intermediate quartz-kodurites and basic kodurites, to manganese-pyroxenites and garnet-rocks. The manganese content in the analysed rocks of this series ranges from 10·50 to 0·98, and in the typical kodurite is 9·08. The calculation of the norms of these rocks presented some difficulty, which was got over by introducing tephroite (manganese-olivine) as one of the normative molecules. Fermor then shows that the manganese, although fourth in importance in the chemical analysis, and third in the norm, does not affect the classification until the ninth subdivision (section of subsection of section of subgrad!) is reached. Moreover the two kodurites analysed fall into different rangs, andase and monzonase, which depend on the ratio of salic alkalis to salic lime; whereas the chief difference between the two rocks depends on the composition of their respective garnets. Fermor therefore concludes [1] that the American Quantitative Classification fails in one instance at least to exhibit the elasticity which is claimed as an advantage of the system. He infers that unusual amounts of other rare constituents, such as baryta, strontia, nickel oxide, etc., would also affect the system adversely. It is to be remarked, however, that a factor (such as MnO) which is of very minor importance in the vast majority of igneous rocks, should also be of minor importance in classification, notwithstanding its occasional abundance in an abnormal and rare series of rocks. Essentially Fermor demands that a classificatory factor should vary in importance according to its significance in the rocks which it is proposed to classify. But no consistent logical classification could be erected on such a basis. Classification must be based on the factors dominant, in significance and most frequently also in mass, in the whole series of igneous rocks; and it is these factors which are used in the American Quantitative Classification, and which will have to be used in some form or other in all quantitative systems.

The American Quantitative Classification itself is not exempt from a similar error; and to this is due an asymmetry of the system which does not yet appear to have been pointed out. I refer to the radically different method of forming the orders in Classes IV. and V., as compared with Classes I., II., and III. In the latter the orders are based on the ratios of quartz or lenads to felspars; in the former on the ratios of pyroxenes plus olivine to the mitic minerals (magnetite, ilmenite, hæmatite, titanite, etc.). This introduces a most confusing break between Classes III. and IV., renders the compartments incommensurate on either side of the partition, and makes the tracing of a petrographic series from Classes I., II., and III. into Classes IV. and V. almost impossible. Employing the figures already utilised, the pigeon-holes in Classes I., II.,

[1] L. L. Fermor, "The Systematic Position of the Kodurite Series, especially with reference to the Quantitative Classification," *Records Geol. Surv. India*, xlii. 1912, p. 210.

and III. are of the same size and shape, and are built of the same material; those in Classes IV. and V. are of entirely different size and shape, and are built of different material. Or three-fifths of our reticulated background is drawn to the same pattern; two-fifths to an entirely different pattern.

The reason for changing the basis of the orders is that quartz, lenads, and felspars cease to be the dominant minerals in Classes IV. and V., and give place to pyroxenes, olivines, etc. The change, however, not only transgresses the principle that a single pair of factors should be applied consistently throughout, but also the principle that classification should be based throughout on factors which are dominant in the whole series of igneous rocks. It can hardly be denied that the salic (or felsic) minerals are by far the most abundant and significant constituents when the whole field of igneous rocks is considered. Taking the general mean of all analyses of igneous rocks as calculated by Clarke,[1] and as recalculated by Cross into the norms,[2] we find that the four salic constituents total 79 per cent., and the femic 21 per cent. If the bulk of the various igneous rocks were taken into account it is possible that the ratio of salic to femic constituents would be still higher.[3] The primary factors in the classification therefore should be the salic ratios, and the femic ratios should only be used after the salic are exhausted.

The convenience, simplicity, and correctness of using the salic ratios right through the Quantitative Classification can be shown by the consideration of some petrographic series which traverse the partition between Classes III. and IV. The magmatic symbols invented by C.I.P.W. are found very useful in this connection. The subrang salemose, for example, is represented by the symbol II.6.3.4., indicating that a rock in salemose falls into Class II. (dosalic), Order 6 (lendofelic), Rang 3 (alkalicalcic), and Subrang 4 (dosodic), and thus has a definite magmatic character. The persistence of some of these numbers, or their sympathetic or antipathetic variation one with the other, in the symbols of a related series of rocks, indicates the persistence or regular variation of certain magmatic characters. The consanguinity of the members of certain

[1] "Some Geochemical Statistics," *Proc. Amer. Phil. Soc.* xli. 1912, pp. 214–34.

[2] *Jour. Geol.* xx. 1912, p. 759.

[3] *Mennell. Geol. Mag.* 1904, p. 263; 1909, p. 212.

petrographic series may be admirably illustrated in this way, as is seen in the lists given below, which are compiled from tables in Iddings' *Igneous Rocks*, vol. ii. These series have been selected from small and isolated areas, so as to ensure, as far as possible, that the rocks belong to a related petrographic series, and that there has been no admixture of rocks of different ages.

I

REUNION ISLAND (Iddings, pp. 589–90)

Phonolitic Trachyte[1]	I'.5.1.'4.	
Phonolitic Trachyte	I'.'5.2.4.	
Quartz-ægirite-syenite	(I)II.4'.1.3(4).	
Akerite	(I)II.5.2.4.	
Olivine-trachyandesite	II.5.2.4.	
Ophitic Basalt	II'.5.3.4.	
Basalt	II(III).5.4.4.	
Essexitic Gabbro	(II)III.'5.3'.4.	
Gabbro	III.5.4.4'.	
Basalt	III.5.'4.4.	
Olivine-gabbro	III'.5.4(5).'5.	
Felspathic picrite	'IV.1'.$_4$.1'.2.	['IV.5.'4.4.]
Basalt	'IV.'2.'$_4$.1'.2.	['IV.5.'4.4.]
Harrisite	IV.1.$_4$.1'.'2.	[IV.5.4.3'.]

II

NEW CALEDONIA (Iddings, p. 652)

Hornblende-anorthosite	I'.5.4.5.	
Anorthosite	I.5.'5.5.	
Hornblende-gabbro	II.5.4.'5.	
Hornblende-gabbro	III.4.4.'5.	
Norite	III.5.'5.4'.	
Ouenite	III.5.5.5.	
Hornblendite	'IV.'2.$_3$.2.2.	['IV.5.4.(4)5.]
Diallagite	IV.1'.'$_2$.2.2.	[IV.5.'4.(4)5.]
Bronzitite	'V.1.$_1$.1.1'.	['V.5.4'.4.]

III

TAHITI (Iddings, p. 653)

Phonolite	I'.5.1'.4.
Tinguaite	I'.6.1.4.
Nepheline-syenite	'II.'6.'2.3.

[1] The use of round brackets in a symbol indicates that the magma is transitional, and near the border line between two compartments. Thus I(II.) indicates a rock in Class I., but transitional to Class II. The use of dashes before or after a figure indicates that the rock is intermediate between the division in which it actually falls and the preceding or succeeding one. Thus II.' indicates a rock in Class II., but intermediate towards III. II.(III.) would indicate a rock in Class II. still closer to III. See C.I.P.W., *Journ. Geol.* xx. (1912), pp. 550–61.

Hauynophyre	II.6′.′2.4.	
Camptonite	II.6′.2.4.	
Nepheline-monzonite	II.′6.(2)3.4.	
Nepheline-gabbro	II.′6.3.4.	
Microgabbro	III.5.3′.4.	
Basalt	III.5.′4.4.	
Felspar-basalt	III.6.3.4.	
Microlitic Picrite	IV.1′.$_{4}$.1′.2.	[IV.5.4.4.]
Essexitic Gabbro	′IV.2.$_{2}$.2.2.	[′IV.6.′4.4(5).]

IV

MONTEREGIAN HILLS, QUEBEC (Iddings, p. 375)

Nordmarkose	I′.5′.1′.′4.	
Laurvikose	I′.5.2.4.	
Laurdalose	′II.6.1.4.	
Akerose	′II.5.2.′4.	
Akerose	II.5.2.4.	
Essexose	II.6.2.4.	
Andose	II.5.′3.′4.	
Andose	II.5.′3.4′.	
Hessose	′II.5.4′.′5.	
Palisadose	IV.1.$_{2}$.2.2.	[IV.5(6).4.4.]
Yamaskose	IV.2′.′$_{2}$.′3.2′.	[IV.5′.′4.4.]
Yamaskite	IV.′3.$_{1}$.(2)3.3.	[IV.5(6).4.4.]

V

HAWAII (Iddings, pp. 654–5)

Trachytic Oosidian	(I)II.5′.1.4.	
Felspathic Lava	(I)II.5.2.4.	
Andesitic Basalt	II.5.3.4.	
Andesite	II.5(6).2.4.	
Trachydolerite	II′.5.3.4.	
Lava	II′.5.3.4(5).	
Hornblende-basalt	II(III).5.2.4.	
"Pelee's hair"	II(III).5.3.4(5).	
Lava	II(III).5.3′.4(5).	
Andesite	II(III).′6.3.(3)4.	
Kauaiite	III.′5.2.4′.	
Basalt	III.5.3.4.	
Basalt-pumice	III.5.3.4(5).	
Basalt-obsidian	III.5.3′.4(5).	
Basalt	III.5.4.4–5.	
Basalt	III′.5.4.4–5.	
Scoria	III(IV).6(7).2(3).4(5).	
Olivine-basalt	(III)IV.1(2).$_{2}$′.1(2).2.	[(III)IV.5.4.4(5).]
Lapilli	(III)IV.2.$_{4}$.2′.2′.	[(III)IV.9.1.4′.]
Basalt	′IV.1(2).′$_{3}$.2.2.	[′IV.5(6).4.4′.]
Olivine-basalt	′IV.1′.′$_{3}$.1(2).2.	[′IV.5.4.(4)5.
Nepheline-melilite-basalt	′IV.2$_{(3)4}$.2(3).2(3).	[′IV.9.1.4.]
Gabbro	IV.1(2).1′.2.	[IV.5.′4.(4)5.]
Nepheline-melilite-basalt	IV.2.′$_{3}$.2.2.	[IV.7′.3.(4)5.]

In each of these series a great contrast is observed between the symbols of rocks in Classes IV. and V., and those in Classes I., II., and III.; but when the salic ratios as used in Classes I., II., and III. are also used for the calculation of rocks in Classes IV. and V., the symbols thus obtained show a great congruity with those obtained for the more salic rocks (compare symbols within square brackets with those of the rocks in Classes I., II., and III. preceding). For example, in the first eleven rocks of Series I. it is clearly seen that, while the Classes range from I. to III. (persalic to salfemic), the orders remain indicated by 5 (perfelic) with the exception of one which is 4′ (intermediate between 4 and 5); and eight of the rocks are dosodic (indicated by 4 in the last figure of the symbol). The remaining two are sodipotassic transitional to dosodic 3 (4), and persodic near dosodic (′5). The third figure, however, varies with the Classes, ranging from peralkalic (1) to docalcic (4), and illustrates admirably the usual increase in the anorthite molecule of the felspars concomitantly with increase in the proportions of the femic constituents. The last three rocks of the series fall into Class IV. (dofemane), and their symbols, calculated by the methods of the Quantitative Classification for rocks in Classes IV. and V., are hopelessly incongruous with those of the other ten. If, however, they are calculated by the methods used for rocks in Classes I., II., and III.—that is, on the ratios obtaining between the salic constituents—their relationships with the rest of the series at once become evident. They are perfelic and dosodic, and thus fall in with the magmatic character of the salic portion of the series. The magmatic character of the whole series is therefore well defined. With a variation from persalic to dofemic there is a sympathetic variation from peralkalic to docalcic, whilst the rocks throughout the series remain dominantly perfelic and dosodic.

Similar analysis of the Series II., III., and IV. affords similar results. The magmatic character of Series II. (New Caledonia) is exceptional in that the rocks remain docalcic to percalcic throughout, although the classes range from persalane (I.) to perfemane (V.). In Series V. (Hawaii) five of the rocks in dofemane (IV) give results which prove them to be the continuation of that part of the series which falls in Classes I., II., and III. The remaining two, however, do not conform to the series so far as is indicated by the magmatic symbol obtained from the salic ratios. It is not clear whether this is due to the rocks belonging to a different series, to a defect in the method of obtaining the symbol, or to defects in the method of calculating the norm.

It is thought that enough has been adduced to show that the utilisation of the salic ratios throughout the classification, instead of only in Classes I., II., and III., as at present, would at least give better results than the present method, especially in indicating serial relationships between rocks. Moreover it would do away with an awkward asymmetry in the Quantitative Classification as at present constituted.

It may be objected that as some rocks in perfemane are totally devoid of salic constituents it would be impossible to treat them as suggested in the foregoing discussion. This is certainly true; it is a defect inherent in a dichotomous method. The difficulty also occurs in the Quantitative Classification in the pure quartz-rocks at the other end of the series. Here it is impossible, or at least inexpedient, to carry the symbol representing the magmatic position of the rock beyond the Order. Thus a quartz-rock from Secucuniland, South Africa, and a quartz-granite from Eskdale, Cumberland, are represented by the symbol I. 1. –. – (see Iddings, *Igneous Rocks*, vol. ii. p. 31). Similarly at the other end of the series, and under the method of calculation adopted in the foregoing discussion, rocks devoid of salic constituents would simply have the symbol V. –. –. –., indicating the Class in which they fall. The matter is of small practical importance. For example, it would affect only five rocks out of 34 belonging to Class V. (perfemane) in Iddings' tables of rock-analyses—that is, five out of over 2,000, considering the whole series of analyses.

Suggestions towards a Quantitative Modal Classification

It will be evident from the foregoing that the writer considers the only classification which will meet the immediate needs of petrographers is one that is at once quantitative and modal. A quantitative classification is admittedly purely utilitarian. However constructed, it will probably "correspond to nothing that has occurred in the evolution and differentiation of igneous rocks," but it is none the less necessary for their comparative study. Petrographers will always require a pigeon-holing arrangement whereby they may accurately docket rocks of mineralogical or chemical similarity. Furthermore a modal classification, whatever its difficulties, is the one based on the characters most readily elicited from the rocks themselves. It will, at any rate, be more convenient, save for special purposes, than a normative classification which requires a tedious process of chemical analysis before a rock can be placed in its proper position.

Difficulties at once present themselves when a quantitative classification on modal lines is considered. The texture of many rocks is such that the mode cannot be elicited at all; and in the case of merocrystalline and porphyritic rocks, Iddings has justly pointed out the anomalies that result from a classification based simply on the identifiable crystalline constituents regardless of the composition of the minutely crystalline or non-crystalline remainder.[1] Furthermore a difficulty arises in the fact that the same mineral species may vary considerably in chemical composition in different rocks. The phenomenon of "occult" minerals is in this connection to be considered as a special case of variable chemical composition.

Before any system can be applied it is necessary, therefore, to consider the methods by which the mode may be obtained from various kinds of rocks. With the holocrystalline granular rocks there is very little trouble. The graphic method of quantitative mineral measurement invented by Rosiwal yields sufficiently accurate results for classificatory purposes in the great majority of types. It might here be said that the degree of accuracy required in the estimation of the mode depends on the size of the classificatory divisions it is proposed to erect. If the latter are to be as comprehensive as, say, the subrangs of the American Quantitative Classification, a great degree of accuracy is not necessary. In many cases the experienced petrographer can estimate the quantitative mineral composition of the rock from a careful examination of the thin sections with sufficient accuracy for this purpose.

The expression of the mode in aphanitic rocks is more difficult, since they are frequently insusceptible to the Rosiwal method. The approximate mode of a quite fine-grained rock, however, may be obtained by this method provided that it is holocrystalline. It must be admitted, however, that with the

[1] *Igneous Rocks*, vol. ii. 1913, p. 72.

material at present available to the petrographer it is inevitable that chemical analysis will have to be resorted to in order to determine the mode of an aphanitic rock. In any modal classification on quantitative lines it seems necessary to assume that the plutonic holocrystalline rocks are those affording the primary units for classification, and to treat the aphanitic rocks as textural modifications of these. In the case of porphyritic rocks the proportions of different kinds of phenocrysts to one another and to the groundmass could easily be estimated by the Rosiwal method. There is need for the chemical investigation of a large series of typical groundmasses with a view to the determination of their average modes. These could be used in calculation in much the same way as the analyses of pyroxenes, amphiboles, and micas collected by the authors of the Quantitative Classification are used for the purpose of modal calculation.[1]

It is admitted that the treatment of the aphanitic rocks on modal lines constitutes a serious difficulty in the way of modal classification. It is a difficulty, however, which may be largely removed by future work. Furthermore, it is believed that an estimation of the mode of many aphanitic volcanic and hypabyssal rocks can be made with sufficient accuracy for classificatory purposes. For example, generally with the aid of a Rosiwal measurement supplemented by calculation from the chemical analysis, Washington has estimated the modes of many fine-grained leucitic and other lavas from Italy.[2] In the description of aphanitic rocks even an approximate estimation of the mode by a careful examination of the thin section would be better than no estimation at all, and would help to fit the rock into one of the larger compartments of the quantitative modal classification.

Notwithstanding the opinion of Williams,[3] the Rosiwal method of estimation is capable of giving extremely accurate results.[4] The writer has recently used it for the investigation of teschenites and related rocks of Central Scotland, and has found the chemical composition calculated from the results of the Rosiwal measurement to be strikingly accordant with that obtained by ordinary chemical analysis.

[1] C.I.P.W., *Quantitative Classification of Igneous Rocks*. Tables XII.-XIV.

[2] *The Roman Comagmatic Region*, Publications Carnegie Inst., No. 57.

[3] *American Geologist*, xxxv. 1905, pp. 34-46.

[4] F. C. Lincoln and H. L. Rietz, *Economic Geology*, viii. 1913, pp. 120-39.

The fact that certain mineral species may vary somewhat in chemical composition ceases to be an objection to modal classification when chemical composition is deposed from the position of chief factor in quantitative classification. For a purely utilitarian purpose of classification the petrographer after all is not so much concerned with comparing the chemical composition of rocks as in comparing their modes. If the rocks be treated simply as classifiable objects, the differences in the chemical composition of certain mineral species become as immaterial as the individual differences between animals of the same species; or if a higher grade of importance be assigned to these variations they may be compared to varietal differences in the same species.

To take a classic example, the hornblende-rock of Gran is as well, or perhaps better, classified with other hornblende-rocks than with the camptonite associated with it in the field, although it is nearly identical in chemical composition with the latter rock. It is better to classify a rock consisting of hornblende with other hornblende-rocks, even though they differ somewhat chemically, than to classify it with a rock consisting essentially of plagioclase and hornblende.

That magmas of identical chemical character may and do crystallise into two or more different mineral aggregates in response to differing conditions is an argument rather for separating the various products in classification than for bringing them together. A chemical classification only takes into consideration the chemical composition of the magma from which the rocks have been derived. A classification taking cognisance of the modal habit of the rock also takes into consideration the conditions under which solidification occurred, and therefore has a more genetic character than the chemical classification. A modal classification treats the rocks as mineral aggregates with a history; a chemical classification treats them merely as magmas.[1]

Having obtained the mode or actual mineral composition of

[1] C. P. Berkey, speaking of the Quantitative Classification, says: "It is a splendid magmatic classification scheme. But in real life we are dealing not with magmas so much as the products of magmas which, because of differing conditions, have given rocks that do not usually agree wholly with their theoretical behaviour."—"Objects and Methods of Petrographic Description," *Economic Geology*, viii. 1913, p. 701.

our igneous rocks, the next step is to consider the best method of subdivision and classification. It is argued in this paper that as the felsic constituents form more than 79 per cent. of the mass of igneous rocks, the dominant factors in classification should be based on them, and that the subordinate mafic constituents should have a correspondingly subordinate place. Consequently the main classes must be based on felsic ratios. Fortunately we have already to hand the main lines of an excellent classification devised by Iddings on this basis in his new book. His principal divisions are as follows:

Division I.	Rocks characterised by quartz.		
,, II.	,,	,,	quartz and felspars.
,, III.	,,	,,	felspars.
,, IV.	,,	,,	felspars and felspathoids.
,, V.	,,	,,	felspathoids.
,, VI.	,,	,,	mafic minerals.

These divisions have definite quantitative limitations. In all save the last, the ratio of felsic to mafic constituents must exceed three to five. Then in

Division I. $\frac{\text{Quartz}}{\text{Felspars}} > \frac{5}{3}$

,, II. $\frac{\text{Quartz}}{\text{Felspars}} < \frac{5}{3} > \frac{1}{7}$

,, III. $\frac{\text{Quartz } \textit{or} \text{ Felspathoids}}{\text{Felspars}} < \frac{1}{7}$

,, IV. $\frac{\text{Felspathoids}}{\text{Felspars}} < \frac{5}{3} > \frac{1}{7}$

,, V. $\frac{\text{Felspathoids}}{\text{Felspars}} > \frac{5}{3}$

,, VI. $\frac{\text{Felsic minerals}}{\text{Mafic minerals}} < \frac{3}{5}$

It is impossible in the limits of this paper to follow out completely the further details of the classification. As a sample the method of dealing with Division III. will be cited. Further subdivision of III. is based on the kind of felspar present. Alkali-felspars (orthoclase, microcline, anorthoclase, albite) are contrasted with lime-soda felspars (oligoclase to anorthite). On this basis the following divisions are made:

A. Syenites . . . $\frac{\text{Alkali-felspars}}{\text{Lime-soda felspars}} < \frac{5}{3}$

B. Monzonites . . $\frac{\text{Alkali-felspars}}{\text{Lime-soda felspars}} < \frac{5}{3} > \frac{3}{5}$

C. Diorites and Gabbros $\frac{\text{Alkali-felspars}}{\text{Lime-soda felspars}} > \frac{3}{5}$

The diorites are characterised by oligoclase and andesine; the gabbros by labradorite, bytownite, and anorthite.

The syenites are subdivided as follows:

Group A1. [Prefelsic syenites] . . $\frac{\text{Felsic minerals}}{\text{Mafic minerals}} > \frac{5}{3}$

1. Alkali-syenites . $\frac{\text{Alkali-felspars}}{\text{Lime-soda felspars}} > \frac{7}{1}$

2. Calcialkalic-syenites $\frac{\text{Alkali-felspars}}{\text{Lime-soda felspars}} < \frac{7}{1} > \frac{5}{3}$

Group A2. [Mafelsic syenites] . . $\frac{\text{Felsic minerals}}{\text{Mafic minerals}} < \frac{5}{3} > \frac{3}{5}$

Monzonites have only been divided into

Group B1. [Prefelsic monzonites] . $\frac{\text{Felsic minerals}}{\text{Mafic minerals}} > \frac{5}{3}$

Group B2. [Mafelsic monzonites] . $\frac{\text{Felsic minerals}}{\text{Mafic minerals}} < \frac{5}{3} > \frac{3}{5}$

Group C.—Diorites and Gabbros—is divided similarly.

Methods of subdivision similar to the above are adopted in all the main Divisions.

It is obvious that this method of classification fulfils the conditions laid down in this paper. It is modal and it is quantitative. The scheme forms an excellent ground-work on which to build up a stable quantitative system of classification based on the mode. Several criticisms of Iddings' arrangement can be made. The first is that his sixth division, that characterised by mafic minerals, is really unnecessary. The rocks included in this division are collected from most of the other divisions, in increasing quantity from the first or second onward, and form merely the domafic and permafic members of these divisions. Division VI. is not co-ordinate with the other divisions, but traverses them, as can be seen from the diagram.

	Rocks characterised by quartz.	Rocks characterised by quartz and felspars.	Rocks characterised by felspars.	Rocks characterised by felspars and felspathoids.	Rocks characterised by felspathoids.
Perfelsic, Dofelsic, Mafelsic	Division I.	Division II.	Division III.	Division IV.	Division V.
Domafic, Permafic	Division VI.				

Division VI., consisting of rocks with dominant mafic constituents, is really a survival of the ultrabasic class of the qualitative classifications. The ultrabasic rocks, however, occur in very small quantity as compared with those of most of the other divisions. They rarely form independent masses, large or small, and are usually encountered as differentiation-facies of rocks belonging to the other divisions, especially III., IV., and V. Moreover, the erection of a division based on mafic minerals contravenes the principle that the main factor in igneous rock classification should be based on the predominant felsic minerals. The proportions of the mafic constituents are sufficiently recognised by the erection of groups based on the felsic mafic ratio (see subdivision of the syenites, ante p. 78). It is in accordance with the results obtained in the foregoing discussion that the rocks of Iddings' Division VI. should be distributed amongst the other five divisions, where they would at once find their place as the domafic and permafic members of these divisions.[1]

A second criticism is that the second factor in the majority of the classes is the ratio of alkalic to lime-soda felspars. The latter may range from oligoclase to anorthite. Albite, which from a mineralogical point of view, forms part of the plagioclase series, is, however, and rightly, treated as an alkali felspar. But as the lime-soda felspars contain the albite molecule in varying proportions, the latter appears in both the quantities that form the ratio. This is neither necessary nor desirable. What is required is to contrast all the alkali-felspar with all the lime-felspar; and to that end the albite molecule included within the lime-soda felspar should be counted with orthoclase and pure albite in the numerator of the fraction. As pure albite rarely occurs independently in igneous rocks, and only to a small extent in solid solution or isomorphously mixed with orthoclase (as soda-orthoclase, anorthoclase, etc.), the ratio used by Iddings essentially contrasts the orthoclase molecule on the one hand with the albite plus the anorthite molecules on the other. This, however, is a meaningless ratio as compared with that of orthoclase plus albite to anorthite. The latter expresses

[1] Holomafic rocks could be treated as suggested for the holofemic rocks; from their affinities and paragenesis it should be possible in the great majority of cases to determine in which Divisions they should be placed.

the common and significant variation in igneous rocks which has given rise to the conception of alkalic and calcic "branches."

It is considered therefore that this ratio should replace that used by Iddings, especially as it is quite as easily obtained. From the optical examination it is now possible to determine the composition of the lime-soda felspars quite accurately; and it is then only necessary to add the amount of the albite molecule to the alkali-felspar already quantitatively determined, and to use the remaining amount of anorthite as the denominator of the fraction, in order to obtain the required ratio between the alkalic and calcic felspars. Complications are introduced by the frequent zonal structure of the plagioclase felspars, making it difficult to estimate the proportions of the albite and anorthite molecules; and by possible perthitic intergrowths of lime-soda felspars with orthoclase. But the errors introduced by these difficulties in the estimation of the felspar ratio will not commonly be so large as to shift the rock from its rightful place in the classification. The grade of accuracy required in the quantitative estimation is governed by the size of the classificatory compartment into which it is desired to introduce the rock; as and it is probable that the compartments of the modal classification would not be smaller than the subrang of the American Quantitative Classification, a high grade of accuracy would not be required.

A third criticism of Iddings' classification is that while it is ostensibly based on the mode it is the norm that is most frequently used to determine the place of any particular rock. It must be admitted that this is largely due to the paucity of data at his disposal as to the modes of igneous rocks. It is a difficulty that only future work will remove; and its discussion here may serve to remind descriptive petrographers how vital it is to systematic work to give as many and as accurate estimations as possible of the modes of the rocks they describe. In the persalic and dosalic classes of the American Quantitative Classification the ratio of salic to femic constituents is, however, nearly the same as that of felsic to mafic. Similarly the differences between the ratios of quartz or lenads to felspars, and between the felspars themselves, are negligible from the point of view of classification. This has been ascertained from the consideration of a number of rocks in which both norm and mode are available. Hence, in the absence of the mode, the norm may be used for modal classification with little possibility of error in rocks belonging to persalane and dosalane.

Whilst the modes of many rocks are normative, they are probably more frequently somewhat abnormative; and this has given rise to some curious anomalies in Iddings' classification. For instance, the anorthite appearing in the norm of a "monzonite" may in part be taken up in the mafic minerals, and therefore fail to appear in the form of lime-soda felspar in the mode. Rocks of this type, therefore, called monzonites in Iddings' classification on the strength of the ratios of alkalic to lime-soda felspars calculated from the norm, may actually be devoid of modal lime-soda felspar. But the presence of the latter in amount roughly equal to the orthoclase, is the essential part of the original definition of monzonite. Many similar anomalies, caused in the same way, could be cited; but until as many modes have been accumulated as there are norms, it is probable that this difficulty will not be completely obviated.

It may be remarked that the range of rocks covered by Iddings' divisions, excluding that based on the mafic minerals, might equally well be treated as divisible into nine equal compartments, based on ratios between quartz or lenads to felspars analogous to those that form the nine orders in Classes I., II., and III. of the American Quantitative Classification. The diagram makes this clear.

Division I. Rocks characterised by quartz.		Division II. Rocks characterised by quartz and felspars.		Division III. Rocks characterised by felspars.	Division IV. Rocks characterised by felspars and felspathoids.		Division V. Rocks characterised by felspathoids.	
Order 1.	Order 2.	Order 3.	Order 4.	Order 5.	Order 6.	Order 7.	Order 8.	Order 9.
$\frac{Q}{F} > \frac{7}{1}$	$\frac{Q}{F} < \frac{7}{1} > \frac{5}{3}$	$\frac{Q}{F} < \frac{5}{3} > \frac{3}{5}$	$\frac{Q}{F} < \frac{3}{5} > \frac{1}{7}$	$\frac{Q \text{ or } L}{F} < \frac{1}{7}$	$\frac{L}{F} > \frac{1}{7} < \frac{3}{5}$	$\frac{L}{F} > \frac{3}{5} < \frac{5}{3}$	$\frac{L}{F} > \frac{5}{3} < \frac{7}{1}$	$\frac{L}{F} > \frac{7}{1}$

A quantitative mineralogical classification of igneous rocks, based on essentially the same principles as that of Iddings, has recently been devised by F. C. Lincoln.[1] The chief difference

[1] F. C. Lincoln, "The Quantitative Mineralogical Classification of Gradational Rocks," *Economic Geology*, viii. 1913, pp. 551–64.

is that Lincoln prefers a three-fold to a five-fold subdivision. His main divisions are as follows:

Division A.	Leucocratic .	. $\frac{\text{Felsic}}{\text{Mafic}} > \frac{2}{1}$
„ B.	Mesocratic .	. $\frac{\text{Felsic}}{\text{Mafic}} < \frac{2}{1} > \frac{1}{2}$
„ C.	Melanocratic	. $\frac{\text{Felsic}}{\text{Mafic}} < \frac{1}{2}$

Divisions A and B are further subdivided on the basis of the ratio of quartz or felspathoid to felspar, and Division C on ratios subsisting between ferromagnesian silicates and ores. It is only possible here to cite the treatment of Division A as a sample of the method. Division A is subdivided as follows:

(*a*) Quartz group . . .	$\frac{Q}{F} > \frac{2}{1}$
(*b*) Quartz-felspar group . .	$\frac{Q}{F} < \frac{2}{1} > \frac{1}{2}$
(*c*) Felspar group . . .	$\frac{Q \text{ or } L}{F} < \frac{1}{2}$
(*d*) Felspar-felspathoid group .	$\frac{L}{F} > \frac{1}{2} < \frac{2}{1}$
(*e*) Felspathoid group . .	$\frac{L}{F} > \frac{2}{1}$

Further subdivision is made in groups (b) and (c) on the basis of the percentage of orthoclase to total felspars present, and in groups (*d*) and (*e*) on the percentage of leucite to total felspathoids, giving rise to thirteen series.

This mode of arrangement may be subjected to precisely the same criticisms as have been applied to that of Iddings. A further criticism is that the ultimate compartments are far too comprehensive. Take, for example, group (c), in which the ratio Q or L to F is less than one to two. This is subdivided into three series on the basis of the percentage of orthoclase in total felspar present as follows:

Series V. Syenite-Trachyte .	Percentage of orthoclase	100–67
„ VI. Monzonite-Vulsinite .	„ „ „	67–33
„ VII. Diorite-Andesite .	„ „ „	33–0

Taking the extreme variations, the syenite-trachyte series may contain rocks of the following composition:

	I.	II.
Quartz	33	0
Orthoclase	67	30
Lime-soda felspars	0	15
Felspathoids	0	22
Mafic minerals	0	33

Both these rocks are leucocratic. In I. quartz forms 33 per cent. of the whole, and in II. felspars make up 67 per cent. of the leucocratic minerals. Hence both rocks fall into group (*c*). In I., moreover, orthoclase forms 100 per cent., and in II. 67 per cent., of the felspar present. Hence they both fall into Series V. On comparison of the mineral composition, however, these rocks are seen to be widely different. One is a hololeucocratic granite; the other is a nearly mesocratic felspathoidal monzonite. Hence it must be agreed that the three-fold method of partition results in far too comprehensive compartments.

A. N. Winchell has recently devised a useful modification of Rosenbusch's classification.[1] His main classes are peralkalic, alkalic, and alkalcic (= alkali-calcic). Symmetry would seem here to demand a percalcic class also. The next co-ordinate used is that of occurrence and incidentally texture. The rocks are divided into plutonic, hypabyssal, and volcanic groups. The hypabyssal is further subdivided into the aschistic and diaschistic types (the latter with both felsic and mafic varieties); and the volcanic into felsitic and glassy types. The third factor in the classification is mineralogical, and subdivides the rocks according to whether they have alkali-felspars, soda-lime felspars, or are devoid of felspars. A further mineralogical division is employed which, however, differs in detáil in each of the three main classes.

No quantitative relations whatever are formulated to regulate the application of the various factors employed; but tables showing the average chemical composition of the principal rock-types are appended to the paper. This is an admirable rearrangement of Rosenbusch's plastic qualitative classification. With one or two further modifications (as, for example, the institution of a percalcic class, and the recognition of the felsic-mafic ratio throughout), and with a certain amount of quantitative stiffening, it would form an excellent classification to present to students in geology classes, from which, in a more advanced stage, they could pass to the more elaborate quantitative classification based on the mode.

[1] Rock Classification on Three Co-ordinates, *Journ. Geol.* xxi. 1913, pp. 208–23.

Conclusions

The present trend of petrological thought is toward an increasing recognition of the quantitative element in the science. A quantitative classification is demanded merely on the grounds of utility, especially for the purpose of comparing and correlating igneous rocks, for which qualitative descriptions afford only a very vague and inconclusive basis. With the exception of chemical and mineralogical composition, all the bases of classification hitherto proposed are insusceptible to the quantitative method. A quantitative classification based on chemical composition is a desideratum for various purposes, but is unsuitable for the everyday working needs of petrographers, because the chemical analysis of a rock usually takes at least a week of the petrographer's time, and it is therefore impossible for him to obtain data for all the rocks he wishes to classify. The mode of a rock is much more easily obtained, and is quite as susceptible to the quantitative method as the chemical composition. The main lines of a suitable modal classification are already formulated in the second volume of Iddings' great work on Igneous Rocks. With a little modification in detail, and elaboration on the scale and with the method of the American Quantitative Classification, it is believed that it will satisfy the immediate needs of petrographers. If and when they are elucidated, the natural or genetic relations of igneous rocks will be as easily expressed in terms of the quantitative modal classification, as various physical relations are expressed in terms of the artificial and arithmetically bounded units of the scale of temperature and other physical properties.

THE CAUSE OF VARIATION

By ARCHER D. WILDE

It is a trite observation, said Darwin, that no two creatures in nature are alike, and fifty years of Darwinism have not made it less so. I will not therefore trouble my readers with any considerable expansion of the theme, but rather adopt it as a postulate, not only that there are no two creatures alike, but also that if two of the likest possible, for example two animals of the same litter, or two cells derived by fission from one, are compared, they differ in every particular. And not only so, but no two parts of the same creature are alike. Like as they seem to the eye, two hairs of the same animal, placed under a microscope, are at once seen to differ; and drawings of cellular structures seen under high microscopic powers show that no two contiguous cells resemble each other exactly. Throughout organic nature general similarity is accompanied by unlikeness in detail. As between parents and their offspring, as between the several offspring of the same parents, and more widely as between the members of any generation of any kind of plant or animal, these differences are called variations, and much labour has been expended in attempts to account for their origin. These I pass over at present, but one hypothetical question regarding such individual differences I wish to ask, in order to answer it in my own way. If by microscopes of continually increasing power we could examine the structure not only of the cells, but also of their component plasms or materials, and again the structure of the units of which these materials might be found to consist, where ultimately should we find these differences end? Not long ago the answer would, I suppose, have been, "In the chemical molecules at all events, if not before reaching them, will be found units absolutely alike in all respects." Recent discoveries in physics have however now given grounds for the belief that not even the atoms of the same element are exactly alike, for even apart from internal motion they may be in different stages of a slow disintegration.

But it is evident that this is going further than there is any need or right to go in biology, and I mention it only to show to what a depth this law of unlikeness goes in natural phenomena. Assuming such differences to exist, then however they may possibly affect life, it is surely not in them that we are to seek even the ultimate factors of the far greater differences between one living being and another. For biological purposes we must assume what is probably not true, that all atoms of the same element are exactly alike. More, that each of the highly complex chemical compounds, in which atoms are united to form the bases of living matter, also consists of molecules, of which every one is exactly like every other in weight, size, shape and the arrangement of its component atoms. When however we consider the manner in which the materials of which a cell is built, or the units, be they what they may, which compose those materials, are constructed out of these theoretically uniform bricks, the chemical molecules, we must suppose that at some stage or other structure comes into play, and that the generally similar homologous parts of two like individuals must differ ultimately in the number and arrangement of the bricks of which they are composed. They may no doubt differ in some degree in the kind of bricks, for while all life is largely composed of the same chemical compounds, and the homologous parts of like animals must be almost entirely so composed, yet it is certain that some forms of life contain chemical compounds absent from others, and it seems not doubtful that two closely allied individuals, even two animals of the same litter, apart from morbid changes, may congenitally contain different chemical compounds resulting in differences in pigmentation and other characters. But as between animals of a kind this difference may be neglected; it is in the others that variations appear in the last analysis to consist. Two like animals differ because their parts differ, these because their constituent tissues differ, these again because their component cells are unlike, and finally these differ because either they, or the ultimate units of which they are composed, are built of unlike numbers of like molecules dissimilarly arranged.

All this is of course far from being new, and it is mainly speculative, having little inductive support derived from observation. What validity it may have is derived rather from necessities of thought. As long ago as 1689 Locke referred our ideas of the primary qualities of bodies to the "bulk, figure, number, situation, and motion of their parts." Since his day we have learnt much about these "parts," and in the light of modern knowledge we may paraphrase his words by the expression "kind, number, and arrangement of the component molecules," because the "bulk and figure" of these depend on their kind, which also determines their motion and the manner of their response to the motions reaching them from the environment. And as the primary qualities of a body are referable to these factors, so also must the differences between any two be referred to differences between these factors. It would appear moreover that the analysis may be carried one step further, resolving the factor of arrangement into the two others. For in the growth of organic matter the positions taken up by the molecules must depend upon their natures and numbers. The differences between any two like individuals are therefore results of the differences in the numbers of the chemical molecules in the cells they grew from—a proposition which, I may perhaps be told, might have been assumed at the outset. An important consequence should however be noted. As the differences between any two such cells consist only in differences in the numbers of the molecules composing them, it follows that all variations must be in one or other of two directions—plus or minus, greater or less—and it is inaccurate and misleading, though very common, to write of them as occurring "all round a centre," "in every possible direction." They must in every particular be above or below a mean, and there is no third alternative.

When the matter is thus considered in a general way, it becomes evident that all that has been said applies not merely to living bodies, but to all bodies, organic and inorganic alike. Locke's words are of course perfectly general. Mountains, rivers and clouds, as well as animals and plants, show striking general resemblances accompanied by endless differences in detail. Not only are no two leaves alike on a tree, but the same may be said of the pebbles on the beach, and with the aid of a microscope the truth is found to hold even of the grains of sand on which they lie. Many rocks look homogeneous enough, but on being examined in thin plates under high powers they reveal a complicated intimate structure. Organic variation being then only the expression in plants and animals of an infinite differentiation common to organic and inorganic nature, it is idle to search for its ultimate causes in phenomena which are subsequent to life. Sex has been suggested as a cause, but is itself a variation of life, as life itself is but a variation of inorganic nature, and therefore it cannot be the cause. The effects of the environment upon a parent, supposed on very inadequate evidence to be transmitted to their offspring, have been pressed into service as causing or contributing to cause variation. But there is no need to spread the net so wide, and it is unreasonable in accounting for a phenomenon everywhere manifested throughout the universe to bring in causes peculiar to life. Variation, differentiation, the production of the unlike out of the like, is a part of the universal scheme of things. The power to vary is a gift passed on to the organic world by the inorganic from which it sprang. The word differentiation was at first part of the formula in which Spencer summed up the course of evolution, and though it was afterwards discarded, its essence remains, and it seems questionable whether the formula gained by the change. If one word could sum up the process of the suns from a homogeneous nebula to the complexity of mundane affairs, that word would be differentiation.

The ultimate cause therefore of organic variation is the same as that of differentiation in general. Variation is a phase of the phenomenon called by Spencer the instability of the homogeneous—the tendency of like things to become unlike, and of unlike things to become more unlike. It may accordingly well be questioned whether the object of our search ought not, instead of the cause of variation, to be the cause of similarity. How is that wonderful constancy to type in all that is essential to life preserved in the immensely complicated organisms of the higher animals? It is a question that is to some extent being answered by the researches of cytologists on the growth and division of cells.

All this however is naturally unsatisfying to the biologist, who wishes to trace not merely the ultimate, but also the proximate cause of variation in the organic world. How is this universal tendency to change and divergence manifested in the world of life, and especially in the higher animals? I believe it is possible by consideration of the mechanics of reproduction in cells in some degree to supply the answer. The proximate cause of variation lies in the differential division of the unfertilised germ-cell. Those who have traced the process of fission under the microscope, describe it as an elaborate arrangement for securing an equal division of every part of the mother-cell between the two daughter-cells. Their descriptions naturally relate only to what they see, but it is difficult to doubt that that process is carried out through every minute part of the cell far beyond the range of vision. It is reasonable to assume that the seen process extends to the unseen, and in no other way can we imagine the powers of the mother-cell to be transferred to both the daughters. The immense complexity of the germ-cell of one of the higher animals is, I suppose, generally assumed. At all events it seems to me a necessary assumption. The brain of an ant has been called the most wonderful piece of matter in existence, but it is nothing in comparison with this speck of comparable size, of which a part only, if the whole be fertilised and provided with proper food and environment, develops into the brain of a man. When the unfertilised cell divides into two daughter-cells, it is as if every part of something far more complicated than a watch were divided each into two equal and similarly shaped parts, and the whole were put together again to make two watches of smaller size. There must be many thousands of factors, each with its special function to perform after fertilisation, should this occur, in the growth of the animal or plant, and every one must be equally divided between the two daughter-cells. Being so numerous, these factors must be almost inconceivably small, yet so much smaller are chemical atoms and molecules that immense numbers may enter into the composition of each, whether they are grouped, as is probable, into intermediate units or not. I have said that on a fission each factor must be equally divided, but here is the point, the division of the molecules of each factor between the two daughter-cells can never be more than approximately equal, or at least the chances are immensely against such an event, and the division is therefore differential. Let us assume what is extremely improbable, but will equally well serve the purpose of argument, that a certain very small factor of a cell about to divide consists not of millions, but of so few as a hundred molecules of one very complex chemical compound, and of nothing else. On a division the chances are much against absolute equality. In the result let us say that 52 molecules pass into one daughter-cell and 48 into the other. Before the next fission these two cells have to grow to normal size—that is, they must be provided with foods from which each factor can draw its appropriate nourishment. All parts of the cell grow at approximately equal rates, and the whole having to be doubled, each factor increases in that proportion. Like the halving process, this process of doubling will of course be inexact, but as there is no reason for assuming a tendency either to excess or defect, an exact doubling must be postulated on an average. The selected factor therefore on the attainment of maturity consists of 104 molecules in the one cell and 96 in the other. On a second division of the two cells into four there can again be no equality. The selected factor in one of the four cells now produced must consist of more than 52 molecules, let us say 53, and in one other of less than 48, it may be 46, the other two being intermediate. On maturity these numbers become 106 and 92, and so on for subsequent divisions. We see then a constant, natural, and inevitable tendency to divergence in respect of this particular factor. And what is true of this imaginary factor is also true of all the many real factors that constitute a real cell. In proportion as the number of molecules in each factor is larger, the argument is the stronger, and although more difficult to follow, it is not really altered if the molecules composing each factor be of many different kinds. These cells are produced in immense numbers, and there must contemporaneously exist among those of a single generation a great variety of combinations of all the factors, each of which has an equal chance of being called on by fertilisation to take part in the continuance of the race.

Briefly the argument is that germ-cells are individuals, and like all individuals they differ, even the twin cells, which result from the fission of a single cell, not being exactly similar, for we know that "no two creatures in nature are alike"; and owing to the method of reproduction by fission, there is a continuous tendency towards a differentiation which accumulates from generation to generation. This tendency appears to me to be not so much a fortuitous as a necessary and universal cause of variation in the animals which at intervals arise from the germs. The hypothesis seems to accord well with the facts of heredity in general. In particular may be cited the congruous fact that twins are often much more alike than brothers or sisters born at long intervals of time. It also affords a simple explanation of the evil effects of close in-breeding, since closely related cells would be apt to resemble each other in the excess or defect of particular factors. It is applicable not only to the higher animals and plants, but to all forms of life which are reproduced by cell-division, including the monads, those lowest forms in which the history of the cell is the whole life-history of the race.

If there is any force in these considerations, it would not be surprising if some check were needed to counteract the strong tendency that we here find towards variation. The germ that nature has elaborated by slow additions through millions of years of unbroken descent in a continuous line from simple beginnings, if any form of matter can be called simple, to its immense present complexity, has to be guarded against a too rapid change in an environment that changes slowly, rather than to be stimulated into variation. To cope with our environment we must come extremely true to a type which is proved by ages of survival to be the fittest. That sex is a device by which such stability is increased is a thesis which I have previously argued elsewhere, and need not here repeat. It is one that appears to me to be supported not only by reason, but by abundant inductive evidence. Briefly the means by which sex effects a reduction of variations is the halving of those differences which we have seen to be established by repeated fission. Except where the two conjugating germ-cells are closely related owing to in-breeding, it is unlikely that the same factors will be much in excess or defect in both; and a factor that is much in excess in one must therefore be assumed to be at about mean size in the other, so that that excess will be halved in the fertilised cell. This halving appears to be ensured by the "reducing division" of the cells which precedes their conjugation.

In this differential division of the cell, if it be an agent in producing somatic variation at all, we have a cause which is certainly constant, and so far as concerns creatures which are reproduced by the fission of cells, one which is also universal. It will be worth while to notice into what troubles and fallacies we are led by the refusal to recognise some such agent. They are well illustrated by Darwin's discussion of the causes of variability in Chapter XXII. of his *Animals and Plants under Domestication*, perhaps the least satisfactory that ever came from his pen. In it he finally decides that the variability of organic beings under domestication, "although so general, is not an inevitable contingent on growth and reproduction, but results from the conditions to which the parents have been exposed. Changes of any kind in the conditions of life, even extremely slight changes, suffice to cause variability." On the other hand, as to two of the most important changes that can be thought of, "a change of climate is not one of the most potent causes," and "it is doubtful whether a change in the nature of food is a potent cause." On the one hand, "of all the causes which induce variability, excess of food . . . is probably the most powerful." On the other hand, "the goose and the turkey have been well fed for many generations, yet have varied very little," while the thorn, hardly cultivated at all, has varied much, and "seeds taken from common English forest trees, grown under their native climate, not highly manured or otherwise artificially treated, yield seedlings which vary much, as may be seen in every extensive seed-bed." Although "we may conclude with certainty that crossing is not necessary for variability," yet "the crossing of distinct species, besides commingling their characters, adds greatly to their variability"; but as against this, "close inter-breeding induces lessened fertility and a weakened constitution; hence it may lead to variability." If on the one hand a plant varies on being cultivated, the change of conditions is supposed to be the cause, but if, as in the case of the Swan River daisy, no conspicuous change occurs until after seven or eight years of high cultivation, this is taken for "good evidence that the power of changed conditions accumulates." Surely all this is very indifferent logic. It almost amounts to this, that the presence of any one of the suggested causes and its absence are equally effective in inducing variability, if and when it occurs. Moreover, in a creature so shielded from the environment as is the germ-cell of the higher animals, all these influences seem too remote. Even in monads, far more exposed to them, changes of food and environment, except perhaps those occurring at the moment of fission, would presumably rather affect the rate of growth and reproduction of the cell than the manner of its division. There seems to be no escape from the conclusion from which Darwin unaccountably shrank, that there exists in all living things an innate tendency to vary independent of external conditions, and in his own language "inevitably contingent on reproduction." "Nevertheless," he says, "when we reflect on the individual differences between organic beings in a state of nature, as shown by every wild animal knowing its mate, and when we reflect on the infinite diversity of the many varieties of our domesticated productions, we may well be inclined to exclaim, though falsely as I believe, that variability must be looked at as an ultimate fact necessarily contingent on reproduction." But while he produces abundant justification for such an exclamation, I can find no grounds for the reservation, "falsely as I believe." The whole argument sadly lacks the beautiful obviousness and simplicity which so distinguish the theory of natural selection. It has a strong flavour of *post hoc propter hoc*. In wild creatures we do not usually know what variations may occur. When they are domesticated, any variation, or at all events any marked variation, is apt to be noticed, and perhaps seized as a peg on which to hang a new variety. When it occurs, anything convenient is set down as the cause; in imported productions change of climate, in home-grown creatures change of soil, change or excess of food, breeding out or breeding in, anything in fact but an innate power to vary. Such vague and contradictory suggestions give no rest or satisfaction to the mind, and incline the student to resort to a cause which, if true, is necessary, general, and in accordance with forces which are manifested, not only in the variations of organisms, but throughout nature, in a process of universal and unceasing differentiation. Such a cause appears to me to exist in the differential division of the cell.

In this passage Darwin finds the cause of variation in change of the environment, nutrition being included in that term. Weismann, who formerly held variation in the metazoa to be effects of sex, and in monads to be due to the action of the environment, afterwards admitted the force of the arguments by which sex is shown to act in the opposite direction, as a cause not of unlikeness but of likeness among the individuals of a species, and ceased to regard it as "the real root of variation itself." This phrase and the following passages, extracted from his *Evolution Theory*, English Translation 1904, show his opinions at that date, and I believe up to the present time:

"Haycraft also finds the significance of amphigony simply in the equalising or neutralising of individual differences which it effects. Quetelet and Galton have attempted to show that intercrossing leads to a mean which then remains constant. Haycraft supposes that a species can only remain constant if its individuals are being continually intercrossed, and that otherwise they would diverge and take different forms, because the 'protoplasm' has within itself the tendency to continual variation" . . . "the fundamental idea, that amphigony is an essential factor in the maintenance . . . of species, is undoubtedly sound . . . but we cannot simply suppose that amphigony and variation are, so to speak, antipodal forces, the former of which secures the constancy of species, the latter its transformation. In my opinion, at all events, there is no such thing as a 'tendency' of protoplasm to vary, although there is a constant fluctuation of the characters—dependent on the imperfect equality of the external influences, especially of nutrition." Thus the "real root of variation" is the same as that alleged by Darwin. Nevertheless the stone here rejected by the builder may yet become the head-stone of the corner. It may be that sex and variation *are* "antipodal forces," in that sex, by ensuring the communication of variations among large numbers of individuals, may check the unlimited divergence that would otherwise ensue. Without sex every individual line of living creatures tends to diverge, like a tree growing into divergent branches; by sex individual lines are bound together like intercrossing threads of a network, and divergence occurs in the large communities called species only in proportion as intercrossing is prevented or is rare. Haycraft's "supposition" is really nothing but a fact. If a species of animal, spreading over two areas, is kept from free intercrossing by an intervening sea, or a beetle by a mountain range, it invariably diverges into varieties, and ultimately into species and genera. But as to the "tendency of protoplasm to vary," this would be better expressed as the impossibility of securing an exactly equal division of the matter of a mother-cell, or of any part of a mother-cell, between the two daughter-cells whenever fission occurs. In a subsequent passage Weismann says that the "ids" (hypothetical constituents of the germ-cell, each of which is supposed to be a "biological unit" virtually containing all the parts of an individual) "differ very little within the same germ-plasm . . . but are only absolutely alike in the case of two ids which have been formed by the division of a mother-cell." To suppose that any two such bodies can be absolutely alike is to run counter to all we know of nature. If not even two atoms of an element are exactly alike, such likeness cannot be postulated between two relatively enormous bodies, each virtually, or let us rather say potentially, containing all the parts of one of the higher animals.

A PROBABLE CAUSATIVE FACTOR IN THE AWAKENING OF POND LIFE IN THE SPRING

By AUBREY H. DREW

The discovery of Auxetics by H. C. Ross has raised some important points in biology, but perhaps one of the most important is that all living matter does not possess any inherent capacity to reproduce itself until it has absorbed an auxetic. Auxetics are substances which cause the multiplication of cells. Some time ago I was able to show that the full development of the spores of *Polytoma granulosa* (1) could be induced by solutions containing auxetics, and I suggested the probability that these organisms, as well as others existing in water, required the presence of auxetics in order to develop. Pond water always contains decomposing organic matter, and from this fact one would suspect that auxetics must be present in solution. Alkaloids have been shown by H. C. Ross to augment the power of auxetics as much as five-fold, and to cause amœboid or kinetic action in leucocytes, and as pond water contains organic matter in course of decomposition the presence of kinetics might reasonably be expected. There is a further question also raised, and that is that it is well known that the organisms present in any given pond vary from time to time. Thus one may find comparatively few organisms in a sample collected from a pond, say, in January or February. A sample collected, say, in March may show Vorticella as the chief yield, while at another time perhaps Coleps or Paramœcia may be the most plentiful. The same thing is seen still better in an artificially prepared infusion, say, of grass. If such an infusion be examined from day to day, it will be found that the first organisms to appear are bacteria, then, somewhat later, flagellates; these are succeeded by ciliates, and it will be noticed that at any given time one particular species of flagellate or ciliate is usually more pronounced than others, and that these often die out, to be replaced by different forms. Finally a period is reached when all life tends to die out in the infusion, but bacteria may persist for a considerable time. No very satisfactory explanation of this phenomenon has been forthcoming as yet, but I think that, considered in the light of recent researches, the point becomes intelligible. H. C. Ross discovered the fact that cells possess what is termed a Coefficient of Diffusion (2). This is measured by estimating the number of units of stain alkali, etc., contained in the jelly film on which the cells are placed, after subtraction of the units of salts present, and by adding the units of heat and time requisite to cause staining of the nucleus. The fact that all cells possess this coefficient of diffusion shows that the medium on which they are living must have the necessary index of diffusion before absorption takes place. It is therefore obvious that supposing an organism to possess a coefficient of diffusion of, say, 20, it would not tend to multiply in an infusion till the fluid possessed the necessary index, time and temperature, of course, being taken into account. The coefficient of diffusion varies greatly in different organisms, and, therefore, supposing two organisms find their way into an infusion, the one possessing the lower coefficient will multiply sooner than the one having the higher. Finally a time might arrive when all the auxetic in the infusion would be used up and development would then cease.

A further important point arises—viz. the great awakening of life in springtime. Several theories have from time to time been put forward such as increase in temperature, or an extra amount of actinic rays from the sun, but none of these explanations really meet the case. Now supposing it to be demonstrated that an increase in either the auxetics or kinetics or both took place towards spring in, let us say, pond water, we should at once have an explanation of that awakening of life in ponds which occurs in spring; and, as it would be unlikely that such an important phenomenon would have more than one cause, we might reasonably conclude that the general development of life in spring was also occasioned by an increase in the auxetics and kinetics in the soil. I therefore determined to conduct a prolonged research to settle the following points. Firstly, whether auxetics and kinetics do occur in pond water. Secondly, whether there is any variation in the amount of these bodies, supposing that they existed, and especially whether the quantity increased as spring approached. Samples of water from different ponds in various districts were therefore examined by the following method. Three litres of the water were filtered through an ordinary chemical filter and were then evaporated to dryness over the water bath. The residue was digested with 5 c.c. of hot organically pure NH_3 free distilled water and filtered. To a tube containing 5 c.c. coefficient jelly 3 units of stain were added and 7 units of alkali, and the total was made up to the necessary 10 c.c. by the addition of 4 c.c. of the sample to be examined (2). The tube was then placed in a beaker of boiling water till the jelly had melted, and the reagents mixed thoroughly with it. It was then boiled in the bunsen till it frothed up, and a drop or two was poured on to a glass slide, where it was allowed to set firmly. Some blood was taken from the finger and mixed with an equal volume of citrate solution (3 per cent. sod. citrate and 1 per cent. sod. chloride) to dilute it, and a drop of this was placed on a cover glass, which was at once put on to the jelly. The specimen was then placed in the 37° C. incubator and allowed to remain there for ten minutes. The coefficient of diffusion of human lymphocytes is 14, and it will be seen from the following equation that this jelly should just stain their nuclei in ten minutes at 37° C. $3\ S + 7\ A + 7\ h + t - (3\ C + N) = 14$. The slide was then taken from the incubator and examined with the microscope.

If division figures were found in the majority of the lymphocytes, the pond was marked down as containing auxetics. In order to determine the presence of kinetics, a tube of 5 c.c. coefficient jelly had 5 units of stain added and 6 units of alkali, the contents being made up to 10 c.c. with 3·9 c.c. of the sample. Blood, as already described, was then placed on the jelly on a slide and examined at room temperature (20° C.), and if the majority of the polymorphonuclear leucocytes showed exaggerated amœboid movement the specimen was marked as containing kinetics. Working in this manner, I examined samples of water from twelve different ponds with the results shown in the following table.

These results were sufficiently satisfactory to make me determine to carry out a somewhat tedious and prolonged research into the question. I determined, therefore, to examine samples from the same ponds monthly, and in order to ascertain whether the substances in solution had any effect on the auxetics and kinetics present, a chemical analysis of the water was made in each case. One of the chief objects in this research has been to show that it is mainly by means of the auxetics and kinetics present in the pond water that the dormant life is caused to reawake in the spring. The following tables show the results of the experiments. For convenience the names of the localities of the ponds are omitted, and the numbers only given. The first table will therefore serve as a key to the remainder. It will be clearly seen from a consideration of these tables that there is undoubtedly a gradual increase in the auxetics and kinetics present as summer approaches. The increase in the kinetics was much more marked, however, than that in the auxetics, which often seemed to remain constant. The chemical analyses were conducted in order to ascertain, wherever possible, if light could be thrown on any rise or fall in the auxetics and kinetics present. I originally intended to conduct these monthly examinations for the whole year, but unfortunately, owing to a press of other research work, I was unable to carry the chemical analyses further than May, although the testing for auxetics and kinetics was continued to the end of June, as was also the examination for albuminoid ammonia. If the chemical analyses are carefully examined, it will be seen that the tendency of the albuminoid ammonia is to increase from January to May, and this is exactly what the kinetics do.

Perhaps this fact is most strikingly brought out by means of a curve, and if this is constructed it will be seen at once that the kinetic curve rises and falls with that of the albuminoid ammonia. Too much stress should not be laid on this point, however, as on an examination of the tables it will be seen that it is not always apparently the exact amount of albuminoid ammonia in any given pond which determines the question whether kinetics will or will not be present. Thus pond No. 1 showed traces of kinetics in January, the albuminoid ammonia being 0·019, whilst pond No. 3 contained no kinetics this month, although the albuminoid ammonia was 0·08. It is evident, therefore, that it is not the amount of albuminoid ammonia itself which causes the kinetics to rise and fall, but something else which, whilst producing kinetics, also influences the albuminoid ammonia in an upward direction. Albuminoid ammonia, of course, does not exist in water as such, but is a laboratory product from the organic matter present in the water in solution. The more organic matter in solution the higher will be the yield of albuminoid ammonia obtained. Now in ponds, where as a rule much decaying vegetable matter accumulates, the chief factor in determining the amount of organic matter in solution will be bacterial action in breaking up the complex and mostly insoluble proteids into simpler bodies, many of which are soluble. The ultimate tendency is for the proteins to be broken up into nitrates, nitrites, and saline ammonia. These substances, of course, will not yield albuminoid ammonia. This being so, it is evident that the time when the albuminoid ammonia should be largest in amount is when the bacteria are breaking up the insoluble proteids into soluble ones, and prior to their complete disintegration. Probably at this time alkaloids of putrefaction are formed, especially if the pond should contain much animal matter, and alkaloids are all powerful kinetics. It is probable, therefore, that the albuminoid ammonia is a criterion of bacterial activity in the pond, a rise showing that a greater amount of decomposition is going on, and consequently a greater kinetic production. Thus although a pond containing 0·019 parts per 100,000 of albuminoid ammonia may show traces of kinetics, while another with a content of 0·08 shows none, the explanation probably is that the former contains more animal matter than the latter, which is probably a more effective producer of kinetics. The kinetic curve, then, probably follows that of the albuminoid ammonia, because the rise of this substance in any sample from a given pond is a measure of the amount of the bacterial activity in that pond.

The explanation of the gradual awakening of life in springtime now becomes intelligible. Auxetics are apparently nearly always present, but the kinetics vary, being altogether absent at times. During autumn leaves and other vegetable matter fall into the ponds, while during the cold of winter there is also undoubtedly a higher death-rate amongst the fauna, all of which supply the organic material for the manufacture of auxetics and augmentors (kinetics). Bacterial activity now comes slowly into play, decomposing the organic matter and producing kinetics, which probably accumulate for a time, and ultimately, by augmenting the action of the auxetics, cause the reawaking of the dormant life. Nor does the matter stop here, for what applies to the pond almost certainly applies to the earth. The dead leaves which are shed in autumn being gradually decomposed by bacteria, the soluble products are washed down into the earth, to be slowly absorbed by the roots of plants, in which they stimulate development. As all cells possess a coefficient of diffusion, so therefore will they take various times to absorb the auxetics and kinetics supplied to them, and will therefore complete their life cycles at different times, giving us a possible explanation of the sequence observed in the vegetable kingdom.

It is possible, in fact probable, that other factors may also be concerned, but I think there can be no doubt that owing to bacterial action, kinetics are produced in larger quantities as spring advances, and augmenting the action of the auxetics cause a large increase in the development of all living matter. It would therefore seem that death is essential for the due maintenance of life, for as all natural auxetics are the products of cytolysis there must inevitably be death to produce the substances essential for the continuance and awakening of life. In autumn and winter, therefore, when all nature seems dying, we may picture the products of this death gradually being absorbed finally to cause the resurrection in spring.[1]

References

1. Drew, A. H., "Auxetic Action on the Spores of a new Species of Polytoma," *Knowledge*, March 1913.
2. Ross, H. C., *Induced Cell Reproduction and Cancer.* (London, J. Murray, 1910.)
3. Cropper, J. W., and Drew, A. H., *Researches into Induced Cell Reproduction in Amœbæ*, The McFadden Researches. (London, J. Murray, March 1914.)

TABLE A

Preliminary Examination for Auxetics and Kinetics

Number of Pond.	Situation of Pond.	Date of Examination.	Auxetics.	Kinetics.
1	Horn Park, Lee, Kent . .	20. 11. 12	yes (slight)	yes (very faint)
2	Southend Village, Kent .	23. 11. 12	yes (trace)	yes (very faint)
3	Burntash Hill, Lee, Kent .	24. 11. 12	yes (good)	no
4	Bushey, near Watford, Herts	28. 10. 12	yes (good)	yes (feeble)
5	Keston, Kent . . .	2. 10. 12	yes (good)	yes (fair)
6	Mitcham, Surrey . . .	4. 10. 12	no	no
7	Crofton Park, Kent . .	12. 11. 12	no	no
8	Norbury, Surrey . . .	15. 11. 12	yes (trace)	no
9	Merryhill, near Watford, Herts	18. 10. 12	yes (good)	yes (fair)
10	Watford, Herts . . .	20. 10. 12	yes (good)	yes (good)
11	Harrow, Middlesex . .	30. 11. 12	yes (fair)	yes (fair)
12	Honor Oak, Surrey . .	26. 11. 12	no (suspicious)	yes (very faint)

[1] Since writing the above (3) it has been shown that the presence of a ferment is probably necessary for an auxetic to act, the kinetics probably activating the ferment; hence the increase in decomposed vegetable matter during autumn probably also supplies an extra amount of enzyme.

TABLE B

Monthly Examination of Ponds for Auxetics and Kinetics

Number of Pond	January.		February.		March.		April.		May.		June.		July.	
	Auxetics.	Kinetics.	Auxetics.	Kinetics.	Auxetics.	Kinetics.	Auxetics.	Kinetics.	Auxetics.	Kinetics.	Auxetics	Kinetics.	Auxetics.	Kinetics.
1	yes (feeble)	yes (trace)	yes	yes (faint)	yes	yes (good)	yes (good)	yes (good)	yes	yes (good)	yes	yes	yes	yes
2	yes (feeble)	no	yes	yes (trace)	yes	yes (fair)	yes (good)	yes (good)	yes	yes (fair)	yes	yes	not determined	
3	yes (good)	no	yes (fair)	no	yes	yes (fair)	yes	yes	yes	yes (good)	yes	yes	yes	yes
4	yes (faint)	yes (feeble)	yes	no	yes	yes (fair)	yes	yes (good)	yes	yes (good)	yes	yes	not determined	
5	yes	yes (feeble)	yes	yes (trace)	yes	yes (fair)	yes	yes (good)	yes	yes (good)	yes	yes (good)	yes	yes (good)
6	no	no	yes (feeble)	no	yes	yes (trace)	yes	yes (fair)	yes	yes (good)	yes	yes	not determined	
7	yes (faint)	no	yes	no	yes	no	yes	yes (feeble)	yes	yes	yes	yes (fair)	not determined	
8	yes (good)	yes (feeble)	yes	yes (trace)	yes	yes (trace)	yes	yes	yes	yes	yes	yes (fair)	yes	yes (good)
9	yes (good)	yes (good)	yes	yes (good)	yes	yes (very good)	yes	yes (very good)	yes	yes (good)	yes	yes (good)	yes	yes (very good)
10	yes (good)	yes (fair)	yes	yes (trace)	yes	yes (good)	yes	yes (very good)	yes	yes (good)	yes	yes (good)	yes	yes (very good)
11	yes	yes (fair)	yes	yes (fair)	yes	yes (good)	yes	yes (good)	yes	yes (good)	yes	yes	yes	yes
12	yes (feeble)	no	yes (faint)	yes (fair)	yes	yes (fair)	yes	yes (good)	yes	yes (good)	yes	yes	not determined	

JANUARY 1913

ALL FIGURES ARE IN PARTS PER 100,000

No.	Total Solids.	Loss on Ignition.	Chlorides.	Nitrates.	Nitrites.	Total Hardness.	Saline NH_3.	Albuminoid NH_3.
1	80	6·3	8·4	4·9	trace	45·1	·12	·019
2	42	4·1	4·7	·77	nil	28·2	·03	·013
3	50	4·02	6·5	·5	nil	38·8	1·6	·08
4	40	4·3	3·9	·3	nil	34·2	·14	·05
5	35	3·8	6·2	·75	trace	27·1	·06	·13
6	38	3·01	4·5	·2	trace	28·4	·13	·20
7	28	2·6	6·4	·42	nil	15·3	·22	·14
8	42	4	2·7	·81	nil	28·2	·16	·08
9	26	2·5	5·5	·3	nil	14·2	·81	·53
10	54	4·3	10·2	·5	nil	28·8	1·65	·80
11	70	5·2	12·3	1·3	trace	48·3	1·51	·85
12	48	3·8	5·8	·85	nil	24·5	·5	·62

FEBRUARY 1913

No.	Total Solids.	Loss on Ignition.	Chlorides.	Nitrates.	Nitrites.	Total Hardness.	Saline NH_3.	Albuminoid NH_3.
1	78	5·8	8·8	3·6	trace	42·5	·14	·028
2	40	4·6	5·3	·64	nil	30·2	·05	·019
3	45	4·1	5·5	·51	nil	34·6	·8	·06
4	42	5·3	4·8	·42	nil	32·4	·18	·08
5	38	3·2	5·2	·52	nil	25·8	·13	·16
6	35	3·1	6·3	·25	nil	24·3	·12	·23
7	26	3	6·8	·34	nil	16·4	·25	·19
8	44	4·5	5·1	·85	trace	28·5	·13	·10
9	23	2·3	6·4	·22	nil	14·5	·68	·48
10	58	5·2	12·3	·55	nil	25·6	1·42	·92
11	65	7·1	12·5	1·5	trace	46·2	1·31	·95
12	45	5	4·8	·64	nil	25·1	·42	·63

MARCH 1913

No.	Total Solids.	Loss on Ignition.	Chlorides.	Nitrates.	Nitrites.	Total Hardness.	Saline NH_3.	Albuminoid NH_3.
1	76·5	5·2	7·5	2·4	nil	40·5	·12	·035
2	43·2	4·3	6·4	·61	nil	32·6	·08	·021
3	44·3	4	5·4	·49	trace	33·8	·54	·08
4	45·5	4·4	5·5	·35	nil	36·3	1·21	·12
5	36·8	3	4·8	·63	nil	25·5	·25	·18
6	38·2	3·1	6·1	·28	nil	26·1	·19	·26
7	28·1	3·3	6·5	·25	nil	16·8	·20	·15
8	46·3	4·2	4·8	·65	trace	25·4	·15	·12
9	25·2	2	6·5	·31	nil	16·1	·48	·51
10	60·1	4·8	10·5	·60	nil	33·2	1·18	1·10
11	58·4	6·5	9·6	·85	trace	40·1	1·01	1·12
12	46·3	4·5	5·01	·55	trace	24·3	·45	·69

APRIL 1913

All Figures are in Parts per 100,000

No.	Total Solids.	Loss on Ignition.	Chlorides.	Nitrates.	Nitrites.	Total Hardness.	Saline NH_3.	Albuminoid NH_3.
1	74·5	5·6	6·4	1·8	nil	38·8	·24	·058
2	40·2	3·8	6·6	·52	nil	30·4	·09	·032
3	40·5	3·2	4·5	·42	nil	32·1	·55	·09
4	46·1	3·5	5·6	·39	nil	36·8	1·35	·18
5	38·3	2·8	4·5	1·2	nil	26·2	·36	·22
6	35·2	2·2	6·2	·15	nil	24·1	·34	·28
7	30·4	2·5	6·6	·20	nil	16·5	·30	·21
8	45·3	3·8	4·5	·51	trace	24·3	·18	·15
9	28·5	2·3	6·8	·24	nil	16·5	·50	·55
10	55·4	4·1	8·4	·55	trace	30·5	1·01	·96
11	52·1	3·6	8·5	·80	nil	35·8	1·20	1·34
12	45·2	3·8	5·3	·56	nil	23·5	·52	·81

MAY 1913

No.	Total Solids.	Loss on Ignition.	Chlorides.	Nitrates.	Nitrites.	Total Hardness.	Saline NH_3.	Albuminoid NH_3.
1	70·2	4·3	5·5	1·42	trace	36·3	·25	·062
2	38·4	3·6	5·8	·42	nil	25·2	·10	·051
3	40·9	—	4·2	·42	nil	30·1	·46	·09
4	47·2	—	6·1	·28	nil	35·5	1·28	·21
5	36·4	—	6·3	·85	nil	25·4	·45	·26
6	36·1	—	6·8	·18	nil	25·5	·42	·31
7	32·5	—	5·5	·25	nil	16·8	·34	·24
8	46·3	—	5·2	·45	nil	25·1	·22	·18
9	30·2	3·1	6·6	·28	nil	15·2	·50	·55
10	52·4	4·1	7·3	·41	nil	26·8	1·21	1·01
11	54·3	—	8·6	1·20	trace	36·1	1·20	1·3
12	42·6	—	5·5	·84	nil	23·8	·68	·92

ALBUMINOID AMMONIA IN PARTS PER 100,000

No.	January.	February.	March.	April.	May.	June.	July.
1	·019	·028	·035	·058	·062	·060	·12
2	·013	·019	·021	·032	·051	·052	·09
3	·08	·06	·08	·09	·09	·08	·12
4	·05	·08	·12	·18	·21	·24	·20
5	·13	·16	·18	·22	·26	·22	·18
6	·20	·23	·26	·28	·31	·30	·25
7	·14	·19	·15	·21	·24	·20	·15
8	·08	·10	·12	·15	·18	·15	·20
9	·53	·48	·51	·55	·55	·45	·48
10	·80	·92	1·10	·96	1·01	·92	·80
11	·85	·95	1·12	1·34	1·30	·50	·65
12	·62	·63	·69	·81	·92	·60	·65

SCIENTIFIC RESEARCH AND THE SEA FISHERIES

By J. T. JENKINS, D.Sc., Ph.D.,
Superintendent, Lancashire and Western Sea Fisheries

During the last few years no less than three Departmental Committees have inquired into and reported on the facilities for scientific research into problems concerning the sea fisheries of these islands. There was first of all the Committee on Ichthyological Research of 1902, then the Committee on Fishery Investigations of 1908, and finally the Committee appointed by Mr. Runciman in 1913 to "advise the Board (of Agriculture and Fisheries) on questions relating to the elucidation through scientific research of problems affecting fisheries."

The recent publication of the "first report" of the last Committee raises anew the whole question of the State aid of scientific investigation of the fisheries. The recommendations of the two former Committees are now of historical interest only, but in order to understand properly the present position it is necessary to consider briefly the position of scientific fishery research at the time these Committees reported. No one cognisant of the facts can fail to be struck with the enormous growth of marine biological research subsidised by the State ostensibly because it throws light on problems concerning the future of our fisheries.

The central authorities for fishery administration in the three kingdoms are in England the Board of Agriculture and Fisheries, in Scotland the Fishery Board, and in Ireland the Department of Agriculture and Technical Instruction. In England and Wales there are also local administrative authorities, usually Joint Committees of maritime County and County Borough Councils. In 1902, when the Committee on Ichthyological Research reported, the central fishery authority for England and Wales (then the Board of Trade) practically undertook no scientific fishery research, although they collected the commercial fishery statistics, for which a special grant was made by the Treasury. Research work was undertaken by a few of the district committees and by one or two scientific bodies, for instance, the Lancashire and Western Fisheries Committee and the Marine Biological Association, the latter body being the only one in England and Wales at that time in receipt of State aid in connection with fisheries work.

In Scotland and Ireland scientific research was undertaken at the cost of the State by the central fisheries authorities for those countries. As the result of their deliberations the Committee of 1902 made certain recommendations, of which the following were the most important. They stated it would be necessary for the State to provide funds for the collection of statistics from trawlers and the examination of material at the ports, for the provision of the necessary assistants at the marine laboratories already in existence, for the provision and maintenance of three research steamers, and for putting the staff in Scotland and Ireland on a permanent basis.

Between 1902 and the publication of the report of the Committee on Fishery Investigations of 1908 a considerable advance was made in the provision of fishery research. An international council for the exploration of the north and neighbouring seas was established as the result of international conferences held at Stockholm in 1899 and Christiania in 1901, to consider programmes for the investigation of the sea by scientific inquiry with a view to promoting and improving the fisheries through international agreements. The participating countries were Great Britain, Denmark, Germany, Holland, Norway, Russia, and Sweden, with (at the second conference) Belgium and Finland. The total expenditure incurred by Great Britain up to December 31, 1913, on the international investigation of the North Sea amounted to £154,919, exclusive of the cost of printing the reports.

In most other respects the position of the various authorities engaged in fishery research was pretty much the same in 1908 that it had been in 1902.

The Committee of 1908 recommended the establishment of a Central Council for the United Kingdom which should have control of public funds for fishery investigations of a national and international character. They also recommended the strengthening of the Board of Agriculture and Fisheries as the Central Fishery Authority for England and Wales, and the provision of additional funds to the Board for the encouragement of local work; the continuance of adequate provision to the Fishery Board for Scotland for local scientific research; the continuance of international co-operation in scientific and statistical investigations upon a definite and permanent basis; and finally, the continuance of the annual grant of £1,000 to the Marine Biological Association of the United Kingdom.

The chief difference between the position in 1908 as compared with 1902 was that the Government were expending about £13,000 per annum on the international investigations. In the Civil Service estimates for 1907–8 we find under the heading of "North Sea Fisheries Investigation" an amount of £12,500, being the sixth instalment on account of expenditure in connection with the international scheme for investigating problems concerning the fisheries of the North Sea and adjacent waters. The agents of the Government for the purposes of these investigations were in Scotland the Fishery Board, and in England the Marine Biological Association, to each of whom the sum of £5,500 was payable annually. In addition the annual sum of £1,250 was paid as a contribution to the Central Bureau which had been established at Copenhagen. The fact that the Board of Agriculture and Fisheries, the central authority for Fisheries in England, had been ignored in the allocation of these grants was a cause of considerable friction between the Board and the Association, as will be seen from a study of the evidence given before the Committee of 1908.

Ultimately the Board gained the victory, and they took over the control of England's share of the international investigations in 1910.

The next important event in the history of scientific fishery research was the passing of the Development and Road Improvement Funds Act of 1909. According to this Act the sum of £500,000 was to be set aside for five years for certain purposes, amongst which was "the development and improvement of the Fisheries." Applications for funds for this purpose were to be made in writing to the Treasury, who would refer the matter to the Development Commissioners for report.

In the first annual report of the Development Commissioners dated July 1911, there is one paragraph which refers to the fisheries, viz., "In respect to the development and improvement of fisheries proper, the Commissioners have received no applications. They learned some time ago that not inconsiderable applications for advances for such purposes had been made to the Treasury and referred to the Government Department or Departments concerned."

The fact is that the Board of Agriculture and Fisheries were quite unprepared with a scheme for the development of the fisheries, and they deliberately hung up schemes prepared by local authorities so that they might prepare and put forward their own scheme first. In the meanwhile, of course, they had the opportunity of considering the schemes forwarded by the local authorities. In one instance an application submitted by a local authority in May 1910 was definitely replied to in March 1912.

In the meantime the "comprehensive scheme" prepared by the Board of Agriculture and Fisheries and submitted by them to the Development Commissioners was rejected by the latter body as entirely unsuitable. It is not an easy matter to get particulars of this scheme *pour rire* of the Board. In fact it is extremely difficult to get authentic information as to its real parents; to quote Shakespeare we might "laugh to think that babe a bastard."

Practically every detail of the application was ruled out by the Commissioners. The Board's application was for a loan of £50,000 and an annual grant for the purpose of putting on vessels to patrol waters at present not properly protected, and they also applied for funds for what was euphemistically called "a Special Commission to inquire into the grievances of the inshore fishermen." It was obvious to any one acquainted with the terms of the Development Act that such an absurd scheme was bound to be rejected, since the Development Fund is not properly available for enabling authorities to perform their statutory duties, and neither can it be utilised to increase the salaries of permanent officials.

The comprehensive scheme of the Board having been rejected, the whole future of scientific fishery research was jeopardised, but the Development Commissioners met the difficulty by making interim grants.

The second report of the Commissioners, published in September 1912, contains some reference to these matters. We learn that the Board's application was considered by the

Commissioners, but as they understood that a number of applications relating to fishery matters had been addressed to the Treasury by scientific bodies, local fishery committees, and other authorities, and were awaiting report by the Board, the Commissioners thought it desirable to obtain these before proceeding with the consideration of the Board's scheme. Not being able to approve of the Board's scheme, they suggested to the Board (after the Commissioners' meeting in September 1911) that its application should take the form of a comprehensive scheme, prepared in consultation with the Scottish and Irish authorities, for the acquisition of further knowledge of the fisheries of the United Kingdom. Interim grants were recommended to the following bodies: The Lancashire and Western Local Fisheries Committee, £1,640; the Marine Biological Association, £500; the Liverpool Marine Biological Committee, £100; and the Eastern Local Fisheries Committee and the University College of Wales, Aberystwyth, £50 each. Of the £78,000 asked for by the Board the sum of £4,100 was granted, £600 for research on the lobster fisheries and £3,500 in aid of the general research work conducted by the Board.

In January 1913 Mr. Runciman appointed a Committee to advise the Board of Agriculture and Fisheries on matters connected with scientific fishery research. While this Committee was considering the situation the Development Commissioners issued their third annual report (in August 1913). In April 1912 the Board submitted to the Commissioners in outline a proposal for the provision of three research steamers which were then estimated to cost £10,000 each, and to consider annual grants of £10,000 for maintenance and £6,500 for the collection and study of material. The estimate of capital cost has since risen to £16,000 for each steamer. This, it will be noted, is an entirely different scheme to that first submitted by the Board. Possibly by now some one in the Department had looked up the provisions of the Development Act! The Commissioners agreed in principle to this scheme of the Board's, but thought it best to defer a grant for the construction or acquisition of the vessels until the scheme for which they are primarily required has been settled by consultation among the fishery authorities of the United Kingdom.

In January 1913 the Commissioners received the Board's application for the year 1913–14. The Board explained that they would not be in a position to submit the general scheme (first asked for in September 1911) before the expiration of the period for which the interim advances had been granted, *i.e.* March 1913, and they asked that the interim grants should be continued for another year, with an additional amount of £1,500 for the Board's research vessel.

In January 1914 the Scientific Research Committee appointed by Mr. Runciman twelve months previously reported, and shortly afterwards information leaked out that a comprehensive scheme had been prepared by the Board acting in conjunction with the Scottish and Irish authorities, and was presumably under consideration by the Commissioners. The Board now ask for £60,000 for the first year, and £25,050 per annum afterwards. Of this only £6,000 per annum is to be devoted to all the local authorities in England and Wales, the remainder being absorbed by the Board. This scheme, so far as it relates to England and Wales, has been prepared without the local authorities being in any way consulted. In addition to the three research steamers asked for by the Board in April 1912, two motor-boats are now considered to be necessary. The estimate of maintenance of the steamers has now gone up to £15,000 (from £10,000). One motor-boat is to cost £1,500, the other £1,000, and in each case the maintenance is fixed at £750 annually. Presumably this enormous expenditure is additional to that already incurred by the Board in respect to England's share in the North Sea fisheries international investigations, which amounts in the Civil Service Estimates for the year ending March 31, 1915, to £7,530. No mention is made in the estimates that any portion of this sum is repayable from the Development Fund, though in other cases "Fishery Development" grants in aid of research work and investigations conducted by the Board amounting to a total of £10,905 are so repayable, except as regards £1,105. If, therefore, the scheme of the Board be adopted by the Commissioners, the Board will spend during the first year's working of the scheme no less than £68,635, and afterwards about £33,000 a year. The bulk of this expenditure, in fact all except a small sum, is new. Most of the proposals are quite unjustifiable, and little or no attempt has been made to utilise existing organisations. The Marine Biological Association has a magnificent marine laboratory at Plymouth, and there are similar institutions at

Cullercoats, Piel, Liverpool, and Aberystwyth. Several of the District Committees have steamers which could be utilised, either partly or entirely, for observations at sea. For the assistance of all these bodies, for the provision of research for the development of the salmon and fresh-water fisheries, and for grants to local institutions for experimental work on and in connection with trade products, a beggarly £6,000 a year is proposed. But what is the worst feature in this preposterous scheme of the Board's is the manner in which the local authorities have been ignored in its preparation. One would have thought that the rejection of the Board's fatuous scheme of 1911 would have indicated the advisability of consulting those authorities having practical experience in the working of detailed schemes of fishery research. Fortunately Lord Richard Cavendish, the Chairman of the Development Commission, is a man of considerable acumen, and indications are not wanting that steps are being taken by the local authorities to enlighten him as to the true significance of the Board's scheme. When promotion in the Civil Service is dependent on the nepotic vagaries of peripatetic Cabinet Ministers, it becomes imperative in the public interest closely to scrutinise schemes which involve the expenditure of large sums of public money.

SOME RECENT WORK ON PLANT OXIDASES

By W. R. G. ATKINS, M.A., Sc.B., F.I.C.,
Assistant to the Professor of Botany, Trinity College, Dublin

In the last ten years the attention of plant physiologists has been largely directed to the study of chemical reactions which take place in the individual cells which go to make up tissues. By the application of suitable reagents it has been found possible to examine many of these qualitatively, and to some extent quantitatively. In such researches the formation of precipitates, amorphous or crystalline, and the production of colour reactions within the cells is examined with the aid of the microscope. Frequently it has happened that one and the same tissue has afforded material for workers with entirely different aims in view. For example, researches on carbohydrate formation and solution may be undertaken by one, while another may examine the processes of oxidation occurring in similar cells. Yet though these may seem very different phenomena, they are really closely connected as constituting together important parts of the life of the cell, the elucidation of which in its entirety is the aim of the physiologist.

Many of these changes can be brought about equally well outside the living cell, though the production of some of them *in vitro* has as yet baffled the chemist. But the methods by which they are effected in the organism are much more direct, and dispense with the high temperatures and concentrations of strong acids of which the chemist has to make use. Accordingly the study of enzymes, as the substances produced by the cells to bring about such specific chemical changes are termed, has become one of the most important branches of biology.

Numerous researches have been directed to the unravelling of the complicated inter-relations of the mechanism by which the fundamental need of oxygen is supplied, and it has been shown that enzymes termed oxidases are concerned in the utilisation of this gas.

It is with some of the more recent results of the study of the oxidases that this paper proposes to deal. The subject as it was known up to 1910 has been exhaustively treated of by Kastle,[1] also by Clark,[2] and Czapek,[3] and to these publications the author is much indebted.[4]

The Nature of Plant Oxidases

On the whole the substances which effect oxidations in plants have the properties of enzymes, though their behaviour is in some ways peculiar. The usual routine adopted in deciding whether a given reaction is enzymic or an ordinary chemical change is to boil the solution. If the reaction is no longer brought about it is concluded that it is enzymic, its cessation being due to the destruction of a thermolabile oxidising agent. But enzymes have two other very important characteristics, firstly, that a small quantity of the enzyme brings about a relatively enormous transformation of the substrate, and secondly, that the rate of this change is proportional to the amount of enzyme present (provided the substrate is in large excess), though the total amount transformed is independent of it if a sufficient time be allowed to elapse. It may be added that enzymes are colloidal, and the reactions they bring about or catalyse are in many cases proved to be reversible, the point of equilibrium being usually very near that of complete change in one direction. Furthermore their action is as a rule specific, one enzyme only acting on one substrate, or on one class of substrates, and may in many cases be inhibited entirely, or reduced in velocity by very minute quantities of paralysors.

Now the oxidising substances of plants are destroyed by heat, though in some cases they may be formed afresh within

[1] J. H. Kastle, Bull. No. 59, Hyg. Lab. U.S. Pub. Health and Mar.-Hosp. Serv. Wash. 1910.

[2] E. D. Clark, *Dissertation*, *Columbia Univ.* 1910 (Eschenbach Co., Easton, Pa.).

[3] Czapek, *Ergebnisse d. Physiol.* 1910, **9**, 587–613.

[4] I have followed the custom of the American authors, and of Fowler (*Bacteriological and Enzyme Chemistry*, Arnold, London, 1911), Moore, and Whitley, in writing "oxidase" rather than "oxydase," to denote the enzyme that splits up a (per)oxide. The spelling "oxydase" has been taken directly from the French, in which both "oxygen" and "oxydant" retain the letter "y." It seems an undesirable anomaly to spell "peroxide" with "i" and "peroxydase" with "y" as is done at present. "Oxygenase," the enzyme which splits up molecular oxygen, if such an enzyme exists, is of course correctly spelt with "y."

a couple of hours from thermostable zymogens, as has been shown by Woods[1] in the case of the oxidase of tobacco leaves. It is remarkable, however, that no other workers have as yet been able to find this zymogen. That they are colloidal and can be precipitated from aqueous solution by the addition of alcohol has also been shown.

The quantitative estimation of oxidases and of their rate of action presents many difficulties. Among the most successful attempts in this direction may be mentioned those of Chodat and Bach,[2] who examined the action of peroxidase upon a mixture of pyrogallol and hydrogen peroxide by weighing the purpurogallin formed under standard conditions. Their results showed that the peroxidase and peroxide take part in the reaction in a definite ratio, and that the weight of purpurogallin produced is proportional to the peroxidase. The above authors also devised a volumetric method. However, the most fundamental quantity to measure seems to be the amount of oxygen absorbed, and this Foà[3] and Mathews[4] have done and Bunzel[5] more recently with an elaborate apparatus and many necessary precautions previously omitted. Bunzel, too, finds that the amount of chemical change is directly proportional to the concentration of the oxidase present, and concludes that the typical plant oxidase with which he worked "is not an enzyme in the customary sense of the word, but rather a substance entering directly into the reaction, and being destroyed in the course of the same." He proposes as a unit to express the oxidase content of a plant juice "a solution of such a strength that one litre of it will be capable of bringing about the consumption by pyrogallol of the equivalent of one gram of hydrogen—*i.e.* a unit of eight grams of oxygen." It seems likely that much valuable knowledge will be gained from Bunzel's systematic quantitative researches at present in progress.

Before going further a distinction must be drawn between the terms oxidase and peroxidase. Originally those tissues which could bring about oxidations of natural chromogens or of artificial ones such as guaiacum resin, benzidine, *a*-naphthol

[1] Woods, Bull. No. 18, U.S. Dept. of Agric. 1902.
[2] Chodat and Bach, *Chem. Ber.* 1904, **37**, 1342.
[3] Foà, *Biochem. Zeitschr.* 1908, **11**, 382.
[4] Mathews, A. P., *Journ. Biol. Chem.* 1909, **6**, 3.
[5] Bunzel, H. H., Bull. No. 238, Bureau of Plant Industry, U.S. Dept. of Agric.

or pyrogallol, were said to contain an oxidase, whilst those which required the addition of a peroxide such as hydrogen peroxide or a spontaneously oxidised essential oil were described as containing a peroxidase. The view that an oxidase consisted of a peroxidase and a naturally occurring peroxide was put forward by Kastle and Lœvenhart,[1] and has gained very general acceptance. Keeble and Armstrong[2] record that in certain flowers the organic peroxide accumulates during darkness, so that apparently the tissues contain oxidase at one time and peroxidase at another. The author's own[3] observations on foliage leaves also point to the variability of the quantity of the peroxide in any tissue. At present it is usual to refer to the "direct" oxidase action, or to the "indirect" action, when the addition of a peroxide is required to bring about oxidation. Strictly speaking it would be more correct to refer to both as peroxidase actions, for the essential is that a peroxide is split up and oxygen derived from it is transferred to an easily oxidisable substance. That hydrogen peroxide does not occur in any appreciable quantity in the tissues is proved by the almost universal presence of catalase, an enzyme which decomposes this substance, but does not attack the closely related ethyl hydroperoxide or any other peroxide, so far as is known.

How far the oxidases are specific in their action seems to be in doubt. Five different oxidases at least have been described as occurring, or rather five different classes of oxidases, viz. laccases, tyrosinases, alcoholases, purine oxidases, and aldehydases. The laccases or phenolases which act on many phenols and are very widely distributed in plants, and the tyrosinases which act on tyrosin or polypeptides containing tyrosin to produce a body which further reacts with amino-acids yielding dark-coloured pigments termed melanins, as shown by Abderhalden and Guggenheim.[4] An example of the alcoholases is furnished by the enzyme which converts ethyl alcohol into acetic acid and is found in certain bacteria. The purine oxidases have been extracted so far from animal tissues only, and the existence of the aldehydases is still hypothetical.

[1] Kastle and Lœvenhart, *Amer. Chem. Journ.* 1901, **26**, 539.
[2] Keeble and Armstrong, *Journ. Genetics*, 1912, **2**, No. 3, 277.
[3] Atkins, W. R. G., *Sci. Proc. R. Dubl. Soc.* 1913, **14** (N.S.), 144.
[4] Abderhalden and Guggenheim, *Hoppe-Seylers Zeitschr. f. physiol. Chem.* 1908, **54**, 331.

It should be mentioned that the production of the organic peroxides before alluded to is by some held to be the work of a special enzyme oxygenase.

It has been pointed out by Bertrand[1] that the aromatic monophenols and monamins are not easily oxidised by laccase, but that substances which it readily attacks are all members of the benzene series containing hydroxyl or amino groups in the ortho or para positions.

Researches in plant physiology deal almost entirely with the two classes of enzyme at the head of the list; there is at present no proof that the laccase or tyrosinase from one species is identical with that from another, though they may produce certain colour reactions in common. This important question is being investigated by Bunzel. The part played by inhibitors in effecting apparent specific action by oxidases will be treated of later on.

For further information, concerning the preparation of artificial oxidases, the effect of small quantities of acids, alkalis and manganese salts upon the activity of oxidases, as well as for discussions of the identity of Medicago-oxidase with a mixture of calcium salts of organic hydroxy acids including glycollic, citric, malic and mesoxalic, the reader is referred to the monographs by Kastle and by Clark, and to Euler's *General Chemistry of the Enzymes*.[2]

The Physiological Functions of the Plant Oxidases

Respiration.—It is a matter of common observation that leaves when killed by frost or by severance from the tree frequently assume a brown, black or red colour, and that local injuries such as punctures by insects or by parasitic fungi take on similar shades of pigmentation. The conspicuous purple red spots appearing on blackberry leaves in autumn are a good example of the effect of the last-mentioned cause, being brought about by the disorders in metabolism due to the attack of *Phragmidium violaceum*, the teleutospores of which are always found as black specks on the lower surfaces of leaves which show such discolorations.

Numerous investigators have established the fact that these

[1] Bertrand, *C. R.* 1896, **122**, 1132.

[2] Wiley & Sons, New York, 1912.

colours are produced by the action of oxidases upon colourless sap-soluble chromogens, the latter probably arising by the hydrolysis of a complex glucoside. Much work has been done by Palladin[1] and his pupils in examining the distribution of such chromogens, which they believe to be of fundamental importance in respiration. They conceived of this process as a taking in of oxygen by a readily oxidisable substance to form a peroxide. The latter is then split up by oxidase, yielding its oxygen for the oxidation of reducing substances elaborated by the protoplasm.

More recently Palladin[2] has brought forward the view that the respiration of a substance such as glucose is a hydrolytic oxidation, whereby the carbon is oxidised anaërobically to carbon dioxide, and the hydrogen thus set free combines with a respiratory pigment, reducing it to a colourless chromogen. In the following aërobic stage oxygen is absorbed, with the production of water and the pigment. These processes are shown in the following equations:

1. Anaërobic stage:

$$\underset{\text{Glucose.}}{C_6H_{12}O_6} + \underset{\text{Water.}}{6H_2O} + \underset{\text{Respiratory pigment.}}{12R} = \underset{\text{Carbon dioxide.}}{6CO_2} + \underset{\text{Chromogen.}}{12RH_2}$$

2. Aërobic stage:

$$12RH_2 + 6O_2 = 12H_2O + 12R.$$

An interesting example of the action of a respiratory enzyme obtained from the spadix of an Aroid has lately been studied by Weevers.[3] The heat-evolution in the spadix had long been known, as had also the facts that it arose from the oxidation of sugars and that the products were carbon dioxide and organic acids. Weevers established that oxidation could be carried out actively by the enzyme in air or in an atmosphere of hydrogen. Glucose was decomposed with the formation of carbonic and citric acids, but without any production of alcohol even when the reaction took place in hydrogen. He concludes, therefore, that the enzyme cannot be a zymase, for in addition to the absence of alcohol the presence of an organic acid was demonstrated.

[1] Palladin, *Ber. d. deut. Bot. Gesell.*, 1908, **26**, 378, 389.
[2] *Ibid.* 1913, **31**, 80.
[3] Weevers, *Kon. Akad. v. Wetenschappen*, Amsterdam, 1911, Oct. 28.

With regard to the chromogens it is found that the quantity of the autoxidised materials normally present is insufficient to impart a colour to the cells which are continuously manufacturing reducing substances. But upon the death of the cells or upon the shortage of supplies of materials necessary for metabolism, the activity of the oxidase is unchecked and many changes are effected, among them being the production of sap-soluble pigments from the chromogens. The latter may be tested for by pressing some sap from the organ under examination. If this appears brown it is invariably found that an oxidase is present, together with organic peroxide and chromogen. If no colour is produced it must be further tested by the addition of hydrogen peroxide. The darkening of the sap then takes place if it contains a chromogen, unless oxidase action is hindered by an inhibitor. In this eventuality a considerable amount of an oxidase preparation has also to be mixed with the sap in order to ascertain whether it will darken or not.

Distribution of Oxidases in Plants.—In the above paragraph it has been assumed that oxidase is present, to some degree at least, in every vegetable cell, and this I believe to be the case in the large majority of land plants. Bourquelot and Bertrand,[1] Zellner,[2] Pringsheim,[3] Kastle,[4] and others, have shown that phenolases and tyrosinases are of almost universal occurrence in fungi. Clark[5] tested a large number of groups of flowering plants and pteridophytes, and has found phenolases to be of very general occurrence: in certain strongly acid saps, however, he failed to detect any oxidase; Moore and Whitley[6] also noted their absence from the pulp of lemons, limes, and oranges.

Some cases are met with in which the usual tests such as guaiacum resin, benzidine, and α-naphthol fail to give any reaction even after the addition of hydrogen peroxide. In such tissues an inhibitor is usually present. For example,

[1] Bourquelot and Bertrand, *Compt. Rend. Soc. Biol.* 1895, **47**, 582, and *Bull. Soc. Mycol. de France*, 1896, **12**, 18.

[2] Zellner, *Die Chemie der Höheren Pilze*, 209, Leipzig, 1907.

[3] Pringsheim, *Zeitschr. physiol. Chem.* 1909, **62**, 386.

[4] Kastle, J. H., Bull. No. 26, Hyg. Lab. U.S. Pub. Health & Mar.-Hosp. Sew. Wash., 1906.

[5] Clark, E. D., *loc. cit.*

[6] Moore and Whitley, *Biochem. Journ.* 1909, **4**, 136.

the young leaves of the Virginian creeper (*Vitis Veitchii*) are red, and give the oxidase reactions. The mature leaves are green and give no oxidase reactions; but in them tannin is present, which is known to act as an inhibitor. Finally, in autumn the leaves again become red, yield positive results with oxidase reagents, and contain no tannin. Another instance examined by the author[1] is the behaviour of the leaves of *Iris germanica*, which contain reducing substances in quantity, and fail to reveal oxidases in the pressed sap, though microscopic examination of sections treated with the usual reagents demonstrates their presence in certain tissues. Moreover, on pouring the sap into alcohol a precipitate is obtained. When this is well washed with spirit and redissolved in water, it is found to give the guaiacum reaction for an oxidase. The inhibitor here cannot be an anti-enzyme of colloidal nature, for after dialysis of the sap in presence of toluene a direct oxidase reaction can be obtained with guaiacum. In *Pteris aquilina* leaf sap too, removal of an inhibitor by dialysis permitted the detection of an indirect oxidase reaction.

In the course of their researches on the production of anthocyan pigments, Keeble and Armstrong[2] showed that certain white flowers owed their colour to the presence of an inhibitor which prevented the action of an oxidase. These authors observed that treatment with dilute hydrocyanic acid and subsequent thorough washing neutralised the action of the inhibitor, so that the oxidase now afforded the usual reactions. I have also found their method effective with the flowers of a number of varieties of Iris,[3] and with the tissues of certain fruits. Thus it is clear that in numerous instances the occurrence of the oxidase may be demonstrated by a variation of the usual procedure, and so negative results must be accepted with caution.

With regard to the oxidases of the fresh water and marine algæ, however, but little is known. It has recently been shown by the author[4] that oxidases of the phenolase type at least are very infrequently met with in this class of plants. Out of about thirty species of the green, brown and red marine algæ only six were found to contain oxidases. Brown algæ of the Laminaria

[1] Atkins, W. R. G., *loc. cit.*
[2] Keeble and Armstrong, *Proc. R. Soc.* 1912, **85** B, 214.
[3] Atkins, W. R. G., *Sci. Proc. R. Dubl. Soc.* 1914, **14** (N.S.), No. 8, 157.
[4] *Ibid.* 1914, **14** (N.S.), No. 11, 199.

class were indeed the only ones to give well-marked indirect reactions with guaiacum resin. Nevertheless most algæ tested give a colour with the benzidine reagent, more especially in their cell walls. But this must not be regarded as always indicating an oxidase, for in some of the cases most thoroughly examined—certain of the green algæ—the blue colour was found to be produced at an equally rapid rate immediately after boiling. These investigations on algæ are at present being continued.

The widespread distribution of oxidases and chromogens in land plants shows that Palladin's views on respiration are of very wide application. The conditions of life of water-dwelling plants are so different from those of sub-aërial vegetation that it would not be surprising if the respiration of the former was carried on in a somewhat modified manner. Reducing substances occur in their tissues, and I have not yet found it possible to detect masked oxidases in them by removal of inhibitors by any of the methods previously described. It is noteworthy that catalase was found in every case, being remarkably active in many.

The Oxidases in relation to Pigmentation.—It has been known for a considerable time that there is a causal connection between the occurrence of oxidases and sap-pigments in stems and the veins of leaves. Reinke[1] investigated the chromogens as far back as 1882. More recently they have been studied by Wheldale,[2] Molisch,[3] Keeble, Armstrong and Jones,[4] Bartlett,[5] and others. The nature of the sap-soluble anthocyan pigments cannot yet be regarded as firmly established. Wheldale regards the anthocyanins as oxidation and condensation products of colourless chromogens which are present in living cells as part of a glucoside molecule. The hydrolysis of the glucoside is considered to be a reversible enzyme action, and only the free chromogen, which belongs to the aromatic group of chemical compounds, can be attacked by the oxidase. This view, which greatly stimulated research, has recently been shown to be in need of some modification, or at any rate not to hold univer-

[1] Reinke, *Zeitschr. f. physiol. Chem.* 1882, **6**, 263.

[2] Wheldale, *Progressus Rei Botanicæ*, 1910, **3**, 457; also *Journ. of Genetics*, 1911, **1**, 133.

[3] Molisch, *Bot. Zeitschr.* 1905, 63, 145.

[4] Keeble, Armstrong, and Jones, *Proc. Roy. Soc.* 1913, B, 87, 113.

[5] Bartlett, H. H., Bull. No. 264, Bureau of Plant Industry, U.S. Dept. of Agric.

sally. Bartlett, after a careful investigation into the nature of a water-soluble ammonia-greening anthocyanin, concludes that it is itself a glucoside, and states that the only non-glucosidal ammonia-greening anthocyanin known to him is insoluble in water. This, it may be remarked, is obtained from Althæa. Molisch records the occurrence of anthocyanin in the solid condition, both as amorphous and crystalline aggregates, in many flowers and leaves. Keeble, Armstrong and Jones have shown that the pale yellow sap-colour of the petals of the wallflower is a mixture of hydroxy-flavone glucosides. From this by suitable treatment a red pigment may be obtained, which is not a glucoside. Oxidations of the hydrolysed products of the yellow glucosides by means of oxidase, in presence of amino-acids, result also in the production of pigments. An interesting research by Willstätter and Everest[1] on the blue pigment of cornflowers has just appeared. They have determined that the blue pigment is the potassium salt of an acid (cyanin), which is violet in the free state, whereas the red pigments are combinations of this acid with simple organic acids. Thus the occurrence of various shades of colour in the flower is explained. These two authors also believe that all anthocyanins are present in flowers as glucosides. Their paper contains many other points of importance.

The beautiful changes of colour which many flowers undergo in fading have arrested the attention of both the scientist and the poet. This phenomenon has been employed in a beautiful simile by "A. E." (*Collected Poems*, p. 9):

> Its edges foamed with amethyst and rose,
> Withers once more the old blue flower of day:
> There where the ether like a diamond glows
> Its petals fade away.

By means of the use of benzidine and α-naphthol as micro-chemical reagents, Keeble, Armstrong, and Jones have studied the distribution of oxidases in flowers, and have shown that when oxidase and chromogen are both present a white colour may still persist owing to the action of an inhibitor. The removal of the latter by dilute hydrogen cyanide permits of the action of the enzyme on one of the artificial chromogens pre-

[1] Willstätter, R., and Everest, A. E., *Liebig's Annalen*, 1913, **401**, 189; and *J.C.S.* Dec. 1913, A. i. 1371.

viously mentioned. Making use of plants of known pedigree they established the occurrence of various kinds of white flowers, such as the type described above in which an inhibitor exists, and that in which an active oxidase is found, unaccompanied by a chromogen. They have also explained the restoration of colour to a flower decolorised in alcohol. For example, the brown wallflower has yellow plastids and a red anthocyanin sap pigment. Alcohol extracts the former, and reducing substances present in the cell destroy the latter. On addition of water, however, the oxidases again become active, and produce the red pigment from the chromogen by oxidation. So the flower then appears red, and not brown, as the yellow is not restored. The author has examined the flowers of about thirty varieties of Iris by means of benzidine and α-naphthol, and on the whole the results obtained follow closely those of Keeble, Armstrong and Jones. The presence of a powerful inhibitor in many of the flowers presents features of interest, and the restoration of the anthocyanin pigment after decolorisation in alcohol is only brought about in the more deeply pigmented forms. The pigment is diffusible, and unless the chromogen is also diffusible the supply ought not to diminish. That it does diminish would appear to point to production of pigment from a chromogen derived from a colloidal pro-chromogen by hydrolysis. In this connection it is noteworthy that in the red algæ there exists a plastid pigment which is seen to be bright red when other pigments have been extracted from the plastids by alcohol. It behaves like an indicator, being red with acids, colourless with alkalis. Though the cell sap is faintly acid, it is decolorised by boiling, apparently going into solution, but addition of an acid restores the colour, probably by hydrolysis of a chromogen. This the author hopes to examine further.[1]

[1] Since this paper was written, the above explanation of Keeble and Armstrong as to the restoration of colour in petals has been severely criticised by Wheldale and Bassett (*Proc. Roy. Soc.* 1914, B. **87**, 300). These authors regard it as due to ionisation changes, such as are undergone by phenolphthalein, rather than as an oxidase action, though they are of the opinion that the enzyme is concerned with the production of the anthocyan pigment in the first instance. The author has satisfied himself as to the validity of the major part of this criticism by repeating the work on Iris petals. It must, however, be pointed out that hydrogen peroxide frequently behaves as a reducing agent; this Wheldale and Bassett have overlooked. The restoration of colour in the red algæ is also brought about by acids, as mentioned above.

In many flowers there is an inhibitor in cells which contain plastid pigments, and in all cells between them and the upper surface. This was observed in Primulas by Keeble and Armstrong, and by the author in varieties of Spanish Iris. The contrast of the dark colour of the benzidine oxidation products in the petals with the colourless inhibition areas is frequently very striking.

The white flowers previously mentioned as containing inhibitors have been shown to be Mendelian dominants, when crossed with coloured varieties, whereas the whites which are white through lack of chromogen behave as recessives.

The Rôle of the Oxidases in Plant Pathology

Since upon the death of the protoplasm oxidases act without the restraints to which they are subject during its life, it might well be supposed that conditions unfavourable to the normal metabolism of the cell might result in increased oxidase activity. This has been found true in a number of instances. It had been observed that when mulberry trees were cut back too frequently an abnormal yellow colour and crinkled appearance resulted in the leaves. Suzuki[1] investigating this found that an excessive production of oxidases had taken place in such yellow areas. He attributed this to the lack of proper nutrition of rapidly growing tissues. Much the same phenomena were observed by Woods[2] in the "mosaic disease" of tobacco plants which have been cut back. He also demonstrated that the condition was rendered more acute by the application of certain manures which increase the rate of growth.

More recently Bunzel[3] has investigated the oxidase content of normal leaves of the sugar beet, and of those affected with the "curly-top" disease, which has been shown by Ball[4] to develop after the bite of an insect, the curly-top leaf-hopper (*Eutettix tenella*). Bunzel ascertained that the leaves of the curly-top plants had an oxidase content two to three times as great as the healthy and normally developed ones. No marked differences, however, could be detected between the roots of the two kinds of plants. Bunzel admits that this

[1] Suzuki, Bull. Agric. Coll. Tokyo, 1900, 4, 167 and 267.
[2] Woods, Bull. 18, Bur. Plant Industry, U.S. Dept. of Agric. 1902.
[3] Bunzel, Bull. 277, Bur. Plant Industry, U.S. Dept. of Agric. 1913.
[4] Ball, Bull. 66, Bur. of Entomology, U.S. Dept. of Agric. 1911, pt. 4, p. 33.

observed increase may be due to a change in the juice by which the pyrogallol oxidising enzyme becomes more active. The extraordinary alterations in oxidase reactions brought about by treatment with hydrogen cyanide and subsequent washing as advocated by Keeble and Armstrong render it very probable that such quantitative estimations are largely influenced by the presence of inhibitors. The author's own work on Iris flowers and other plant tissues has afforded additional evidence of the widespread occurrence of these bodies. However, this does not alter the fact that the functional activity of oxidase is greatly increased in the affected leaves, producing, as it were, a state of "fever," according to Bunzel. In this connection it may be remarked that many apparent specific actions by oxidases, such as the oxidation of benzidine by a tissue though it fails to oxidise guaiacum, can be proved, by treatment with hydrogen cyanide, to be due to inhibitors. Whether these act as genuine inhibitors, or by being themselves more readily oxidisable than the added artificial chromogens, is as yet undecided. Bunzel's direct measurement of oxygen absorption would appear to be the most promising method of attacking this problem.

The Bearing of Oxidase Investigations on Technology

In addition to the interest of oxidase study for the silk and sugar industries mentioned in the last section there are several other more direct applications. The researches of Yoshida[1] in 1883 showed that an oxidase was concerned in the production of the well-known black varnish obtained from the milk-like sap of the lac tree, *Rhus vermicifera* and allied species. He obtained from the sap an acid, urushic acid, which when oxidised by the enzyme becomes black and forms the basis of the varnish which is so much used in China and Japan. Eleven years later Bertrand[2] confirmed and extended Yoshida's work, giving the name laccase to the enzyme, and pointing out the relationship of urushic acid to the hydroxy derivatives of the benzene series.

In the preparation of tea also an oxidase has been proved by Mann[3] to play an important part. In green tea the leaf is

[1] Yoshida, *Journ. Chem. Soc. Trans.* 1883, **43**, 472.

[2] Bertrand, *Compt. Rend. Acad. Sci.* 1894, **118**, 1215.

[3] Mann, H. H. Quoted from Fowler's *Biological and Enzyme Chemistry* (Arnold, London, 1911).

roasted immediately after picking, before any appreciable oxidation takes place. In black tea, on the other hand, an oxidation of the tannin is effected by an oxidase of the laccase class, resulting in the production of a soluble brown substance to which the colour of the infusion is due. The pungency, on the contrary, is dependent on the amount of unoxidised tannin, while the flavour is caused mainly by an essential oil. By carefully regulating the different processes of withering, rolling, and oxidation the qualities of the tea may be altered within limits. All the operations involved must be carried out with the utmost cleanliness to avoid bacterial contamination as far as possible, as such gives rise to sourness, rendering the tea unfit for consumption.

In the procuring of cocoa beans, too, fermentation processes are involved which loosen the seeds in the fruit. In these stages yeasts and acetic acid producing bacteria are active. Oxidases are also at work both during the fermentation and subsequent drying, as shown by Loew,[1] the change from the violet colour of the fresh bean to a deep brown being due to their agency.

The curing of tobacco again involves a fermentation. The leaves after a preliminary withering are "sweated" in moderate sized heaps, and fermented in very large heaps containing many tons. It was at one time thought that this was a bacterial process, but owing to the work of Loew[2] and other American chemists it has been shown to be mainly one of respiration of starch, sugars, and tannin, brought about by oxidases in conjunction with hydrolytic enzymes.

Another instance of the importance of oxidases in commerce is that of the blemish known as "sap stain" in lumber, by which considerable portions of the wood of certain trees when exposed to the air by sawing into planks or beams were discoloured, and so depreciated in value considerably. It has been demonstrated by Bailey[3] that this is brought about by an oxidase.

The preparation of ensilage has been investigated by Russell[4]

[1] Loew, Ann. Report, Porto Rico Agric. Expt. Station, 1907, quoted from Fowler *loc. cit.*

[2] Loew, *loc. cit.* Quoted from Fowler, *loc. cit.*

[3] Bailey, *Bot. Gaz.* 1910, **50**, 142.

[4] Russell, *J. Agric. Science*, 1908, vol. ii. pt. 4.

within the last few years. He studied the changes taking place in green maize stems packed closely together to form a "silo." Bacterial action sets in, and also action of the respiratory oxidases and other enzymes of the plant cells. So large an amount of heat is generated by the oxidases that the temperature rises to such a degree as to inhibit further bacterial growth. The hydrolytic and proteolytic enzymes still retain their activity, however, and a complicated series of changes ensues, in which organic acids appear among the products.

In the foregoing account of the oxidases the chief difficulty experienced by the author has been that of deciding what to omit, as the subject is of such dimensions and of such rapid growth. Furthermore it has not been possible to present both sides of all the questions involved, owing to limitation of space and a desire to give a connected account of the various researches.

PLANT CHIMÆRAS

By MACGREGOR SKENE, B.Sc.
Lecturer on Vegetable Physiology, Aberdeen University

THE practice of horticulture and the study of heredity have led to the production of innumerable hybrids, plants having their origin in the fused sex-cells of two distinct races, species, or even genera, and frequently betraying their dual nature by exhibiting characteristics of both parents. From these legions of intermediate forms, as to the sexual origin of which there is no doubt, there stand apart a very few isolated cases of hybrids which seem to have arisen in another fashion. By reason of their supposed mode of origin they have long been labelled *graft-hybrids*; and both because of this origin, which was doubtful, and because of various peculiarities which they constantly exhibited, a considerable amount of attention has been paid them during the last eighty years or so. Only five years ago, however, did the various problems involved prove capable of an experimental solution. Professor Winkler, of Hamburg, has succeeded in replacing the airy castles of theory by an edifice built of solid fact enough, but of a nature more curious than that of any of its predecessors.

The most notable of the so-called graft-hybrids is the shrub known as *Cytisus Adami*, which is supposed to have arisen in the following way. It is a common practice of gardeners to graft the purple broom, *Cytisus purpureus*, on to the laburnum, *Cytisus Laburnum*. *Cytisus purpureus* bears on its spiky stems many tufts of fine purple flowers, but the stems are low-growing and the plant makes no great show. Grafted on the laburnum, however, it artificially raises its purple head and thereby greatly enhances its decorative value. In the year 1829 the Parisian gardener Adam observed that a bud from one of these grafts had produced a novelty. The flowers on the new shoot were indeed purple in colour, but instead of occurring in small erect tufts they occurred in the fine drooping clusters so characteristic of the laburnum. In other words, the inflorescence exhibited characters of both parents, the purple broom and the laburnum. This was not confined to the flowers; leaves and stem also showed an intermediate structure. There could be no doubt that this *Cytisus Adami*, as the new plant came to be styled, was of hybrid origin; and if the account of Adam were correct it would seem that there could be equally little doubt that it had arisen during, or subsequent to, the process of grafting. That doubt has for various reasons been thrown on Adam's story is no reflection on the individual, but merely results from the general fact that practical men keep no exact records of the plants passing through their hands, and in particular do not carry out their operations under the carefully controlled conditions which modern science demands.

Apart from actual *proof* there are strong reasons supporting the "graft" origin of the hybrid. Perhaps the most important is the fact that no attempt to raise seed from the laburnum pollinated by the purple broom, or from the purple broom pollinated by the laburnum, has ever succeeded: the two plants are mutually absolutely sterile. And then we have the very remarkable behaviour of the hybrid itself. *Cytisus Adami* is now a common ornamental shrub; it has been possible to multiply the original shoot indefinitely by grafting, and its appearance and characteristics are widely known and may be studied in numerous private gardens. Its most striking feature is that it does not maintain the intermediate character in all its branches. From a shoot of typical *Adami* will arise, apparently without reason, a branch of pure laburnum, or one which possesses the characters of the purple broom alone. Not only this, but even a single flower will show one half hybrid, and the other belonging to one of the parents: a single petal may have the same mixed constitution, or a single leaf. This production of "vegetative throwbacks" is extremely rare in sexual hybrids.

Striking as these facts are, so eminent an authority as Prof. De Vries wrote in his "Species and Varieties" that there was no evidence to show that *Cytisus Adami* had not arisen as a sexual hybrid. Against the fact that the two parents are mutually sterile he points out that although, since 1829 *C. purpureus* has been grafted on *C. Laburnum* many thousands of times, yet in no single case has this resulted in the formation of a hybrid. That such had occurred on a single occasion is just as probable, or as improbable, as that in one isolated instance an ovule of laburnum had been fertilised by pollen from the purple broom giving a seed of hybrid origin: that this latter is the true explanation was Dr. Vries's belief.

We must encounter, too, the difficulty of explaining the mechanism by which hybridisation during grafting could take place. The stock never influences the specific characters of the scion. The more luxurious growth of a delicate scion placed on a sturdy stock is due solely to an improved food supply. Prof. Winkler in an exhaustive memoir ("Pfropfbastarde," Part I.) has shown that there is no well-authenticated case of a specific change in stock or scion resulting from the influence of the co-partner. There remained, seemingly, the supposition that a fusion of cells of the two parents had given rise to an organism bearing the characters of both. But without definite proof it is not possible to believe that ordinary vegetative cells of two highly organised seed-plants could assume the characters of sex-cells.

One line of evidence, the nature of its progeny, might have led to a solution; but *Cytisus Adami* is sterile. In the long course of its history it has produced two seeds, and both grew up into typical laburnums. But the number is far too small to make it permissible to draw conclusions from this fact.

Cytisus Adami is the best known and most widely distributed "graft" hybrid. But one or two other similar cases are known. Of these we may mention the hybrids between the hawthorn and medlar to which the name *Cratægomespilus* has been given. Of these no less than three are known; one is intermediate between the two parents, of the two others one resembles more closely the hawthorn, the second the medlar. Their origin is shrouded in even greater mystery than that of the *Cytisus Adami*; but it is equally certain that the parents are mutually sterile, and that the hybrids produce vegetative throwbacks.

When Prof. Winkler commenced his investigations some seven years ago our knowledge of the origin of these curious plants was sadly indefinite. That they had arisen by grafting seemed improbable; that they possessed properties seen in no other hybrids made a sexual origin equally doubtful.

Prof. Winkler saw that it was useless to attack the problem by grafting laburnums or hawthorns; that had been tried times without number, with uniform lack of success. He looked round for other plants which might proffer greater hopes of positive results. In the first place, the subjects of experiment must graft easily, and be readily reared in large numbers; in the second, they must have the property of producing large numbers of *adventitious* buds from wounded surfaces. This was of great importance, because he recognised that it was in *new* buds arising from the graft, and not in the original scion, that a modification was to be looked for. His studies in regeneration enabled him to single out the most suitable plants, and these he states are the poplars, the herbaceous members of the *Capparidaceæ*, and the *Solanums*. His results were obtained with these last.

Briefly, the method employed is as follows. Two to three month old seedlings of *Solanum lycopersicum*, the tomato, are decapitated, and the apex of the stump is split open about a couple of inches. Into this slit is inserted the wedge-shaped end of the tip of a seedling of *Solanum nigrum*, the nightshade. The two are bound together with bast, and kept moist and poorly lighted, till the wounded surfaces have grown well together, and the graft is complete. If left in this condition the nightshade thrives excellently, and produces flowers and fruits on the tomato stump. But if the system be again decapitated at the point of union of the two plants, and if the precaution be taken to remove all the buds from the axils of the leaves of the tomato stump, then, after about a fortnight, a large number of adventitious buds develop on the cut surface. If the decapitation has been performed at the proper point there will be on the cut surface a small wedge of nightshade tissue, grown firmly between the two halves of the tomato stem.

Now all buds that developed on the tomato gave shoots of pure tomato, and all buds that developed on the wedge of nightshade gave pure nightshade. But in August 1907 Prof. Winkler observed a bud which arose from a point on the line of junction of the tissues of the two plants, and which developed into a shoot of a unique character. The first leaf was a nightshade leaf, and so were the fourth, fifth, and seventh leaves, and these all arose from the side of the stem towards the wedge of nightshade tissue; but the second, third, and sixth leaves, which were on the tomato side, were all tomato leaves; and the eighth, ninth, and eleventh, occupying positions opposite the line of junction, were leaves of which one longitudinal half was tomato, the other nightshade. In other words, the plant was a composite one, the one longitudinal half being a nightshade, the other a tomato.

The leaves of the tomato are large, feather-compound, hairy of surface, thick in texture, light green in colour, and with notched edges: those of the nightshade are smaller, simple, almost glabrous, thin in texture, dark green, and entire. There can arise no doubt as to the identity of the one or the other.

This plant Prof. Winkler aptly named a *Chimæra* in imitation of Homer's "mingled monster of no mortal kind."

Following on this partial success, the ultimate aim of the experiments was attained in succeeding years, when buds developing similarly, at the line of junction of the two parent tissues, produced shoots which were plainly of a hybrid nature. Of these hybrids no less than five distinct types have been described and named by Prof. Winkler, while other investigators have obtained forms differing from any of these.

The first experimentally produced "graft hybrid" was named *Solanum tübingense.* It possesses leaves which are simple, like those of the nightshade, but with the notched margins, and the hairy surface of the tomato. Without entering into details, it may be stated that the flowers, the fruits, and the stem are also intermediate in character: the plant is easily rooted and maintained by means of cuttings. *Solanum Kœlreuterianum* resembles the tomato, but has leaves with the glabrous nightshade surface. Between these two extremes lie *S. Gærtnerianum*, *S. Darwinianum*, and *S. proteus.*

Of great interest is the fact that, like *Cytisus Adami*, these plants produce vegetative throwbacks to one or other parent: these are most abundant among shoots from adventitious buds on cut surfaces; but they also occur spontaneously. Happily several of the hybrids are fertile, and the curious fact came out that the seeds of any particular hybrid always give rise to plants bearing the characters of one of the parents. Thus *tübingense* and *Gærtnerianum* always give nightshade; *proteus* always gives tomato. Further, *tübingense* and *Gærtnerianum* are fertile with the nightshade, giving nightshade, but not with the tomato; while with *proteus* the reverse is the case. The tomato and nightshade are mutually sterile.

The possibility of producing plants of a hybrid nature by the process of grafting is therefore proved beyond all doubt.

But we are left with the difficulty of explaining the mechanism by which this occurs. Prof. Winkler was at first inclined to believe that his hybrids were *Hyperchimæras*, plants in which the tissues of the two parents were mingled together in a very intimate manner, instead of being isolated in two distinct longitudinal strips. This view he gave up, after a discussion in the German Botanical Society had brought out the difficulties which stood in its way. He then expressed the opinion that they had arisen by a cell-fusion at the point of grafting—a view vigorously combated by Strassburger. To Prof. Erwin Baur belongs the credit of having made the suggestion which subsequent investigation has proved to be the correct solution of the problem.

He was engaged in the study of the heredity of the *Pelargoniums*, and he found, on examining anatomically the leaves of those forms with white margins, that the organ consisted of a core of green tissue surrounded by two or more layers of cells, in which the chloroplasts were degenerate and contained no chlorophyll—a hand of green tissue in a glove of colourless. At the margin of the leaf the number of layers of cells is reduced till finally only those with colourless chloroplasts persist, and thus is produced the white margin. The idea of applying this arrangement to explain the properties of the *Solanum* hybrids was shortly afterwards made public. Thereupon Prof. Winkler examined his plants to test the truth of the hypothesis. As it happens, this is fairly easily done. *Solanum lycopersicum* possesses 24 chromosomes, *S nigrum* 72: it was only necessary to count the number of chromosomes in the different layers of the vegetative points of the intermediate types.

Prof. Baur's theory proved to be correct. The hybrids are indeed plants in which a core of one parent is enclosed in a skin of the other. *Tübingense* consists of nightshade covered by a single layer of tomato; *proteus* has two layers of tomato; in *Kœlreuterianum* and *Gærtnerianum* the tomato is the core covered by one and two layers of nightshade respectively. We are dealing, then, with plants which are not *hybrids* in the exact sense of the word at all; they are, in fact, *chimæras* just as much as the original object to which that term was applied, with this difference—that, as the tissues are laid one over the other, and not side by side, the result is a plant which exhibits characters intermediate between those of the parents: to express this particular arrangement the term *Periclinal chimæra* has been adopted.

Let us see in what way this knowledge helps us to explain the peculiarities of these plants. In the first place, the particular form will resemble the one or other parent more closely, as the one or other parent enters more largely into its composition: a reference to the characters and structure of *tübingense* and *Kœlreuterianum* shows this to be the case. Secondly, the phenomenon of vegetative throwbacks is readily understood when we see that a slight accidental injury or derangement of the tissues of one partner will permit of the appearance of the other in its pure state: this is, of course, more particularly the case with adventitious buds, arising from a wounded surface. And, finally, the fact that the intermediate form always breeds true to one parent becomes self-evident when we remember that the sex-cells are always produced from the sub-epidermal layer, and therefore always belong to the one parent: that the chimæra is sterile with the other parent is explained by the same considerations. The parent to which the seeds of the chimæra revert will be the parent which constitutes the sub-epidermal layer, and this is in fact the case.

This explanation is admirably simple: can it be applied to these original "graft hybrids," the origin of the whole discussion? Anatomical investigation has shown that it can. The arrangement of the pigments in the petals of *Cytisus Adami* shows that that shrub is a chimæra, which consists of a core of laburnum with an epiderm of purple broom. The characters of the epiderm and cortex of the fruit of *Cratægomespilus asniersii* prove it to be a hawthorn with the skin of a medlar. Minute investigations of the anatomy of various other parts of the two plants have only confirmed these results. And we recall the fact that the two seeds which *C. Adami* bore both grew up into laburnums.

The uniformity of these results is broken only by *Solanum Darwinianum*, the nature of which is not yet clear. Prof. Winkler states that it is not a periclinal chimæra, and has promised further investigations: pending the publication of these, we cannot say how it is built up.

The theoretical interest of these results is of course very great. A very awkward exception to various laws of heredity has been removed. The opening paragraphs of a new chapter on regeneration have been written; and an entirely unsuspected power of accommodation in the most highly organised plants has been discovered.

What the practical interest of the chimæra may be, it is hard to say. The difficulty of its production, and the small number of plants from which results may be expected, will probably prevent its becoming more than a curiosity. We would hesitate to suggest the possibility of a naturally blended tobacco, did we not recognise that the suggestion is scarcely more fantastic than the chimæra itself.

Apart from all this, it is some satisfaction to know that, after all, Adam was right. He did obtain his hybrid from a graft—by some accident, probably, a bud of purple broom became hollowed out, and into the cavity grew laburnum tissues. And he was to be excused if he did not recognise that his hybrid was no hybrid, but that it was the materialisation of a very ancient myth.

COLOURED THINKING AND ALLIED CONDITIONS

By DAVID FRASER HARRIS, M.D., D.Sc., B.Sc. (Lond.), F.R.S.E.
Professor of Physiology and Histology in Dalhousie University, Halifax, N.S.

There are certain persons in whom sounds are invariably and inevitably associated with colours. Whether these sounds are those of the human voice or the notes of various musical instruments, they are all heard as coloured. This kind of thing is known as coloured hearing; in French *audition colorée*, in German *farbiges Hören*.

The linking together of any two kinds of sensation is called synæsthesia; of all the possible synæsthesiæ the linking of colour and hearing is the commonest. A larger number of persons than might be supposed are the subjects of coloured hearing. As long ago as 1864 the chromatic associations of one of these coloured hearers were described by Benjamin Lumley (2). "I know a person," he wrote, "with whom music and colours are so intimately associated that whenever this person listens to a singer, a colour corresponding to his voice becomes visible to his eyes; the greater the volume of the voice the more distinct is the colour." This person heard Mario's voice as violet, Sims Reeves' as gold-brown, Grisi's as primrose, and so on.

But there is also a small number of persons who, whether they hear in colours or not, always think in colours. These persons, called coloured thinkers, do not have any sensation of colour when voices or notes are heard, but they invariably associate some kind of colour with such things as the names of the days of the week, the hours of the day, the months of the year, the vowels, the consonants, etc. This faculty is coloured thinking, or chromatic conception, and has been called psycho-chromæsthesia. A typical coloured thinker, who will tell you, for instance, that Sunday is yellow, Wednesday brown, Friday black, may not experience any sensation of colour on hearing the organ played or a song sung. Certain persons are indeed coloured hearers as well as coloured thinkers; but we should distinguish the person who has linked sensations, a synæsthete from the person whose thoughts are coloured, whose mentation is chromatic, who is, in fact, a psychochromæsthete.

The literature of synæsthesia is much more extensive than any one would be inclined to think who had not made it a special study. Nor is the condition described only in technical publications; there is an increasing tendency to recognise it in current fiction. Thus in *Dorian Grey* we have: "her voice was exquisite, but from the point of view of tone it was absolutely false. It was wrong in colour." Musicians, it would appear, are particularly liable to hear in colours: "The aria in A sharp (Schubert) is of so sunny a warmth and of so delicate a green that it seems to me when I hear it that I breathe the scent of young fir-trees." The musical critic of the *Birmingham Daily Post* thus once complained of a lady's singing: "Her voice should have been luscious like purple grapes." *Punch* has, of course, not failed to notice this tendency in musical criticism. A writer in the *Daily Telegraph* had thus expressed himself: "To a rather dark-coloured, deep, mezzo-soprano voice, the singer joins a splendid temperament." *Punch* remarked: "We ourselves prefer a plum-coloured voice with blue stripes, or else something of a tartan timbre."

Monsieur Peillaube (53), editor of the *Revue Philosophique*, has reported on four persons who have well-marked coloured hearing for organ notes, and he calls attention to the numerous cases amongst musicians of definite associations between notes and musical instruments on the one hand, and colours on the other, as well as between whole pieces of music and colours. Thus Gounod, endeavouring to express the difference between the French and Italian languages, and giving his preference for the former, used terms relating to colours: "Elle est moins riche de coloris, soit, mais elle est plus variée et plus fins de tintes."

Theoretically any two sensations may be linked, so that coloured hearing is only one particular variety of synæsthesia (coupled sensations, secondary or dual sensations, *Secondärempfindungen*). No doubt the linking of colour with sound is the commonest of these dual sensations, which, following Bleuler (31), might be called sound-photisms. When a taste produces light or colour we have a taste-photism; similarly there are odour-photisms, touch-photisms, temperature-photisms and pain-photisms recorded in the annals of abnormal psychology. A good example of a pain-photism occurs in a recent novel, *The Dream Ship* (66). The whole passage is so appropriate to our subject that it may be quoted in full:

"Blair" (a boy) "decided all his likes and dislikes by colour and smell. His favourite colours were yellow, red, green, and wet-black. The last was very different to (*sic*) ordinary black, which was the colour of toothache. Little rheumatic pains which he sometimes got in his knees were grey. The worst pain you could get was a purply-red one, which came when you were sad, and gave you the stomach-ache. He had once solemnly stated that the only colour he hated was yellowy-pink, but, as he always called yellow pink, and pink yellow, no one had been able to solve the riddle of this hated colour."

The black colours of toothache and the grey of rheumatism were this boy's pain-photisms. Something of the reverse order is indicated where a disagreeable colour is described as producing a pain in the stomach. When Baudelaire said that musk reminded him of scarlet and gold, he had an odour-photism.

When the reverse linking occurs we have an analogous series. If light or colour produces a sound, it is a light- or colour-phonism. This is what occurred in the case of the blind man alluded to by Locke (1), to whom "scarlet was like the sound of a trumpet"; he had a colour-phonism, the colour presumably being of the nature of a memory. When a taste is coupled with a sound we have a taste-phonism, and there may exist odour-, touch-, temperature-, and pain-phonisms respectively. Sometimes the secondary sensation linked is of a more vague character, as when screeching sounds produce disagreeable general sensations very difficult to describe. They have been called secondary sensations of general feeling, and they may be akin to those unpleasant sensations evidently experienced by dogs and other animals when they hear music. The late Mr. Grant Allen was evidently alluding to this kind of thing when he wrote, in an article on "Scales and Colours," that "Chaos was in dark and gloomy colours, whereas light was treated in white" in such a work as Haydn's "Creation."

Bleuler believes that phonisms of high pitch are produced by bright lights, well-defined outlines, small and pointed forms, whereas phonisms of low pitch are produced by the opposite conditions. An interesting thing may be mentioned in connection with the difference in colour aroused by spoken words and by whispering. Dr. Hélène Stelzner (51) tells us that in her own case full-toned speech appears as a coloured picture, whereas whispering, with its much less resonant vowels, appears like a copper-plate engraving, that is as non-chromatic.

Quite apart from all these things—synæsthesiæ—is coloured thinking or chromatic mentation. Here it is not a question of a sensation being present at all, it is that certain persons who have this power, faculty, or disability cannot visualise any concept without seeing it in the mind's eye as coloured in some way or other. Indeed the majority of the coloured thinkers questioned by the author do not experience colours when they *hear* sounds or musical tones, but they cannot think of anything definitely, the month, the day, the hour, without its being thought of as red or yellow or black or white or brown or green or blue. There is no approach towards unanimity in the colours thought of in association with any one concept or word; for instance, for Saturday the colours selected at random from records in my possession are white, yellow, steel-grey, white-grey, crimson, brown. The coloured thought may be called a psychochrome, and persons who think in colours psychochromæsthetes, the faculty or disposition to think in colours being psychochromæsthesia.

Apparently the concepts to be most commonly coloured are those for the vowels, the consonants, the months, the days, and the hours of the day. Thus the vowel "a" as in "fame" is mentally coloured in the following five ways in five different persons—red, black, green, white-grey, and white respectively. Or take the vowel "u" as in "usual"; we find it psychically coloured as grey-white, yellow, black, brown, blue, and green in six different coloured thinkers. Similarly, whole words are associated with colours in the minds of this class of thinkers. One person says he divides all words into two great classes, the dark and the light. Random examples of dark words are—man, hill, night, horse, Rome, London; and of light—sea, child, silver, year, day, and Cairo. Or again, another coloured thinker divides up the numerals into those associated with cold colours, grey, black, blue, green, and those with warm, red, yellow, orange, brown, purple, and pink. The odd numbers have the cold colours, the even the warm. In some cases, as might be expected, the coloured concepts are appropriate or natural, as when the word scarlet is scarlet, black black, and white white. But an examination of psychochromes shows us that this reasonableness does not necessarily always occur. Thus the word "apple" is to one coloured thinker a slate-grey, which is not the colour of any real apple, and the word "cucumber" to the same person is white; now only the inside of the vegetable itself is white.

Some kind of method, however, may be traced in this chromatic madness, for, according to Bleuler (31), high-pitched notes produce the lighter tints of colour, but low-pitched the darker shades. According to this authority the colours oftenest aroused in the synæsthesia, sound-photism, are dark brown, dark red, yellow, and white, which is not at all the statement of the frequency of occurrence in coloured thinking. From the records of the psychochromes of two brothers, the relative order of frequency of the colours is white or grey, brown, black, yellow, red, green and blue; violet and indigo not occurring. Dr. Hélène Stelzner (51) says that green is the colour least commonly thought of. But individual differences are extreme: thus both purple and violet are such favourites with some coloured thinkers that they hardly ever think in terms of any other colours. The present writer (55) has examined the psychochromes of two men, one woman, and one child, with the result that the relative order of frequency of occurrence comes out as white, brown, black, yellow, green, blue, red, pink, cream, orange, and purple. It is thus clear that the colours thought of are not exclusively the pure or spectral ones, for certain non-spectral colours like brown, pink, cream, white, and black are quite commonly reported. The novelist Ellen Thorneycroft Fowler, in a private communication to the author, wrote: "The colour which I always associate with myself, for no earthly reason that I can discover, is blue. Therefore 'E.,' my initial letter, is blue, April the month of my birthday is blue, and 9 the date of my birthday is blue." This is known as "colour individuation," and has been made a special study of by Paul Sokolov (47) in his paper "L'individuation colorée," read before the Fourth International Congress of Psychology held at Paris in 1900. Some people, in short, have their favourite colours, and with these they invest their pleasant thoughts, while their unpleasant thoughts they find coloured by the tints they are not fond of.

Apart, however, from whether certain colours are favourites or not, some few persons have the consciousness of a colour more or less present with them. Thus R. L. Stevenson had, so he tells us, a feeling of brown which, during his attacks of fever, was unusually distinct. It was "a peculiar shade of brown, something like sealskin."

As might be expected, so acute an observer as Mr. Rudyard Kipling has not failed to notice coloured thinking. In his very curious story "They" (52) he describes the colour concepts experienced by a blind old lady, who opens an interview by complaining that certain colours—purple and black—hurt her. Her visitor asks, "And what are the colours at the top of whatever you see"? "I see them so," she replies, "white, green, yellow, red, purple; and when people are very bad, black across the red, as you were just now." The old lady goes on to say that ever since she was quite a child some colours hurt her, and some made her happy. "I only found out afterwards that other people did not see the colours." So unfamiliar is coloured thinking to the ordinary person that a critic wrote (*The Academy and Literature*, October 8, 1904), "Such tales as 'They' are sheer conundrums." Another writer asked more pertinently, "Are the colours the blind woman described the colours of different thoughts?"

In Mrs. Felkin's novel *In Subjection* (43) (1900) the heroine, Isabel Seton, is evidently a coloured thinker. Some of her colour associations are given on page 149. The novelist, in a letter to the writer, was good enough to explain that these experiences of her heroine are based on those of an actual prototype, some of whose additional psychochromes she kindly mentioned. Isabel Seton has synæsthesia also, for the actual sounds of voices call up colours. Thus, soprano voices are to her pale blue or green or yellow or white, contraltos are pink or red or violet, tenors are different shades of brown, while basses are black or dark green or navy blue.

In the novel *Christopher*, by Richard Pryce (61), there is an interesting allusion to a boy who is described as not morbid, although he is evidently a synæsthete and a coloured thinker. He talks of playing the sunset on the piano (a colour-phonism), and of smelling moonlight (a light-olfaction). In a novel, *Youth's Encounter* (64), published only last year (1913), we are told that to one of the characters "Monday was dull red, Tuesday was cream coloured, Thursday was dingy purple, Friday was a harsh scarlet, but Wednesday was vivid apple-green, or was it a clear, cool blue?"

It is difficult to express the character of these coloured concepts to persons—and they are the majority of people—who never experience this sort of thing at any time. The colours are not present so vividly as to constitute hallucination. Coloured visualisings never become hallucinatory, possibly because they are of the nature of thoughts, rather than of subjective sensations. Chromatic conception belongs to the physiology, not to the pathology of mind. Coloured thinkers are not continually plagued with phantasmagoria. Mental colourings do not obtrude themselves into our mental life; they are habitual, natural, chromatic tincturings of one's concepts, and have been so long present to one's consciousness that they have long ago become part of our mental belongings. They are invariable and definite without being disturbing.

One coloured thinker has thus expressed himself: "When I think at all definitely about the month of January the name or word appears to me reddish, whereas April is white, May yellow, the vowel 'i' is always black, the letter 'o' white, and 'w' indigo-blue. Only by a determined effort can I think of 'b' as green or blue, for me it always has been and must be black; to imagine August as anything but white seems to me an impossibility, an altering of the inherent nature of things." There is, thus, an inherent definiteness, finality, and constancy about each thinker's psychochromes that is very striking. But it is not alone letters and words that are habitually thought of as coloured, certain coloured thinkers always associate a particular colour with their thoughts about a particular person.

The author of *The Corner of Harley Street* remarks (p. 251): "If only we could use colours now to express our deeper attitude on these occasions, as some of your fellow clergy wear stoles at certain seasons, with what pleasant impunity could we write to one another in yellow or purple or red, leaving black for the editor of the *Times*, or the plumber whose bill we are disputing."

"Our alphabet is not rich enough for the notation of the cockney dialect," writes Mr. Richard Whiteing in *No.* 5, *John Street*. "I can but indicate his speech system by a stray word which, if there is anything in the theory of the correspondence between sounds and colours, should have the effect of a stain of London mud." This is evidently an allusion to coloured thinking; there is unfortunately no theory at all as yet, but there is the fact of chromatic conception. Quite recently (1913) there was in the *British Review* (65) a vivacious article dealing with coloured thinking from the popular standpoint. The literature that contains the most systematic discussion of coloured thinking is that of the decadent poets of France, the symbolards, as they are called. Some account of their psychochromes is given in Lombroso's *Man of Genius* (30). The eccentric poet Paul Verlaine belonged to this school. It evidently includes synæsthetes as well as coloured thinkers for, for them, the organ is black, the harp white, the violin blue, the trumpet red, and the flute yellow. Further they think of the vowel "a" as black, "e" as white, "i" blue, "o" red, and "u" yellow. One of them, Stephane Mallarmé, has explained in his pamphlet *Traité du verbe* how these things have come to be.

The following verses—for I hesitate to call them poetry—seem to be an attempt to express the associations of emotions symbolised by the mental colourings of the vowels.

VOYELLES

A noir, E blanc, I rouge, U vert, O bleu, voyelles,
Je dirai quelque jour vos naissances latentes;
A noir corset velu des mouches éclatantes
Qui bombillent autour des puanteurs cruelles.

Golfes d'ombre E, candeur des vapeurs et des tentes,
Lances des guerriers fiers, rois blancs, frissons d'ombelles,
I pourpres, sang craché, rire des lèvres belles
Dans la colére les ivresses pénitentes.

U cycles vibrement divins des mers virides,
Paix des pâtis semès d'animaux, paix des rides
Que l'alchemie imprime aux grands fronts studieux.

O, suprème clairon plein de strideurs étranges,
Silence traversée des Mondes et des Anges,
O l'omega, rayon violet des ses yeux.

J. A. RIMBAUD.

We are now, perhaps, in a position to make some inquiry into the characteristic features of coloured thinking. The first point that strikes one is the very early age at which these associations are fixed. This was a feature recognised by Galton in his classic examination of the subject in 1883 (10).

The present author's observations fully confirm this point; he has in his possession many letters from coloured thinkers in which the details of their psychochromes differ in the widest possible manner, but all agree in that they testify to the very early age at which the associations were formed. After the publication of the writer's article in the *Scotsman*, December 29, 1908 (59), he received a number of letters spontaneously sent, all emphasising this feature in such phrases as, "ever since I can remember," "ever since childhood I have always had it," "I do not remember the time when I had not," etc. A writer in *Nature* in 1891 (29) reports on the psychochromes of his daughter when seven years old, at which age she had specifically different colours for the days of the week, namely, blue, pink, brown or grey, brown or grey, white, white, and black. The months of the year were coloured in the following way by a girl of ten who had so thought of them ever since she could remember—brown, olive-green, "art blue," green-yellow, pink, pale green, pale mauve, orange, orange-brown, grey, grey outlined in black, and finally red.

A boy ten years old is reported in the article on Colour Hearing in the *British Review* (65) to have "noticed that the number 8 invariably provoked in him the sensation of apricot yellow, and the number 15 that of peacock blue." There seems not the slightest doubt that these colour associations are amongst the earliest that are formed in the child mind of the coloured thinker.

The second characteristic of coloured thinking is the unchangeableness of the colour thought of. Middle-aged people will tell you that there has been no alteration in the colours or even in the tints and shades of colour which for many years they have associated with their various concepts. Galton remarked on this in his original monograph; "they are very little altered," he said, "by the accidents of education." Galton's phrase was, "they result from Nature not nurture." Just as their origination is not due to the influence of the environment, so the environment exercises no modifying influence on them during a long life.

The third characteristic of psychochromes is their extreme definiteness in the minds of their possessors. Contrary to what might reasonably be expected, the precise colours attached to concepts are by no means vague or incapable of accurate verbal description. A coloured thinker is most fastidious in the choice of terms to give adequate expression to his chromatic imagery. One of these is not content, for instance, with speaking of September as grey, he must call it steel-grey; another speaks of a dull white, of a silvery white, of the colour of white watered silk, and so on. One child speaks of March as "art blue," whatever that is, another of 6 p.m. as pinkish. The degree of chromatic precision which can be given by coloured thinkers to their visualising is as extraordinary as any of the other extraordinary things connected with this curious subject.

The fourth characteristic is the complete non-agreement between the various colours attached to the same concept in the minds of coloured thinkers. Thus, nine different persons think of Tuesday in terms of the following colours—brown, purple, dark purple, brown, blue, white, black, pink, and blue. Again, September is thought of as pale yellow, steel-grey, and orange by three different coloured thinkers respectively. Once more, the vowel "i" is thought of as black, red-violet, yellow, white, and red respectively by five persons gifted with chromatic mentation. Unanimity seems hopeless, agreement quite impossible; the colours are essentially individualistic.

The fifth characteristic of psychochromes is their unaccountableness. No coloured thinker seems to be able to say how he came by his associations; "I cannot account for them in any way" is the invariable remark one finds in letters from persons describing their coloured thoughts.

The sixth characteristic is the hereditary or at least inborn nature of the condition. Galton's phrase was "very hereditary." The extremely early age at which coloured thinking reveals itself would of itself indicate that the tendency was either hereditary or congenital. The details of a case of heredity from father to son have been reported for coloured hearing by Lauret and Duchassoy (22, 44, and 50); a case of coloured thinking reported by the present writer was one of heredity also from father to son (55). But these related coloured hearers did not see the same colours for the same sound, nor did the two coloured thinkers think in the same colours. From the writer's inquiries, coloured thinking is certainly congenital even when it cannot be proved to be hereditary. This point will come up again in connection with the origin of the condition, but we may at present note that those who have studied the subject are unanimous in denying that, at any rate, coloured thinking is due to environmental influences.

It may now be asked what manner of people are they who are coloured hearers or coloured thinkers or both. The late Mr. Galton told us that they are rather above than below the average intelligence. The writer's observations would in the main confirm this; they are at least invariably well educated persons who confess to being coloured thinkers. In his book Mr. Galton gave a few names of distinguished persons of his acquaintance, and his list might be brought up to date by the addition of some names quite as distinguished. But all persons who have coloured hearing or coloured thinking are not necessarily distinguished—a large number, as we have seen, are yet children—but they are all probably more or less sensitive. Possibly they are more given to introspection than is the ordinary person. At any rate, what is quite certain is that both synæsthetes and psychochromæsthetes belong to the group of strong visuals or "seers" as Galton called them. Seers are persons who visualise or exteriorise their concepts either as uncoloured forms or as coloured in some way or other. The uncoloured thought-forms are very curious, some of which Galton gave as examples in the appendix to his work. One distinguished neurologist always sees the numerals 1 to 100 in the form of a ladder sloping upwards from left to right into the sky. As this concept is not coloured it cannot be called a psychochrome, but it might be called a psychogram. A psychogram is, then, the uncoloured thought-form of a concept, and people who have psychograms must be strong visualisers.

The school of symbolist poets in France to which Ghil, Malarmé, Rimbaud, and Verlaine belong, appears to lay a great deal of stress on the so-called meaning of colours. The school evidently includes both coloured hearers and coloured thinkers; but whereas the majority of coloured thinkers derive no particular meaning from their psychochromes, the symbolists attach considerable significance to the colours which happen to be associated with their thoughts. The different vowels, for instance, mean to them or represent for them particular emotions or states of mind not in virtue of the sound of the vowel but entirely through the related colour. The particular emotion symbolised by any given colour seems to the ordinary person rather arbitrary if we judge by the details in Rimbaud's poem; but we are all aware that there has always been a tendency to represent emotional states in terms of the language of colour. Homer spoke of "black pains"; we constantly speak of a black outlook, a black lie, a white lie, a black record, a grey life, a colourless life, and so on. There is, in fact, growing up in England a school of musicians who hold that it should be possible and pleasurable to represent music chromatically. Whether the general public will ever enjoy silent music seems very doubtful, but it is notorious that most people derive a great deal of pleasure from the display of coloured lights, illuminated vapours, coloured steam, "fairy fountains," Bengal lights, a house on fire, and other exhibitions in the open air. People undoubtedly do like to see great surfaces or masses vividly coloured as in the rainbow, the sunrise or sunset, the afterglow on snowy mountains, the streamers of the northern lights, etc. But whether they would care to have audible music suppressed and to have offered them a succession of coloured surfaces or patches of colour even following one another in the sequence or rhythm required by music, is open to serious question. Such, however, is the intention of Mr. A. W. Rimington, as explained in his book *Colour in Music* (63), in which there is much that is true and interesting. "It is undeniable," he writes, "that as a nation our colour sense is practically dormant. . . . Compare our colour sense with that possessed by the Japanese, the Indians, or even the Bulgarians and Spaniards. . . . To my mind a widespread, refined colour sense is more important than a musical one." Long before Mr. Rimington's work was published, there appeared a little book, privately printed at Leith in Scotland, called *Chromography or Tone-colour Music* (23). The author assigned a colour to each of the notes of the scale thus—Do = red; re = orange; mi = yellow; fa = green; sol = blue; la = violet-purple; ti = red-purple.

Many persons have synæsthesia in connection with musical tones (sound-photisms); two reported on by Albertoni (24) associated blue with the sound of Do (C), yellow with mi (E), and red with sol (G). But it was discovered that they were colour-blind for red (Daltonism). Now, whereas they could recognise and name the other notes, they could not name G, a disability which Albertoni thinks was related to the Daltonism; he has accordingly called it auditory Daltonism (*Daltonismus* *auditivus*), a psychical deafness depending on the red-blindness, since the note to which they were psychically deaf was the one which called up mentally the particular colour, red, to which they were actually blind.

It might now be asked whether we have any explanation of the causes or causal conditions of coloured thinking, why may thoughts be coloured at all, and why should particular thoughts come to be associated with particular colours? Why should only a few persons be found to be coloured thinkers? The answers, if answers they can be called, are extremely disappointing, for we have no satisfactory explanations of any of these matters. The very arbitrariness of the associations defies theoretical analysis.

If it is the function of science merely to describe, then our work is done; but in a subject such as this, to make no attempt to account for the abstruse phenomena observed would be a distinctly feeble conclusion of our studies. It has been suggested that the cause of coloured thinking is no more recondite than the influence of some picture-book which in early life determined for us ever afterwards the colours of certain concepts. Now though many people do regard their coloured thinking as a childish survival, the picture-books will account for very few of the best established psychochromes. In some few cases, environmental influences do seem to have been causal. Thus, in one case known to the writer, the colour of February as white was accounted for by the influence of the surroundings. The earliest February remembered was snowy, and, through the whiteness of snow, the concept of February came to be and ever afterwards remained white. But it is clear that if environmental influences are operative in anything like a large number of cases, the colours for such concepts as the months of the year ought to be far more uniform than they are. No common origin of external source can make one person think of August as white, another as brown, and yet another as crimson. If August is white to one person because it is the month of white harvest, then it ought to be white to all persons capable of receiving any impressions as to the colours of harvest. But to the vast majority of people it is perfectly absurd to talk of August having any colour at all; and, to the few who think it coloured, it has not by any means the same colour; all seems confusion.

Monsieur Peillaube (54) has made a suggestion of a different kind as likely to explain some of these colour associations. Monsieur Peillaube became acquainted with a Monsieur Ch——, who had *audition colorée* as well as coloured thinking. Monsieur Ch—— had an excellent memory, and was able to submit his conceptions to searching introspection, with the result that he seems to have discovered what may be called the missing link in the associational chain of mental chromatic events. To this coloured thinker, the lower notes of the organ were of a violet colour. This seems to have been brought about in the following way: low notes of any kind were sweet and deep (*douces et profondes*), the colour violet is sweet and deep, therefore it came to pass that the low notes were associated with violet. Similarly to Monsieur Ch——, the vowel sound of "i" was suggestive of something *vive et gaie*, the colour green had always been associated with liveliness and gaiety, therefore he thought the vowel "i" was green. These conclusions were reached only after considerable introspection, for it must be understood that the link between the low notes and the colour violet was by no means an explicit or definite presentation in this person's mind at the time that Monsieur Peillaube suggested the inquiry. Peillaube's theory, then, is that these apparently arbitrary and instantaneous linkings of sounds (x) to colours (y), or of thoughts to colours, are really after all cases of association of two terms through the intermediation of a third factor an emotional link (l) now subconscious but revivable. The sequence was x—l—y, but in course of time the "l" had dropped out of consciousness, leaving the "x" and the "y" apparently indissolubly joined together.

Finally, it may be asked, would the capability of coloured thinking cause its possessor to be classed as mentally abnormal? The answer is in the negative. Coloured thinkers may not conform to the usual or most commonly met with mental type, but they deviate from that type only in the same way that geniuses deviate from it. Inasmuch as they deviate from the normal, coloured thinkers are of course abnormal, but there is nothing in them that is allied to instability of mental balance. Some coloured thinkers may no doubt belong to families in which some degree of mental instability is present; or, on the other hand, some relatives of coloured thinkers may possess a high degree of artistic or musical ability, of scientific or philosophical insight, that quality, of genius in fact, so exceedingly difficult to define. Genius is something notoriously not conferred by training or education; if not inborn it cannot be acquired; exactly the same may be said of coloured thinking. Our studies have at least shown us this, that it is not in the ordinary type of mental constitution, but in the recesses of the slightly supernormal that this recondite problem of psychology presents itself for analysis and explanation.

Appendix

Being the Psychochromes in an Actual Case

"*a*. Blue-white (like a dead tadpole).
b. Dark brown-red.
c. Brighter red.
d. Pea-green.
e. Fawn-yellow.
f A yellow, brighter than *e*.
g. Dark brown, nearly black.
h. Black.
i. Chocolate brown.
j. A dull red (not the same shade as the other reds).
k. Bright brick-red.
l. Black.
m. Bright yellow.
n. Dark brown (nearly black).
o. White.
p. White with just a tinge of blue.
q. Pale blue-green.
r. Black (nearer to *h* than to *i*).
s. White.
t. Mustard colour (ugly).
u. Brown-yellow.
v. Olive-green.
w. Red (like *c*).
x. Green.
y. An ugly yellow.
z. Very bright scarlet.

Sunday. Red.
Monday. Pea-green.
Tuesday. Fawn-yellow.
Wednesday. Black.
Thursday. Fawn (not as bright as Tuesday).
Friday. Green (a very ugly bile colour).
Saturday. White.

January. Dull red.
February. Fawn.
March. A green mustard colour.
April. Blue-white.
May. Sunshine colour.
June. Dull red.
July. A slightly darker red.
August. Olive-green (more yellow than *n*).
September. White.
October. Green.
November. Black-brown.
December. A blue shot with green.

Christmas. White.
Whitsun. Nearly a rose-pink.
Easter. Mauve with something white in the middle.

One. Black.
Two. Blue-white.
Three. Fawn.
Four. Dark red.
Five. White.
Six. Bright yellow.
Seven. Black.
Eight. White.
Nine. Green.
Ten. Mustard green.
Eleven. Brown-yellow-green.
Twelve. Pale brown."

Bibliography

1. 1690. Locke, John, Philosophical Works. London, 1872, vol. ii. p. 26.
2. 1864. Lumley, B., Reminiscences of the Opera. London, 1864, pp. 98, 99.
3. 1873. Bruhl, *Wien. med. wochens.* Nos. 1–3.
4. 1879. Lewes, G. H., Problems of Life and Mind. Third Series, London, 1879.
5. 1880. Galton, F., *Nature*. London, 1880, vol. xxi. p. 252.
6. 1880. —— *ibid.* p. 494.
7. 1881. Bleuler, E., und Lehmann, K., Zwangmässige Lichtempfindungen durch Schall und verwandte Erscheinungen. Leipzig, 1881.
8. 1881. Steinbrügge, Ueber secondäre Sinnesempfindungen. Wiesbaden, 1881.
9. 1882. Mayerhausen, Über Association der Klänge speciell der Worte mit Farben, *Klin. Monatsbl. f. Augenheilk*, 1882.
10. 1883. Galton, F., Inquiries into Human Faculty and its Development. Macmillan, London, 1883, pp. 145–77.
11. 1883. Lussana, Sur l'audition colorée, *Arch. Ital. de Biol.* 88.
12. 1883. Pedrono, De l'audition colorée, *Annal d'Oculiste*, 1883.
13. 1883. Schenkl, Ueber Association der Worte mit Farben, *Prager Medic. Wochenschr.* 1883.
14. 1884. Hilbert, *Klin. Monatsbl. f. Augenheilk.*

15. 1885. DE ROCHAS, A., Audition colorée, *La Nature*, April, May, September, 1885.
16. 1885. GIRANDEAU, De l'audition colorée, *L'Encephale*, No. 5, 1885, p. 589.
17. 1887. BARATOUX, Audition colorée, *Progrès Médicale*, 1887.
18. 1887. FERÉ, C., La vision colorée et l'équivalence des excitations sensorielles. Soc. de Biol. Paris, 1887.
19. 1887. PICK, Mendel's Neurolg. Centralbl. 1887, p. 536.
20. 1887. URBANTSCHITSCH, Sitzungsbericht. d. Gesellsch. d. Aertze in Wien, October 1, 1887.
21. 1887. —— *Münch. med. Woch.* October 25, 1887, p. 845.
22. 1888. LAURET et DUCHASSOY, *Bull. Soc. de psychol. physiol.* Paris, tome iii. p. 11; Abstract in *Centralbl. fur Physiol.* Leipzig und Wien, 1888 s. 125.
23. 1888. WATT, WILLIAM, Chromography or Tone-colour Music. Leith, 1888.
24. 1889. ALBERTONI, Abstract in *Centralbl. f. Physiol.* Leipzig und Wien, 1889, s. 345.
25. 1889. KAISER, Association der Worte mit Farben, *Arch. f. Augenheilk*, 1889.
26. 1889. RAYMOND, P., Une observation d'audition colorée, *Gazette des Hôpitaux*, 1889.
27. 1890. DE MENDOZA, SUAREZ, L'audition colorée. Paris, 1890.
28. 1891. DELSTAUCH, Une observation d'audition colorée, *Annal. des maladies de l'oreille*, 1891.
29. 1891. HOLDEN, *Nature.* London, 1891, vol. xliv. p. 223.
30. 1891. LOMBROSO, C., The Man of Genius. London, W. Scott, 1891, p. 231.
31. 1892. BLEULER, Article, Secondary Sensations (Pseudochromæsthesia), *Dict. psych. med.* Edited by Tuke, London, Churchill, 1892.
32. 1892. CALKINS, MARY W., *Amer. Journ. Psych.* vol. v. No. 2, November 1892.
33. 1892. GRÜBER, L'audition colorée, *Proc. Intern. Cong. Exper. Psych.* London, 1892.
34. 1892. KROHN, Pseudochromæsthesia, *Amer. Journ. Psych.* October 1892.
35. 1893. CALKINS, MARY W., Statistical Study of Pseudochromæsthesia and of Mental Forms, *Amer. Journ. Psych.* July 1893.
36. 1895. MIRTO, G., Contributo al Fenomeno di Sinestesia visuale. Palmero, 1895.
37. 1895. ROBERTSON, W. J., A Century of French Verse. London, 1895. (For translation of Arthur Rimbaud's poem "Les Voyelles.")
38. 1897. ZEHENDER, Ein Fall von Geschmaksphotismus, *Klin. Monatsbl. f. Augenheilk*, 1897.
39. 1899. SYMONS, A., The Symbolist Movement in Literature, 1899.
40. 1899. CLAVIERE, J., L'audition colorée, *Ann. Psychol.* vol. v. 1899, pp. 161–78
41. 1899. —— *Ann. de Soc. Psych.* vol. ix. pp. 257-71, 1899.
42. 1900. DAUBRESSE, Audition colorée, *Rev. Philosoph.* 1900.
43. 1900. FOWLER, E. T., In Subjection. London, Hutchinson, 1900, p. 149.
44. 1901. LAIGNEL & LAVASTINE, Audition colorée familiale, *Rev. Neurolog.* 1901.
45. 1901. LEMAITRE, Audition colorée et phenomènes connexes observés chez des écoliers. Genève, 1901.
46. 1901. SOKOLOV, P., Individuation colorée, *Rev. Philosoph.* 1901.
47. 1901. —— L'individuation colorée, *Trans. IV. Cong. intern. de psychol. tenu à Paris*, 1900. Paris, 1901, pp. 189–93.
48. 1902. ROMBY, L'Hysterie de Ste. Thérèse, *Arch. de Neurol.* vol. xiv. 1902.

49. 1903. Dressler, L'audition colorée, evolue-t-elle? *L'Annee Biol.* vol. viii. 1903, p. 406.
50. 1903. Lemaitre, Cas d'audition colorée hallucinatoire cas héréditaire, *L'Année Biol.* vol. viii. 1903, p. 421.
51. 1903. Stelzner, Hélène, F. Fall von akustisch-optischer Synaesthesie, *Graefe's Arch. Ophth.* Leipzig, vol. lv. 1903, pp. 549-63.
52. 1904. Kipling, R., Traffics and Discoveries. London, 1904.
53. 1904. Peillaube, *Rev. Phil.* Paris, November 1904, p. 675.
54. 1904. —— Communication to the VI. International Congress of Physiology. Brussels, September, 1904.
55. 1905. Harris, D. Fraser, Psychochromæsthesia and certain synæthesiæ. *Edin. Med. Journ.* December, 1905.
56. 1905. Lomer, G., Farbiges Hören (Auditio Colorata), *Arch. f. Psychiat.* Berlin, 1905, vol. xl. pp. 593–601.
57. 1905. Colville, W. J., The Human Aura and the Significance of Colour. London, 1905.
58. 1908. Harris, D. Fraser, Coloured Thinking, *Journ. Abn. Psychol.* Boston, June-July, 1908, p. 97.
59. 1908. —— Coloured Thinking. *Scotsman*, December 29, 1908.
60. 1908. —— Coloured Thinking. *Rev. Neurol. and Psych.* Edin. September, 1908.
61. 1911. Pryce, R., Christopher. London, Hutchinson & Co., 1911, p. 81.
62. 1911. Bashford, H. H., The Corner of Harley Street. London, Constable, 1911, p. 251.
63. 1912. Rimington, A. W., Colour in Music. London, 1912.
64. 1913. Mackenzie, C., Youth's Encounter. Toronto, Bell & Cockburn, 1913.
65. 1913. Martindale, C. C., Colour Hearing, *British Review*, April 1913.
66. 1913. Stockley, C., The Dream Ship. London, Constable & Co., 1913, p. 229.

PHOTOGRAPHIC AND MECHANICAL PROCESSES IN THE REPRODUCTION OF ILLUSTRATIONS

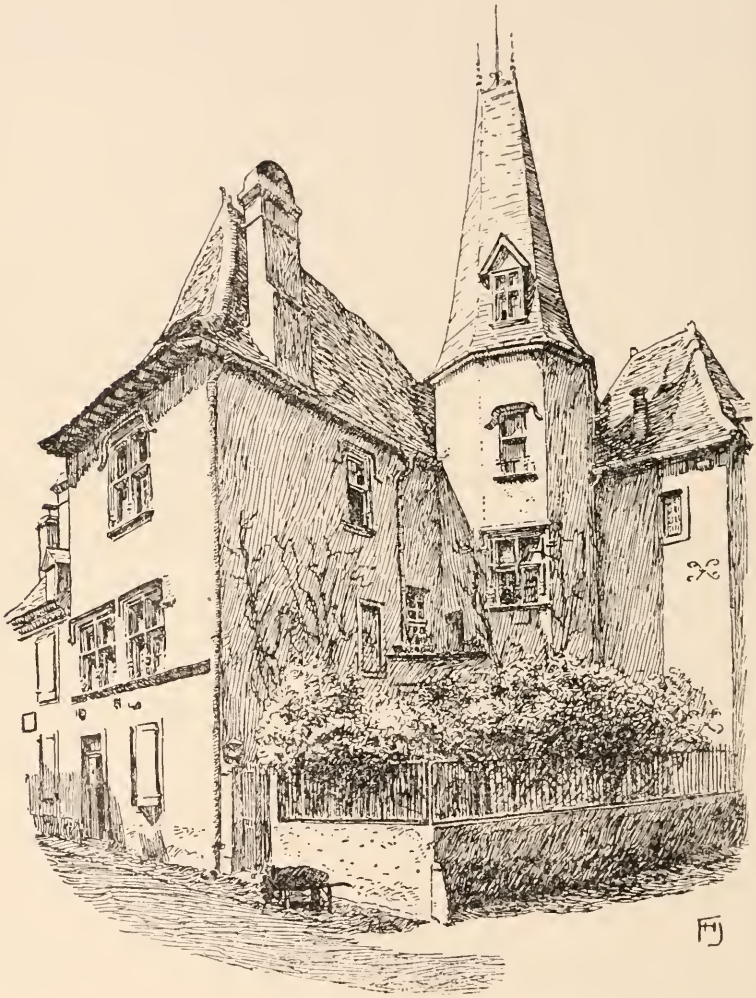
By ROBERT STEELE

There are very few books or articles which would not be the better for a certain amount of illustration, while on the other hand there are very few authors who could not suggest illustrations or produce them themselves. Unfortunately, the steps that lie between the rough sketch, the photograph, or the water-colour drawing as it lies in the author's portfolio, and the finished illustration as it is to appear in the book, are a mystery to him, and he has no means of deciding what kind of illustrations he can get from the material he has by him. It is with a view of helping such an one to decide this question that the following survey of modern methods of production has been written.

All mechanical reproductions to-day are based upon photography. The ordinary process of taking a photograph depends on the use of a lens to form a clear image of the object to be photographed on the surface of a film at the back of the camera. This film contains a compound of silver, which is altered by the action of light so as to become insoluble in a liquid which we call the "developer." The tones of the picture on the sensitive film are thus represented by the varying thicknesses of altered silver, and after the plate has been sufficiently immersed in the developer, all the unaltered silver is dissolved out, and a reversed picture or negative is left on the film, in which the lights of the real picture are black and the shadows are light.

Line Blocks

When the illustration required is in pure line, as, for example, a diagram or a drawing, the process of reproduction is comparatively simple. A negative is produced in the ordinary manner, except that it is reversed in a well-known way by a mirror. A highly polished sheet of zinc is covered with a film of albumen treated with bichromate of ammonia, and a print of the negative obtained on it. In the negative the lines of the original drawing are absolutely clear, and the remainder is dense; in the resulting



Specimen of an illustration reproduced by line process.

From an illustration by F. H. Jackson in *Rambles in the Pyrenees.*

print the lines are hardened by the action of light, while the remainder is unaltered. The plate is then taken into a dark room, inked with a roller, and allowed to soak in a bath of cold water till all the unaltered albumen is sufficiently softened to allow it to be easily wiped away, and nothing is left but the hardened ink-covered lines. Asphalt is then dusted on them, and gently heated till it melts and forms a protective covering for the lines of the design. The plate is then varnished on its back and sides and immersed in acid, which eats away its surface except where it is protected by the film. During this process ink is continually being added to the lines to protect their sides from being undercut by the acid. When a sufficient depth of zinc has been removed, the lines of the drawing stand high above the surface, and form the block, which is then mounted on wood or metal to raise it to the level of the type with which it will be printed.

A drawing for reproduction in this manner should be made with a good black ink on white paper. It is essential that the ink should be of the same colour throughout. Chinese white should never be used to correct the drawing, as it comes out grey in the photograph: a "process" white for use in its place is sold by colourmen. The cost of an ordinary block is from 2½*d.* to 6*d.* the square inch, with a minimum price of 2*s.* 6*d.* to 6*s.* The variations in price correspond to the difficulty of the subject and the technical skill of the workman employed in its reproduction: the lower prices only applying to the very roughest work.

Half-tone Blocks

An ordinary photograph contains, besides its pure white high lights and deep black shadows, a very large number of intermediate shades of light, called technically "half-tones," which cannot be adequately reproduced in a line block, and for which the half-tone process is used if cheapness is desired, while, if better results are necessary, photogravure or collotype processes should be employed. The fundamental difference between these methods is that in the first the difference of tone is obtained by the varying size of the white spaces scattered over the reproduction as seen by a powerful glass, in the second by the amount of ink deposited in each part of it from the plate.

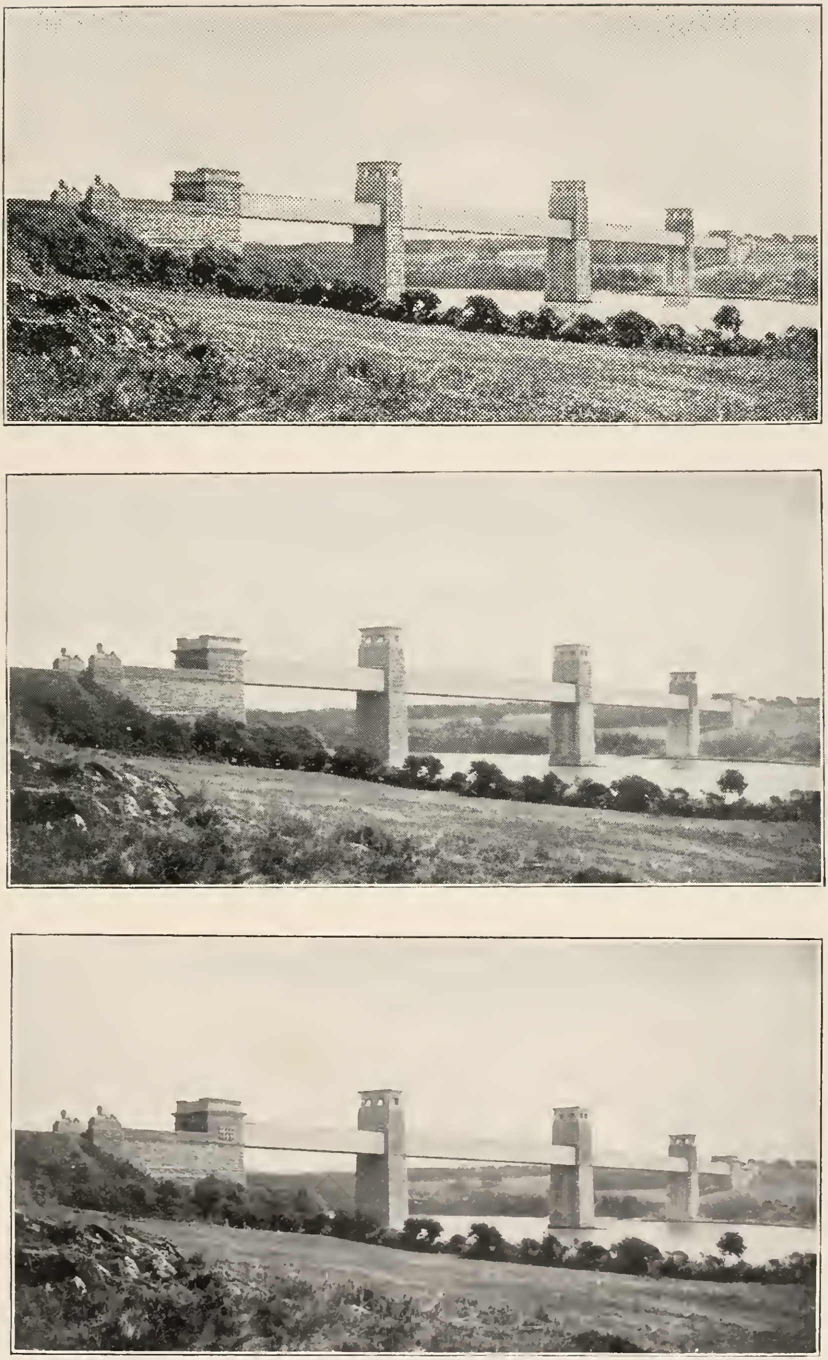
An ordinary illustration in a magazine, looked at through a magnifying glass, is seen to be made up of a large number of black dots of varying size but uniform distribution. When the dots are large and run into each other, leaving small white spaces among them, we get shadows; when they are small we get lights. This effect is produced by introducing into the camera, just in front of the sensitive film, a glass plate cross-ruled in parallel lines from 1/50th to 1/200th of an inch apart.

As the picture from the lens passes through the little squares of the ruled screen, thousands of minute cones of light are produced. Where the bright part of the picture falls on the sensitive plate there is greater action, where the darkest parts fall there is often very little, and thus some of these minute spots of altered silver in the negative may meet each other on the sensitive film, while others do not. In this way the varying size of the white spaces which produce the half-tones is obtained.

The negative, when ready, is printed on a highly polished piece of copper which is coated with fish-glue treated with bichromate of ammonia, and the unaltered parts of the print are washed away as in the case of the line block. The plate with its gelatin picture is heated until the lines and dots, now insoluble in acid, adhere firmly to the metal, and is then placed in a bath of ferric chloride, which etches away the unprotected surface and leaves the lines and dots standing free. The first etching is a rough one, and, to bring out contrast, it is usually necessary to re-bite portions of the plate several times, the remainder being protected from the bath by coats of varnish. In cases where special care is required, handwork engraving on the block is resorted to, but this adds greatly to the expense.

Photographs of every kind, sepia sketches, black and white wash drawings, chalk drawings, line engravings, mezzotints, and photogravures can be reproduced to great advantage by this process. A pencil drawing hardly ever makes a good reproduction. The chief objection to the half-tone block is that it cannot reproduce the actual range of the artist's expression. A pure white is impossible without engraving on the block, and light tints are degraded by the screen lines, robbing the print of brilliancy.

When an oil-painting or water-colour sketch is to be reproduced, a new difficulty arises. Colours affect photographic plates in a very different way from that in which they strike the eye. The yellow in a picture will come out as black, while the blue will act as a white would. To obviate this difficulty, either special plates which give approximately equal values to



Specimen of a photograph reproduced in three different screens, in 65, 100, and 175 to the inch.

the colours are employed, or in extreme cases colour filters are used to lessen the importance of the predominant one.

The ruling of the screen used in making the original negative is of importance. In ordinary newspaper illustrations the lines run 60 to the inch, in magazines they run 133 or 150 to the inch. The finest screen in general use is 175 to the inch. It would be impossible to print from a block with such fine dots as these on any ordinary paper, as the result would be a blur. A specially smooth surface is required: this can only be obtained by the use of paper which has been faced with china clay paste by being passed through a bath of the material and rubbed in by metal brushes to attain a high polish. As a result, not only is the eye wounded by the glistening smoothness of the paper on which the illustration is printed, but there is a certainty that the paper will fall to pieces in a comparatively short number of years, as some of the earliest specimens of it have already done.

The question of the proper illustration of a book is largely a question of expense. From every point of view it is better that the illustrations should form part of the book itself, and not be inserted later. The process of insertion brings an added cost; there is always a risk of loss of the inserted plate; it is usually inharmonious with the body of the work; it is, in short, an excrescence on it. The ideal illustration for a printed book is a wood engraving, whose capabilities in skilled hands are hardly to be over-estimated; and a line block can be printed with the text in a very satisfactory way, but all other processes require separate printing. The objection to the half-tone process—that it destroys the unity and durability of the book—is one that has not been overcome, and promises to cause its supersession.

The expense of an ordinary half-tone block from a coloured original varies, of course, with the amount of work put into it—say from 5*d.* to 9*d.* per square inch, with a minimum charge for 12 square inches. When there is much cutting away to be done, the minimum cost would be much higher.

Collotype

Collotype or phototype is a very satisfactory process of reproduction of which examples are familiar to every one in the best picture postcards. The preliminary expense of their reproduction is small. A rough film is spread upon glass, and on this is laid a second film of gelatin treated with bichromate of potash. An image is printed on this through a reversed negative, in which each part is hardened in the ratio of the amount of light that passes through the negative.

To use the film for printing it must be kept at a uniform degree of moisture. The hardest parts absorb none, the others absorb moisture in the ratio of the action of the light on them. When an inked roller is passed over the film, the ink is taken up freely by the hard, unmoistened parts, while the other parts take it up in inverse proportion to the moisture they hold, and print off an impression exactly reproducing the values of the original photograph. In the hands of an experienced printer this process gives very artistic results. One great advantage of the collotype is that it can be printed on any printing paper. It is especially suitable for use in the cases of small editions, as the prime cost of making a block is avoided.

It is usual to obtain an estimate in this process for making the collotype and printing of the reproductions together, as the success depends equally on the character of the original negative taken and on the skill and experience of the printer.

Photogravure

Photogravure is the most important of the mechanical reproductive processes, and the most costly. A photogravure plate, when completed, resembles the old mezzotint, in which the parts that yield the impression are hollows in the metal, instead of being ridges on it, as in line or half-tone blocks, or flat surfaces, as in collotype and lithography. It is excellent for portraiture, and gives the nearest approach to a facsimile reproduction in black and white that has yet been obtained, chiefly because it provides a mechanical basis on which an artist may work.

In this process the first thing required is to produce a grain on a highly polished copper plate, for the purpose of holding the ink. This is usually done by exposing the plate to a dust-cloud of bitumen, and then heating it sufficiently to attach the particles to it without entirely melting them. The plate is then covered by the usual film of bichromatised gelatin, and printed with the subject required from a "positive"—that is, a glass film in which the lights are transparent and the shadows dark. The ordinary hardening process takes place, and the unhardened gelatin is washed away. The plate is now bitten in with ferric

SPECIMEN OF AN ILLUSTRATION PRINTED IN COLLOTYPE.

THE DAUPHIN FRANCOIS.

From "CHANTILLY IN HISTORY & ART" by Mrs. J. P. RICHTER.

chloride, which eats through the gelatin on the plate, its action being carefully supervised by the workman in charge.

It is obvious that there is little mechanical certainty in the working of this process, and not uncommonly several plates have to be made to get a successful result. Each reproduction has to be printed separately, as all copper and steel plates are, and it is usual to employ a thick paper called "plate paper." In printing the paper is damped, and forced into the hollows of the plate, which has to be inked and wiped by hand. The cost may be assumed as a minimum of from two to three guineas per plate for a crown or demy 8vo book, according to the difficulty of the subject and the skill and standing of the engravers who are employed to finish the plates. Among these latter are, at least in the case of one firm, exhibitors at the Royal Academy.

Colour Processes

If it is desired to reproduce an illustration in its original colours a variety of processes are now at command. Though chromo-lithography with a dozen or more printings for each plate was, up to the last twenty years, the only means of reproducing a picture, at the present time very good results are obtained by the three-colour process, which is founded on the fact that by the combination of the three primary colours—red, green, and violet—any other colour may be produced. A pigment must be distinguished from a colour: it is a substance which absorbs all the coloured light that falls upon it, except its own. The primary pigments are yellow, red, and blue, and a perfect combination of them should produce black, just as a perfect combination of primary colours should produce white. If, therefore, three photographs could be obtained, one showing only the blue elements of the coloured object, a second showing only the red, and a third showing only the yellow, and impressions from each of them in a pure pigment combined into a single picture, the colours of the original would be reproduced. All modern colour processes may be said to depend on this principle.

In the ordinary three-colour process three half-tone blocks are made, one each for the yellow, red, and blue, all the other colours being strained off by a light filter in the camera as each negative is being made. The manufacture of the negatives requires the most skilful manipulation, and the slightest defect or inaccuracy is fatal to a good result. When the blocks are completed proofs are taken, which are supplied with them to the purchaser. These are usually a proof in the proper yellow ink of the yellow block, a progressive proof of the red block in red, and another of it superimposed on the yellow impression, and of the blue block in blue, and another as printed over the yellow and red. Between each of these printings several hours must elapse. These proofs are the models for the colour printer to work up to.

When making water-colour sketches for reproduction the drawing should be kept within well-defined limits, avoiding vignetting. The colours should not be mixed with white, and Chinese white should never be used for the high lights.

With good blocks and good printing every colour and every shade of colour in the illustration can be reproduced with great exactness, though any three fixed colours will not suit equally all subjects, while the paper on which it is printed must have been in store for a considerable time to become thoroughly seasoned.

The price of blocks made from a painting or sketch may be taken as about 3*s.* per square inch (1*s.* each block), with a minimum of about £3. If the photographs have to be made directly from the objects themselves the cost would be at least one-third more, say 4*s.* per inch.

The principle of making three negatives by the light filters for the primary pigments can be applied to the collotype process with the most exquisite results, some of the most artistic reproductions of the day being made by pure collotype methods. Coloured photogravures, like engravings, are inked in colour by the hand on the plate itself, and are so extremely costly as to be usually out of the question for book illustration.

Lithography

Before passing to some modifications of the colour processes in use we must glance at the method of lithography. By it we are able to produce facsimiles of drawings made on a prepared transfer paper, or directly on a surface of zinc or stone. The drawing is made with a fatty substance, which combines with the surface of the stone, which is then damped. When an inked roller is passed over the stone the lithographic ink does not adhere to the moist parts, but only to the dry marks of the

A Three-Colour Block in

(a) YELLOW BLOCK.

(b) YELLOW AND RED BLOCKS.

various stages of Printing.

(c) YELLOW, RED, AND BLUE BLOCKS

CHARING.

From "The Pilgrims' Way," by Julia Cartwright. Illustrated by A. H. Hallam Murray.

drawing. The stone is then passed through the press, the ink is printed off on the paper, and the stone is ready for a new impression. Coloured inks may be used.

A lithograph can be made to give great variety of tone if its surface is roughened, or given a grain, as the grain on the stone repels moisture in proportion to the amount of ink upon it, and so gives accurately all the intermediate tones of the drawing.

In photo-lithography a negative—line or half-tone—is printed on a transfer paper made sensitive by a coating of bichromatised gelatin. The paper is then rolled with transfer ink and placed in a bath of water, where the unaltered gelatin softens and can be wiped away, leaving the print upon the paper, which can be transferred to stone in the ordinary way.

In chromo-lithography a separate stone is provided for each colour, and the artist draws each by hand, adding new tints as the work approaches completion. The usual number of stones for posters or commercial work is seven, but as many as fifteen, and sometimes twenty, may be employed in the finest work. The difference between the chromo-lithograph and the three-colour process is that in the former the artist's eye essays to do what is automatically performed by the light-filters in the camera.

Photo-lithography is especially useful for the reproduction of large work, such as charts, diagrams, architectural drawings, etc., as for any small edition it is cheaper than a line block of the same size.

Combination Methods of Colour Printing

Many very pleasing effects are produced by a combination of various methods. It is quite usual to print the colours of a reproduction by lithography and then to print the subject over the colour either from a collotype or from a half-tone block. This process is used in many of the most effective of the coloured postcards of the Continent. Line blocks are sometimes used when a block of solid colour has to be added to a design.

In cases of special difficulty in three-colour reproduction it is not unusual to use a fourth block of a grey or light black tone to produce a softening effect, while when it is not desired to reproduce natural colours, but to make an effective picture, very

11

good results are obtained by using a two-colour process by which a warm tint is added to the original half-tone block.

Other combinations are sometimes found necessary to produce the best effects or to remedy the faults of the original drawings, but all such cases add very largely to the cost of reproduction, and can only be suggested by the experience of the blockmaker.

Large Impressions

Of recent years photogravure printing has been applied to the production of large impressions in what is known as the Rembrandt intaglio process. In ordinary photogravure printing the plates are filled with ink and wiped by hand, and the paper is put over the plate. In this process the plate is prepared with a coarser grain than that of the bitumen dust, obtained by the use of a very fine screen negative. The plate is bent into a cylinder, which rolls over the paper, and leaves a print upon it. The cylinder is inked mechanically and wiped as it revolves under a series of flexible knives which scrape the ink from its surface without removing any from the hollows. It will be seen that this is only possible when the cylinders are turned absolutely true and the machinery is perfect. This process in one form or another is now being adopted by a number of weekly newspapers for their more important illustrations, and an illustrated newspaper abroad is entirely produced by it. The prints at their best are softer and smoother than ordinary photogravures, but lack their brilliancy and depth. The great initial cost of the process is, however, a barrier to its use under present conditions, unless a very large number of prints are required. An initial cost which when spread over 100,000 copies is trifling is prohibitive when only 1,000 are required. It is a process which will certainly play a great part in the book illustration of the future. Colour tints are often added to it by combination with other processes.

Offset Process

Another process which seems destined to affect the illustration of books is the "offset" process. An ordinary offset is caused when the impression on one sheet of paper is imprinted on another facing it; it is usually due to over-inking. In this process the impression from the block and the type is not made on the paper directly, but on an india-rubber plate or cylinder;

and it is from this impression that the actual printing on the paper is made. The offset process is especially useful in the case of photographic work ; the rubber fits itself into the grain of the original block and takes off every particle of ink, which it deposits on the paper. By it reproductions of the finest half-tone blocks, either in black or three-colour, can be obtained on ordinary paper. It is also used to advantage with lithography in which a small loss of sharpness and brilliancy is not so apparent. At present it can hardly be used for book printing, because it tends to soften the outline of the type-impression, which should be as sharp and clear as possible, but it is possible that this objection may be overcome.

As regards the prices here quoted, it is important to remember that they are mere approximations, and that it is not unusual, for example, to find that the photographs, drawings, etc., sent in have to be re-made in order to get a good reproduction. In such cases the prices will be much larger.

NOTES

Proposed Union of Scientific Workers

We continue to receive replies to our notice regarding the emoluments of scientific workers; and they emphasise the opinions which have already been expressed in the leading article of the April number of this Quarterly. For example, one worker, a London graduate with first-class honours, who has published original research work, and is now a demonstrator working two or three days a week, and who also gives two courses of post-graduate lectures with demonstrations, and does other work, receives the generous salary of fifty pounds per annum—much less than most unskilled labourers will work for. We hear that in one British university, out of two hundred members of the junior staff in all departments (that is, all members of the teaching staff who are not full professors), not more than six receive a stipend greater than two hundred and fifty pounds a year. There appears also to be some fear amongst junior staff workers that if they divulge particulars of their salaries they will lose their posts; and in one case we are informed that some highly specialised workers seem even to have lost the ambition ever to earn a reasonable wage. In addition to the poorness of the pay, complaints are made regarding the entire absence of any provision for adequate pension, and also regarding the state of serfdom in which men of science are kept under boards and committees composed of persons who frequently have no qualifications for the exercise of such authority. The whole picture is a melancholy, not to say a disgraceful, one for so wealthy a country, which also imagines that it possesses the hegemony of the world. On the other hand, much sympathy is expressed on behalf of any endeavours that may be made to remedy these evils, and men of science appear to be awakening to the fact that they should attempt some combined effort in this direction. We note especially an excellent article on the Income and Prospects of the Mathematical Specialist by Prof. G. H. Bryan, F.R.S., in the April number of the *Cornhill Magazine*, and an admirable lecture on the Place of Science in Modern Thought by George Idle, Esq., M.I.N.A., delivered at the Royal College of Science, Dublin, on January 27, which suggests at least the position which scientific work should hold in a modern State. Moreover, the lay press is beginning to consider the subject, entirely with sympathy for the scientific worker; and we should like to give special commendation to the efforts being made by the *Morning Post* in its series of articles and letters published during May and June.

The question now arises as to what had best be done under the circumstances; and it has been suggested that those who wish to do so would be wise to form a union of some kind with a programme specifically aimed at improving the position of the workers themselves. At present there are numerous societies which are supposed, more or less indirectly, to attend to this very necessary work, but which certainly have not achieved much success in it. We should therefore like to receive any suggestions upon the subject, together with the names of those who may feel inclined to join such a movement if the programme ultimately decided upon meets with their approval.

The British Science Guild

The eighth annual meeting of the Guild was held at the Mansion House on May 22, the Right Honourable the Lord Mayor presiding and the President, the Right Honourable Sir William Mather, P.C., LL.D., being present. A précis of the annual report was read by Sir Boverton Redwood, Bart., D.Sc., showing the excellent work being done by the Guild. We note particularly the action of the Guild with regard to the Panama-Pacific Exposition, 1915, the organisation of anthropological teaching in universities, the work of the Ventilation and Medical and other Committees—all of which is of value. Especial mention should be given to the facts that the Guild is paying attention to the remuneration of members of Royal Commissions and committees, and that the report refers to the inquiry made by SCIENCE PROGRESS as to the emoluments of scientific workers. In commenting with approval upon our leading article in April last, *Nature* suggests that the subject of it might come within the province of the Science Guild to deal with.

At the annual meeting Sir Ronald Ross gave a short address on the encouragement of discovery, in which he emphasised the point that the public omits the main consideration as regards such encouragement by failing to pay men of science for benefits received from them, and his address has been made the text of a considerable correspondence in the lay press from writers who very often agree with him. In the evening a banquet was given at which many good speeches were made, especially a very noble one from the President, who remarked to the effect that after all religion was best served by a study of the beauty of God's works. Sir William Patrick Byrne, K.C.V.O., C.B., of the Home Office, made some very pertinent observations on the subject referred to in our leading article in the April number, of the employment of scientific experts by Government without payment, and expressed his hearty concurrence in the opinion that payment should in future be given whenever possible. The time appears to be propitious for a general movement towards the betterment of scientific work in Britain, and so far as we can judge all workers are in favour of some reform in this respect.

Institut für Schiffs- und Tropenkrankheiten

On May 28 the Institut für Schiffs- und Tropenkrankheiten at Hamburg held a ceremony in connection with the opening of its magnificent new buildings and hospital in Hamburg. The ceremony was attended by many delegates from abroad, who read or delivered messages of congratulation. Amongst these there was a message from the Right Honourable the Secretary of State for the British Colonies, one from the President of the Royal Society of Medicine, and other messages from the Liverpool and London Schools of Tropical Medicine. The new buildings and organisation of the staff show how far Germany exceeds Britain in its management of scientific affairs. The laboratories and hospital exhibit the newest developments in building and appliances—both for the treatment of the sick and for scientific investigation—on a scale which leaves the original British Schools entirely in the shade. This is only what is to be expected, since the Hamburg institution is paid for and maintained entirely by the State of Hamburg and the Imperial German Government, whereas the British Schools have been founded principally on private benefactions, nobly given, but collected with much difficulty and over a long period of time. The annual income of the German institution amounts to about £20,000 a year, which is probably more than double the incomes of both the British Schools put together. The appointments of all the higher workers are comparatively well paid, are put upon a national footing, and are endowed with pensions varying from 40 per cent. to 100 per cent. of salary, according to length of service. The German Secretary of State for the Colonies and the President of the Hamburg Senate were present on the occasion; and we are glad to note that the Director of the Institute, Professor Dr. Nocht, received a special decoration from the Emperor on the occasion as a recognition of his fine work now extending over many years. Although the total German Colonies in the tropics are very small compared with those of Britain, nevertheless Germany has done more in recognising clearly the importance of tropical medicine and hygiene. Yet, as we showed in our January number, it was really Britain which originated this great work. The efforts of private individuals, however distinguished they may be, cannot hope to keep pace with a well-organised and scientific government with the immense backing of a whole nation behind it.

The Value of Logic

At the April meeting of the Aristotelian Society there was an interesting discussion on the value of logic both as a branch of philosophy and as a subject of education in the university curriculum.

Dr. A. Wolf opened the discussion by reading sections of a long paper in answer to Dr. Schiller's well-known attacks in "Formal Logic." He regarded it as a work of supererogation to defend the teaching of a subject which had behind it the traditions of 2,000 years. He himself had found that students of economics thought it more valuable than mathematics. His own experience was that, for students who were not going to study other branches of philosophy, the more formally it was taught the better. He attempted to prove that

Dr. Schiller's book showed ignorance of a number of technical logical points.

Dr. Schiller replied and maintained his original contention that the fundamental basis of formal logic as commonly taught and understood was unsound. As it might conceivably be presented there was a case for its continuance, but in its present incoherent state it was worse than useless. He remarked that Dr. Wolf's accusation of ignorance was easily answered by a careful reading of the criticised passages in their context, and that Dr. Wolf's case was not improved by the violence of his language.

Dr. Bosanquet gave a general support to Dr. Wolf, but did not altogether approve of the argument from the tradition of 2,000 years. His own experience was that the most educative exercise for the student was not so much the working of the formal logic as the putting of arguments into logic.

Dr. Mercier remarked that none of the speakers in the course of their arguments had made any use whatever of the obverse, or the contrapositive, or the syllogism, or any of the technical machinery of ordinary formal logic.

Mr. Carveth Read turned the discussion to the scientific side. He remarked that the attempt to teach methodology was not usually very successful. Most of the time of the teacher was taken up in explaining the meaning of the illustrative examples, that is in teaching chemistry, physics, or biology. The students were usually quite incapable of finding other examples for themselves.

Prof. Brough owned that formal logic, in its present state, was open to many criticisms, but hoped that Dr. Schiller would see that his views were somewhat too extreme.

Mr. Shelton commented strongly on Dr. Wolf's contention that what is asserted by science should be accepted unconditionally by logic. He said that no man of science would make such a claim. Methodology, as commonly taught, was metaphysics and, on that basis, no special section was required. Unless the methodologist were prepared to make some positive addition to the concepts, methods, and results of science, it would be better to delete the subject of methodology from the university curriculum.

Dr. Schiller and Dr. Wolf briefly replied.

The Constitution of the Atom

The discussion at the Royal Society on the "Constitution of the Atom" on March 19 was notable in disclosing real experimental advances of very far-reaching consequences in the most fundamental of all problems, the ultimate structure of matter. Sir Ernest Rutherford, in opening the proceedings, reviewed briefly the chief new methods of attacking the problem —the transformation of one atom into another in radioactive changes, the phenomena observed when high-speed helium atoms or electrons, such as the α- and β-rays respectively, transverse matter and either penetrate it or are scattered and reflected by it, and, lastly, the regular reflection of the X-rays by crystals. In addition to and distinct from the extraordinary advance in our knowledge of the structure of the *molecular* or *crystalline* unit of solids, the last method, by enabling the wave-lengths of the so-called characteristic X-rays to be determined, has thrown an entirely new light on the structure of the atoms of which these X-rays are characteristic.

The well-known model atom of Sir J. J. Thomson, in which negative electrons are supposed to be distributed through a uniform sphere of positive electricity, cannot give a permanently stable atom, owing to the continual radiation of energy from the revolving electrons, and, moreover, it is unable to explain the facts met with in the scattering of α-particles. The deflection of a very small proportion of the α-particles through large angles by single encounters with heavy atoms of matter necessitated the existence in these atoms of a very concentrated charge, large in amount but small in volume. This led Sir Ernest Rutherford to picture the atom as composed of a nuclear positive charge, excessively small compared with the sphere of action of the atom, and consisting of a number of unit charges equal to about half the atomic mass. A similar number of separate negative electrons are distributed in the external shell, the precise nature of the distribution being still an open question. This model satisfactorily explains the observed laws of scattering of α-particles over the immense range of conditions for which it has been tested. Recently Marsden has studied the passage of α-particles through hydrogen, and has found that some of the hydrogen atoms acquire a velocity by collision so great as to be capable of producing weak scintillations over four times the distance traversed by the α-particles themselves. These results indicated that the nuclei of the hydrogen and helium atoms must approach during collision to within a distance of $1 \cdot 3 \times 10^{-13}$ cm., or less than the diameter usually accepted for the electron. It might be that the mass of the nucleus was, like that of the electron, electro-magnetic in character and was so much greater than the mass of the electron because of this greater concentration of the charge. On such a view the hydrogen nucleus might be really the long-looked-for positive electron.

Results obtained in other fields showed that the charge of the nucleus was probably the "atomic number," which usually is rather less than half the atomic weight. The atomic number is the number of the element in the series when all the places of the periodic table are arranged in sequence, that of hydrogen being one, of helium, two, of lithium, three, and so on. Recent results of Soddy and others, based upon the chemical work of Fleck, showed that the value of the nuclear charge entirely controlled the chemical properties of the atom. When a radio-element expelled an α-particle the nuclear charge was reduced by two units, and when it expelled a β-particle the nuclear charge was increased by one unit. The first produced a change in chemical nature corresponding with a change of two places in the periodic table in one direction and the second with a change of one place in the opposite direction. But after one α-particle and two β-particles had been expelled, the product was chemically indistinguishable from the parent.

The constitution of the nucleus was a difficult problem which might be left to the future generation of physicists, but it was natural and indeed necessary to suppose in the case of the radio-elements that the helium nucleus was one constituent, and, possibly, the hydrogen nucleus was another. But the theory of Dr. Bohr which attempted to reconcile the older mechanical theory with the newer conception of energy quanta undoubtedly had some relation to the experimental facts and had achieved notable successes in accounting for the wave-lengths of the luminous spectra of the simpler elements and for the wave-lengths of the characteristic X-rays.

Mr. Soddy, who followed, said he had found the nuclear atom of great help as a guide in following the sequence of radioactive changes. It was clear that particles moving like the α-particles, at what might be termed ultra-material velocities, revealed the atom as a nebula with a hard point in it. But there was no known property of electricity which would explain how a charge of nearly a hundred units could be concentrated within a volume less than that occupied by an electron. The theory endowed electricity with the attributes of matter rather than explained the attributes of matter in terms of those of electricity. The sequence of radioactive changes and the chemical characterisation of the successive products showed that in the last twelve places of the periodic table, from uranium to thallium, there were nearly forty elements, all those falling in the same place being chemically indistinguishable, or "isotopic." The presumption was that the same was true in the other parts of the table where no means of detecting it yet existed. The results threw doubt, in particular, on the homogeneity of the element lead, but this point still had to be experimentally settled. Apart from any hypothesis, the radioactive evidence proved that, between thallium and uranium, the successive places in the periodic table corresponded with unit increases in the positive nuclear charge of the atom. A model was shown of the periodic system of the elements embodying, in the well-known "figure of eight" arrangement of Sir William Crookes, many new features in accordance with the actual state of our knowledge of the elements to-day.

Mr. Moseley then contributed a most valuable set of entirely new results for the values of the nuclear charges of the elements, as determined from the wave-lengths of their characteristic X-radiation, which fully bore out and extended the conclusions drawn from his initial examination, by this method, of the elements between calcium and zinc, the results of which were published as recently as last December. Accepting the nuclear charge of aluminium as thirteen as the basis, the wave-lengths of the characteristic X-rays of all the others correspond with their atomic numbers, that is, to the number of the place assigned to them by chemists in the Periodic Table. Gaps were indicated in the sequence corresponding with the two missing homologues of manganese, between molybdenum and ruthenium and between tungsten and osmium. In the rare-earth group, places for sixteen elements, including lanthanum and cerium, were indicated, instead of the fourteen included in the International List of Atomic Weights. The work extends our knowledge of the absolute value of the nuclear charge almost from one end of the periodic table to the other, from aluminium right into the region of the heaviest elements, worked out by radioactive methods. The total number of distinct chemical elements between hydrogen and uranium is probably ninety-two, and not more than five or six of these remain still unknown. This is an extraordinary testimony to the completeness and thoroughness with which the elements have been investigated by chemists.

The discussion was continued by Prof. Hicks, who, whilst accepting in general Soddy's theory of the existence of isotopic elements identical with one another in chemical character, pointed out the difficulty in supposing that such elements would prove to be also spectroscopically indistinguishable. He had shown that the atomic mass enters directly into the series relationships of spectra. The lines of the very complicated spectrum of thorium, for example, might in some cases be really due to ionium, which would explain the apparent identity of the spectra of ionium and thorium.

Prof. Nicholson, in a mathematical criticism of the nucleus theory and especially of Bohr's hypothesis, made an interesting reference to coronium, the spectrum of which he had found could be derived accurately on the view that the nuclear charge was five, or the same as that possessed by the terrestrial element boron. He suggested that coronium and other stellar elements might have a type of nucleus entirely distinct from that of the terrestrial elements, and possibly these elements belonged to a different stage in the general evolution of matter. Certainly there is no room in the periodic table, as now understood, for any of the supposed stellar elements, asterium, coronium, nebulium, etc. Prof. Silvanus Thompson raised the question of the magnetic properties of matter and the magneton hypothesis, and referred to recent researches of Weiss, which indicated that the magnetic properties of the elements differed from one another in such a manner as to be capable of expression by a set of integral numbers. Mr. Allen brought forward some interesting results obtained in the mathematical theory of the nuclear atom, and Sir Ernest Rutherford very briefly replied.

Municipal Insanitation

The report upon the state of public health of the city of Dublin during 1912 issued by Sir Charles Cameron, the Chief

Medical Officer of Health for the city, does not convince the reader that the Dublin Corporation manages its sanitary matters very effectively. The housing of the poor appears to be in a wretched condition, and the reason for this is probably that the Dublin Corporation is much influenced by the slum landlord, who is generally a man who tries to get as much out of his property as possible—a common thing. Apparently the corporation seldom compels him to expend money on repairs and on sanitary improvements. The infant mortality in the slums is very great, and the total death-rate of the city was as high as 37·8 per thousand in 1880. During the last ten years it has fallen to 24·8 per thousand—while in London the annual rate is only about 14 per thousand. Even in the North Dublin Union Workhouse, where one would expect proper sanitation, the arrangements appear to be very unsatisfactory. Recently a very strong report on the subject has been issued by a deputation of working men from Manchester, who describe the slums as being of "incredible squalor" (*Morning Post*, May 11, 1914).

It is not possible to say whether the sanitation of Dublin, bad as it appears to be, is very much worse than that of many towns in the United Kingdom. Certainly it is not worse than that of many towns in British colonial possessions. The subject is one of very great importance, because it is open to question whether municipal government is not granted too easily to populations which seem scarcely competent to use it properly. The theoretical considerations upon which such powers have been given are perhaps sound. It is supposed that in an ideal State every person will possess political morality and political wisdom; and even where this ideal is not reached, that municipal government will exert an educating effect upon the people. But, in the world as it is, a very small proportion of the public possess both political morality and political wisdom. Even if many are entirely honest, few can possibly have the detailed knowledge of municipal administration which fits them to conduct such administration in the council of their municipality. This must especially be the case in very small municipalities; and the result is that in them we often find all sanitary matters to be in a deplorable condition. One can understand that a great city will possess enough competent citizens to regulate its sanitary administration; but this becomes more and more unlikely with smaller towns—and we can often see municipal government granted under British administration to what are really mere villages. In such, the management falls into the hands of those who know nothing about scientific administration, and, very often, into the hands of those who seek election only for personal purposes. Thus the funds are in such places allotted very inexpertly or upon the principle of "graft." As nothing is more generally unpopular than sanitation, this important branch of administration is neglected; slum owners, fraudulent food vendors, worthless contractors, and jobbing tradesmen and private citizens are allowed full scope; and the death-rate mounts up into the thirties per mille.

It is time that the whole of this question of granting municipal government to such bodies should receive the careful consideration of Parliament and of the great machinery of Imperial administration. Incompetent municipalities become a danger to the State, and annually sacrifice through ignorance more human life than may be destroyed in a chronic state of war—causing death and sickness to untold thousands. It is extraordinary that this state of things should continue to be allowed. If one city can reduce its death-rate to 14 per mille, other cities can certainly reduce theirs to a figure not much higher; and when they do not do so, it is generally their own fault. We should not think of giving local judicial powers of life and death to small municipalities—and even the police are generally removed from their influence; but British administration does not hesitate to allow them much greater powers of life and death in the sanitary line.

The remedy is to keep and to use full powers of censure and even of suppression against municipalities which show a high death-rate, or which exhibit incompetence in other lines. That this can be done even under present laws is certain; and in fact sanitary administration has actually been recently taken out of the hands of one municipality, namely that of Freetown, Sierra Leone. For years that body failed to make a sufficient reduction in the malaria which abounds in the town, though several expeditions were sent in order to instruct them regarding the full details of the work. The same thing can be said regarding many other such bodies in British tropical possessions. It is time that more discipline and science were introduced into British administration generally. We are too lax, and give in on every occasion to the half-educated, to the wire-puller, and to the faddist. It seems a great thing to talk about the Pax Britannica; but, as a matter of fact, this is often won by such subserviency. So-called political liberty is justifiable only when it is accompanied by efficient administration, and civilisation is now advancing too fast to allow politicians any more to maintain that the first is superior to the second. In fact, in the opinion of many the whole idea of popular government requires some revision in the interests of the life and prosperity of those who are supposed to govern themselves, but are really too often governed by self-seekers, talkers, impostors, and wholly ignorant persons.

REVIEWS

The Viscosity of Liquids. By A. E. DUNSTAN and F. B. THOLE. [Pp. vi + 89, with diagrams.] (London: Longmans, Green & Co., 1914. Price 3*s*. net.)

THIS book is a useful addition to the monographs on Inorganic and Physical Chemistry edited by Prof. Findlay. The text is divided into nine chapters, of which the first is devoted to the "Development of a Working Formula," the next three to a discussion of the methods used in measurements of viscosity. The subsequent chapters deal with liquid mixtures, electrolytic solutions, and colloids, and with the relations which have been found between viscosity and chemical constitution.

It is unfortunate that the large amount of research which has been carried out in this field has provided little more than a collection of disconnected facts. In the book under review attention is drawn to a number of relationships which have been found to exist between viscosity and chemical constitution, such as the definite influence of certain chemical groups, and to experiments indicating that chemical combination is accompanied by maximum values of the viscosity. However in none of these cases has any attempt been made to give a physical interpretation of the results, though a few hypotheses have been thrown out, such as the statement that the influence of the hydroxyl group in raising viscosity is no doubt intimately connected with the potential quadrivalence of the oxygen atoms inducing association. The viscosity of a series of unsaturated organic compounds is also vaguely referred to the degree of residual affinity in the compounds.

The discussion of the influence of temperature is limited to the consideration of a number of empirical formulæ to which no theoretical basis has been assigned, and no attempt is made to justify the general practice of referring all comparative measurements of viscosity to the values obtained at the boiling-points of the liquids.

In the chapter dealing with electrolytic solutions, the data have been presented in a form showing the significant relationships which may be expected between viscosity and the condition of the ions in solution, such as their degree of hydration and the extent of molecular association. It is very noteworthy that at the present time no rigid relation has been established between viscosity and any other physical property.

The disconnected nature of the results in this field render the compiling of a monograph on this subject a task of considerable difficulty. In spite of this, the authors have contrived to write a very readable book, which gives a good idea of the present state of this branch of physical chemistry. J. N. P.

Intermetallic Compounds. By Dr. CECIL H. DESCH. [Pp. vi + 116, with 17 figures.] Monographs on Inorganic and Physical Chemistry. (London: Longmans, Green & Co., 1914.)

THOSE who know the author's "Metallography" will require no assurance as to the merits of this, its junior partner. The present volume is small, but in it is embodied a very large bulk of work ; and it is only by reason of the skill with which they are presented that the facts can be brought into so narrow a compass without confusion. The subject-matter lies for a great part beyond what was discussed in the larger volume referred to ; hence, although there must be some overlapping between them, the two books should be taken together. Here are few experimental details, but it is evident that all results have been submitted to the author's keen criticism before being admitted. The aspect of the subject discussed in this volume is one which will greatly interest chemists, quite apart from metallographers.

I. M.

The Synthetic Use of Metals in Organic Chemistry. By A. J. HALE, B.Sc., A.I.C. [Pp. xi + 169.] (London : J. A. Churchill, 1914. Price 4*s*. 6*d*. net.)

THIS is a book which should make its chief appeal to the practitioner of organic chemistry rather than to the student, but there is much which is of value to both. To the student it is (or should be) a matter of minor concern whether a given organic process works better with, let us say, barium hydroxide instead of calcium hydroxide, unless the fact illustrate some fairly wide matter of principle. To the researcher, on the other hand, such knowledge may be vital.

The book is divided up in a way which seems a little arbitrary, although the author's experience must presumably have found it to be the best ; thus the chapters are arranged according to the various kinds of metals employed. By this method quite different processes are often classed under one heading, and also essentially similar processes are scattered among different chapters simply because more than one kind of metal can be used. Thus reactions of zinc alkyl compounds are in Chapter IV., whilst the corresponding magnesium Grignard reactions are in Chapter III.

The subject-matter is not limited to the metals themselves, but is extended to include their inorganic compounds. One result is that under "Sodium and Potassium" we find such diverse and unrelated reactions as those of metallic sodium on alkyl halides, potassium cyanide on ketones, and potassium hydroxide on aldehydes.

From the point of the student (for whom the work was chiefly written) this book is useful in showing what a very large bulk of all manner of organic syntheses is effected by the use of metals and metallic compounds, and it also provides numerous well-selected practical examples to be worked on in the laboratory. The book will also prove convenient for original workers and for teachers who wish for material to exemplify their own systematic treatment of organic synthesis.

Quantitative Analysis in Practice. By JOHN WADDELL, D.Sc., Ph.D., etc. [Pp. vi + 162.] (London : J. A. Churchill, 1913. Price 4*s*. 6*d*.)

THIS is a book which ought to prove very useful to students beginning quantitative chemical analysis. Primarily, it forms an introduction to technical methods, but this really adds to its value as an aid to those who take up the academic side ; for of the three essential factors in technical work which are neatly summed up in the introduction as honesty, accuracy, and speed, the first two are always to be insisted upon in "pure" chemistry, whilst the third is often apt to receive too little attention. Further, most teachers notice that students are often more interested in analysing everyday substances than in performing similar operations upon pure chemicals ; accordingly, here we find the examination of clay, cement, coal, iron ore, and such materials forming a great part of the book. The accounts of the principles, no less than of the detailed practice, of analyses, are very carefully and clearly given, and the student is not ordered to take a given precaution without being shown exactly why he should do so. In fact, the whole scheme is rational and satisfactory, and there are a great many features, such as the indication of the time for each analysis, which lend special worth to the book.

I. M.

Photochemistry. By S. E. SHEPPARD, D.Sc. [Pp. ix + 461. Illustrations and figures.] Text-Books of Physical Chemistry. (London : Longmans, Green & Co., 1914. Price 12*s.* 6*d.*)

It is no easy matter to review a book like this ; for the reviewer cannot claim to be an authority on photochemistry, hence he feels some diffidence in expressing views which are in the nature of unfavourable criticism. No such criticism would be offered if the series of which this book is one were designed for the reading of specialists only ; but the volumes already issued appeal to a wider public, and therefore it is legitimate to regard the work from a not too exalted standpoint.

Firstly, the treatment throughout is of a very physical kind, and although obviously a great deal of photochemistry is pure physics, a more chemical outlook upon it than we find here would have been welcome. The chemical reader wishes to have, in the first place, information as to the *facts* of the reactions set up or modified by light, and he expects that information to be conveyed in ordinary chemical phraseology. He is then prepared for whatever theories and hypotheses may be forthcoming as to the reasons for such actions, together with the further facts, chemical and physical, which lend support to these. This is doubtless a limited aspect of the matter ; but the point is that this limited aspect is photochemistry, in which science physics must play an auxiliary and not a preponderating rôle. In this book there is a large amount of extremely interesting matter, but its relevance to chemistry is not always plain. And although by diligent study the reader will come to an understanding of what is set forth, he will find that considerable rearrangement, including discrimination of well-grounded theories from speculation and analogy, occupies much of his time after reading.

It would be presumption to deny that an author whose knowledge is as profound and whose enthusiasm is so great as Dr. Sheppard's has the right to choose his own way of attacking his own subject, even though the result may turn out to be caviare to the general. But (and this is the last criticism) it seems to the writer a vast pity that so much interesting and stimulating material should be set forth in a style which jars constantly upon the literary sense of an ordinary individual, and which by its obscurity (in reality due to a meticulous regard for the *mot juste*) is a real check to the appreciation which the author surely should receive.

It must be insisted on once more that these remarks represent the views of a chemist only ; those who specialise in the chemical effects of light cannot fail to gain by the collection together into one volume of so much that is significant in the subject.

I. M.

An Introduction to the Chemistry of Plant Products. By P. HAAS and T. G. HILL. [Pp. 401. With 5 text figures.] (London : Longmans, Green & Co., 1913. Price 7*s.* 6*d.* net.)

It should be said at the outset that this book will be of very great value to the botanist ; published at such a commendably reasonable price, it should find a considerable public. There is no other English book in which the reader will find such ample treatment of the large amount of recent work upon the chemical problems involved in plant metabolism. Most of the chief groups of organic substances of importance in the plant are passed in review in turn, and discussed in the first place from the chemical standpoint, and then in relation to their rôle within the plant.

Strictly speaking, the scope of the book would be better indicated by such a title as "Materials for the Study of Plant Products," as it is essentially a compilation, and lacks that very valuable element in an introduction to the subject, the challenging statement of current hypothesis and controversy which leads the reader to pursue the subject further.

Regarded as a compilation, there are certain criticisms which seem to the reviewer valid.

In the first place, in spite of the statement in the preface that a knowledge of elementary chemistry is assumed, there is some very elementary chemical explanatory matter in the text. It is questionable whether it is a healthy tendency in the botanist that he should be satisfied to have one of the most fundamental underlying sciences of his subject presented to him entirely through the medium of his special literature. To take a concrete case, it is surely not desirable that the student of plant physiology should arrive at the conception of an ester simply through a consideration of the very brief exposition of the formation of salts which precedes it on page 5. Far better that, if the conception of an ester be a new one, elucidation should be sought from general chemical literature.

The book also suffers from one of the defects, perhaps constitutional to compilation, that the various sections do not seem adequately welded together. This is perhaps a defect in the subject-matter at the present stage rather than in its treatment, but the chapter on colloids, for instance, arrives most unexpectedly, and seems to remain very aloof from the many physiological problems it must ultimately help to solve.

The publication of a book of this type, with its valuable references to original papers, tends to make the reader think that the citation of literature is exhaustive; but papers appear on these subjects in such widely different journals that completeness of treatment is probably impossible. In some sections, indeed, the present work is rather misleading, owing to the absence of reference to important papers upon the subjects discussed. The tests given for the detection of small quantities of formaldehyde, for instance, by no means exhaust the possibilities of the subject, and Bokorny has given a more complete account.

In the section upon the quantitative estimation of sugars, no reference is made to important series of investigations by Brown and Morris and by Parkin, in which the quantitative distribution of sugars in plant tissues was studied Their conclusion that cane sugar is to be regarded as the essential sugar in the up-grade series also receives inadequate treatment.

The section on proteins has necessarily been so condensed that the presentation of the subject is incomplete on many points. It will obviously mislead if the few paragraphs upon the formation of salts by proteins are regarded as an adequate treatment of the subject.

Finally, it seems clear that if books are to keep pace with the rapid progress of the subject, it is to the special monograph, readily revised, that we must look for accurate information. Thus it is probably hardly the authors' fault that, in producing a work of this scope, important facts, in the light of recent work, are already incorrectly stated. Thus the impression given on page 227 that the best extracts of chlorophyll are obtained by the action of *water-free* solvents on dried leaves is quite incorrect.

J. H. P.

A Text-Book of Experimental Metallurgy and Assaying. By ALFRED ROLAND GOWER, F.C.S. [Pp. xiv + 163.] (London: Chapman & Hall, 1913. Price 3*s.* 6*d.* net.)

THIS little book is modelled on earlier editions and forms a concise and well-arranged guide for the beginner in practical assaying and metallurgical work. The explanatory text is very brief, and, as the author states in his preface, the knowledge, on the part of the student, of elementary theoretical chemistry is assumed. The first portion of the book is given over to the description of simple experiments, such as may be carried out in any assaying laboratory, dealing with the various processes connected with the production of metals from the oxides, sulphides, and other compounds; the use of fluxes and the formation of slags; and the formation of alloys. The second half of the work presents a series of experiments that serve to illustrate the methods adopted for the assay of the precious metals and for the dry and wet assays of the baser metals and alloys.

Several appendices are placed at the end of the book and comprise weights and measures, melting-points and specific gravities of the metals, and a table of international atomic weights. Appendix D, which occupies eight pages, is a list of chemical formulæ purporting to represent the principal reactions which take place in metallurgical operations. It is, however, difficult to see that any useful purpose is served by tabulating such matter as this, for the formulæ are necessarily incomplete and give no idea of the conditions under which the changes are supposed to take place.

Appendix E gives a brief description of the chief ores and contains what might be termed "rule of thumb" methods for their discrimination. Other appendices comprise a table of factors and other aids in the solution of the ordinary assay-problems.

The book as a whole is eminently practical, but the practical side has been given so much prominence that the scientific aspect of the metallurgical methods is somewhat lost sight of. The book, however, should prove of great value to the elementary student if used with caution. Its use should certainly follow on a ground-work of elementary chemistry and an appreciation of the scientific principles that underlie and control the various reactions to which metallurgical science owes its existence.

H. H. T.

The Antiquity of Man in Europe. Being the Munro Lectures, 1913. By JAMES GEIKIE, LL.D., D.C.L. [Pp. xx + 328. With 21 plates, 9 other figures, and 4 maps.] (Edinburgh: Oliver & Boyd. Price 10*s.* 6*d.* net.)

DR. ROBERT MUNRO founded the "Munro Lectureship in Anthropology and Prehistoric Archæology" at Edinburgh University about two years ago, and was himself the first lecturer. Prof. J. Geikie is thus the second scholar to hold the position, and his ten lectures are now issued as a book which, in spite of certain somewhat serious omissions, is absorbingly interesting from beginning to end.

It is unfortunate, however, that the lectures cannot be considered quite up-to-date. They were delivered after the famous Piltdown discovery was made known to the world (though, we believe, before the Dawson-Woodward paper was actually published), and yet they contain virtually no reference to that epoch-making event, a fault which is aggravated by nearly a year's delay in publication. This is the more disappointing in that there is of course no one more competent than the author to answer the important question as to whether the Piltdown relics—one of the two oldest discoveries of man in Europe—should be attributed to the first or to the second interglacial epoch.

The work deals, as its title implies, with the purely geological side of pre-historic anthropology, anatomical and archæological matters being only referred to so far as is necessary to make the geological story intelligible. The extreme views on the question of the antiquity of man obtain scant recognition from Prof. Geikie. Here we have no talk of Oligocene, Miocene, or even of Pliocene humanity. The claims of the eoliths are dismissed in a few paragraphs, and the Ipswich and Galley Hill skeletons are not deemed worthy of mention. Even the Pliocene "rostro-carinate implements," championed by Ray Lankester, are regarded as more than doubtful. The problem is thus narrowed down to the Pleistocene, and the opinions expressed are representative of the more cautious school of anthropologists.

The dramatic story of the Glacial Epochs is told us once again, and with a wealth of detail which makes the book of value to the student as well as to the general reader. We are given a vision of the musk-ox, the banded lemming (well described as "the warmth-hater"), and other denizens of the tundras coming south through twenty degrees of latitude, of icebergs stranded on the Azores, and of vast Swiss glaciers boring huge trenches hundreds of feet deep. The successive lectures deal with the Pleistocene fauna and flora, with the testimony of the caves, the river-drifts, and the great morainic accumulations, with the interglacial strata; and finally a chronological history of the Pleistocene is given. There is a thorough treatment of the flora as well as of the fauna, and both animals and plants are illustrated by numerous charming plates. The botany of the Pleistocene is very important, because the plants afford a more reliable index to climatic changes than the animals can supply. No doubt in many instances, where remains of animals apparently belonging to different climates are found in juxtaposition, the creatures were not really contemporaneous—different strata have become mixed up; but this is not so in every case, and there is little doubt that the lion and the hyena, for instance, ranged into colder climes in the Pleistocene than they do now, and the converse appears to be true of the arctic fox.

As is well known, Prof. Geikie's scheme includes six glacial epochs, not four as in the more generally accepted classification. His differences with Penck and the other great German pioneers are, however, scarcely more than verbal. His first four epochs correspond entirely to their four, and it is not denied on the one side that there were several subsequent returns of cold conditions, nor is it contended on the other side that such recurrences approached in severity any of the four great ice-ages. Moreover, it is known for certain that Palæolithic man and the distinctively Pleistocene fauna vanished before the fifth glacial epoch of Geikie. It is a moot point whether the last cold phases were sufficiently severe to be styled "Glacial Periods." There are proofs of three returns of cold conditions in the Alps, but the first of these is indistinguishable from the fourth glacial period in Scotland. Thus the author's first glacial epoch is evidenced by the Chillesford Clay and Weybourne Crag, which used to be classed as Pliocene, and his last two are Neolithic.

The correlation of the human relics with these glacial and interglacial epochs is an interesting if difficult subject. There is no unanimity yet among scholars, but here, as elsewhere, the author represents the dominant school. The Heidelberg Jaw is placed in the first interglacial phase, and the Chellean and Acheulean cultures flourished in the second genial interval, which was very long and warmer than the present day. The Mousterian implements carry us through the third ice-age to the next interglacial epoch, at the end of which the more advanced Aurignacian and Solutréan cultures appeared. The Magdalenians lived through and during the decline of the fourth glacial epoch. Then comes the hiatus between Palæolithic and Neolithic man, which the author regards as a reality in Northern and Central Europe, if not farther south, the so-called Azilian culture being typically Neolithic. The Neolithic tribes appeared, however, before the fifth ice-age. It is unfortunate that the author groups the two oldest cultures, the Mesvinian and Strépyan, with the Chellean. The Mesvinian implements have been placed by some writers in the first interglacial epoch, and since they are the oldest artifacts generally recognised as such, we should have welcomed a discussion of their exact age. We infer that Geikie places them in the second interglacial epoch with his comprehensive "Chellean," but the point is not discussed.

We think the anatomical summary is too meagre even for the author's special purpose, and we find it stated on p. 47 that the Magdalenians were short. One of the Magdalenian races no doubt was short, but there is strong evidence (accepted by most writers) that the tall "Cro-Magnon" race was living during this period, and Prof. Geikie ought certainly to have given his reasons for dissenting from this view.

The book closes with the usual guesses at the duration of the various epochs in years, the Heidelberg Jaw being given an antiquity of about half a million years.

A. G. THACKER.

The Childhood of the World. A simple account of man's origin and early history. By EDWARD CLODD. New and revised edition. [Pp. xiii + 240. With 26 figs.] (New York: The Macmillan Co., 1914. Price 4*s.* 6*d.* net.)

THIS well-known and successful little work, first published in 1872, is divided into three parts, dealing respectively with man's physical and material evolution, his mental and religious development, and with his advance in scientific ideas. The book's success (it has been translated into seven languages) is no doubt largely due to the author's admirable style of writing, which, being both simple and graphic, enables him to reach not only the general public but even juvenile readers. Occasionally, however, in aiming at simplicity he becomes almost unintelligible, as in his avoidance of the word "Pleistocene." The work of bringing the book up-to-date has been fairly thoroughly done, but there are a few rather serious mistakes. For instance, the erroneous impression is given that the distribution of land and sea in N.W. Europe was almost constant during the Paleolithic Age; the phylogenetic tree of the higher Primates on p. 14 is misleading and inadequate, the subdivision of the Old Stone Age on p. 50 is quite out-of-date, and we are told that there are no fossils known from Pre-cambrian rocks. The section dealing with religion is written from the naturalistic standpoint. There is a good bibliography appended.

A. G. T.

Australasian Fossils. A Student's Manual of Palæontology. By FREDERICK CHAPMAN. With an Introduction by PROF. E. W. SKEATS. [Pp. 341. Illustrated.] (Melbourne: G. Robertson & Co., 1914.)

STUDENTS of palæontology in Australia and New Zealand have hitherto had to contend with the disadvantage that the text-books on their science are mainly written either from the European or the American standpoint, and consequently take but scant notice of Australasian formations and fossils. To a certain degree this is undoubtedly a hindrance to local workers, and it has accordingly been deemed that the time has come to collect and arrange in a handy form the main facts of the subject as exemplified from the Australasian point of view. The result is the admirable little volume now before us, the author of which, from his official position as palæontologist to the National Museum at Melbourne, enjoys special and unrivalled opportunities for undertaking a task of this nature.

Not only will the volume stimulate workers in Australia and New Zealand, but it will likewise have a very considerable value to workers in this country as an up-to-date sketch of the leading facts in Antipodean palæontology. The Australasian student may, however, be reminded that the publication of Mr. Chapman's volume does not by any means imply that palæontological and geological works written from the European standpoint are to be permanently shelved, and that he will find all he wants in the local text-book. As the author himself would doubtless be the first to acknowledge, precisely the contrary is the case; and since the British rock series, with its included fossils, is the typical basis for the geology and palæontology of the world in general, students in all quarters of the globe must always make this their starting-point and standard of sequence.

In connection with this sequence of strata in Europe and the Antipodes certain very important and interesting remarks are made in the Introduction by Prof. Skeats with regard to the late Prof. Huxley's doctrine of "homotaxis." Throughout the world there is no exception to the rule that strata containing Devonian and Carboniferous fossils overlie those of a Silurian type, and that Palæozoic are succeeded by Mesozoic formations, and these again by beds of Tertiary age. But it has long been a question whether, let us say, the Silurian and Devonian strata of Australia or Africa were strictly contemporaneous with the typical European representatives of those strata. Huxley was strongly disposed to consider that they were not, basing his opinion on the supposition that the migration of one fauna—say the Silurian—from one area to the other would occupy such a long period of time that when it reached its new habitat the succeeding Carboniferous fauna would have developed in the original area. Consequently, if this were so, a Devonian fauna and flora in Britain might have co-existed with those of Silurian age in North America, and with Carboniferous forms of life in Africa.

But, remarks Prof. Skeats, "this could only be true if the time taken for the migration of faunas and floras was so great as to transcend the boundaries between great geological periods. This does not appear to be the case, and Huxley's idea in its extreme form has consequently been generally abandoned." Hence we may now regard at least some of the Silurian fossils of Australia as being the actual contemporaries of their European namesakes.

Commencing with a discussion on the nature of fossils, the means by which these are preserved, their various modes of occurrence, and the characters and sequence of the rocks in which they are embedded, Mr. Chapman includes in this portion of the volume a table showing the correlation between the geological horizons of Australia and those of Europe. A persual of this table will at once show that while there is a comparatively full development of the Palæozoic strata and their fauna in the southern island-continent, the Middle and Upper Mesozoic formations are much less fully developed than in Europe, while the Eocene is apparently wanting. In consequence of these deficiencies, Australia has practically no record of the great development of terrestrial reptilian and mammalian life which took place during these epochs in other parts of the world ; thereby emphasising our remarks with regard to the importance of European palæontological text-books to Australian workers. Before leaving the stratigraphical table, reference may be made to an unfortunate error on p. 47, where the Permian and Carboniferous are classed as Mesozoic in place of Palæozoic.

The various groups of animal fossils are treated in zoological order, from the lowest to the highest, and there is also a brief chapter on palæobotany, in which it is mentioned that the existing ginkgo, or maiden-hair, tree of China and Japan was represented in the Jurassic of Victoria and Queensland.

As regards their vertebrate land faunas both Australia and New Zealand come under the category of what have been happily termed "biological asylums"—a feature largely due to the long isolation of both areas. There was, however, a time when Australia formed part of "Gondwanaland," and shared its thylacines and horned-tortoises with South America, its batrachians with South Africa, and its ferns with India; and it would have been well, we think, had these former land-connections been shown on a map. In regard to the systematic position of the Australian aborigines the author is thoroughly up to date, although he is a bit "wobbly" with regard to the introduction of the dingo.

Taken as a whole, the volume is admirably planned, and the plan admirably executed.

R. L.

The Snakes of Europe. By G. A. Boulenger, LL.D., D.Sc., Ph.D., F.R.S. [Pp. xi + 269. With 14 plates and 42 text figures.] (London: Methuen & Co., 1913. Price 6*s*.)

Until the publication of this book there was a remarkable gap in English zoological literature; for, as is pointed out in the preface, there is no book treating of European reptiles in the English language. This present volume on the snakes in part fills the gap; and it is to be hoped that the remaining European reptiles will be dealt with ere long by an author as well equipped for his task as Mr. Boulenger.

The book is divided into two parts—one an introduction dealing with the characteristics of snakes in general, and the second occupied with a systematic account of the various species. On the whole, the book is remarkably free from errors; but to make a sentence, "Further remarks on this subject in the chapter on Dentition" (p. 6) needs some addition, and "in relation with" is used instead of "in relation to" on p. 8. There is a tendency to use technical words not clearly explained by the context rather freely in certain parts, *e.g.* Lepidosis (p. 17), and some of the chapters are consequently rendered very stiff reading for the layman. The chapters dealing with general subjects, such as those on Colouration, Habits, and Snakes in Relation to Man are extremely interesting. To crowd a large amount of information into a small compass is always a difficult task, but it has been accomplished here with great success. The nervous system, however, is somewhat briefly treated, and the whole of it is dismissed in about six lines.

The statement on p. 77 that "the right systemic arch gives off the carotid artery, which in many snakes—the common grass-snake, for instance—may branch into two, or in others be double from its origin," is somewhat misleading, since it implies that in this snake there are two common carotid arteries. One of the most interesting points in the arterial system of the grass-snake, however, is the fact that as the right common carotid is reduced during embryonic development to a very small twig which supplies only the thyroid gland, the whole of the blood-supply to the head is carried by the left common carotid. Three special arterial anastomoses are developed in order to allow the blood to be conveyed to the right side of the head.

The key to the identification of the various species by means of their external characters and the list of the species found in the different regions of Europe are very handy. Although it is stated on p. 135 that the vipers are the only very dangerous snakes, this information would be more easily referred to if the individual species in either of the above lists had been marked according to whether they were slightly, considerably, or not at all poisonous.

The systematic part contains a full yet concise description of all the snakes found in Europe, together with an account of their distribution, local varieties, and habits. This is indeed very useful for reference, and its value is greatly enhanced by the completeness of its illustrations, mostly from the very accurate drawings by Prof. Sordelli of Naples.

The few criticisms offered above do not alter the fact that the volume is a very desirable addition to our knowledge. It is certainly a work that should find a place on the bookshelf not only of the naturalist, who will learn much from its well-filled pages, but also of the general reader who wishes to know something of the general characteristics of snakes or the particular forms to be found in Europe from one who has command of a facile pen and a remarkable knowledge of his subject.

C. H. O'D.

Les Zoocécidées des Plantes d'Europe et du Bassin de la Méditerranée. Tome 3: Supplément 1909-12. By C. HOUARD. [Pp. 312. With 1567 figures and 8 portraits.] (Paris: A. Hermann et Fils, 1913. Price 10 francs.)

THE vast majority of the parasites which attack plants are either fungi or insects, and the result of the attack is either (1) a drain on the food-resources of the host, which if severe may so weaken it that it succumbs to adverse conditions otherwise easily overcome, or (2) the production of injurious substances which cause local or even general death, or (3) a stimulus to local growth. As regards (1) and (2) it may be said that general death as the result of parasitic attack is a rare occurrence in the higher plants, only taking place when the parasite blocks up the wood vessels, thus cutting off the water-supply to the leaves, or invades the whole plant. In most cases, death is likely to be merely local, since plants present a striking contrast with animals in the fact that they have no means for the rapid distribution of poisons locally produced nor any regulatory centres whose injury upsets the entire system.

The location of a parasite in a plant is often marked by deformities which frequently appear as circumscribed swellings, or galls, and are usually conspicuous structures of peculiar and fantastic or beautiful form, especially in the case of animal-induced galls (zoocecidia). The chief animal gall-producers, apart from the nematodes (eelworms) whose attention is usually confined to the roots of plants, are insects belonging to various families of hemiptera, diptera, hymenoptera, and a relatively small number of coleoptera and lepidoptera; while the plants attacked range from Algæ and fungi to the Compositæ—indeed there are probably very few species of flowering plants on which galls may not be found either frequently or occasionally. Hence it is not surprising that the study of animal-induced galls (zoocecidology) is pursued by a large and enthusiastic body of workers, mainly field-naturalists who confine their attention to external form and the determination of the species of insect and plant concerned in gall production. The investigation of the histology and physiology of galls has, as might be perhaps expected in a branch of biology which appeals both to botanists and zoologists, and is therefore apt to be neglected by laboratory workers in both departments, lagged behind the purely systematic study of zoocecidology, though it offers a most attractive field for research and the results already obtained are of the greatest interest from many points of view, both biological and biochemical. The work of Peyritsch, Beijerinck, Küster, and others has shown that the detailed study of galls and gall-formation may throw light upon many problems in the physiology and pathology of plants, and the considerable and increasing literature of the subject indicates that this study has before it a future of great promise.

Meanwhile, biologists owe a debt of gratitude to the labours of M. Houard in compiling his great work, of which the volume now issued is the third part, consisting of pp. 1249 to 1560, with figures 1366 to 1566. As the author points out, since the appearance of the first two volumes the study of galls has shown extraordinary development, and this supplementary volume, based on four years of cecidological work, contains descriptions of 1,300 galls, giving the names of over 500 species of gall-producing animals and of 300 new plant "hosts," with an extensive bibliography, and finally zoological and botanical lists which facilitate reference to the body of the work. This volume, like its predecessors, is indispensable to all who are interested in galls; it is admirably arranged, and the price is very moderate.

F. Cavers.

Cabinet Timbers of Australia. By R. T. Baker, F.L.S. [Pp. 186. With 68 coloured plates.] (Sydney: Technological Museum, 1913.)

This handsome volume forms No. 18 of the valuable Technical Education Series published by the New South Wales Government, and contains an Introduction written by the State Minister of Public Instruction, who aptly remarks that the colour, figure, and other characters here portrayed of the various species, by colour photography, may come as a revelation to those not intimately acquainted with the timbers themselves. The letterpress is practically limited to brief but adequate descriptions of the various timbers, which are illustrated by sixty-eight exquisite coloured plates. These fine plates alone render the book of great value; and it is greatly to be desired that plates of this kind, showing the natural colour and graining of timbers, may be published in other timber-producing countries. The importance of works like this is far-reaching; for apart from their more immediate value in the technology of timber and in cabinet-making, the bringing together of useful and beautiful timbers in this particular form should do much to stimulate the movement for the setting aside of forest reserves and for extensive afforestation in regions where valuable forests are in danger of extinction.

F. Cavers.

Rubber and Rubber Planting. By R. H. LOCK, SC.D. [Pp. xiv + 246, 10 plates and 18 text figures.] (London: Cambridge University Press, 1913. Price 5*s*. net.)

THOUGH no other vegetable product has been put to so many and varied uses as rubber, and none has risen with equal rapidity from insignificance to such high commercial importance, the science and practice of rubber planting are alike, as the author points out, still in their infancy. Dr. Lock is to be congratulated on having succeeded in compressing into the small compass of this handy and well-illustrated book a remarkably complete summary of what is accurately known in both the botanical and the commercial branches of the subject. His well-known researches on the physiology of latex, together with his close and long-continued personal acquaintance with the plantation industry in Ceylon, render the present work authoritative and accurate in both branches, while it is written in a simple and attractive style which should ensure a wide circle of readers.

The subject of rubber and its cultivation is one that appeals not merely to those connected with the rubber industry, but to all who are interested in natural products and particularly in one like rubber, in the development of which British invention and British capital may be said to have played the predominant part almost throughout. The first patent for the employment of rubber for anything further than such uses as the removal of pencil marks dates back only to 1791, when it was applied to waterproofing purposes by Thomas Hancock, of the firm of Charles Macintosh & Co.; but the modern extensions of rubber manufacture only became possible after the discovery of vulcanisation—the process of combining rubber with sulphur—was made, about seventy years ago, independently and almost simultaneously by Goodyear in America and Hancock in England. In his chapters on the chemistry of rubber and the manufacture of rubber goods the author gives details concerning vulcanisation, which ranks among the most important of all industrial discoveries, since it not only makes rubber practically unaffected by changes in temperature and immersion in water, but enables the manufacturer to vary the physical properties of the finished product, according to the proportion of sulphur used, from those of the softest elastic up to those of the hardest vulcanite. The importance of the rubber-planting industry to this country may be gauged from the fact that over £100,000,000 of British money are invested in it, quite apart from the almost innumerable manufacturing industries in which rubber plays a part.

The author discusses the botanical sources of rubber, the physiology of latex production, and experiments in tapping; then follow four chapters on *Hevea brasiliensis*, dealing respectively with planting and harvesting operations, factory work on the estate, and the pests and diseases of *Hevea*. A chapter is next devoted to the cultivation of rubber-yielding plants other than *Hevea*, in which, after a brief but judicious summary of the results obtained from *Castilloa*, *Manihot*, *Funtumia*, and *Ficus elastica* in various parts of the tropics, he arrives at the conclusion that in the future the world's supply of rubber will probably depend more and more upon the *Hevea* plantations. The chapter on rubber chemistry gives a remarkably clear and concise account of recent work on a substance whose chemical composition and behaviour are among the most difficult problems facing the organic chemist.

F. CAVERS.

The Diseases of Tropical Plants. By MELVILLE THURSTON COOK, Ph.D. [Pp. vi + 317.] (London: Macmillan & Co., 1913. Price 8*s*. 6*d*.)

THE rapid growth of plantation industries in the tropics, and the great advances made in tropical agriculture generally, within comparatively recent years have resulted in the occurrence of epidemic diseases of various kinds which always tend to accompany the cultivation of individual plants on a large scale. Many of the diseases are of fungal nature, and these are often those most difficult to cope with. Every crop is liable to attack sooner or later, however resistent it may appear to be on its first introduction. When tea was introduced into Ceylon as one of the principal crops, about a quarter of a century ago, it was often said that the main thing to be careful about was to get the right variety, as once in, it was difficult to get out. No disease of any consequence appeared to attack the bushes, and after the awful experience of coffee it seemed as if at last a plant had been secured which was easily going to hold its own against all parasitic invasion. But tea is not immune, any more than is any other crop, and as the years go on the number of plant diseases referable to fungi continually increases with the growth of intensive culture.

A book on diseases of hot-country plants, their nature, prevention, and curative treatment, will appeal with force to those whose business lies in the tropics, where cultivation is carried on under climatal conditions peculiarly favourable to the spreading of pests of many kinds, while their eradication is attended by corresponding difficulties.

Dr. Cook is already known as a writer on plant pathology, and his experience in Cuba and elsewhere has enabled him to gain a first-hand and extensive knowledge of the subject. The broad lines on which his book on *The Diseases of Tropical Plants* is laid down are similar to those of other writers who have dealt with the subject in connection with the vegetation of temperate climates. He considers the general nature and symptoms of disease in general, and, in order that his readers may be the better able to follow him, gives a brief outline of the structure of the higher plants. This is followed by a classification of the fungi, and a short account of the chief disease-producing animals. The greater part of the volume is devoted to a description of the particular diseases (and the organisms which cause them) of individual economic plants, whilst in a special chapter the chief fungicides and forms of spraying apparatus are described.

The book is a useful one; but perhaps it will appeal more to the scientific expert, or the plant-sanitation officer, than to the planter. The latter will find much that he would scarcely understand or appreciate without a previous botanical training. It does not, however, follow that this is to be regarded as a blemish, for, after all, plant-sanitation work is a highly expert business, and it can hardly be undertaken by an amateur without grave risk. If one desired to urge a point of criticism on general grounds it would be that the author seems to have tried to cater for both classes of readers, the planter and the pathologist. Perhaps this was inevitable. At any rate, Dr. Cook has done his work well, as far as a book of modest dimensions would allow, and the planter will doubtless derive much instruction from it. He will be able to appreciate the urgent importance of extending our knowledge of a subject which touches his own material interests so closely.

The book is well printed and illustrated, but the misprints are somewhat more numerous than they should be.

Philosophy of the Practical, Economic and Ethic. Translated from the Italian of Benedetto Croce by DOUGLAS AINSLIE, B.A. (Oxon), M.R.A.S. [Pp. xxxvii + 585.] (London: Macmillan & Co., 1913. Price 12*s.* net.)

THE volume translated by Mr. Ainslie is entitled *The Philosophy of the Practical.* Those who are not acquainted with the peculiarities of modern metaphysics must, however, be warned that the title may be misinterpreted. The philosophy is not practical in the ordinary sense of the word. In the words of the author, "*The Philosophy of the Practical* cannot be *practical philosophy*." The author, indeed, in some passages expresses the opinion that it is unphilosophic to attempt to deduce a practical conclusion from a philosophical premise. The volume is an essay in metaphysics, beautifully written, and the author is fortunate to find a translator, himself a poet, who has so keen a sense of literary form. The reviewer is unable to judge the accuracy of the translation, but can pay the compliment that the volume reads like an original work.

Unfortunately, however, the translator insists on writing a preface. And the preface attracts attention, irritates, and excites opposition. How far the author would appreciate the remarks is doubtful. One remembers Swift's account of the meeting of Aristotle and his critics in the truthful atmosphere of the underworld. On the other hand, we must not forget the more practical side. If metaphysicians were not admired by pupils who misunderstood them,[1] they would have no readers at all. We are, however, interested to note that "the so-called Synthetic Philosophy (really psychology) of Herbert Spencer was one of the many powerful influences abroad, tending to divert youthful minds from the truth path of knowledge. . . . Spencer tries to force Life into a brass bottle of his own making, but the genius will not go into the bottle." Other philosophers wait for the guileless youth (who is foolish enough to prefer philosophy to sport?) with mask and rapier at the corner of every thicket. "Croce alone has defined and allocated the activities of the human spirit, he alone has plumbed and charted its ocean in all its depth and breadth." This is somewhat crude. It is perfectly legitimate, in philosophy, to express any opinion whatever, but statements of this kind should be disguised in the recognised setting of dialectic ornamentation.

Concerning the book itself, it is not now possible to describe the system or to compare it with those of other philosophers. The statement quoted above is descriptive to the extent that Croce duly discusses the views of many great philosophers and substitutes his own for theirs. We must assume that these systems satisfy some demand of the human mind. No one has recognised the necessity for successive metaphysical systems and their tentative nature more clearly than Mr. Bradley. ". . . existing philosophies cannot answer this purpose. For whether there is progress or not, at all events there is change; and the changed minds of each generation will require a difference in what has to satisfy the intellect. Hence there is as much reason for new philosophy as there is for new poetry. In each case the new production is usually much inferior to something already in existence, and yet it answers a purpose for it appeals more personally to the reader" (*Appearance and Reality*, p. 6).

To extant and recent metaphysicians, Bradley, James and the pragmatists, Bergson, we must now add Croce. Whether or no the newcomer is likely to satisfy the quasi-æsthetic need which calls for metaphysics, only time can show. The reviewer is strongly opposed to the trend of that part of the argument which asserts so absolute and impassable a gulf between a philosophy of the practical and practical philosophy. But that fundamental we cannot discuss here.

H. S. S.

[1] It is as well to state that I am not accusing Mr. Ainslie of any specific misunderstanding of Croce. The sense of the remark is that of the proverbial definition of metaphysics: "When a man who does not know what he is talking about addresses a number of people who do not understand him, that is metaphysics."

Marine Engineering. By ENGINEER-CAPTAIN A. E. TOMPKINS, Royal Navy (Retired). [Pp. ix + 812.] (London: Macmillan & Co., 1914. Price 15*s.* net.)

THIS work is pre-eminently of a practical character, covering in a very satisfactory manner the whole range of machinery usually coming under the care of marine engineers.

The various organs of a ship are described, and their action explained in clear and readable style, inspiring the reader with confidence as to the practical acquaintance of the author with the machines he deals with.

Adequate treatment is given to the more recent developments of marine engineering, namely, the use of the steam turbine and various types of internal combustion engines.

The chapter on Steam Turbines is particularly well arranged, and the writer may be, in this instance, forgiven his excursion into the history of a subject about which, at present, very little has been written; though there is little justification for the introductory matter on the early development of the steam engine, or on definitions and units, or on the elementary thermo-dynamics of heat engines, concerning all of which there is already a wealth of able literature.

The book includes a very detailed section on the various kinds of marine boilers, embodying much useful information on the several types of water-tube boilers now in use in the Royal Navy.

There is a section on combustion, which brings out the great importance of this process on the economy of working, and includes a chapter on the combustion of liquid fuel, which is now becoming more and more general, especially on the ships of the Royal Navy.

The reciprocating steam engine receives careful attention in every particular, and all its immediate auxiliaries are given due consideration.

The chapter on indicator diagrams strikes one as being extremely useful, developing as it does the uses of this instrument for detecting the nature of the trouble with faulty engines.

The screw propeller is described, the author, in this case, wisely confining himself to practical matters and common phenomena, leaving all but the very elementary theory to more specialised works.

The auxiliary machinery, other than that employed in actual propulsion, is well treated, so that an engineer reader has in this work a good deal of information on the ever-increasing and varied auxiliaries which come under his charge, such as electrical generating sets, steering gears, refrigeration plant, etc.

In conclusion, it may be said that this volume may be thoroughly recommended to every marine engineer, whether he be a naval man or in the merchant service, as a sound and readable work.

Malaria: Etiology, Pathology, Diagnosis, Prophylaxis, and Treatment, by GRAHAM E. HENSON, M.D., with an Introduction by PROF. CHARLES C. BASS, M.D. [Pp. 190 and 27 Illustrations.] (St. Louis: C. V. Mosby Company, 1913.)

THE literature of Malaria is so very large that it is impossible to write a complete text-book upon the subject except at great length. At the same time short text-books are useful for medical men and sanitarians who come only occasionally into contact with the disease; and Dr. Henson's little work is extremely useful for this purpose. It gives a good *résumé* of most of the important departments of the subject, and discusses some of them as completely as possible within its narrow limits. It begins with a short historical and geographical chapter, continues with a study of the parasites and a discussion of the carrying agents, the Anopheline mosquitoes, and describes the recent cultivation of the parasites by Bass and Johns, recently confirmed by D. Thomson and Ziemann and others. It also gives a good *résumé* of the various theories regarding relapses. In this last connection, I think that the hypothesis of intracorpuscular conjugation has been much over-rated. As long ago as 1898, I supported Mannaberg's ideas from my own observations in connection with the so-called *Proteosoma* of birds, in which many corpuscles contained five or six small parasites which appear to coalesce in order to produce the sexual forms. But it is very difficult to give a genuine proof of their really doing so, while the supposed process appears to be very contrary to what we should expect from our general biological knowledge. But even if we admit that this conjugation does occur, it is still impossible to understand how it can have any effect in continuing the species of parasites in the host. The conjugation is supposed to produce the sexual forms, and these, for reasons which I have frequently given, cannot be proved to be concerned in such continuation of the species—though of course they continue the species in the mosquito. Rather an unnecessary fuss is made upon this question, because, after all, the long continuance of the malaria parasites in the host is precisely similar to the continuance of other organisms, and probably depends upon immunity questions. The author gives also a short review of my analytical study of the factors concerned in the spread of malaria in a community, though he does not deal with my actual general epidemiological equations (given in the second edition of my book on *The Prevention of Malaria* and also in *Nature*, October 5, 1911, and February 8, 1912). It is singular that though these equations are of vital importance in the whole of epidemiology, they have received scarcely any attention from any of the very numerous epidemiologists who instruct us on these matters. Perhaps medical men are too little given to exact thought to make very good epidemiologists. Their processes of reasoning are generally more qualitative than quantitative, and their mistakes are, still more frequently, correspondingly astonishing. Nevertheless, Dr. Henson shows more exactness than usual, and his book does not admit of a detailed study of this subject. Much recent work on malaria is open to a logical fallacy connected with microscopical work which I have also ventured to point out. An observer examines a large number of objects one after another. What he sees is often described correctly enough; but when he attempts to connect the different objects by means of a mental thread which he calls a life-history, he is apt to forget that this thread is hypothetical, and is finally tempted to imagine that it is as real as the objects which he sees. This is an explanation of the extremely inconclusive work which is done on the life-history of many microscopical organisms. For instance, scores of observers have come to different conclusions regarding the life-history of various trypanosomes, at a great expense of money and time; and yet our knowledge is very defective. What is wanted is new methods, especially those of cultivation and correct enumeration. As regards malaria, these new methods are already yielding fruit, and will probably revolutionise our study of the subject very soon. In the meantime Dr. Henson's book ought to be in the hands of all those who require a condensed knowledge of the subject.

R. Ross.

Sanitation in India. By J. A. TURNER, M.D., D.P.H., Executive Health Officer, Bombay Municipality, with Contributions by B. K. GOLDSMITH, M.B., D.P.H., S. C. HORMUSJI, L.R.C.P., M.R.C.S., M.D., D.P.H., K. B. SHROFF, L.M.S., D.P.H., D.T.M., and L. GODINHO, L.M. & S., M.D., D.P.H. [Pp. viii + 1014. Illustrated.] (Bombay: *The Times of India*, 1914.)

THIS is a book of the same size, but is more technical in its structure. It is dedicated to the Municipal Corporation of Bombay, and is written by the Executive Health Officer of that great city—which is now become perhaps the second city in the Empire. There are contributions by four able medical men. The work begins with an outline of sanitary administration in India, and then continues with the disposal of town refuse and sewage, water supply, food and milk, infectious diseases and their prevention, malaria and other diseases due to animal parasites, disinfection, dangerous trades, school hygiene, housing, and vital statistics; and there are specially useful chapters on habits and customs in relation to health and on the routine work of sanitary officials. The matter appears to be very correct as a rule, but it is unfortunate that parthenogenesis is definitely described as being a *third method of reproduction* in malaria. Such statements are examples of the way in which what were originally merely conjectures, even of a wild description, become gradually crystallised by the petrifaction of time into absolute truths. Once this has happened, scarcely any amount of criticism of the supposed truth is effective in getting it eliminated from the text-books. The work will be a necessary part of the health officer's library, especially now that Mr. J. A. Jones's book is out of print.

R. R.

Hygiene and Diseases of India. A Popular Handbook. By LIEUT.-COL. PATRICK HEHIR, I.M.S., M.D., D.P.H., D.T.M. Third Edition, Revised and Illustrated. [Pp. ii + 1003.] (Madras: Higginbothams, Ltd., 1913. Price 6-8 Rs.)

COLONEL HEHIR, a distinguished officer of the Indian Medical Service, has written many useful works relating to the prevention of disease, medical administration, etc. His book under review (third edition) is stated to be a popular handbook, and should therefore be considered as such. This does not mean that the work is not worthy to be a book of reference for medical sanitarians. It is full of useful information; but the necessary table of detailed contents is not given, and the reader is obliged to rely mostly upon an index. The book is, however, divided into three sections, namely, General Hygiene, Personal Hygiene, and Diseases of India, and is very suitable for public instruction.

R. R.

BOOKS RECEIVED

(*Publishers are requested to notify prices*)

Theory of Functions of a Complex Variable. By Dr. Heinrich Burkhardt, O. Professor, Technical School, Munich. Authorised Translation from the Fourth German Edition, with the Addition of Figures and Exercises by S. E. Rasor, M.Sc., Professor of Mathematics, the Ohio State University. London: D. C. Heath & Co., 2 and 3, Portsmouth Street, Kingsway, W.C. (Pp. xiii, 432.) Price 12*s*. 6*d*. net.

Spectrum Analysis, applied to Biology and Medicine. By the late C. A. Macmunn, M.A., M.D., Author of "The Spectroscope in Medicine," etc., Articles in the "Encyclopædia Britannica" and "Quain's Dictionary of Medicine." With a Preface by F. W. Gamble. With Illustrations. Longmans, Green & Co., 39, Paternoster Row, London; Fourth Avenue and 30th Street, New York; Bombay, Calcutta, and Madras, 1914. (Pp. xiv, 112.) Price 5*s*. net.

I. K. Therapy, with Special Reference to Tuberculosis. By W. E. M. Armstrong, M.A., M.D., Dublin; Bacteriologist to the Central London Ophthalmic Hospital; Late Assistant in the Inoculation Department, St. Mary's Hospital, Paddington, W. London: H. K. Lewis, 136, Gower Street, W.C., 1914. (Pp. x, 83.) Price 5*s*. net.

Interpretations and Forecasts: a Study of Survivals and Tendencies in Contemporary Society. By Victor Branford, M.A., sometime Honorary Secretary of the Sociological Society. London: Duckworth & Co., 1914. (Pp. 410.) Price 7*s*. 6*d*. net.

Bio-Philosophy, or The Meaning of Comparative Physiology, Comparative Psychology, and Organic Evolution. By Joel N. Eno, A.M. Published by the Author. Printed by the New Haven Printing Company, New Haven, Conn., 1913. (Pp. 30.)

Researches into Induced Cell-Reproduction in Amœbæ. By John Westray Cropper, M.B., M.Sc., Liverpool, M.R.S.C. Eng., L.R.C.P. London, and Aubrey Howard Drew. With Illustrations. The John Howard McFadden Researches, vol. iv. London: John Murray, Albemarle Street, W. April, 1914. (Pp. 112.) Price 5*s*. net.

Plague and Pestilence in Literature and Art. By Raymond Crawfurd, M.A., M.D. Oxon., F.R.C.P., Fellow of King's College, London. Oxford: At the Clarendon Press, 1914. (Pp. viii, 222.) Illustrated. Price 12*s*. 6*d*. net.

The Quaternary Ice Age. By W. B. Wright, of the Geological Survey of Ireland. Macmillan & Co., Limited, St. Martin's Street, London, 1914. (Pp. xxiv, 464.) Illustrated. Price 17*s*. net.

Entomology, with Special Reference to its Biological and Economic Aspects. By Justus Watson Folsom, Sc.D. (Harvard), Assistant Professor of Entomology at the University of Illinois. Second Revised Edition with 4 Plates and 304 Text-Figures. Philadelphia: P. Blakiston's Son & Co., 1,012, Walnut Street, 1913. (Pp. vii, 402.)

Modern Problems of Biology. Lectures Delivered at the University of Jena, December 1912. By Charles Sedgwick Minot, LL.D. Yale, Toronto, and St. Andrews, D.Sc. Oxford; Director of the Anatomical Laboratories, Harvard Medical School; Exchange Professor at the Universities of Berlin and Jena, 1912–3. With 53 Illustrations. Philadelphia: P. Blakiston's Son & Co., 1012, Walnut Street, 1913. (Pp. viii, 124.)

The Principles of Biology. By J. T. Hamaker, Ph.D., Professor of Biology, Randolph-Macon Woman's College. With 267 Illustrations. Philadelphia: P. Blakiston's Son & Co., 1012, Walnut Street, 1913. (Pp. 459.)

X Rays. An Introduction to the Study of Röntgen Rays. By G. W. C. Kaye, B.A., D.Sc., Head of the Radium Department at the National Physical Laboratory, Examiner in Medical Physics for the Universities of London and Glasgow, Member of Council for the Röntgen Society. Longmans, Green & Co., 39, Paternoster Row, London; Fourth Avenue and 30th Street, New York; Bombay, Calcutta, and Madras, 1914. (Pp. xix, 252.) Price 5*s*. net.

Annual Magazine Subject-Index, 1913. A Subject-Index to a Selected List of American and English Periodicals and Society Publications not elsewhere Indexed. Edited by Frederick Winthrop Faxon, A.B. (Harvard). Compiled with the co-operation of Librarians. Boston: The Boston Book Company, 1914.

Of Spiritism, *i.e.* Hypnotic Telepathy and Phantasms—Their Danger. By the Hon. J. W. Harris. London: Francis Griffiths, 1913. (Pp. 126.) Price 2*s.* 6*d.*

The Simpler Natural Bases. By George Barger, M.A., D.Sc., formerly Fellow of King's College, Cambridge; Professor of Chemistry in the Royal Holloway College, University of London. Longmans, Green & Co., 39, Paternoster Row, London; New York, Bombay, and Calcutta, 1914. (Pp. viii, 215.) Price 6*s.* net.

Nucleic Acids: Their Chemical Properties and Physiological Conduct. By Walter Jones, Ph.D., Professor of Physiological Chemistry in the Johns Hopkins Medical School. Longmans, Green & Co., 39, Paternoster Row, London; New York, Bombay, and Calcutta, 1914. (Pp. viii, 118.) Price 3*s.* 6*d.* net.

The Internal Secretory Organs: Their Physiology and Pathology. By Prof. Dr. Artur Biedl, Vienna. With an Introductory Preface by Leonard Williams, M.D., M.R.C.P., Physician to the French Hospital, Assistant Physician to the Metropolitan Hospital. Translated by Linda Forster. London: John Bale, Sons & Danielsson, Ltd., Oxford House, 83–91, Great Titchfield Street, Oxford Street, W., 1913. (Pp. viii, 606.) Price 21*s.* net.

The Instinct of Workmanship, and the State of the Industrial Arts. By Thorstein Veblen, Author of "The Theory of the Leisure Class." New York: The Macmillan Company, 1914. (Pp. viii, 355.) Price 6*s.* 6*d.* net.

The School and College Atlas. 103 Maps, Physical, Political, and Commercial, Index. London: G. W. Bacon & Co., Ltd., 127, Strand, W.C.; 14, Union Court, Old Broad Street, E.C.

Preliminary Report on the Treatment of Pulmonary Tuberculosis with Tuberculin. By Noel D. Bardswell, M.D., Medical Superintendent. With a Prefatory Note by Prof. Karl Pearson, F.R.S., Director of the Galton Laboratory of Eugenics, University of London. Presented by the Medical Superintendent to the Council and published at the request of the Consulting Staff. London: H. K. Lewis, 136, Gower Street, W.C., 1914. (Pp. xviii, 142.) Price 6*s.* net.

Physiological Plant Anatomy. By Dr. G. Haberbandt, Professor in the University of Berlin (formerly in the University of Graz). Translated from the Fourth German Edition by Montagu Drummond, B.A., F.L.S., Lecturer in Plant Physiology, University of Glasgow. With 291 Figures in the Text. Macmillan & Co., Ltd., St. Martin's Street, London, 1914. (Pp. xv, 777.) Price 25*s.* net.

Kinship and Social Organisation. By W. H. R. Rivers, M.D., F.R.S., Fellow of St. John's College, Cambridge. London: Constable & Co., Ltd., 1914. (Pp. 96.) Price 2*s.* 6*d.* net.

An Introduction to the Study of Integral Equations. By Maxime Bôcher, B.A., Ph.D., Professor of Mathematics in Harvard University. Cambridge: at the University Press, second edition, 1914. (Cambridge Tracts in Mathematics and Mathematical Physics.) General Editors, J. G. Leathem, M.A., E. T. Whittaker, M.A., F.R.S. (Pp. 71.) Price 2s. 6*d.* net.

Some Minute Animal Parasites, or Unseen Foes in the Animal World. By H. B. Fantham, D.Sc. (Lond.), B.A. (Cantab.), A.R.C.S., F.Z.S., Lecturer in Parasitology, Liverpool School of Tropical Medicine, Formerly Assistant to the Quick Professor of Biology, Cambridge University, and Annie Porter, D.Sc. (Lond.), F.L.S., Beit Memorial Research Fellow, Quick Laboratory, Cambridge. With Frontispiece and 56 Text Figures. Methuen & Co., Ltd., 36, Essex Street, London, W.C. (Pp. xi, 319.) Price 5*s*. net.

A Text-book of Medical Entomology. By Walter Scott Patton, M.B. (Edin.), I.M.S., Membre Correspondant de la Société de Pathologie Exotique, King Institute of Preventive Medicine, Guindy, Madras ; Lately on Special Duty for the Investigation of Kala Azar in Madras, and of Oriental Sore in Cambay, and Francis William Cragg, M.D. (Edin.), I.M.S., Fellow of the Entomological Society of London, Central Research Institute, Kasauli, Punjab ; Lately Assistant to the Director, King Institute of Preventive Medicine. Christian Literature Society for India, London, Madras, and Calcutta, 1913. (Pp. xxxiii, 768.)

The Open Court Company, of 149, Strand, have just ready " Problems of Science," by Federigo Enriques, Professor in the University of Bologna. The authorised translation by Katherine Royce, with an introduction by Prof. Josiah Royce, of Harvard University.

The Johns Hopkins Press, Baltimore, Maryland, U.S.A., announce that the General Index to Vols. 21–50 of the American Chemical Journal is now ready for delivery. Price $1.50. The index can only be had by purchase. There have also been issued Indices to Vols. 1-10 and 11-20. Price $1.00.

INSTRUMENTS

Messrs. Prouds, Ltd., Electric Clock and Scientific Instrument Makers, 336, Kent Street, Sydney, N.S.W., announce a New Meteorological Instrument, namely, Murday's Thread-Recording Electrical MICRO-BAROMETER. The price of the Micro-Barometer, as described, with two rolls of paper (one year's supply), is £75. Extra rolls of diagram paper, 5*s*. 6*d*. each.

Messrs. Adam Hilger, Ltd., 75A, Camden Road, London, N.W., announce their SPECTROPHOTOMETERS for the Ultra-Violet, Visible, and Infra-Red Regions. Prices as per catalogue.

ANNOUNCEMENTS

The Sixth International Congress of Mining, Metallurgy, Engineering, and Economic Geology will be held in London from Monday, July 12, to Saturday, July 15, 1915. Secretary, 28, Victoria Street, London, S.W.

The International Congress of Neurology, Psychiatry, and Psychology will be held at Berne, Monday, 7th, to Saturday, 12th September, 1914. Secretary, Dr. L. Schnyder, 31, Rue Monbijou, Berne, Switzerland.

ANNOUNCEMENTS—*continued*:—

The Napier Tercentenary Celebration will be held in Edinburgh on Friday the 24th July and following days. General Secretary, Royal Society of Edinburgh, 22 George Street.

The *Morning Post*, 346 Strand, London, has published a large card giving a copious list of Congresses of Learned Societies and other bodies in 1914, 1915, etc.

Those willing to assist the Radiotelegraphic Investigation of the British Association are requested to write to Dr. W. Eccles, University College, London, W.C.

NOTICE

THE EMOLUMENTS OF SCIENTIFIC WORKERS

It is proposed to undertake an inquiry regarding the pay, position, tenure of appointments, and pensions of scientific workers and teachers in this country and the Colonies. The Editor will therefore be much obliged if all workers and teachers who hold such appointments, temporary or permanent, paid or unpaid, will give him the necessary information suggested below. The figures will be published only in a collective form, and without reference to the names of correspondents, unless they expressly wish their names to be published. The Editor reserves the right not to publish any facts communicated to him. Workers who are conducting unpaid private investigations must not be included. The required information should be sent as soon as possible, and should be placed under the following headings:

(1) Full name
(2) Date of birth. Whether married. Number of family living
(3) Qualifications, diplomas, and degrees
(4) Titles and honorary degrees
(5) Appointments held in the past
(6) Appointments now held, with actual salary, allowances, fees, and expected rises, if any. Whether work is whole time or not
(7) Body under which appointment is held
(8) Conditions and length of tenure
(9) Pension, if any, with conditions
(10) Insurance against injury, if any, paid by employers
(11) Family pensions, if any
(12) Remarks

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury.

SCIENCE AND THE STATE: A PROGRAMME

The action recently taken by Science Progress in calling attention to the sweating of science in this country has received much public attention, and many of our scientific contemporaries, especially *Nature*, have generally supported our remarks. The *Morning Post* opened its columns during May and June to a long discussion on the subject of Science and the State; and the British Science Guild has appointed a special committee to examine thoroughly into the matter.[1] It is a popular complaint against men of science that they never seem to know exactly what they want; and we therefore now propose to define exactly some of the steps which may be suggested for the betterment of science in Britain and elsewhere.

Suggested Programme

(1) Improved payment of scientific workers in Universities and other State-aided institutions, including:

- (*a*) Adjustment of the minimum salaries of the most junior workers;
- (*b*) Rises of salary depending upon length of service in scientific or academical work;
- (*c*) Adequate pensions;
- (*d*) Security of tenure and better organisation of efficiency.

(2) Special arrangements for stimulating research in Universities and other State-aided institutions, and for attracting and retaining distinguished investigators in them.

(3) The more careful regulation of selection for appointments, especially as regards the due consideration of distinction in research; and the placing of professorships upon a State-regulated standing.

(4) Abolition of the present method by which the State

[1] See p. 353.

often obtains expert evidence or temporary expert assistance at Commissions, Committees, Advisory Boards, etc., without payment or, sometimes, even the refunding of expenses.

(5) Payment of compensation by the State for proved pecuniary losses incurred by investigators in consequence of researches which have been unremunerative to themselves but of admitted benefit to the public or to Government Departments.

(6) Payment of special rewards or pensions by the State to investigators whose researches have been unremunerative to themselves but of pecuniary advantage to Government Departments, or of general advantage to the public at large.

(7) A higher place for science in national education.

Of course this programme does not by any means include the whole list of reforms which may be considered, but it will suffice for immediate discussion; and we will now proceed to examine the items in detail.

With regard to the first item in the programme, we have already called attention (in our last April number) to the very bad payment of scientific teachers and investigators in our universities—and some of the cases can be described only as "sweating" of the worst type. Beginning at the entry of newly graduated persons into the academic arena, we should first point out that the system of scholarships is really utilised as a kind of bait to induce young men into these unremunerative paths. A scholarship of from £150 to £250 a year may seem quite generous to a young graduate; and he commences his labours without thought of the future. Later on, however, he discovers that while he has been engaged upon the researches required by his scholarship his fellow students who were not so easily beguiled are perhaps already thriving in the practice of their profession, while he himself has lost time, and is behindhand in the race. As a result of this, and because he does not wish to waste his scientific experience, he next generally determines to devote himself to an academic career—with the result that he finds himself caught in the net and condemned for the rest of his life to the poor salary of a demonstrator, assistant, or lecturer, with only some possible chance of obtaining ultimately a badly paid professorship.

Regarding the actual pay of demonstrators, lecturers, and professors, a good idea can be obtained by any one who troubles to examine the advertisements of vacancies which appear in scientific and technical journals—and we have already mentioned some figures. Dr. W. Makower has made a study of this kind from the reports of the Board of Education for the year 1911–12, and has published his analysis in the following table:[1]

	Number of Professors.	Average Salary of Professors.	Number of Lecturers and Demonstrators.	Average Salary.
		£		£
Birmingham	12	704	33	195
Bristol	4	556	27	142
Newcastle	6	660	18	137
Leeds	10	661	32	164
Liverpool	6	853	28	126
Manchester	10	888	44	160
Sheffield	9	710	76	109
Nottingham	6	552	36	118
Southampton	4	325	24	82
University College, London	14	680	52	130
King's College, London	10	529	23	176
East London College	5	520	20	157
Aberystwyth	6	320	7	94
Bangor	5	498	8	107
Cardiff	5	440	8	158
Average		£628		£137

He explains that "the figures for the different institutions are not in all cases exactly comparable since the subjects are grouped differently in the reports from the different universities, but they relate, as far as possible, to the ordinary science subjects taught at the universities, and include data for mathematics and engineering, but not for the medical sciences. The figures taken from the reports include the salaries of 'part-time' professors and lecturers, but as the figures for these are not given separately it has been found necessary to include them in drawing averages." We think that the average salaries are likely in some cases to be based upon selected professorships, and therefore to be probably much over the correct figure; but according to the table the average salary of a professor in Britain is only £628 per annum, and that of lecturers and demonstrators only £137 per annum. We hear that in one British university, out of two hundred members of the junior

[1] *Morning Post*, June 4, 1914.

staff in all departments (that is all members of the teaching staff who are not full professors), not more than six receive a stipend greater than £250 a year! Some of the salaries given, especially for part-time work, amount to little over £1 a week—and this, be it remembered, often to senior persons who have spent a large sum of money in obtaining full degrees, besides special acquirements. It should be also remembered that rises of salary according to length of service are seldom arranged for, and that the pensions, if any, are extremely small and are mostly established upon a contributory basis. The proportion of assistants, lecturers, and demonstrators who ever become professors is not large; and, as a matter of fact, many of these persons end by drifting into mercantile laboratories, or return at a late age to the practice of various arts and professions—in which they are at a disadvantage compared with those who started in the same more lucrative lines of work at an earlier age.

Regarding the British professorships themselves, we should note that many of them are temporary or assistant professorships, tenable only for a short term of years, after which the holders may be cast loose without any donation or pension. Even in the case of full professorships, superannuation at the age of sixty-five, with a pension amounting to from £100 to £200 is often insisted upon; while in many cases the professor must be re-elected every few years. As we have pointed out, such scales of payment compare most unfavourably with the emoluments of work in nearly all other professions. Thus, Government employment is generally associated with a bonus on retirement at an early age, and a pension on retirement at a later one. The highest salaries and pensions in academical life are extremely small compared with those given in military service or the law—and the position is also comparatively lower. In Germany, most of the professorships are held for life, with pensions varying from 40 per cent. to 100 per cent. of salary, according to length of service—so that an aged professor can often retire upon his full salary when he is no longer capable of performing the duties of his chair. Nothing like this appears to hold in Britain, where an old man is often driven out of his post on payment which is less than that of many an enterprising chauffeur. We should add that cases have occurred in which professors have been obliged to leave their universities after fifteen or more years of service without any pension whatever, and that without fault or disability on their part.

Such treatment of the best educated and sometimes some of the most valuable men in the community is a gross scandal. It seems to be due principally to the fact that the universities utilise additional funds which they may receive from private subscriptions and from the Board of Education, not for consolidating and improving the position of their staffs, but for starting new lectureships and professorships and for building magnificent new structures in order to obtain *réclâme.* In some cases, the only persons who seem ever to enjoy increase of salary and adequate pensions are, not the working teachers and investigators, but the persons who hold what are called administrative posts, and who seem often to be selected according to some extraordinary principle which ignores distinguished past work as a recommendation.

A little while ago, during the preliminary discussions of the Universities and Colleges with the Advisory Committee of the Board of Education as to the possibility of establishing a federated superannuation system, the appointment of a small committee to discuss the details was suggested; and this committee issued in June 1913 a "Federated Superannuation System for Universities." But the system is really very little better than an ordinary insurance scheme, to which the universities do not appear to subscribe as much as they ought to do. A member of the system can scarcely expect to obtain a pension of more than £200 a year on payment of premiums amounting to one-tenth of his income, starting at an early age, and the pensions do not appear to be guaranteed in any way by the State or by the universities themselves.

It seems to us that the proper way to obtain reform regarding the whole of item 1 of the programme is for the Board of Education to insist, as a condition of their grants to the universities and other State-aided institutions, that such bodies shall consider it to be one of the first charges upon their income to make provision for adequate rises of salary and pension on retirement for all members of their staff. Such action will check the waste of funds just complained of upon buildings and unnecessary new ventures, and will set the pace for all institutions employing scientific workers. We should remember that many of the British universities are managed by business men, who adopt commercial methods for making as big a show as possible out of their receipts, and succeed in this chiefly by sweating their scientific and learned employees in the manner described; and we may add in passing that any reform in this respect should include a very strict reform as to the persons who are allowed to obtain positions upon the councils of these universities, and who at present are frequently men who have not even benefited their institutions by considerable pecuniary grants, much less distinguished themselves by any achievements in science and art.

Regarding the very difficult subject of security of tenure—1 (*d*) of the programme—we had better merely remark at present that it implies an adjustment between two different interests, namely that of the university or institution and that of the individual. It is certainly the case that many individuals who find themselves in too secure a position thereupon cease to do good work; but, on the other hand, the absence of security tends to impair the working efficiency of the individual. Probably the best way is to retain more power for the discharge of members of the staff on condition that such power can be exercised only on compensation by means of a bonus or pension graduated according to length of past service. Thus a university should be able to insist upon superannuation in consequence of failure of powers at any given age, but should not be allowed to do so without giving suitable compensation as defined.

Another principle should be held in mind. Most teachers and workers at the universities are at present often obliged to shift from one institution to another, because, in fact, academical life is becoming a kind of general public service. At present such shifts frequently imply a loss of seniority; and arrangements should be made to avoid this. Indeed, the Federated Superannuation System for Universities just referred to has already considered the point; and the question arises whether the whole of the academical profession should not be converted into a Government service, or at least into a service which is very carefully controlled by Government. Until something like this is done, cases of exploitation or hardship are likely to continue.

Prof. Soddy has suggested a scheme, well worthy of attention, for directly stimulating research in university departments by increasing the grants for each department *pro rata* according to the research work done in them. There are of course difficulties to be met, but these are probably not insuperable; and something of the nature should be considered at an early date. We are all of us familiar with certain departments in which no research at all is done, and also, fortunately, with those which distinguish themselves greatly by it; and it is not fair that payment should be on the same scale in both. Of course, in many departments research is not obligatory at all; and we would suggest that where it is made obligatory it should certainly be specially paid for in addition to the payments made for teaching by itself. Otherwise such proposals will merely result in further sweating.

The suggestion under heading 3 of the programme is a difficult but weighty one. It can properly be met only by attention to the suggestions already made regarding State-supervision of the universities. In most countries on the continent of Europe a professor is an important person, because he is supposed to possess the most detailed knowledge possible of his subject. In Britain and America, however, a university professor appears to be ranked with "professors" of boxing and dancing, and to be looked upon with some indulgent contempt. At the same time, little care is exercised with us in the selection of men for these posts—which are often given to local candidates of not much account, or to any person who possesses the *savoir faire* to apply himself to the proper persons. The whole system of election is faulty—as was well pointed out in a paper published some years ago in the *University Review*. At present vacancies are generally advertised in the press, and applicants are told to send in testimonials, however important the post may be. Thus a number of applicants are kept on tenterhooks of doubt for months, and are also obliged to tout amongst eminent men for testimonials, and to go to the expense of printing them afterwards. As only one of the candidates obtains the coveted post, this amounts to quite a serious infliction; and it is time that the system is changed for appointment by invitation, supervised by some censorship by, say, the Board of Education. Indeed, we think that the whole status of the professor should be raised by placing every professorship upon something like a *regius* standing, with a selection ultimately approved by Government, and fixity of tenure guarded by a right of compensated removal upon failure of function. Temporary professorships are becoming an abuse, inasmuch as they are really something of a pretence on the part of a university that it has a larger staff of professors than it really possesses, and also because the name tends to deceive candidates as to the nature of the appointments.

One notes with regret how seldom in British administration the most important investigations lead to the highest appointments. Thus many professorships are given, not so much for discoveries made, as for the compilation of successful textbooks, or even for attendance on committees or for good after-dinner speaking! At least, there is some general feeling that this is the case, though, of course, proof is always difficult—and the same charge lies with regard to the very highest university posts open to workers in science and art. Genius and achievement seem to have small claims on election committees in comparison with the less important arts of life; and many cases are known in which men who have actually made a new line of work have been excluded in favour of some nobody who never has done and never will do anything at all. Undoubtedly, here, a spirit of jealousy often enters into the election—especially when the electors themselves have been chosen rather by personal influence than according to achievement; and this adds one more reason to the contention that the State should strictly supervise such matters in future. The same defect, however, is apparent outside university life, and even in many Government appointments—in which the selections sometimes made fill experts with amazement. It is of course impossible to furnish instances without giving offence; but such instances are much too clear and common. Large scientific departments of Government have been known to be directed for years by series of men not one of whom has really ever shown distinction in scientific work; and, indeed, it is often declared that scientific ability is a direct impediment to advancement. This appears to be based upon the absurd pretence that a man of scientific ability is not good at administration. The truth is that if a man is capable of doing high scientific work he is almost certainly capable of the much easier arts of administration; and the case is rather that authorities do not find men of distinction to be sufficiently subservient to them and their frequently out-of-date notions. Achievement is put upon one side, and the world is astonished to find the prize of a service given to some absolutely unknown person who has merely pleased by his personal address, and who is known to be one who possesses not a single ideal or idea.

The frequency with which departments of State obtain expert scientific evidence or temporary expert assistance at commissions, committees, advisory boards, etc., has been already referred to in our number for April last (page 605). We said: "For example, a Government department wishes for expert advice on some matter—it ought to form a commission of its own, and honestly pay the expert members of it. Instead of doing this the Government department goes to some learned society and asks it to advise on the scientific question at issue. The society is honoured by the request, and obtains the advice gratis from its own members. Thus the Government gets what it requires for nothing, the learned body is overpowered with the honour rendered to it, and the unfortunate worker is the loser." Frequently Government departments appoint unpaid experts on their committees without the smallest apparent feeling of honesty, and the experts give their services under a sense of patriotism. Of course, officials say that this matter is ruled by the custom of the market; but such excuses do not absolve them, because they are really exploiting an honourable sense of devotion to duty in the expert. The country is quite rich enough to pay such men, and we are especially glad to see that the British Science Guild has taken up the matter, and not less to learn that several high officials are in favour of a change being made. We shall therefore say no more under this heading at the moment.

In order to understand items 5 and 6 of the programme, we should classify the different kinds of research now being done. In the first place, many investigations are the result of directly paid employment *ad hoc*. A second class, such as many researches in physics, chemistry, and engineering, lead to profit by way of patentable articles. A third class, especially medical and surgical investigations, give indirect but sometimes very large remuneration by enhancement of practice. On the other hand, it must be clearly understood that many discoveries, and indeed some of the most important discoveries, are not paid for, and bring no remuneration at all, while indeed some cause direct pecuniary loss, either for apparatus or assistants or by loss of time which might be spent more lucratively in practice—a consideration applying especially to professional men such as doctors and engineers. Here, too, we must remember that very few of the teaching staffs of our universities are specifically employed for investigation in addition to teaching. Some think that investigation is nevertheless a part of the duties of a professor; but unless this is laid down directly in the contract, such a view is merely a pious opinion, and as a matter of fact many teachers do no investigation at all, their time being fully taken up with their teaching. We thus see that on the whole a very small part of the research work done is remunerative to the worker in this country.

Scientific discoveries may or may not be directly advantageous to the public or the State, though they always increase the stock of human knowledge. For example, we could hardly claim that physical astronomy, or studies on the constitution of the atom, or some physiological work, confer such benefits on humanity as can be directly measured in terms of pounds, shillings, and pence. On the other hand, a large number of discoveries are so measurable, either in the advantage given to the public at large, or to their governments, or by direct saving of pecuniary loss to the latter. We may instance here such cases as the recent improvements in tropical medicine and sanitation, which reduce the sick-rate amongst the people in the tropics, and also save governments considerable expenditure formerly due to the death and invaliding of officials and soldiers. Edward Jenner's discovery of vaccination for the prevention of small-pox is another very good example—and it is easy to think of many more.

On comparing these points we shall see that all discoveries may be classed into groups, according to whether they have or have not been remunerative to the workers, and at the same time have or have not been of direct measurable benefit to humanity or to governments. Men of science may therefore quite justifiably claim that if their discoveries have been of measurable benefit to the State, but not to themselves, the State should make some effort to reward or compensate them. This is especially the case when such discoveries have involved direct pecuniary loss, either as regards apparatus and assistants or loss of professional time. We maintain that in such cases it is an obvious duty, and indeed should be a point of honour, for the State to pay compensation. Apart from this ethical consideration, the State should see that its interest lies in doing so, if only to encourage such individual work in the future. Of course it may be doubted whether men who have been directly paid for their researches possess any claim for such remuneration; but even in such cases, where a Government obtains immense pecuniary advantage from any researches made by private individuals, it seems to us that a certain moral claim lies for some pecuniary recognition in return. But the case is very much more clear where researches have been totally unremunerative to the worker, or, indeed, have occasioned him direct pecuniary loss. Indeed, Parliament fully recognised this obligation when it gave Edward Jenner the sum of £30,000 in 1802 and 1805 as a result of his petition. That precedent still holds to-day, and the moral obligation will never be abrogated. These cases are therefore provided for directly in items 5 and 6 of the programme.

Many of those who have written on this subject complain that the defects in our present organisation for science are due to the general attitude of the public, which appears to take little interest in science and scientific work. Others maintain that this attitude is due largely to our faulty methods of education, which instil into children, not the laborious pursuits of science, but rather grammatical, dialectical, historical, political, and literary ambitions. There is much truth in this, but a consideration of the details would be out of place here. Suffice it to say that the eyes of British education seem to be too constantly fixed upon the past rather than upon the future, and that this tends to give the whole intellectual attitude of the nation a backward aspect. A knowledge of the facts of nature, ascertained by centuries of effort, forms the best groundwork for advance. At present there is some truth in the epigram that the one ideal of the British schoolmaster seems to be to develop in his scholars only the arts of literary criticism and party politics. There are loftier fields of work, but we can reach them only through the hard-won and difficult passes of true science.

> To the solid ground
> Of nature trusts the mind which builds for aye.

That there is something wrong with the whole state of science in Britain is admitted by nearly every one. The fault has lain largely with men of science themselves, who fail to claim for their work the position which it should hold in the thoughts of the nation. Our learned societies seem to be concerned with little else than providing places for the reading and printing of scientific articles, or occasionally supplying some small funds for working expenses. But really the only way to encourage science and art is to encourage the men who make them; and here men of science themselves take no worthy stand for betterment. They let things drift. They content themselves too often with talking futilities about the high aims of science and the glory of being allowed to do scientific work for nothing. All this is an attitude of fakirism—the lofty but vain unpracticalness which leads to nothing but airy dreams combined with beggary. In this world, for science as for every movement, straight thinking and hard pushing are the only things which win.

The future lies with the younger men. They should see to it that some kind of corporate body is formed to do more than give opportunities for work—to attend to the interests of the workers themselves, and to help those who are climbing the precipitous heights of knowledge. They may allow the beggar to sit still dreaming by the roadside, but themselves should advance, staff in hand, to the more arduous task. With them lies, not only their own future, but the future of their country. We do not—and perhaps the whole world does not—fully recognise the great principle that of all forms of human effort, those efforts which result directly in discovery, whether in science or in art, are by far the most important efforts for humanity; greater than the hunt for gold, the petty cries of party politics, or the shouts of small nations at war. Discovery is not for one time or one people, but for all time and the whole world. It is the duty of men of science not only to make discoveries themselves, but to see that everything is in train to encourage discovery in others.

SOME LOGICAL IMPOSSIBILITIES

By CHARLES A. MERCIER, M.D., F.R.C.P.

"THERE are impossibilities logical, but none natural." This quotation from Huxley in Mr. Hill's article in the April number of SCIENCE PROGRESS is a capital instance of the equivocation of terms. There are logical impossibilities and logical impossibilities. Some, such as a round square or a present past, are truly impossible; but there is a huge class of "logical impossibilities" that are impossibilities only in the sense that logicians declare that they are impossible, but that are, in fact, surmountable with ease, not only by the non-logical public, but also by logicians themselves when they forget, as they often do, that they have declared them impossible. It was said by the late Lord Salisbury that certain politicians seem to live in a balloon, so oblivious are they of the actual state of affairs with which they have to cope. Logicians may be said in this sense to live in a balloon; for not only do they profess ignorance of the ways in which other people are accustomed to perform their reasonings, but also they declare that these ways are impossible. It is impossible, they say, to walk, for are we not cooped up in a little basket, in which there is no room to use the legs? In vain we, who are not logicians, point out that every one does not live in a balloon, and that the earth beneath is quite large enough to walk about on. The answer of the logician is conclusive. My balloon, he says, is the only means of locomotion; it is the only way of getting from place to place; and any one who attempts any other mode of locomotion will immediately lose his way, and be bogged in the slough of fallacy. It makes not the slightest impression on him that no one in practice adopts his means of locomotion, and that he himself, when he is out of his balloon—his book on Logic—uses his legs and walks about like other people. He still holds, and asserts with the confidence of infallibility, that it is impossible to get from place to place except by balloon—that it is impossible to get from premisses to conclusion except by the methods of Logic.

If any one else should assert that a thing is impossible, the doing of that thing before his eyes would at any rate raise doubts in his mind whether it was really impossible; but the logician is unmoved. He wraps himself in his mackintosh of logical doctrine, and the rain of facts pours off him like water off a duck's back. Traditional Logic has many professors, but few students. It has many professors because the bounty of past ages, when Logic was the most important of the three subjects that alone entered into a liberal education, has endowed many professorial chairs of Logic. That it has few students is due to the general appreciation of its uselessness; but this appreciation is vague, and is due, not to any analysis or exposure of the futility of Logic, but to the cumulative effect of many indications, more felt than explicitly acknowledged, among them the following:

Every one who has ever looked into a book of Logic says to himself, This is all very well: these simple arguments are valid, no doubt, but what piffle they are! They will do very well for the nursery, but when am I coming to the important methods of reasoning? These are not the kinds of argument that men use in real life: when am I coming to the methods men do use? But he never comes to them, or if he does find some of them in the chapter on Induction, he finds them explained in a way that thoroughly puzzles him, and does not convince him in the least. Those who have learnt Logic, and especially those who teach Logic, are not better reasoners, nor have they greater mastery of clear statement, than others who know nothing of Logic: on the contrary, taken as a whole, the arguments of logicians are more wanting in cogency, and their statements are more confused. So wanting are their arguments in cogency, that much space in every book on Logic is occupied in exposing the fallacies perpetrated in other books on Logic. So wanting are their statements in clearness, that the two professors of Logic who are most followed at the present time, and who are credited with the greatest profundity of original thought, write so abominably that they are always difficult to understand, and sometimes completely unintelligible. The business of Parliament and of Courts of Law is carried on mainly by argument, but no form of argument that is taught in Logic is ever used in a Court of Law or in Parliament; and if any one were to use in any of these Courts a "logical" argument, he would be laughed out of Court. In every other walk of life any difficult point is submitted to an expert, and his judgment is taken; but who ever heard of a professor of Logic being consulted as to the cogency or validity of an argument? Men of science are arguing with one another all the year round, and many of them are attached to universities, and must sometimes come into contact with professors of Logic—cannot at any rate be ignorant that professors of Logic exist; but when did ever a controversialist in any other branch of science appeal to a professor of Logic to decide on the validity of an argument? These, it seems to me, are the considerations inarticulately and unavowedly present in the minds of those—the vast majority of educated men—who ignore the existence of Logic; who, if they have learnt it, make haste to forget it the moment they shut their text-book or leave their class-rooms; and if they have not learnt it, feel no curiosity about a subject which has produced no visible beneficial effect on the minds of those who have.

It is well that Logic has few students; for, if we may judge by its professors, it does not conduce either to skill in reasoning or to clearness of exposition; and it certainly produces in all who have learnt it a despairing helplessness in the face of difficulties that to the non-logical mind are not difficulties at all, but to the logician are "logical impossibilities." Far be it from me to say that logicians are impostors. Those who are known to me are high-minded men and my very good friends; and I know full well that they believe as implicitly in the fads and the formulæ, the prohibitions and impossibilities, of their pseudo-science as any Christian Scientist, or astrologer, or flat-earth crank, or circle-squarer of them all; but that traditional Logic is an imposture I hope to satisfy my readers before I have done.

I quite realise the gravity of this accusation, and am fully alive to its *primâ-facie* improbability. The science of Logic was founded by a man who stands for all time as the embodiment and great exemplar of reasoning power; of whom it was said by Averroes that "his doctrine is the perfection of truth, and his understanding attained the utmost limit of human ability: so that it might truly be said of him that he was created and given to the world by Divine Providence that we might see in him how much it is possible for men to know." This is very much the opinion that prevails to this day in Oxford and among logicians the world over; and Aristotle's system of Logic has been accepted as impeccable for two thousand years. It is true that there have been a very few sceptics and scoffers, from Sextus Empiricus in the third century A.D. to Peter Ramus and Sennertius in the sixteenth; but they had no followers, and their influence was neglectable. If a consensus of authority could establish the truth of any doctrine, no doubt the truth of traditional Logic would be unquestionable; but a consensus of authority alone, even of two thousand years of practical unanimity, is not enough to establish the truth of a doctrine. There is conclusive proof that it is not. Not for two thousand years only, but for six thousand years, and not the Western nations only, but also the highly intellectual civilisations of India and China, accepted as unquestionably true the doctrines of judicial astrology. As in the case of Logic, these doctrines were from time to time questioned by isolated sceptics, such as Aristarchus of Samos, Martianus Capella, Favorinus, Juvenal, and others, but, as in the case of Logic, these exceptional protests carried no weight and exercised no influence. Down to the time of Milton, who often uses astrological phraseology, and even down to Dryden, himself a convinced astrologer, the doctrines of judicial astrology were accepted with practical unanimity. The belief in witchcraft was even more ancient and more widespread, yet who believes in it now? It is difficult now to find any adherent either of witchcraft or judicial astrology, and at some future time it will be difficult to find any one who is interested in the doctrines of Logic, except as a matter of historical curiosity. How far off this future may be it is impossible to say, but I hope to do something to hasten its advent.

The impossibilities of Logic are many, and to cover the whole field, to surmount them all, and to show that every one is merely a bogey, requires a volume of considerable size. Such a book I have written, and no logician has ventured to deny that I have achieved his impossibilities. I select here but a few, and I take them from the central and most important doctrine of Logic, the doctrine of the syllogism.

The central doctrine of Logic is that there is only one mode of reasoning or inference, only one way in which we can reason from premisses to a conclusion, and that this is by means of the syllogism. There are, it is true, certain minor processes, called immediate inferences, but these are so simple that many logicians deny to them the title of inference, and are so seldom used that they may, for practical purposes, be neglected. They are open, however, to objections just as damaging as those I shall make to the syllogism itself, which I now proceed to examine.

The model syllogism, as given in every book on Logic, is as follows:

I. All men are mortal,
Socrates is a man;
Therefore Socrates is mortal.

The impossibilities with which the syllogism is fenced about are to be found in its rules, which are eight in number. I will take them seriatim.

The first rule is that it is impossible to construct a syllogism with more or fewer than three propositions—two premisses and a conclusion—as in the model given above. This rule must be read in connection with the doctrine that the syllogism is the only form of mediate inference; and when so read, it means that it is impossible to construct any argument with more than three propositions, or any argument except the four immediate inferences with less than three propositions. In fact, nothing is easier than to argue from three or four premisses, and so to construct an argument with at least four propositions; moreover, the same premisses will often yield two or three conclusions, and thus the whole argument may easily contain double the number of propositions that Logic allows. For instance:

II. If Some of the flowers in this bed are geraniums,
and Others are calceolarias,
and Others are stocks,
and The rest are lobelias;
then The bed contains no begonias,
and It contains no violas.

Nor is it difficult to construct an argument which is not one of the immediate inferences known to Logic, and which yet contains but two propositions, thus:

III. If The bed contains nothing but geraniums and violas,
then It contains no asters.

The second rule declares that it is impossible to construct a syllogism with more or fewer than three terms. This must

15

be interpreted together with the assumption that the syllogism is the only mode of argument. It will be seen that argument II. contains no fewer than ten terms—some of the flowers, others of the flowers, the rest of the flowers, the flower-bed, geraniums, calceolarias, stocks, lobelias, begonias, and violas.

The third rule of the syllogism is that it is impossible to construct a valid argument unless the middle term is distributed in at least one of the premisses. To render this rule intelligible it is necessary to explain. According to traditional Logic, the conclusion of an argument must contain two terms and no more. One of these terms must appear in each premiss, and the premisses must be completed by another term, the middle term, which must be the same in both premisses. Thus, in the model syllogism (I.) the terms in the conclusion are Socrates and mortal, and the middle term, which appears in each premiss but not in the conclusion, is man. The third rule provides that in at least one premiss the middle term should be "distributed," by which is meant that it must include or refer to the whole of a class. In the model syllogism, the middle term in the first premiss is "All men," which really means "every man," and conforms to the rule by including the whole class of men. In the second premiss the middle term is "a man." It is evident, therefore, that the third rule comprises several rules, and declares several impossibilities, as follows:

1. It is impossible to construct an argument without a middle term.

2. It is impossible to construct an argument unless the middle term appears in both premisses.

3. It is impossible to construct an argument unless the middle term, in at least one premiss, expresses or refers to the whole of a class.

4. It is impossible to construct an argument in which the middle term appears in the conclusion.

Let us now perform these impossibilities.

1. It is impossible to construct an argument without a middle term. Both the arguments II. and III. achieve this impossibility, and so does the following:

IV. If His hands were tied behind him,
then He could not wipe his nose.

No doubt a logician will object that as this argument has only two propositions, it is no argument. Let us therefore try one with three:

V. If There was a little man,
and He had a little gun;
then He was a match for a big man without a gun.

Again logicians may object that since the little man appears in both premisses, he is the middle term. I don't know how they would reconcile this with his appearance in the conclusion, but to meet the objection let us try again:

VI. If Little Bo-Peep has lost her sheep,
and Little Boy Blue let the cow into the corn;
then They were both inefficient,
and They ought both to be smacked.

2. It is impossible to construct an argument unless the middle term appears in both premisses. This impossibility is of course achieved in the preceding arguments which contain no middle term.

3. It is impossible to construct a valid argument unless the middle term, in at least one premiss, expresses or refers to the whole of a class, or, in other words, is "distributed." Let us try:

VII. If Hannibal crossed the Alps,
and The part of the Alps that he crossed is impassable for elephants;
then He took no elephants across with him.

It would appear to the uninitiated that this argument conforms to rule, for the whole class of the Alps is mentioned in the first premiss; but in Logic this does not count, for the Alps is the predicate of an affirmative proposition, and therefore, although we do in fact refer to the whole class of the Alps, the convention of Logic requires us to suppose that we do not. The argument is therefore invalid in Logic, though it is convincing enough to any one who is not a logician. Besides, the argument is quite irregular, and not "logical" for other reasons. For one thing, the term in the minor premiss, "impassable for elephants," does not appear in the conclusion. Let us try again.

VIII. If Many soldiers fought,
and Few soldiers were killed;
then Some soldiers survived.

This seems to the non-logical mind a good, sound, valid argument, but the non-logical mind is mistaken. It is not only not a valid argument, but it is not an argument at all, and this for so many reasons that it would be tedious to enumerate them all. In the first place, not one of these pseudo-propositions is a "logical" proposition. No "logical" proposition can be constructed except with the verb "is" or "are," and none of these propositions contains either of these. This is bad, but this is not the worst; for though we may grant, as every logician does in practice grant, though he strictly denies it in principle, that propositions can be formed with other verbs than "is" and "are," yet neither in practice nor in principle will any logician allow for a moment that any argument can be conducted, or any "logical" proposition be constructed, with such quantities as "many" and "few." Logic allows that it is possible, and even meritorious, to affirm, deny, and argue about All soldiers, about Some soldiers, and about No soldiers; but to reason about Many soldiers or Few soldiers, or even to make any statement about them, is among the many impossibilities of Logic. This seems a little unreasonable, but let us play the game according to logical rules, and admit into our premisses no quantity but the logical "Some."

IX. If Some logicians admit the verb "were" into their propositions,
and Some do not;
then Logicians are divided in opinion about "were."

Here we have avoided Scylla only to be swamped in Charybdis. We have got the orthodox "quantities" into the premisses, but alas! in our conclusion the term "Logicians" is not preceded by either of the orthodox quantities, All, Some, and None. It is indesignate, and with an indesignate proposition no "logical" argument can possibly be constructed.

4. It is impossible to construct an argument in which the middle term appears in the conclusion. It is possible that this rule is not imperative. It has never, I believe, been actually formulated, but for two thousand years every "logical" argument has conformed to it. Lately, however, one logician, greatly daring, has allowed his middle term to appear occasionally in his conclusions; but as far as I know, no other logician has ventured to adopt this revolutionary innovation. There are,

however, arguments in which it is very useful to get the middle term into the conclusion. Here is one:

X. If Cherries will grow on calcareous soil,
and Rhododendrons will not grow on calcareous soil;
then Calcareous soil is not equally suitable for all plants.

And here is another:

XI. If Logicians may not argue about Many,
and Logicians may not argue about Few;
then There are two useful quantities forbidden to logicians.

So much for the third rule of the syllogism. The fourth rule declares that it is impossible for a term to be "distributed" in the conclusion unless that term is "distributed" in a premiss. Unless, therefore, the term in the premiss refers to or includes the whole of the class, that term must not include or refer to the whole class in the conclusion. I say that

XII. If Some of the crew manned the jolly boat,
and Others of the crew manned the long boat;
then The whole of the crew were enough to man both these boats.

And I say that I have got the whole class of the crew in the conclusion although part only of this class was in any premiss; but here again I run up against such a multitude of logical impossibilities that I am fairly ashamed of myself. For reasons already given, the propositions are none of them "logical." In Logic they are impossible. It is as impossible in Logic to reason about "Others" as about "Many" and "Few." "The whole of the crew" is not a "logical" term, and cannot enter into a "logical" argument. You may argue, indeed, about "All the crew," but only if you mean by all the crew every one of the crew taken separately. All the crew may mean every one of the crew, or it may mean the whole of the crew taken together. It is one of those nice, ambiguous, confusing terms that reflect so faithfully the confusion of "logical" doctrine, and therefore it is always used by logicians, although there are precise unambiguous terms ready to hand and begging to be used. Now, in this argument the term in the conclusion means the whole of the crew taken together, and as the term is not a "logical" term, the proposition is not a "logical" proposition, and the argument is not a "logical" argument. Why this should be so, Logic does not explain. If it were obliged to explain, I suppose it would say that term, proposition, and argument are all inconsistent with Aristotle's teaching, and therefore must be wrong.

The fifth rule of the syllogism is that it is impossible to draw any conclusion from two negative premisses. Unless one premiss is affirmative, there can be no conclusion. Thus I achieve the impossibility.

XIII. If No one ever reasons by "logical" methods,
and No one always reasons erroneously;
then It is quite possible to reason correctly by non-logical methods.

Of course, logicians will say that since the conclusion says nothing about either All, Some, or None, the reasoning is not reasoning and the conclusion is no conclusion; but the non-logical reasoner will not much mind this fulmination, for he will see, first, that the conclusion is in fact a good sound proposition, and second, that it follows from the premisses. However, to obviate the logical objection, I will put my conclusion into another form, and say, Some non-logical reasonings are correct. This, however, will not satisfy my logical friends. They will say that since the premisses are not constructed with the verb "is" or "are," the propositions are not propositions, etc. It is as difficult to keep within the narrow boundaries of Logic as to walk on a tight rope, but I will try again:

XIV. If No argument from two negative premisses is valid,
and No argument from two particular premisses is valid;
then No argument from two particular negative premisses is valid,
and All arguments from particular premisses resemble all arguments from negative premisses in being invalid.

This specimen is extremely interesting in several ways. In the first place, a single pair of premisses yields two conclusions, and might easily be made to yield more, a fertility which is unknown to the arguments of Logic. In the second place, the conclusions are highly favourable to the pretensions of Logic; but in the third place, Logic is unfortunately forbidden by its own rules to admit that the conclusions are valid. In the fourth place, the argument is unique in the remarkable feature that the attainment of a conclusion is a flat contradiction of the first premiss; and in the fifth place, in spite of this contradiction, the argument is unquestionably valid. Such are the remarkable consequences of "logical" rules.

The sixth rule of the syllogism declares two impossibilities, which are as easily surmountable as the other impossibilities of Logic. This rule declares that it is impossible to draw an affirmative conclusion if one of the premisses is negative, or a negative conclusion if both the premisses are affirmative. The first impossibility has already been surmounted in arguments IX. and XI., and the second by arguments II. and VII. The following also are to the point:

Affirmative conclusion from premisses, one of which is negative.

XV. If Some Scotchmen play the bagpipe,
and No Englishman plays the bagpipe;
then Some Scotchmen do what no Englishman does.

It is easy to do better than this, however, for we can draw an affirmative conclusion from premisses both of which are negative, thus:

XVI. If Not a drum was heard,
and Not a funeral note;
then All the bands were silent.

Negative conclusion from affirmative premisses:

XVII. If Most of them were drowned,
and The rest died of exposure;
then None of them survived.

This argument employs two quantities, Most and The Rest, which Logic does not recognise. I do not see why we should bind ourselves in the fetters that Logic loads its votaries with, but to leave logicians the less excuse, I offer the following argument:

XVIII. If Some of them travelled by express,
and Some followed by slow train;
then All of them did not arrive together.

Here again, in the conclusion, I have employed a non-logical quantity. Logic knows of no negative but "None are" and "Some are not," and here I conclude that All did not. To arrive at any conclusion about what all did not do is another "logical" impossibility; but, as the reader will have realised by this time, nearly every mode of statement or argument that is of any use is "logically" impossible.

The seventh rule of the syllogism is that it is impossible to draw any conclusion from premisses both of which are particular; and, as already explained, by a particular premiss is meant a statement about "Some," or part of a class. Nearly all the arguments already given achieve this impossibility, and so does the following:

XIX.	If	Some of the army were infantry,	
	and	Some of the army were cavalry;	
	then	The army consisted of at least two arms,	(Affirmative.)
	and	The army did not consist wholly of infantry.	(Negative.)

The eighth and last rule of the syllogism proclaims the impossibility of reaching a universal conclusion (that is, a conclusion about the whole of a class) if one premiss is particular (that is, refers to part only of a class). This impossibility is achieved by the arguments II., III., VI., IX., XII., XVII., XVIII., and XIX., so that it is unnecessary to give another; but to complete the tale I offer the following argument:

If	Some of them are infantry,
and	Others are cavalry,
and	Others are artillery,
and	The rest are naval officers;
then	None of them is a civilian.

This argument breaks the first rule of the syllogism, for it contains more than three propositions; it breaks the second, for it contains more than three terms; it breaks the third rule in several pieces, for it contains no middle term, and no term in the premisses is distributed; it breaks the fourth rule, for the conclusion contains a term, "None of them," which is distributed, though the term "them" is not distributed in any premiss; it breaks the sixth rule, for the conclusion is negative, though every premiss is affirmative; it breaks the seventh rule, for it reaches a conclusion from premisses every one of which is "particular"—refers to part only of a class; and it breaks the eighth rule, for it reaches a universal conclusion—a conclusion about the whole of a class—from premisses of which, not one only, but all are particular. The only rule it does not break is the fifth, which forbids us to draw a conclusion from two negative premisses. Yet this argument, which is in flat violation of seven out of the eight rules of the syllogism, is undeniably and incontestably valid. In five lines, in one-and-twenty words, it performs seven impossibilities.

Any one who is unacquainted with logicians and their ways would suppose that when their doctrines had been shown to be *primâ facie* false, logicians would find some answer to the charge, or would modify their doctrines; but any one who would make such a supposition knows little of Logic or of logicians. In *A New Logic* I have surveyed the whole field of Logic, have examined every one of its doctrines, and have shown that every one of them *primâ facie* requires justification as much as the doctrine of the syllogism. These demonstrations have been before logicians for three years. The book has been sent for review to a professor of Logic in nearly every university in the kingdom, and one and all have declined to take any notice of it. One or two minor logical luminaries have denied in general terms my accusations, but no one has examined my instances or tried to refute them. The only logician who has examined my charges in detail is Miss E. E. C. Jones, the Principal of Girton, and she admits that some of them, at any rate, are incontrovertible. It remains on record that for three years the whole fabric of traditional Logic has been accused, in general and in detail, of error and inadequacy, and that for three years the charges have remained unanswered. It is nearly time that judgment should go by default.

I do not deny that, if the presumptions of Logic were true, something might be said for its rules and its impossibilities. If it were true that we can speak, think, reason, and argue of no "quantities" but All, Some, and None; if it were true that no proposition can form the basis of argument or statement, or can even be constructed, without the use of "is" or "are" as its principal verb; if it were true that the only mode of reasoning is to include the thing reasoned about in a class, or to exclude it from a class, then some of the doctrines of Logic would be true, although even then some of them would not be true. But I deny the validity of these presumptions, and I appeal to the universal experience of mankind to say whether we cannot, and do not frequently speak, think, reason, and argue of Few, Many, Most, This, That, Certain, The First, The Next, The Last, More, Fewer, All Together, Enough, Others, The Rest, and many other quantities; whether we do not constantly form, use, reason, and argue about propositions constructed with other verbs than "is" and "are"; whether we do not frequently argue and reason in other ways than by including things in classes and excluding things from classes.

The stranger to Logic will naturally ask how it is that presumptions so remarkable, so easily examined, and so easily refuted have remained unquestioned for two thousand years. Is it not true that during that time Logic has been studied by some of the acutest and profoundest thinkers that the human race has produced? and is not this *primâ-facie* evidence that they are right? To this I reply at once, Yes, this does afford *primâ-facie* evidence, but *primâ-facie* evidence is not proof. It is open to refutation if reasons can be adduced against it; and surely, of all sciences, Logic should be the least afraid of an appeal to reason. But it is afraid. The fabric of Logic will hold together if, and only if, its presumptions are admitted without question. They always have been admitted without question; and thus is its long life explained. The efforts of logicians, however acute and however distinguished, have been devoted entirely to elaborating the fabric of Logic that has been built upon certain foundations. The foundations have never been examined. The authority of Aristotle, who laid them, has always over-awed every subsequent thinker, and to question his authority has been held a kind of profanity. In making the following remarks I shall be looked upon much as Tom Paine was looked upon by our grandfathers, as a ribald infidel for whom the rack and the stake would be too merciful.

Aristotle was unquestionably an acute and original thinker, and his researches into the processes of thought were remarkable for the time at which he lived, and are perhaps the most considerable that have ever been made by any one man; but even so, his researches did not go very far; he was not always right; and in subsequent ages his doctrines have been so corrupted and vitiated that if he could read a modern text-book he would indignantly repudiate many that are attributed to him. Nevertheless, so great is his authority that the text-books accept with reverence not only what he said, but also what he is said to have said.

This partly explains the absence of any criticism of the presumptions of Logic; another part of the explanation is that man has no natural or spontaneous tendency to examine his presumptions or the grounds of his beliefs. On the contrary, the process is distasteful and repellent; it is irksome, and even painful; it is never attempted except under the compulsion of circumstances, and those who bring about these circumstances and compel us to reconsider our presumptions are so abhorred and detested that they are punished as severely as we have power to punish them. This has been the fate of reformers in all ages and in all countries, and we need not go beyond the limits of Logic for an example. In the sixteenth century Peter Ramus was formally tried for the offence of differing from Aristotle; he was deprived of his professorship, his books were destroyed, he was threatened by command of the king with corporal punishment if he did not cease his attacks upon the Stagyrite, he was driven from his country, and at last, under the pretence that it was for his Protestantism, he was murdered. Yet the utmost departure that he had ventured to make from the doctrine of Aristotle was the purely technical difference of dividing the figures of the syllogism according to the place of the middle term in the premisses instead of according to the relation of the middle term to the major and minor terms, as Aristotle taught.

With this example before my eyes, it is with natural trepidation that I have proposed a sweeping revolution in the doctrines of Logic; but on reflection I take heart. The only tribunal by which I could be tried for my logical heresy is the Court of King's Bench, and by this Court only if some indignant logician were to sue me for libel. If such an action should be brought, I should call upon the plaintiff to argue his case exclusively by the syllogism. This he could not do, for a logician can no more conduct an argument by syllogistic reasoning than a professor of Latin can converse in the language he teaches. If he refused, the jury would no doubt stop the case. If he accepted, he could not keep his undertaking; but if, by a miracle, he should overcome this real logical impossibility, the jury would at first laugh at him, and before the first month of his argument was ended they would all be dead of acute boredom, and my adversary would himself be on his trial for manslaughter.

If an appeal were made to King George V. to deprive me of my professorship, I should rely with no less confidence on the justice of my cause and the good sense and clemency of His Majesty than on the fact that I hold no professorship, though no doubt I ought to hold one; and though I know full well the weakness of logicians in logical argument, I scarcely think that nowadays they would care to reinforce it by the *argumentum ad baculum*; and if they did wish, I am sure they would not obtain the sanction of the King. If they should burn my book I should rejoice, for under modern conditions they would have first to purchase the whole stock from the publisher, and it is only in such circumstances that it will ever go to a second edition; and lastly, though some infuriated logician may attempt my life, I am confident that it will not be on any religious ground; for when, in a moment of despondency at the boycotting of my *New Logic*, I proposed to an experienced Oxford Don to emulate the example of Martin Luther, and nail the book to the door of St. Mary's Church, he assured me that I could not choose any place in which the logicians of Oxford would be less likely to see it. I cannot help thinking that in this he was wrong. It seems to me that SCIENCE PROGRESS is the very last place in the world to be visited by the professors of THE UNPROGRESSIVE SCIENCE.

VITAMINES[1]

By H. W. BYWATERS, D.Sc. (London and Bristol)

In the days when physiology was just beginning to be recognised as a distinct science, the articles of our food were regarded as made up of three classes of materials—fats, carbohydrates, and proteins—and it was thought that if these materials were present in the diet in sufficient quantity, the maintenance of healthy life was ensured.

With the lapse of time the importance of "quality" as well as "quantity" has gradually dawned upon us, and we now know that food must contain not only proteins, carbohydrates, and fats, but certain definite kinds of these principles, together with small quantities of mineral salts, if it is to be considered satisfactory from the maintenance-of-health point of view. If the protein element, for instance, is deficient in certain amino-acids, especially in aromatic amino-acids, such as tyrosine and tryptophane, no superabundance of other amino-acid constituents will compensate for the deficiency, and the food is unable to maintain the integrity of the living tissues.

The essential factors of a complete diet are therefore more numerous than was formerly suspected, and the recognition of the limitation of the synthetic powers of the living organism has suggested the possibility that other substances may be present in the food—occurring, it may be, in only small quantities—which are nevertheless absolutely indispensable, whose withdrawal from the diet would be attended with eventually fatal results. We know that there are certain mysterious substances in the body—the so-called "internal secretions," hormones, enzymes, and so forth—of which very small traces bring about changes of immense importance to the living organism. These substances are being constantly destroyed and renewed, and the peculiarity of their structure suggests that their elaboration is dependent upon the presence in the food of materials essentially

[1] Based on a Public Lecture given at the University of Bristol on March 6, 1914.

different from the common proteins, carbohydrates, and fats. If these essential materials are persistently absent from the diet, the normal metabolic processes are likely to become disturbed and deranged, culminating in pathological changes of a more or less pernicious character.

The justification of this hypothesis is to be found in the remarkable light which it throws upon our knowledge of a number of diseases which appear to be caused by a too rigid restriction of diet. Such diseases have been grouped together under the term "Deficiency Diseases," and include beri-beri, pellagra, scurvy, rickets, and other less well-defined conditions. We shall see that a certain amount of evidence has been accumulated to show that, in each case, the condition is attributable to the absence from the diet of an essential material, termed by Casimir Funk a vitamine, which is more or less specific in its action in preventing the onset of the disease.

Beri-Beri

Beri-beri is a disease which used to be common in Japan, the Malay Peninsula, and the Philippines—countries where rice is the staple article of diet. That rice consumption was really the cause of beri-beri was suggested as early as 1878, but Eykman was the first to bring forward, in 1897, evidence which seemed to establish a close connection between the use of "polished" rice and the appearance of the disease.

The rice grain, as will be seen from the accompanying figs. 1 and 2 (from Dr. Casimir Funk's article in the *Ergebnisse der Physiologie*, vol. xiii. 1913), consists of an inner part and an outer husk. The inhabitants of the regions just referred to live almost entirely on rice which has had its husk removed—polished rice—and Eykman showed that the addition of the missing husk, rice bran, or the substitution of unpolished for polished rice, was sufficient to effect the cure of the disease and prevent its subsequent recurrence.

The nature of the evidence is interesting. Eykman found that if birds are fed on polished rice, a condition is produced which is analogous to the disease of beri-beri in man. The characteristic symptoms in man are such as arise from degeneration of peripheral nerves, viz. paralysis, muscle atrophy, contraction of the extremities. Death ensues from heart

FIG. 1.—Raw or unpolished rice.

FIG. 2.—Polished rice.

(From the *Ergebnisse der Physiologie*, vol. xiii. 1913.)

weakness, and, *post mortem*, the peripheral nerves, the vagus, spinal cord, and cranial nerves all show signs of degeneration, whilst the muscles, including the heart muscle, are also degenerated and atrophied. In birds, the paralysis of wings and legs is most apparent, and the head is usually pressed back in a characteristic manner by contraction of the muscles in the neck. When these characteristic symptoms appear, the birds, if undisturbed, seldom live for more than twenty-four hours. Eykman found that if, when such a condition has become developed, the rice bran, or an extract of it, is given to the bird, it rapidly recovers, and regains its normal condition.

Eykman's work has been confirmed and extended by subsequent researchers. Sandwith, for instance, has carried out similar experiments on human subjects. Some men were fed exclusively on polished rice; beri-beri developed in two or three months, and was promptly cured by reverting to an unpolished rice diet. The discovery by Braddon that one class of natives in the Malay Peninsula—the Tamils—who prepare their food from unpolished rice, are free from the disease, also falls into line with the rest of the evidence.

In the meantime, the knowledge thus acquired has been applied in various fields with interesting results. A Government Report from Siam, which was recently published, contains the following information bearing on this point.

Effect of alteration of diet of prisoners from polished to unpolished rice in the year 1909:

Deaths from beri-beri in	1906–7	113
,, ,, ,, ,,	1907–8	104
,, ,, ,, ,,	1908–9	122
,, ,, ,, ,,	1909–10	3
,, ,, ,, ,,	1910–11	3
,, ,, ,, ,,	1911–12	0

Similar striking results were obtained in the Culion Leper Colony when the usual diet of polished rice was altered to one of unpolished rice. The deaths from beri-beri among the lepers fell from nearly 100 per month to 1 or 2 per month. To make sure that the change in the diet was responsible for this wonderful improvement, a fresh quantity of polished rice was distributed in November 1911 amongst the lepers. For the next month or so nothing noticeable occurred. In January

1912 the death-rate was still 2, then suddenly in February it rose to 36. The issue of polished rice was immediately stopped, but beri-beri had now set in, and in March the number of deaths was 60. The death-rate then fell in April to 3, and in the succeeding months to even less.

The above facts are most simply explained by the view originally put forward by Gryns (1901), which states that the husk of the rice contains something which is essential for the maintenance of the proper metabolism of the peripheral nervous system. This mysterious "something" has been termed "vitamine" by Casimir Funk, who has succeeded in extracting it from rice bran and obtaining it in a more or less pure state. Later on, the same or a similar vitamine was found to be obtainable from yeast. It was crystalline and melted at 210° C.

The curative properties of this substance were investigated in the following way: A number of pigeons were fed from ten to twenty-one days on polished rice until they developed the characteristic symptoms of polyneuritis. In control animals it was found that the usual length of life after these symptoms had developed was from six to twelve hours. One of these birds, represented in fig. 3 (reproduced from Dr. Casimir Funk's article in the *British Medical Journal*, 1913), received 8 milligrammes of the vitamine by injection into the pectoral muscle. Two hours later, the bird, instead of being dead, flew away to its cage, and had to be photographed on its perch. It seemed as well as ever. The diet of polished rice was therefore continued, and in six days the symptoms of the disease were again apparent, a fresh injection being again sufficient to restore life and energy to the bird.

In fig. 5 we have another bird showing typical signs of polyneuritis. Four milligrammes of the vitamine were injected, and the result is seen in fig. 6.

In another bird (fig. 7,) 8 milligrammes of vitamine were sufficient to bring about complete recovery in three hours.

The anti-beri-beri vitamine has also been detected in milk, oats, wheat, barley, maize, and beans; in cabbage and other vegetables; in common white bread and in ox brain. It is soluble in water and alcohol, and passes through a semi-permeable membrane. It is destroyed by heating to 100° C.

As regards its chemical structure not much can at present be said. It appears to be a mixture or combination of three

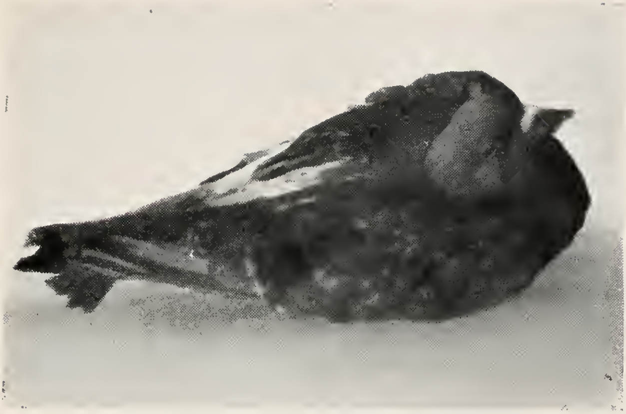


FIG. 3.—Bird before injection of vitamine.

Fig. 4.—Same bird as fig. 3, after injection of vitamine.

(From the *British Medical Journal*, 1913.)

substances, one of which is possibly allantoin or a simple derivative of it. It has been found that certain pyrimidine compounds have also a small curative effect when administered to birds with polyneuritic symptoms and possibly a similar nucleus is contained in the vitamine.

These experiments, striking as they are, may yet receive a different interpretation to that put forward by Casimir Funk. The alternative view was suggested originally by Eykman, who regarded the ill effects observable after feeding with polished rice as due to a toxin contained in the grain whose action is normally neutralised by a corresponding anti-toxin contained in the husk. The curative power of the husk is, from this point of view, due to the anti-toxin contained in it, which removes the toxic condition brought about previously by the administration of the polished rice. Some have ascribed the toxic action to the presence of microbes and others to faulty processes occurring in the grain; and a considerable amount of work has been done with the object of deciding between these contending views.

Most of the known facts, however, are as much in favour of the one as of the other theory. It has been remarked, for instance, that an infant whose mother is only taking polished rice may get beri-beri before the mother shows any signs of the disease. If the diet of the mother remain unaltered the infant will probably die, but it will recover if it be fed on other milk —cow's or even condensed milk. Further, if given an extract of rice bran, its recovery is prompt, although it continues to be nursed by its mother. The restoration to health can, of course, be explained on the ground that the deficient vitamine has been supplied, or equally on the ground that the anti-toxin given in the milk has overcome the induced toxic condition of the child.

A strong point in favour of the toxin theory was advanced by Abderhalden and Lampé, who found that boiling the rice —and thereby presumably weakening or killing the toxin— diminished its power to produce polyneuritis, although it did not prevent it altogether. But Funk argues that the more rapid the assimilation of vitamine-free food, the more quickly would one expect the store of vitamines in the body to become exhausted. Now pigeons are unused to boiled food, and Funk supposes that therefore it is not so easily assimilated as the uncooked rice. Under these circumstances, less vitamine is used up on feeding with the boiled rice and consequently we have the observed delay in the onset of the polyneuritic symptoms.

Funk supports his main proposition by experiments in which birds were fed on quantities of polished rice ranging from ½ to 30 grammes per diem. Those receiving the largest amount of food and in which the greatest assimilative work had to be done, developed symptoms of polyneuritis long before those receiving the smaller amounts of food. Braddon and Cooper have also found in feeding experiments on pigeons that an amount of vitamine in the food sufficient to prevent polyneuritis may become inadequate if more carbohydrate is added to the diet. It would seem, in passing, that the vitamine is particularly concerned with the assimilation of the carbohydrate element of the food.

The fact that prolonged boiling of the polished rice does not completely inhibit, but only delays, the production of the disease is really another point in favour of the vitamine theory, and indeed, I think we are justified in regarding this theory as the one that at the present time offers the most simple and reasonable explanation of all the facts.

Based on this hypothesis a plausible explanation of the symptoms of beri-beri has been advanced by Casimir Funk. The vitamine being necessary for the maintenance of the metabolic processes, particularly of the nervous tissues, the store of it in the body becomes, on feeding with vitamine-free food, gradually exhausted. First the store in the muscles is called upon, then that in the liver, and finally the heart, brain, and nerves themselves become involved. In this way we can account for the onset of the marked nerve degeneration occurring towards the end of the disease.

Pellagra

This disease is practically unknown in this country, but in Spain, Italy, Egypt, and other parts of the world it claims many victims. The first symptoms of the disease are observed in the spring, and consist of severe pains in the spine and joints, accompanied by general feelings of depression. An eruption or skin disease then appears, but towards autumn the symptoms subside, to reappear in an exaggerated form in the following spring. The attacks thus recur regularly every year with increasing severity until paralysis and dementia set in, resulting ultimately in death.

The cause of this disease cannot at present be definitely stated, but there is a certain amount of evidence tending to show that it is due to a deficiency of vitamine. The cereal, maize, constitutes the staple article of diet among the poorer classes in the districts where the disease is most commonly met with, and, as in the case of rice, in the method of preparing the grain for food, the outer layers are often largely removed. These outer layers have been shown by Casimir Funk to be particularly rich in amino compounds, and probably contain an essential vitamine. Feeding animals on polished maize has so far not led to the appearance of symptoms which can be regarded as in any way analogous to those of pellagra, but persons suffering from pellagra may often be cured or greatly benefited by replacing the maize of the diet by other cereals.

It would seem that maize is not the only cereal which may lead to an outbreak of pellagra. According to a recent report by Dr. Stannus at Zomba in Nyassaland, pellagra is not uncommon in the prison at this place and in the surrounding district, although maize is unknown. The diet is, however, very restricted, consisting of rice and salt with little or nothing else. On the other hand, in many regions where maize is the principal foodstuff, pellagra is unknown, which may be accounted for by the vitamine theory by the assumption that the necessary vitamine is being supplied from another source, or that the maize used still retains sufficient of its husk to furnish the necessary vitamine.

An alternative theory, put forward by Sambon, ascribes the disease to a totally different cause, viz. to the action of certain protozoa which are spread through the agency of sand-flies of the genus *Simulium*. He observed that, at any rate in Italy, the disease was only prevalent in the neighbourhood of swift-flowing streams—the favourite haunts of these flies. The beneficial effect attending removal from such a neighbourhood can therefore be readily understood.

The chief objections which can be raised against Sambon's theory are that it does not account for the beneficial effects of a change in diet and that it would lead one to expect the disease to be of a more or less infectious nature, which however does not seem to be the case. It has been observed, for instance, that although the prisons in Italy may contain many "pellagrins," no warder ever catches the complaint. The diet would appear to be, in this case also, the more probable source to which to look for the explanation of the disease.

Scurvy

The diseases thus far dealt with do not occur frequently in our own country, because our people are not confined either to rice or maize as their sole foodstuff. We come nearer home when we refer to scurvy, although this also is one of the "seldom-seen" diseases. At the port of London, ten cases were reported during the years 1899-1909.

The most characteristic symptoms of scurvy are the swellings of the legs and thighs, and the livid patches which are caused by the hæmorrhages under the skin. The gums are usually swollen and congested, and the teeth become loose and drop out. The bones are brittle and there is arrest of fresh bone formation. Similar symptoms may be produced in guinea-pigs, rabbits, dogs, and pigs by feeding them on a diet consisting exclusively of bread. The same subcutaneous hæmorrhages are produced, the gums become swollen, the teeth loose, and the bones brittle, so that we may say that scurvy may be experimentally produced in these animals, thus allowing the disease to be carefully studied.

It is a matter of experience that scurvy is due to defective quality of food, and not merely to a reduced supply. It is most liable to occur on board ship or on expeditions where there is an absence of fresh vegetables or fresh meat. Fresh meat and fresh vegetables are both preventives and curatives of scurvy. In the first Scott Expedition to the South Pole scurvy broke out but was effectively cured by the administration of fresh seal meat, fresh vegetables not being available. In Shackleton's Expedition and Nansen's Expedition across Greenland, where fresh seal meat was usually obtainable, no outbreak of scurvy occurred. In the Arctic Expedition of 1875-6 scurvy broke out because neither fresh vegetables nor seal meat were obtainable.

The conditions determining the appearance of the disease in animals have been shown by Hölst and Frohlich (to whom most of our knowledge of experimental scurvy is due) to be the

FIG. 5.—Bird before injection of vitamine.

FIG. 6.—Same bird as fig. 5, after injection of vitamine.

(From the *British Medical Journal*, 1913.)

same as those which lead to the disease in man. These experimenters found that guinea-pigs fed exclusively on bread, groats, or unpeeled grain, all died of scurvy within thirty days. Other guinea-pigs who received the same diet with the addition of fresh lemon juice showed no signs of scurvy, although a few of them died of starvation. Another series received only fresh cabbage. No sign of scurvy was detected in these animals, but they all died of starvation because of the lack of nutriment in the food. If, however, the fresh cabbage was strongly heated, or was kept for any considerable length of time, before being given to the guinea-pigs, the guinea-pigs all died of scurvy. These experiments undoubtedly prove that there is some constituent in fresh vegetables which prevents scurvy, and, further, that this anti-scorbutic substance or vitamine is easily decomposed by heat or by long keeping. Hölst and Frohlich were inclined to ascribe an enzymatic character to the substance owing to its capacity for producing a large effect although present in such small amount. Other experiments made by these investigators also prove that after the scurvy condition has been produced the giving of fresh vegetable or vegetable juices gradually removes the symptoms, whilst if heated, dried, or long kept, these materials lose their healing properties.

The exact nature of the anti-scorbutic vitamine or vitamines has not yet been established. The experimental study of scurvy has shown conclusively that they are not necessarily potassium salts, as suggested by Garrod, nor are they effective because of their acid-neutralising properties, as demanded by Sir Almroth Wright's theory of acid intoxication. It would appear at first sight that there must be several different anti-scorbutic vitamines because of the variable stability of the curative principle in different foodstuffs. As a rule, heating the food material—milk, for example—to 100° C. is sufficient to destroy the anti-scorbutic vitamine contained in it. But lime-juice—one of the most efficient anti-scorbutics—may be heated for an hour at 110° C. without affecting its curative power. Preserved vegetables are useless as preventives of scurvy, but lime-juice retains its power for years. It is not impossible, however, that the real agent is identical in each case, the environment being really responsible for the observed differences in the behaviour under the influence of heat and other conditions. The presence of the 7 per cent.

citric acid in lime-juice, for example, may confer a stability on the active principle which is not apparent in the slightly alkaline milk, where, it will be noted, simple sterilisation is sufficient to destroy it.

The fact that heating the milk destroys the anti-scorbutic vitamine accounts for the appearance of scurvy in infants fed on artificial substitutes for human milk. An interesting case is recorded in the *Lancet* of 1911 by Brachi and Carr, where a female infant, one of twins, developed the disease at the age of seven months. For the first six weeks of life the child had been breast fed, but after that had been brought up entirely on the Chelsea Borough Council sterilised milk. As there were no teeth, there was no marked congestion of the gums, but the legs and thighs were characteristically swollen and very painful and tender. The X-rays revealed large sub-periostial swellings situated at the lower ends of both femora. The treatment consisted in giving fresh milk, diluted with water at first, with orange juice and raw meat juice. Within a week, the swellings began to subside, and in a fortnight the tenderness had disappeared. The child left the hospital a month afterwards completely cured. Cheadle and Poynton in their work on this subject state: "There is nothing in the whole range of medicine —not even excepting the effect of thyroid extract in myxœdema —more striking and remarkable than the immediate and rapid recovery which follows the administration of fresh vegetable material and other fresh elements of food in these cases of infantile scurvy." It may be well to draw attention in passing to the probability that since boiled or sterilised milk often enters largely into the diet, many children suffer, not perhaps from typical scurvy itself, but from slight manifestations of the disease, fortunately arrested, often fortuitously, by a trifling alteration of the diet or by the addition to it of what may even be deemed indigestible fresh foodstuff.

There appears to be a close connection between beri-beri and scurvy. Feeding hens, doves, ducks, etc., with polished rice causes polyneuritis, whilst dogs and guinea-pigs on a polished rice diet develop not beri-beri but scurvy. Pigs similarly treated suffer from both diseases. It would seem that polished rice is devoid of both the anti-beri-beri and the anti-scurvy vitamines, and that each type of animal, except the pig, is more susceptible to the absence of one than the other, the one to which they are more susceptible differing in the case of different animals.

The vitamine responsible for the prevention of beri-beri is quite distinct from that which inhibits scurvy. The anti-beri-beri vitamine is more stable; it is not destroyed by heat, and can be treated with alcohol and ether without losing its virtue. Certain foodstuffs, such as yeast, oats, and barley, which are rich in anti-beri-beri vitamine, do not contain any anti-scurvy vitamine.

Rickets

Rickets is another common disease found in all parts of the world, and especially in the temperate zone, which is undoubtedly due to a lack of some material in the diet, probably of a vitamine nature. The disease has been attributed to deficient clothing, to deficient fresh air, to indigestion, to syphilis, and to inherited tendencies. But it is not inherited, not due to syphilis or any of the evils I have mentioned. They are possibly predisposing influences, but they cannot be considered as actually responsible for the disease. The vast majority of the cases of rickets may be directly traced to defective feeding; indeed, we may say that a faulty diet is the common and, in most cases, the sole source of the mischief. And just as scurvy produced by a scorbutic diet is cured by change of diet, so here a suitable change of diet is attended by an arrest of the rickety condition and an opportunity is afforded for Nature to bring about restoration to health.

What is the substance the omission of which from the diet leads to the outbreak of rickets? The most striking feature of the disease is the weak bones—the evident failure of the ossification process—and this led to the view being expressed and long entertained that the cause of the disease was a deficiency of calcium or lime salts in the food. Investigation has however shown that this is not really the case. Feeding animals with food deficient in lime salts certainly leads to weakness in the bones, but the rickety symptoms are otherwise absent. Moreover, there is conclusive evidence that rickets may, and often does, develop when there is an abundance of lime salts in the food. Rickets is quite common in limestone districts, and yet the drinking water may be loaded with lime salts, and the children living on patent foods made up with such drinking water could not but receive large quantities of lime salts as a daily constituent of their diet. Further, analysis shows that most of the patent farinaceous foods on the market contain large quantities of lime salts as calcium carbonate or phosphate, but very often recourse to such foods does not prevent the outbreak of rickets. It is, of course, possible to maintain that the fault lies not in the deficiency of lime salts in the food, but in a failure to absorb the lime salts into the system, but in this case the real point we have to consider is the factor which brings about the loss of absorptive power.

Another common belief is that the condition is due to a lack of fat in the diet. It is known that it is the children who have been brought up on patent starchy foods with little or no fresh milk who become subject to rickets. The experiments of Mr. (now Sir John) Bland-Sutton on the animals at the London Zoo are also quoted as supporting this theory. He found that young monkeys removed from their mother and fed on vegetable food became rickety, whilst two young bears fed almost entirely on rice and biscuits died of the same disease. The most striking results were obtained with the lion cubs. It had been the custom to remove the young cubs from their mother at quite an early period of their existence, and to feed them on raw horse-meat instead of on mother's milk. But they invariably became rickety to such a degree that it was impossible to rear them. The condition was typical of rickets—the same feebleness of muscle, general debility, bending of bones, etc. With a single exception, the successive litters of young cubs lived but a few weeks and then succumbed. More than twenty litters had been lost in this way. Then Mr. Bland-Sutton appeared on the scene. The litter of cubs then being reared had been weaned at the end of two weeks and put on the horse-flesh diet. They had rapidly become rickety, and one had already died. Mr. Bland-Sutton ordered a mixture of milk, pounded bones, and cod liver oil to be added to the horse-flesh, the other conditions being exactly the same as before—the same den, the same amount of warmth, light, air, etc. The results were most startling; improvement immediately set in, and in three months all signs of rickets had disappeared. The animals grew up strong and healthy—a unique event, it is stated, in the history of the Zoological Society.

But are these results to be attributed to the beneficial effect

FIG. 7.—Bird before injection of vitamine.

Fig. 8.—Same bird as fig. 7, after injection of vitamine.

(From the *British Medical Journal*, 1913.)

of the fat as fat contained in the cod liver oil? Is it not more likely, as Casimir Funk has pointed out, that a "vitamine" present in the cod liver oil was really the active agent in bringing about the cure? From the chemical standpoint it is difficult to see how the presence or absence of fat—an organic compound containing a store of energy liberated in the body by breaking down into carbon dioxide and water—can affect the laying down of calcium salts in the bony tissues or in any way bring about the collateral symptoms of rickets. We know that a child may be brought by starvation to death's door without having shown any sign of rickets, and conversely, it is common knowledge that it often is the fat, overfed child which suffers from the complaint. If rickety children may have a superabundance of fat, how can the lack of it be the cause of the disease? As a matter of fact, experience has shown that children, and lion cubs, may recover from rickets and grow up without the addition of any special fatty constituents to the diet.

We have seen that the error in scurvy is the absence of a minute quantity of essential vitamine and not the lack of any of the physiological classes of foodstuffs. It seems probable that rickets also is due to the absence of a vitamine which associates itself with the fatty portion of the diet. If fat be removed from milk, and the child be fed on the skim milk, rickets will probably ensue because the vitamine has also been removed. But it is not necessary to remove the fat to produce an unsuitable diet. Simply boiling the milk will produce the same effect, although the fats are thereby practically unaffected. It cannot, therefore, be lack of fat that accounts for the outbreak of rickets, but, instead, the absence of a vitamine similar to those responsible for the prevention of beri-beri and scurvy.

Accessory Substances Necessary for Growth

Recent work has revealed the presence of traces of substances in food which regulate the growth of young animals. The discovery of these substances was made by Gowland Hopkins during a series of experiments which had for their object the maintenance of animals upon artificial mixtures of pure carbohydrates, fats, proteins, and salts. The researches of Gowland Hopkins in this country, Osborne and Mendel in

America, and various other workers in Germany and elsewhere, have shown that although an artificial diet may be constructed which will maintain full-grown rats in health for a few weeks, yet, after this time, they invariably begin to decline and slowly die. What is even more remarkable is that it is impossible in the case of young rats for normal growth to take place when only an artificial diet is supplied, but that the addition of small

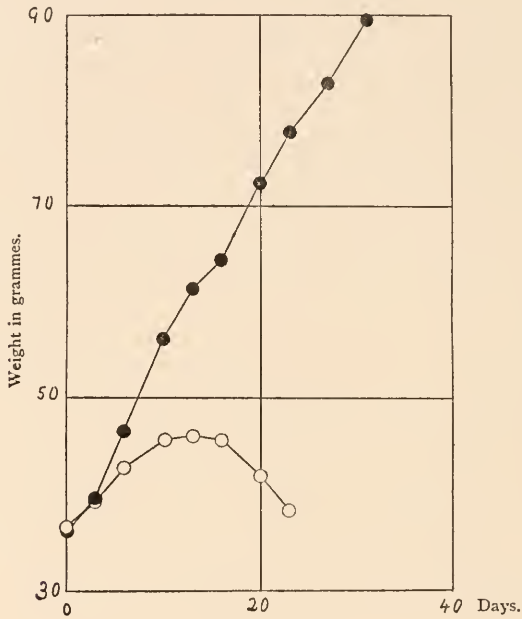


FIG. 9.

White circles :—Rats on artificial diet alone.
Black circles :—Rats on artificial diet + 2 c.c. milk per rat per diem.

(From the *Journal of Physiology*, vol. xliv. 1912.)

traces of other substances, such as fresh milk, is quite sufficient to transform the inadequate into a most nourishing diet.

Hopkins's experiments were carried out on young rats about 35 to 50 grammes in weight, *e.g.* of such a size as would normally double itself in about twenty days. The rats were kept in cages, two in a cage, and were weighed every three or four days. The artificial diet consisted of casein, starch, cane sugar, lard, and salts, mixed up with a little water into a stiff dough; and the amount of food daily eaten was accurately estimated, the average for each of a series of rats under observation being then calculated.

In the preliminary work it was found that the rats devoured large quantities of the artificial food; but they did not grow until a modicum of milk was also added. The nature of the results can perhaps best be made clear by means of graphic representations taken from Hopkins's published work in the *Journal of Physiology*, vol. xliv. 1912. In fig. 9 we have the

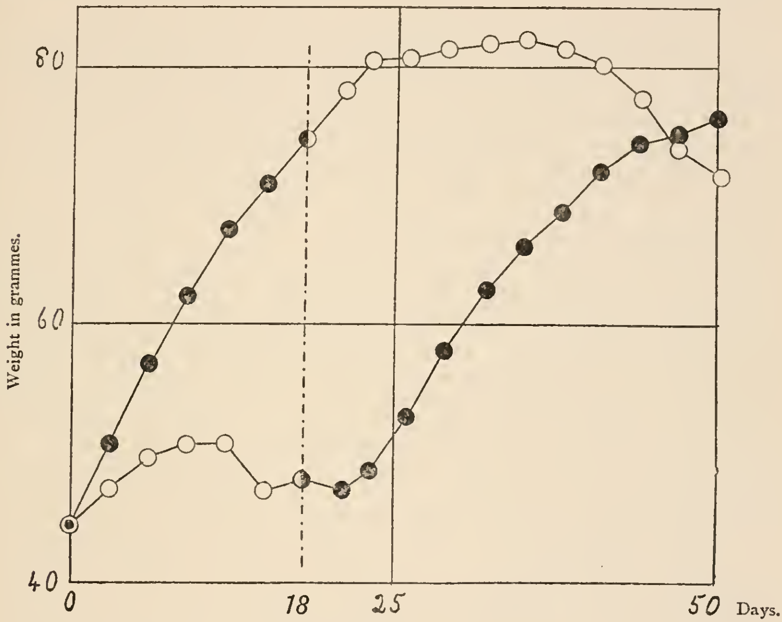


FIG. 10.

White circles :—Rats on artificial diet alone.
Black circles :—Rats on artificial diet alone + 3 c.c. milk per rat per diem.

(From the *Journal of Physiology*, vol. xliv. 1912.)

record of the average weight of six rats fed on artificial food compared with the average weight of six other rats similarly treated, but receiving in addition 2 c.c. of milk per rat per diem. The blackened circles denote the set receiving milk, whilst the light circles constitute the record of the set fed on the artificial diet alone. In these experiments the constituents of the diet were first carefully purified to eliminate traces of activating substances before being mixed together to form the basal artificial food.

It will be observed that the set of six rats fed on this diet grew slowly until the thirteenth day, when growth ceased. By the twentieth day loss of weight had set in, and a week later five of the six rats had died. Compare now the record of the six rats fed on the same diet with the addition of 2 c.c. of milk per rat per diem, represented by the line of blackened circles. A steady growth in size and weight has occurred—a growth resulting in the doubling of the weight of the animals in less than twenty days—in other words, a thoroughly normal growth.

In the next figure is contrasted the effect produced by feeding eight rats on the purified artificial diet with another eight rats fed on the same diet plus 3 c.c. of milk per rat per diem. On the eighteenth day the milk ration was transferred from the one set to the other. The arrest of growth of the first set and the setting in of the delayed growth of the second set are most apparent effects of the change.

In all the experiments, of which the above are only typical examples, it must be realised that the amount of solid matter in the added milk was almost negligible in comparison with the weight of artificial food taken, and amounted usually to from 1 to 4 per cent. of the total food. Moreover, by far the larger part of this additional material consisted of lactose or protein, both of which were shown by separate experiments to be totally devoid of growth-activating properties.

Further experiments of a similar nature have shown that the same accelerating effect on growth may be obtained by adding to the artificial diet traces of the protein-free alcoholic extract of the milk solids. A still further refinement has been possible in that it has been found that the ethereal extract of the alcoholic extract of milk solids is just as efficient as the raw milk. Evidently the responsible agent is not inorganic in nature, and it must also be very powerful in its action. The substance is thermostable, because boiled milk is just as effective as unboiled milk.

The experiments of Osborne and Mendel have confirmed the work of Hopkins, and have extended it in several directions. They also have found that rats cannot be maintained on an artificial diet containing amounts of protein, fat, carbohydrates, and salts which are more than sufficient to satisfy the energy requirements of the animals. If, however, dried protein-free milk (a commercial article consisting of lactose and salts, with only a small amount of fat and protein) be added to the diet, the resulting mixture is adequate to keep the rats in excellent

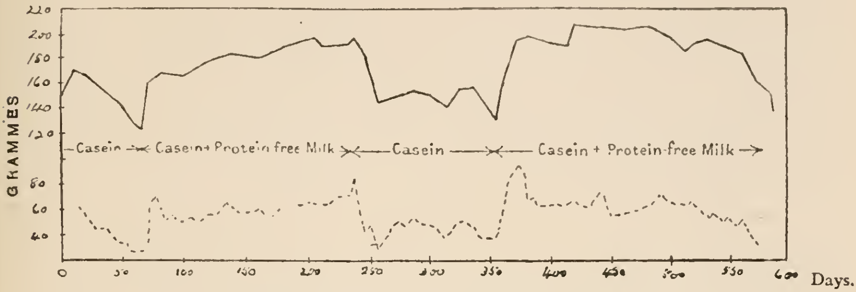


FIG. 11.—Curve showing variation of weight of rat on different foods. The lower dotted line shows weight of food eaten by rat.

(From the *Journal of Biological Chemistry*, vol. xiii. 1912.)

condition for many months. Some of their experiments with individual rats have extended over 600 days.

As an example of their work, I have selected fig. 11 (reproduced from the *Journal of Biological Chemistry*, vol. xiii. 1912). During the first 60 days, on a diet of casein, starch, sugar, lard, and salts, the rat was evidently being insufficiently nourished.

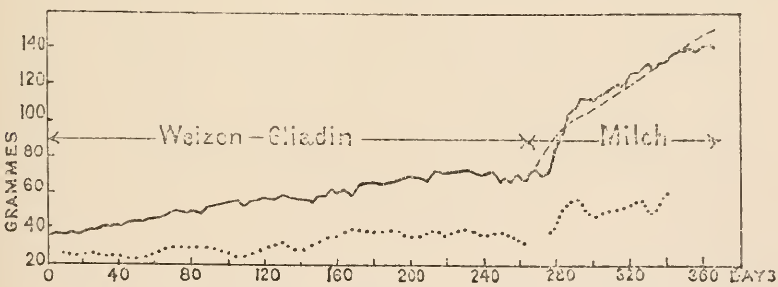


FIG. 12.—Curve showing variation of weight of rat before and after addition of milk to diet.

(Aus Thomas B. Osborne und Lafayette B. Mendel unter Mitwirkung von Edna L. Ferry, Beobachtungen über Wachstum bei Fütterungsversuchen mit isolierten Nahrungssubstancen in Hoppe-Seyler's *Zeitschrift für Physiologische Chemie*, Band lxxx. Verlag von Karl J. Trübner, Strassburg.)

Substitution of protein-free milk for the sugar restored the rat to its original weight. After 170 days the casein food was again tried, and immediately the weight of the rat began to fall, to be again restored when the diet was supplemented by

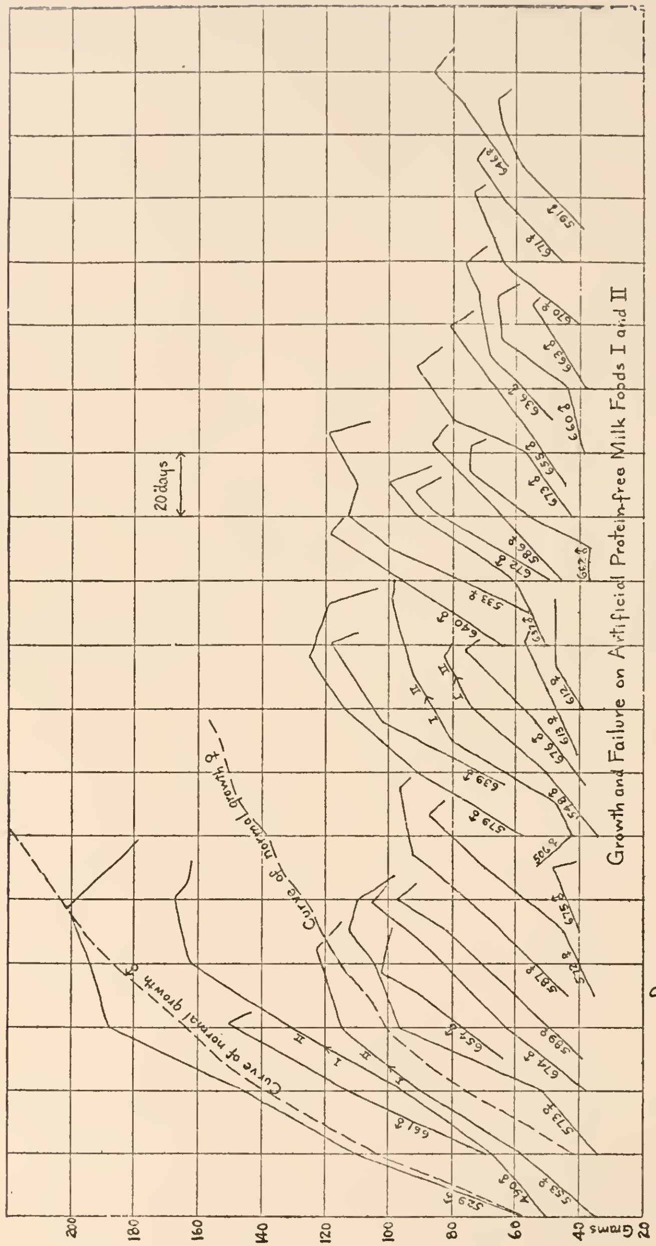


FIG. 13.

(From the *Journal of Biological Chemistry*, vol. xv. 1913.)

the protein-free milk. The animal eventually succumbed to diseased lungs after 587 days of experimental feeding. Provided that a suitable protein, as casein or edestine, is included in the diet, it can be said that rats may be maintained practically

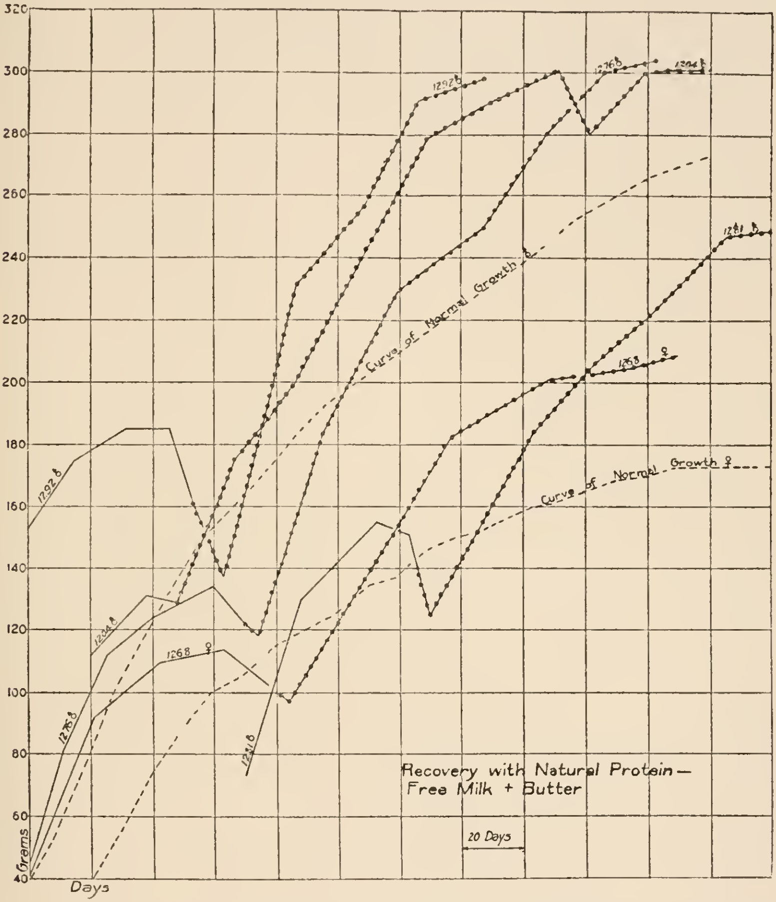


FIG. 14.—The plain line shows weight during feeding with artificial diet, and the crossed line the weight after the change to milk diet.

(From the *Journal of Biological Chemistry*, vol. xvi. 1913.)

indefinitely so long as the maintenance vitamine, such as is found in dried protein-free milk, is present.

But maintenance is one thing and growth is quite another, and Osborne and Mendel have found that, although the addition of protein-free milk to the artificial diet is sufficient to render it capable of maintaining the health of fully grown rats, it is

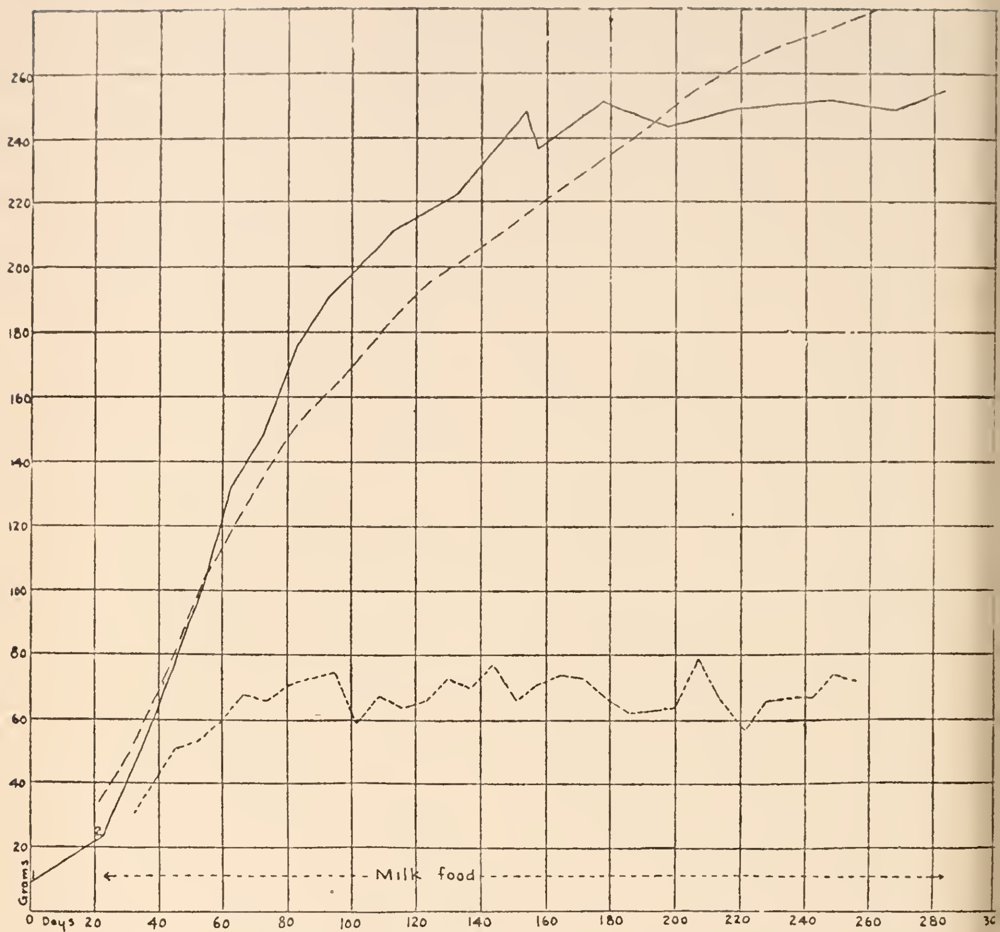


FIG. 15.—Rat on milk diet. The broken curve indicates normal growth of rat on ordinary food. The dotted line shows amount of food eaten by rat.

(From the *Journal of Biological Chemistry*, vol. xii. 1912.)

inadequate to bring about the normal growth of a young animal. For growth to take place some other vitamine must be present which is believed to be contained in the fatty portion of the milk, *i.e.* in the portion that would constitute the ether-alcohol extract found to be active in this respect by Hopkins.

In the first example, fig. 12, the young rat was fed for 265 days on an artificial diet containing gliadin as its sole protein. Dry protein-free milk was also added. The weight of the rat very slowly increased. On the 266th day when the normal rat had long since reached maturity and ceased to grow, some milk was added to the diet. Normal growth now set in, and this old rat started growing so fast that it put on as much weight in 26 days as in the previous 265 days of inadequate diet. (The broken line indicates the growth of the normal animal.) Evidently in the added milk there is the vitamine necessary for growth.

The next diagram shows the commencement of growth of a

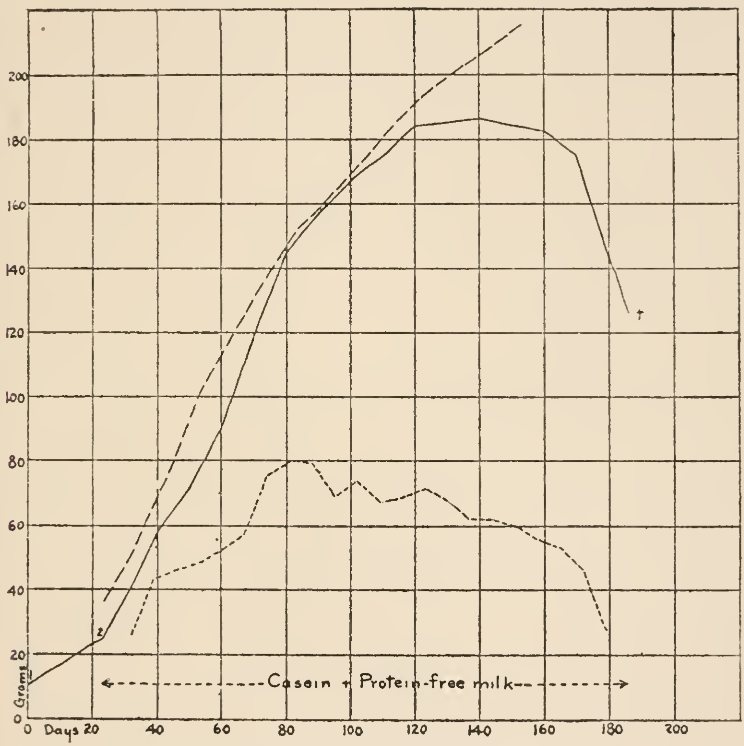


FIG. 16.—Rat on casein and protein-free milk.
(From the *Journal of Biological Chemistry*, vol. xii. 1912.)

number of young rats, and their inevitable death when the diet consisted of an artificial mixture of proteins, fats, carbohydrates, and salts, with dried protein-free milk. This food, it may be once more emphasised, has been proved to be quite adequate to maintain adult rats in good health for at least one and a half to two years.

The next example shows that the addition of the growth vitamine present in the fatty portion of fresh milk restores the growing power to young rats and enables them to attain to their normal size and weight. These animals were fed on the previously mentioned artificial food, and when they showed signs of declining preparatory to death, butter was added to the diet. Very shortly afterwards the decline was arrested, and growth commenced once more.

An interesting illustration of the fact that the activating substance for growth is specific is afforded by one of Osborne and Mendel's experiments where a rat became pregnant whilst on the protein plus protein-free milk diet, and in due season

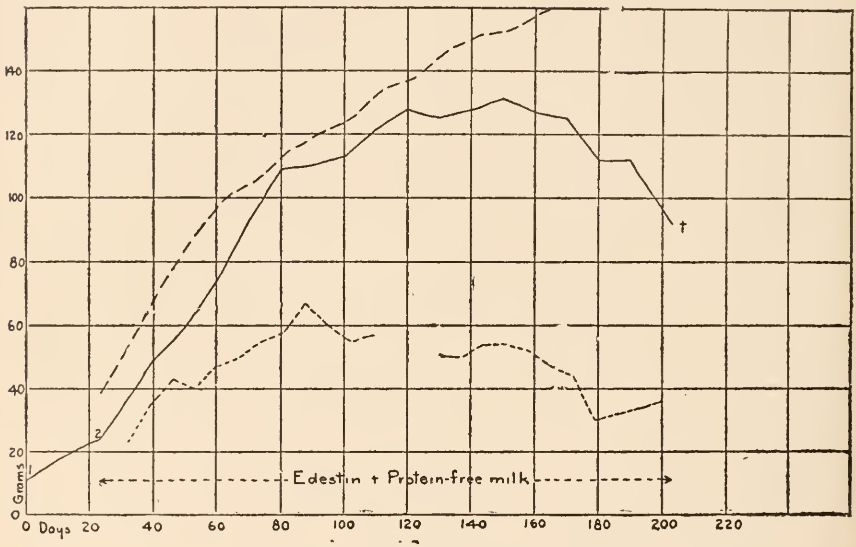


FIG. 17.

(From the *Journal of Biological Chemistry*, vol. xii. 1912.)

brought forth four young rats. The young rats were suckled by the mother for 30 days, and during this time they seemed to grow about as rapidly as the young of normal parents. About this time, as a rule, young rats are wont to begin to depend upon extraneous food for nourishment, and so three of them were removed and fed separately. One of them received a diet containing all the ingredients of milk. Note the normal growth as represented in fig. 15. The other two rats were fed on protein plus protein-free milk. Their curves (figs. 16 and 17) reveal an attempt at growth, followed by collapse and death.

The fourth rat was allowed to remain with its mother, sharing the same food (fig. 18). For 50 days scarcely any growth took place, *i.e.* the food that had been sufficient to enable the mother to produce young and secrete sufficient milk, both as regards quantity and quality, for bringing up the offspring for 30 days, was not able to induce normal growth after this time. The essential growth vitamine was missing, and though the

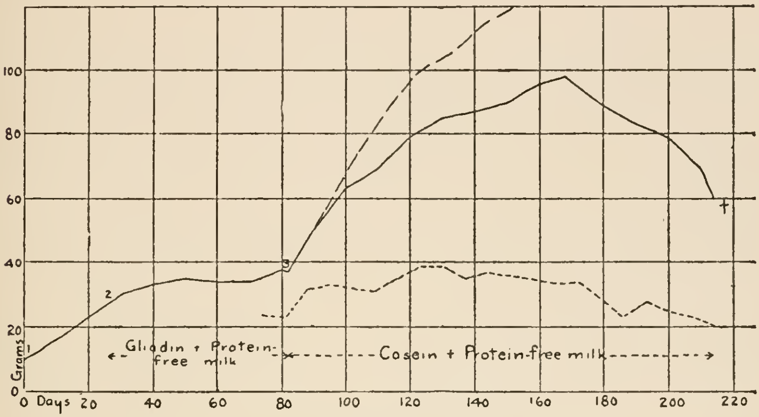


FIG. 18.

(From the *Journal of Biological Chemistry*, vol. xii. 1912.)

substitution of milk protein for gliadin prolonged life for some time, the animal eventually died before it was half grown.

The generation of these young rats whilst the mother rat was living on a diet containing a single protein is really most remarkable. During this time a large new formation of body-tissue must have occurred, involving the synthesis of purines, nucleic acids, pyrimidines, phospho-proteins, casein, lipoids, etc., and probably the actual synthesis of the growth vitamine, inasmuch as normal growth of the young rats was maintained whilst they were being suckled by their mother. As, from the same diet, the young rats themselves were unable to form the growth vitamine, the production of the growth vitamine is manifestly a function of the mature female.

These experiments all go to show that for growth a mysterious "something"—the growth vitamine—is necessary. This vitamine is associated with the fatty constituents of milk, and is not destroyed by heat. Everything points to the probability of its being a different substance from those responsible for the prevention of scurvy and beri-beri, for not only do the various vitamines possess different properties, but the omission of any one of them brings about its own specific result. A striking result of this specificity has been recently advanced by Funk. He has shown that a certain diet may lead to cessation of growth without the production of beri-beri or scurvy. Thus the two chickens represented in fig. 19 are each two months old, but the one fed on red rice has scarcely grown at all, and has not shown the slightest sign of polyneuritis. Here the diet contains anti-beri-beri and anti-scorbutic vitamines, but the growth vitamine is absent.

It may be asked in what way do these vitamines act. Do they affect the absorption of food from the intestine? Careful investigation by Hopkins showed that in his experiments involving the addition of small quantities of milk, not the slightest difference in the percentage extent of absorption could be discerned, whether the milk was added or not. The rats without the milk absorbed as much food as those with, but evidently the food in the former case was not being properly applied within the body.

It is also not a question of appetite. The rats without the milk ate as voraciously as the others, and it was only when the rats began to lose weight that the amount of food consumed began to grow less. In many cases it was conclusively demonstrated that the animals on the milk-free diet continued to eat and absorb a quantity containing an ample supply both of protein and energy for the continuance of growth at a time when their growth had wholly ceased. The loss of appetite followed the cessation of growth and did not precede it.

The minuteness of the quantities of these vitamines which are requisite to maintain normal processes of life suggest that they must have something to do with the production of some of the essential hormones, internal secretions, enzymes, etc., in the animal organism. Careful post-mortem examination of polyneuritic pigeons by Casimir Funk has shown that this condition is associated with an extreme atrophy of the thymus, which suggests that the vitamine in rice husk has to do with the stimulation of this organ, which we know is largest, and evidently exerts its chief functions, during the period of growth.

It eems probable that other vital processes may also be

Fig. 19.—Two chickens each two months old. The smaller one has been fed on red rice, the other on ordinary food.

(From the *Lancet*, 1914.)

dependent upon vitamines in the food. Consider, for instance, the transformation of carbohydrate into fat, which takes place on such a large scale when animals for market are fed on certain starchy foods. The starch and soluble carbohydrates in all such foodstuffs are reduced to glucose in the alimentary tracts of these animals; and yet the food containing the largest amount of assimilable carbohydrate is by no means necessarily the most fattening. Something else is necessary to bring about the change from glucose to fat—something which, it is not unlikely, may prove to be a vitamine, present in different amounts in different foodstuffs, and contained possibly in the outer layers or husks of the grain. The difference in the fattening properties of the two well-known adjoining fields in the Romney marshes, with almost, but doubtless not quite, identical vegetation, may, as has been already suggested by Prof. Armstrong, be explained in a similar way.

In conclusion, reference may be made to one other problem on which this new knowledge may possibly shed some light. In cancer we have a pathological new growth of tissue, in which, as Casimir Funk has suggested, a special vitamine may be concerned. He raises the question as to whether it is possible to inhibit this new growth by careful regulation of the diet with a view to the exclusion of the special vitamine. Experimental investigation so far seems to show that the vitamines for normal and cancerous growths are *not* identical, and it would therefore seem possible to frame a diet that shall allow normal growth to proceed but shall inhibit cancerous growth.

I am greatly indebted to Dr. Funk, Professor Gowland Hopkins, Professors Osborne and Mendel, and the proprietors of the various journals for permission to reproduce the curves, etc., used in the illustration of this article. My grateful thanks are also due to Professors Osborne and Mendel for the loan of their excellent blocks.

Literature

For excellent summary of, and references to, previous work see Casimir Funk's article on "Vitamines" in *Ergebnisse der Physiologie*, vol. xiii. 1913.

Braddon and Cooper:
British Medical Journal, 1914, p. 1348.

CASIMIR FUNK:
Biochemical Journal, vol. 7, pp. 211 and 356, 1913.
British Medical Journal, April 19, 1913.
Journal of Physiology, vol. 45, p. 489, 1913; vol. 46, p. 173, 1913; vol. 47, p. 389, 1913.
Lancet, January 10, 1914.
Münchener Med. Wochens. Nos. 36 and 47, 1913.
Die Naturwissenschaft, 1914, p. 121.
Proc. Roy. Soc. Med. vol. 7, p. 9, 1913.
Zeits. physiol. Chem. vol. 88, p. 352, 1913.

HOPKINS:
Journal of Physiology, vol. 44, 1912, p. 361.

OSBORNE and MENDEL:
Journal of Biological Chemistry, vol. 15, 1913; vol. 16, 1913.

STANNUS:
Trans. Soc. Trop. Med. and Hygiene, vol. 7, No. 1, p. 32.

THE BIOCHEMISTRY OF RESPIRATION

By H. M. VERNON, M.A., M.D.

An article upon the "Mechanism of Tissue Respiration" was published in this journal[1] by the writer some seven years ago, but so great has been the advance in our knowledge of the subject in recent years that it seems worth while to write a second article, dealing chiefly with the fresh research which has appeared in the interval.

And, firstly, it should be pointed out that the clearer insight which we now seem to possess into the fundamental chemical processes which underlie the internal respiration of the tissues is due to the gradually increasing recognition of the hypothesis—first suggested by Hoppe-Seyler[2] in 1876—that in all organisms respiration is primarily anaerobic. Detmer[3] put it more clearly by stating that all vital processes consist in a breakdown of labile compounds, the decomposition products of which subsequently undergo oxidation. But these views were not generally accepted, as it was evident that in the great majority of organisms oxidation processes are of much greater importance in the production of energy than hydrolytic or other changes taking place in the absence of oxygen, and that in all but a few of the lowest forms of life, such as certain bacteria and parasites, oxygen is essential for the continuance of life. The fact that carbon dioxide continues to be discharged for minutes or even hours by frogs, snails, and other animals kept in an oxygen-free atmosphere was attributed by Engelmann, Pflüger, Verworn, and others to the existence of a store of intramolecular oxygen bound up in the tissues. Winterstein[4] pointed out that this hypothesis has no solid experimental evidence to support it, and there is no doubt that much of the supposed evidence is better explained in other ways; but, as we shall see later on,

[1] Vernon, SCIENCE PROGRESS, **2**, N.S. p. 160, 1907.
[2] Hoppe-Seyler, *Arch. f. d. ges. Physiol.* **12**, p. 1, 1876.
[3] Cf. Detmer, *Jahrbuch. f. wiss. Bot.* **12**, p. 237, 1879.
[4] Winterstein, *Zeit. f. allgem. Physiol.* **6**, p. 315, 1907.

there are still good reasons for accepting the doctrine in a modified and reduced form.

The fundamental character and universal distribution of anaerobic respiration is supported by a large body of evidence, much of which is described in an article published three years ago in this journal by C. D. Snyder[1] on "Life without Oxygen." Snyder points out that what appear to be the simplest forms of life are anaerobic still, and that the majority of lower organisms, both plant and animal, can live under anaerobic conditions more or less continuously. He states that the fundamental chemical processes of the cell in all organisms, even the highest, are anaerobic, and that phenomena of oxidation are secondary, and have been gradually built up in the course of evolution. Need of oxygen appears to increase *pari passu* with increasing complexity of chemical and morphological organisation.

But little direct evidence is adduced by Snyder in favour of the fundamentally anaerobic character of the vital process, so it is desirable to discuss the hypothesis in some detail. It is supported chiefly by two lines of experimental evidence. One, the production of CO_2 by animals kept anaerobically, will be referred to later, whilst the other concerns the existence, in many organisms, of hydrolytic enzymes which in the complete absence of oxygen break down food material with the liberation of heat and other forms of energy. This evidence begins with Buchner's[2] discovery of zymase in 1897. After mixing washed yeast with sand and grinding with a heavy mortar, a juice can be expressed containing numerous intracellular enzymes, the most interesting of which is that responsible for the alcoholic fermentation typical of living yeast cells. When mixed with a solution of glucose, lævulose, or galactose, the yeast juice decomposes the sugar according to the equation:

$$C_6H_{12}O_6 = 2C_2H_6O + 2CO_2$$

Disaccharides as cane-sugar and maltose, and polysaccharides such as glycogen, are likewise acted on, but they are hydrolysed to monosaccharides by the intracellular invertase, maltase, and amylase enzymes present before being attacked by the zymase.

[1] C. D. Snyder, SCIENCE PROGRESS, **6**, N.S. p. 107, 1911; also **4**, p. 579, 1909. See also Lesser, "Leben ohne Sauerstoff," *Ergeb. d. Physiol.* **8**, p. 742, 1909.

[2] Cf. *Die Zymasegahrung*, by E. Buchner, H. Buchner, and M. Hahn, 1903.

Recent research has shown that the process of hydrolysis is by no means so simple as that indicated in the above equation. Harden and Young[1] find that the hexose interacts with the phosphates always present in yeast juice to form a hexose-phosphate of the type $C_6H_{10}O_4(PO_4R_2)_2$, simultaneously with CO_2 and alcohol. Subsequently the hexose-phosphate splits up into free hexose and phosphate. Also they find that zymase is quite unable to ferment sugar unless a "co-enzyme" is present. This co-enzyme is a thermostable substance which can be removed from yeast juice by dialysis. It can be hydrolysed by lipase and so appears to be an ester, but it has not been isolated and its chemical nature is unknown.

The conversion of hexose into alcohol and CO_2 is probably not a direct one. Buchner and Meisenheimer[2] formerly thought that lactic acid is an intermediate product in the decomposition, but they have now abandoned this view, and believe that the evidence points rather to dihydroxyacetone as the chief intermediary. Another product appears to be glycerin aldehyde, and this may be converted into methyl glyoxal, CH_3—CO—CHO, which in turn is split up into pyruvic acid and pyruvic alcohol. The pyruvic acid, CH_3—CO—COOH, splits up into CO_2 and acetaldehyde, the acetaldehyde being reduced to the final product, ethyl alcohol.[3] Traces of lactic acid are formed as a by-reaction, perhaps by the hydration of some of the methyl glyoxal, for Neuberg[4] has shown that minced liver and muscle can effect this conversion. In any case the lactic acid appears to be an end-product, and is not an intermediate step in the decomposition.

In addition to forming CO_2 and alcohol, yeast juice converts about 5 per cent. of the sugar decomposed into glycerol. Also small quantities of acetic acid, formic acid, acetaldehyde, and formaldehyde are produced, but the amyl alcohols and succinic acid liberated by the action of living yeast cells do not appear in yeast-juice fermentation. Yeast juice is likewise capable of liberating CO_2 from certain organic acids. For instance, it can convert pyruvic acid into CO_2 and acetaldehyde, and as much as 40 per cent. of the theoretical quantity of the aldehyde has

[1] Cf. Harden, *Alcoholic Fermentation*, London, 1911.
[2] Buchner and Meisenheimer, *Ber.* **43**, p. 1773, 1910.
[3] Cf. Oppenheimer, *Die Fermente*, p. 696, 1913.
[4] Neuberg, *Biochem. Zeit.* **49**, p. 502, 1913.

been isolated.[1] Neuberg considers that this reaction is induced, not by zymase, but by a distinct "carboxylase" enzyme, for the action of zymase can be paralysed by antiseptics and destroyed by high temperatures which leave the carboxylase unaffected. Yeast cells liberate CO_2 from acetic, butyric, lactic, oxalic, tartaric, and other acids; but there is no evidence to show that this is due to the specific action of the carboxylase enzyme.

Zymases inducing alcoholic fermentation have been shown to exist in many vegetable organisms, not only bacteria and moulds, but also in the higher plants. Stoklasa, Palladin,[2] and others have demonstrated them in seeds, leaves, flowers, and roots of numerous plants, and hence it seems probable that they are of universal distribution. In addition to CO_2 and alcohol, lactic, acetic, and formic acids are produced, apparently as intermediate products in the decomposition, whilst according to Stoklasa some free hydrogen is liberated.

In animal tissues the existence of zymases is not definitely proved. Stoklasa and others have brought forward evidence in their favour, but the majority of investigators[3] have failed to obtain positive results when the possibility of bacterial infection was excluded. However, Kobert[4] found that if the yolk of tortoise eggs or sea-urchin ova were ground up and incubated with glucose in the presence of toluol and 1 per cent. NaF, appreciable quantities of alcohol were formed. If *Ascaris*, earthworms, and other invertebrate animals were ground up with kieselguhr and incubated sixteen hours with toluol and NaF, they likewise yielded some alcohol on incubation, so he concludes that these organisms, and also the ova, contain zymase. Still these results have not been confirmed, and even if they are correct it does not necessarily follow that they hold for higher animals.

The evidence in favour of the existence of glycolytic enzymes in animal tissues is somewhat stronger, though it is not above suspicion. Cohnheim[5] found that if glucose were incubated with

[1] Neuberg and Karczag, *Biochem. Zeit.* **36**, p. 68, 1911; Neuberg and Rosenthal, *ibid.* **51**, p. 128, 1913; **61**, p. 171, 1914.

[2] Cf. Vernon, *Intracellular Enzymes*, p. 109, 1908.

[3] Cf. Harden and Maclean, *Journ. Physiol.* **42**, p. 64, 1911.

[4] Kobert, *Arch. f. d. ges. Physiol.* **99**, p. 116, 1903.

[5] Cohnheim, *Zeit. f. physiol. Chem.* **39**, p. 336; **42**, p. 401; **43**, p. 547; **47**, p. 253, 1906.

muscle press juice, or with pancreas juice, in presence of toluol, little or none of it disappeared. If, on the other hand, fresh muscle and pancreas were minced together, the mixed juice squeezed out from the two tissues possessed distinct glycolytic power. Extracts of boiled pancreas, if mixed with muscle juice, were as efficient as those of fresh pancreas, so the activating power of the pancreas was not dependent on an enzyme. Though these experimental results have been contradicted by some investigators, they have been confirmed in the main, especially by Hall.[1] Other investigators have found that glycolytic power is not confined to the muscles and pancreas. Liver possesses it too, and is able to act to some extent in the absence of the pancreas. The special glycolytic power attributed to the pancreas is founded on the well-known discovery of v. Mering and Minkowski that extirpation of the pancreas causes diabetes. This was generally supposed to prove that the pancreas forms an internal secretion which enables the tissues, especially the muscles, to burn up sugar; but the recent work of Starling and Patterson[2] upon the sugar consumption of the heart of dogs suffering from diabetes as the result of pancreas extirpation, proves that there is very little loss of glycolytic power. The internal secretion of the pancreas appears rather to prevent the over-production of sugar by the organism than to stimulate its consumption, so the experiments on glycolysis *in vivo* show no correspondence with those made with tissue juices and extracts *in vitro.*

It should be pointed out that in most of the observations on the glycolytic power of animal tissues the evidence of glycolysis consisted only in the disappearance of some of the sugar added. Still some investigators[3] found that CO_2 was evolved. Levene and Meyer[4] maintain that the disappearance of sugar on incubation with muscle plasma and pancreas extract is due to a condensation of the glucose molecule rather than to its degradation. They found that some of the sugar was converted into a biose, and that if the mixture were boiled with dilute acid its original reducing power was restored.

[1] Hall, *Amer. Journ. Physiol.* **18**, p. 283, 1907.

[2] Starling and Patterson, *Journ. Physiol.* **47**, p. 137, 1913.

[3] Cf. Arnheim and Rosenbaum, *Zeit. f. physiol. Chem.* **40**, p. 220, 1903, and Feinschmidt, *Hofmeister Beitr.* **4**, p. 511, 1904.

[4] Levene and Meyer, *Journ. Biol. Chem.* **9**, p. 97, 1911.

Evidence of another kind in favour of a glycolytic enzyme has been obtained by Weinland.[1] He found that not only did intestinal worms (*Ascaris lumbricoides*), when kept in the complete absence of oxygen, form CO_2 and valerianic acid at the expense of their store of glycogen, but that the expressed juice of the worms effected a similar decomposition on glycogen and glucose.

It will be gathered from this brief statement of the evidence that at present we cannot definitely assume the existence of a zymase in animal tissues, though we are probably justified in accepting the existence of other glycolytic enzymes. It is by no means improbable that zymases are present, but they may be even more unstable than the zymase of vegetable tissues, and so require special methods for their demonstration.

In addition to the hydrolytic decomposition of tissue constituents by enzyme action, it seems probable that in muscle, if not in other tissues, some of them can undergo sudden disintegration by intramolecular changes induced by an external stimulus. By means of a very delicate thermopile V. T. Hill[2] has shown that if a frog's sartorius muscle be kept in an atmosphere of nitrogen, the heat produced on excitation occurs only during, or shortly after, the mechanical response, and not during long periods after it. There are good grounds for thinking that the evolution of heat and mechanical energy are due to the sudden decomposition of an unstable chemical substance which forms lactic acid as its chief decomposition product. From his thermo-electric data Hill[3] calculates that this lactic-acid precursor possesses about 10 per cent. greater total energy than the lactic acid itself. Hence it cannot be glucose, as this possesses at most only 3 per cent. more energy than lactic acid. The precursor is not to be confounded with Hermann's "inogen," for this labile muscle molecule was supposed to break down into lactic acid, CO_2 and water, or to undergo combustion at the expense of intramolecular oxygen, which had previously been taken up and stored within it. So far from accepting this hypothesis Fletcher considers that there is no good evidence that the lactic precursor forms any CO_2 whatever at the moment

[1] Weinland, *Zeit. f. Biol.* **42**, p. 55, 1901; **43**, p. 86, 1902.
[2] V. T. Hill, *Journ. Physiol.* **42**, p. 29, 1911; **46**, p. 28, 1913.
[3] V. T. Hill, *ibid.* **44**, p. 466, 1912.

of its decomposition. Thus he showed[1] that muscles kept in nitrogen give out very little extra CO_2 on tetanisation for twenty minutes, the very small increase of output observed being due, in all probability, to the liberated lactic acid turning it out from the carbonates present in the muscle. Similarly the CO_2 discharged from muscle when caused to pass into rigor by heating to 40° C. is pre-existent CO_2 expelled from the carbonates by the lactic acid formed during the onset of the rigor, and if, previous to the induction of the rigor, the muscle is kept for several hours in nitrogen, whereby its CO_2 is expelled by the steadily accumulating lactic acid, there is practically *no* CO_2 given off.[2] In fact Fletcher considers that the increased CO_2 output which accompanies the contraction of muscles supplied with oxygen is not due to the combustion of the lactic acid at all. Other suitable material is burnt, and its oxidation supplies energy for the building up of the lactic acid into its precursor, which is then able, when required, to break down again with the evolution of heat and mechanical energy, and the re-formation of lactic acid. Thus Hill has shown that muscle, when kept in oxygen, gives out about twice as much heat on contraction as when kept in nitrogen, but the extra amount of heat is not evolved at the time of contraction. It is produced slowly during several minutes subsequent to it, and it is accompanied by the extra CO_2 output. Similarly Verzàr[3] found that the oxygen consumption of an active mammalian muscle may occur largely after the contraction is over, and Barcroft and Piper[4] showed that the oxygen consumption of the active submaxillary gland is considerably delayed.

Fletcher and Hill point out that their results lend no support to the hypothesis of intramolecular oxygen. In fact the interpretation they put on their experiments is diametrically opposed to it. The continued evolution of CO_2 by frogs, snails, worms, and other animals when kept in nitrogen was held by Pflüger, and has since been held by most other physiologists except Detmer and Winterstein, to be formed at the expense of intramolecular oxygen; but as I pointed out in my previous article,

[1] Fletcher, *ibid.* **23**, p. 10, 1898; **28**, p. 474, 1902; Fletcher and Hopkins, *ibid.* **35**, p. 247, 1907.

[2] Fletcher and Brown, *ibid.* **48**, p. 177, 1914.

[3] Verzàr, *ibid.* **44**, p. 252, 1912.

[4] Barcroft and Piper, *ibid.* **44**, p. 372, 1912.

it might arise entirely from the carbonates of the tissues and tissue fluids of the animals, which were gradually decomposed by the steadily accumulating lactic acid. This suggestion, so far as it applies to frogs' muscles, is supported by Fletcher and Brown's recent experiments; but it is difficult to believe that it holds for all other animals. For instance, Thunberg[1] showed that the snail, *Limax agrestis*, and the mealworm, *Tenebrio molitor*, when kept in nitrogen, gave out about 1,000 c.c. of CO_2 per kilogram, or five times more CO_2 than the frog does under similar conditions. Also I found[2] that if an excised mammalian kidney were perfused with oxygenless saline it gave out about 100 c.c. of CO_2 per kilogram. This CO_2 did not arise from the carbonates of the tissues, as they had already been washed out by a previous perfusion with oxygenated saline. Moreover, the addition of lactic acid to such saline led to no increase of CO_2 output.

Part of the large volume of CO_2 evolved from the anaerobic snails and worms is probably formed by glycolytic enzymes. Thus Lesser[3] found that when earthworms were kept in nitrogen they lost a large amount of their glycogen. In six hours at room temperature from 5 to 37 per cent. of it disappeared, and about half of it was shown definitely to be converted into CO_2 and a fatty acid, probably valerianic acid. Frogs kept in nitrogen lost on an average 17 per cent. of their glycogen. A small portion of it was converted into glucose, but what happened to the rest of it is unknown. Lesser thought that it could not have been oxidised to $CO_2 + H_2O$ by intramolecular oxygen, as he found that the heat produced by anaerobic frogs, per unit of CO_2 evolved, was only a third as great as that produced by frogs kept in air. Still this result affords no evidence as to the origin of the CO_2. Part of it was doubtless expelled from the carbonates of the tissues by the accumulating lactic acid, and with practically no production of heat. The remainder may have been formed partly at the expense of intramolecular oxygen or partly by glycolytic enzymes. In any case it is evident that the evolution of CO_2 by animals kept under anaerobic conditions does not, by itself, afford good evidence of the existence of zymase and other

[1] Thunberg, *Skand. Arch. f. Physiol.* **17**, p. 133, 1905.
[2] Vernon, *Journ. Physiol.* **35**, p. 53, 1906.
[3] Lesser. *Zeit. f. Biol.* **51**, p. 287; **52**, p. 282; **56**, p. 467, 1911.

glycolytic enzymes in animal tissues. It is necessary in addition to analyse the tissues before and after anaerobiosis, as Lesser has done.

The second respiratory mechanism to be discussed is that concerned in oxidation processes. Such processes are much more efficient than those of hydrolysis as a source of energy. For instance, the complete oxidation of sugar into $CO_2 + H_2O$ causes the evolution of nine times more energy than is formed by its conversion into CO_2 and alcohol. The mechanism of oxidation occurring in living tissues is probably very similar to certain chemical reactions which have been investigated *in vitro* of recent years, and it will be convenient to describe these first. It was shown by Fenton[1] in 1894 that hydrogen peroxide, in presence of a trace of a ferrous salt which acted as a catalyst, could oxidise tartaric acid even at room temperature. Cross, Bevan, and Smith[2] found that dextrose, lævulose, and other carbohydrates could be similarly oxidised, whilst Dakin[3] has shown that the reaction can be extended to a large number of fatty and other acids. Ammonium butyrate, for instance, is readily oxidised at body temperature to the oxybutyric acids. The α-acid is then broken down step by step to CO_2 and water. Propionic aldehyde and CO_2 are formed first, and the aldehyde passes in turn through the stages of propionic acid, acetaldehyde $+ CO_2$, acetic acid, formic acid $+ CO_2$, and finally $CO_2 + H_2O$. The β-oxybutyric acid is partly converted into aceto-acetic acid and acetone. These gradual oxidation processes hold for all the fatty acids right up to stearic acid, the higher members of the series being oxidised less easily than the lower members, but all of them forming CO_2, aldehydes, lower fatty acids, and ketones. Also amino acids such as leucin, alanin, and glycin are readily oxidised—alanin, for instance, giving $CH_3 . CHO + NH_3 + CO_2$. Hence one may say that all the chief classes of food stuffs, if first hydrolysed by appropriate enzymes, can be oxidised by $H_2O_2 + FeSO_4$, and that in many cases this oxidation is complete. Furthermore these oxidations can take place at body temperature.

As the result of their observations on vegetable enzymes,

[1] Fenton, *Journ. Chem Soc. Trans.* 1894, p. 899.

[2] Cross, Bevan, and Smith, *ibid.* 1898, p. 459.

[3] Dakin, *Journ. Biol. Chem.* **1**, p. 171, 1906; **4**, pp. 63, 77, 91, and 227, 1908; **5**, p. 409, 1909.

Bach and Chodat[1] came to the conclusion that two classes of oxidising enzymes exist, namely, *oxygenases*, or enzymes which take up molecular oxygen and become converted into peroxides or transfer it to other suitable organic substances with peroxide formation, and *peroxidases*, or activators which assist in the transference of this active peroxide oxygen to oxidisable substances. Hence oxygenases are directly comparable to H_2O_2, and peroxidases to activators such as $FeSO_4$. The existence of these two classes of enzymes, both in plants and animals, is generally accepted, though the evidence concerning animal peroxidases is rather contradictory. Almost all animal tissues, unless very thoroughly washed, contain hæmoglobin, and this substance can function as an activator just like other iron salts; but its activity is not much affected by heating, whilst that of true peroxidases is destroyed at about 60° C. Peroxidases are usually tested for the addition of H_2O_2 and guiaconic acid, which leads to the production of guiacum blue, but the test is not a safe one. Hydriodic acid is oxidised by peroxidases to free iodine in presence of H_2O_2, but this test is likewise uncertain in its action.[2] Battelli and Stern[3] found that in presence of ethyl hydrogen peroxide—which is preferable to hydrogen peroxide—peroxidases oxidise formic acid to $CO_2 + H_2O$, and the volume of CO_2 evolved affords a rough quantitative measure of the peroxidase present in a tissue. Liver proved most active of all, whilst blood came next and oxidised about half as much formic acid as liver, though it contained only the "pseudo-peroxidase" hæmoglobin. Kidney, lung, and spleen were about half as active as blood, whilst muscle and brain were only a quarter as active.

For testing the direct oxidases or oxygenases of the tissues a number of different colour reactions have been employed, but for the most part they have been used only qualitatively. As a quantitative method one of the best reactions appears to be the indophenol test, first used by Röhmann and Spitzer[4] in 1895. If solutions of α-naphthol and p-phenylenediamine be mixed in presence of sodium carbonate, oxygen is absorbed from the air and the purple indophenol is gradually produced,

[1] Bach and Chodat, *Biochem. Centralb.* **1**, p. 417, 1903.
[2] V. Czyhlarz and v. Fürth, *Beitr. z. chem. Physiol. u. Path.* **10**, p. 358, 1907.
[3] Battelli and Stern, *Biochem. Zeit.* **13**, p. 44, 1908.
[4] Röhmann and Spitzer, *Ber.* **38**, p. 567, 1895.

but if minced animal tissue be added, the rate of indophenol formation is accelerated ten- to a hundred-fold. By extracting the indophenol with alcohol and estimating its amount colourimetrically, a measure is obtained of the oxidase content of a tissue.[1] The writer found that in practically all the mammals and birds examined the heart muscle is richest in indophenol oxidase. Next in order come kidney, brain, liver, and lung. The oxidasic power seems, as a rule, to vary with the magnitude of the oxidation processes occurring in the tissues. Thus the grey matter of the brain contains at least three times more oxidase than the white matter, and the renal cortex ten times more than the renal medulla. The oxidase content of muscles is extremely variable. The pectoral muscles of vigorous flying birds are nearly as rich in oxidase as the heart muscle, whilst their leg muscles contain less than a third as much. The tame duck, on the other hand, has much more oxidase in its leg muscles than in its pectoral muscles. In mammals, red muscles such as the diaphragm and tongue, which are in a constant state of activity, are very much richer in oxidase than the white muscles. Taking the organism as a whole, its oxidasic power varies with the magnitude of its respiratory exchanges. For instance, the CO_2 output of the canary and sparrow is about eight times larger than that of the goose, whilst the oxidase content of their tissues is twice as large. The starling, pigeon, and duck have an intermediate CO_2 output and intermediate oxidase content. Of mammals, the harvest mouse has the greatest respiratory exchange and greatest oxidase content of its tissues, whilst next in order in respect of both qualities come the common mouse, the rat, and then larger mammals such as the rabbit, cat, dog, and ox.

At the same time it must be admitted that the apparent oxidase content of a tissue does not vary in absolute measure with its gaseous metabolism. One reason of this is that the oxidase reaction which is taken as a measure of the oxidase content of the tissues is to some extent interfered with by the reducing substances which are always present. These substances tend to absorb the available oxygen or abstract it from the indophenol blue formed, so that some tissues, such as the white muscles of certain animals, may fail to oxidise the indophenol reagent completely, though undoubtedly they

[1] Vernon, *Journ. Physiol.* **42**, p. 402, 1911 ; **43**, p. 96, 1911.

contain oxidase. Hence the method of estimating the oxidasic power of the tissues employed by Battelli and Stern[1] may ultimately prove the best one, though it has fallacies of its own. These investigators found that minced tissues have the power of oxidising succinic acid, $C_4H_6O_4$, to malic acid, $C_4H_6O_5$, with the absorption of molecular oxygen, and that as a rule the volumes of oxygen so absorbed closely correspond with those taken up by the tissues when mixed with *p*-phenylenediamine. This substance is probably oxidised in the same way as the indophenol reagent, and many of Battelli and Stern's determinations of oxygen absorption by various tissues show a rough correspondence with my estimations of indophenol formation: but some tissues, especially liver and skeletal muscle, show relatively much greater absorption of oxygen than production of indophenol, probably because they are specially rich in reducing substances.

At the same time it is doubtful how far the oxygen absorption of a minced tissue is a true measure of its oxidase content. For instance, Battelli and Stern found that minced brain, when mixed with *p*-phenylenediamine, absorbed two or three times more oxygen than when mixed with succinic acid, or to quote actual figures, 100 gm. of dog's brain when mixed with diamine absorbed 225 c.c. of oxygen in half an hour at 39° C., but when mixed with succinic acid absorbed only 85 c.c. Is this difference due to the presence of different and specific oxidases for each of the two substances, or is it due to the occurrence of oxidation processes other than that produced by oxidase action? It seems more probable that such additional oxidation processes do occur, and that one and the same oxidase is responsible for both these oxidations and for that of the indophenol reagent, for the oxidase of all these substances is quite insoluble in water or saline, and it is so thermolabile as to be entirely destroyed in half an hour at 60° C.[2] According to Battelli and Stern the oxidase is a single enzyme, and not comparable to the oxygenase + peroxidase of vegetable tissues. In any case the soluble peroxidase described above takes no part in its activities, but it is possible that it consists of an insoluble peroxidase bound up with an insoluble oxygenase.

[1] Battelli and Stern, *Biochem. Zeit.* **30**, p. 172, 1910; **46**, pp. 317 and 343, 1912.
[2] Vernon, *Journ. Physiol.* **44**, p. 152, 1912; Battelli and Stern, *Biochem. Zeit.* **46**, p. 343, 1912.

If we admit the existence of organic peroxides in animal tissues, it is evident that we accept to some extent the doctrine of intramolecular oxygen. It is true that the actual quantity of oxygen loosely bound up in the tissues in the form of peroxide may be extremely small, but such as it is it corresponds to the intramolecular oxygen of Pflüger and Hermann. Ehrlich[1] in 1885 made a number of interesting observations on *intra vitam* straining which seemed to support the doctrine strongly. He found that on injection of the somewhat stable alizarin blue into rabbits, a few tissues such as liver and lung reduced it to alizarin white, and so he concluded that they had a high degree of oxygen avidity, or possessed very little store of intramolecular oxygen. Other tissues, such as glands and some muscles, could reduce the less stable indophenol blue to indophenol white, whilst others, such as muscle of heart, tongue and diaphragm, renal cortex and cerebral cortex, failed to reduce either pigment. Hence he concluded that these tissues contained the largest store of intramolecular oxygen. Corresponding to these results, the writer found that all the tissues of this latter class are very rich in indophenol oxidase (*i.e.* organic peroxide), whilst those of the liver and lung class contain very little oxidase, and those of the intermediate class intermediate amounts. It seems very probable that there is some measure of truth in Ehrlich's hypothesis, but no final conclusions can be arrived at until it is found possible to estimate the exact share in the processes taken by the reducing substances of the tissues.

In addition to the insoluble indophenol oxidase, animal tissues contain several soluble oxidases which appear to be specific. The most striking of these is the uricolytic enzyme[2] or uricase which oxidises uric acid to allantoin and CO_2. This enzyme is present in considerable amount in the liver and kidney of all the animals examined except those of man, from which it is entirely lacking. Corresponding to this fact we find that human urine does not contain a trace of allantoin, whilst that of the dog contains most of its purin nitrogen in this form. Uricase, like indophenol oxidase, is not assisted in its action by peroxidase. It is not to be confounded with the oxidase, first described by Horbaczewski,[3] which oxidises hypoxanthin and

[1] Ehrlich, *Das Sauerstoff-Bedurfniss des Organismus*, Berlin, 1885.

[2] Cf. W. Jones, *Nucleic Acids*, London, 1914.

[3] Horbaczewski, *Monatschr. f. Chem.* **10**, p. 624, 1889; **12**, p. 221.

xanthin into uric acid. Another oxidase which is present in watery extracts of dog's liver has the power of oxidising β-oxybutyric acid to aceto-acetic acid,[1] and this aceto-acetic acid is in turn split up still further by some other enzyme. Still another soluble oxidase is able to oxidise ethyl alcohol to acetaldehyde and acetic acid.[2] However, it is extremely feeble in its action, 100 gm. of fresh horse liver oxidising only 0·1 gm. of ethyl alcohol in an hour at 40°, whilst it attacks other alcohols still more slowly. The so-called aldehydase described by Schmiedeburg and others is really not a true oxidase at all, but merely a hydrolytic enzyme which converts salicylaldehyde into equal quantities of saligenin and salicylic acid.[3]

Many invertebrate animals, such as lepidoptera, worms, and certain Cephalopods, contain a specific enzyme which oxidises tyrosin to a brown melanin. This oxidase is also present in the skin of the frog, whilst an enzyme can be extracted from the skin of rats and guinea-pigs which will oxidise tyrosin to various coloured pigments if a trace of iron sulphate is added as activator.[4] V. Fürth and Schneider[5] think that tyrosinase is responsible for the formation of melanotic pigments in all animals, but there is at present no definite proof of this hypothesis, except that Gessard[6] found tyrosinase in a melanotic tumour. In addition to tyrosinase, some animal tissues contain phenolases which oxidise guiaconic acid to guiacum blue, pyrogallol to purpurogallin, and effect other phenol oxidations, but they are not nearly so powerful or so widely distributed as in vegetable tissues. They are present in the hæmolymph and blood of various invertebrates, and in some of the tissues, whilst in vertebrates they seem to be confined to leucocytes and a few secretions as the saliva.[7]

It will be noted that the oxidations effected by the oxidising enzymes of the tissues are for the most part not nearly so powerful as those induced by $H_2O_2 + FeSO_4$. In one or two instances they led to the production of CO_2, but food stuffs such as carbohydrates and fats are practically untouched by oxidising

[1] Wakeman and Dakin, *Journ. Biol. Chem.* **6**, p. 373, 1909.
[2] Battelli and Stern, *Biochem. Zeit.* **28**, p. 145, 1910.
[3] Battelli and Stern, *ibid.* **29**, p. 130, 1910.
[4] Miss Durham, *Proc. Roy. Soc.* **74**, p. 390, 1904.
[5] V. Fürth and Schneider, *Hofmeister's Beitr.* **1**, p. 229, 1901.
[6] Gessard, C. R., **138**, p. 1086, 1903.
[7] Cf. Battelli and Stern, *Ergeb. d. Physiol.* **12**, p. 157, 1912.

enzymes. This may be because the protein and other tissue constituents with which the oxidases are inevitably mixed inhibit their activity, but it is natural that a doubt should arise as to whether the oxidases are really responsible for the main oxidation processes which occur in living organisms. In order to obtain further information on the subject, the writer[1] has made a number of comparisons of the respiratory powers and oxidase content of living tissues treated in various ways, in order to see how far they correspond. The experiments were made with freshly excised rabbits' kidneys, which can readily be perfused with oxygenated saline solution, and the gaseous metabolism of which can be estimated by analyses of the gases in the inflowing and outflowing saline. It was found that on exposure of kidneys to high temperature their respiratory powers and their oxidase content were destroyed to about the same extent. After half an hour in saline at 50° C. both were reduced to about half their initial value, whilst at 53° they were reduced to a fifth, and at 55° to about a tenth their value. Again, on perfusion of kidneys with saline containing 0·1 to 0·4 per cent. of lactic acid, or 1 per cent. of phenol, both respiration and oxidase were destroyed to about the same extent; but other poisons acted less destructively on the oxidase of the tissues than on their respiration. With chloroform the difference observed was moderately great; with mercuric chloride and ammonia more considerable, whilst with formaldehyde it was greatest of all. For instance, perfusion with 0·3 per cent. formaldehyde did not affect the oxidase at all, whilst it reduced the respiration to a seventh the normal. Perfusion with 0·5 per cent. formaldehyde destroyed 40 per cent. of the oxidase, but 92 per cent. of the respiratory power. Even more striking than this is the effect of mincing up the kidney tissue and keeping it two days. Its respiratory powers are thereby entirely lost, whilst its oxidase content is unaffected. There seems to be an unstable linkage of some kind in the chain of oxidation processes which is readily destroyed by poisons and in other ways, without the oxidases being necessarily affected at all. Perhaps this linkage is a lipoid membrane which binds together enzyme and substrate, and so permits the normal oxidation processes of the tissues to proceed. Thus the writer[2] found that on perfusing

[1] Vernon, *Journ. Physiol.* **44**, p. 150, 1912.
[2] Vernon, *ibid.* **45**, p. 197, 1912.

various concentrations of alcohols through kidneys, washing them out and estimating their respiratory powers and oxidase content, the respective concentrations of ethyl, propyl and butyl alcohols which first permanently injured the respiration were approximately those which lack red blood corpuscles. The oxidase was first affected by rather greater concentrations than these, but it was completely destroyed by the same concentrations which completely destroyed the gaseous metabolism. In addition to destroying respiration and oxidase, the alcohols caused great disintegration of the kidney tissues, so much so that 9 to 29 per cent. of the total protein contents of the kidneys were washed out during the half-hour's perfusion.

Apart from respiratory processes, however, the action of indophenol oxidase itself seems to depend on lipoid membranes. Thus the writer[1] found that whilst increasing concentrations of various alcohols and ketones, ether, chloroform, chloral hydrate, and other narcotics have, up to a certain point, no action upon the oxidase, at slightly greater concentrations they act destructively on it, and at about twice the concentrations which first affect it they destroy it completely. Now the concentrations of narcotics acting in this way bear a nearly constant ratio to the narcotising concentrations for tadpoles and other organisms, and as in accordance with the Meyer-Overton hypothesis it is generally supposed that such narcotics act by dissolving in the lipoids of the cells, it looks as if the activity of the insoluble indophenol oxidase is likewise dependent on lipoids.

Though it must be admitted that up to the present it is not definitely proved that the respiration of the tissues is dependent on oxidases, it can be shown that at least a part of it is independent of the continued vitality of the tissues. Battelli and Stern[2] found that if fresh muscle, liver, kidney, and other organs were minced up and were shaken with blood or saline in presence of oxygen, they had at first a considerably greater gaseous metabolism than when they formed parts of the living animal. This "chief respiration," as they called it, rapidly dwindled down and ceased in an hour or two. Doubtless it depended on the continued vitality of the cells and cell fragments,

[1] Vernon, *Biochem. Zeit.* **47**, p. 374, 1912; **51**, p. 1, 1913; **60**, p. 202, 1914; Battelli and Stern, *ibid.*, **52**, p. 226, 1913; **63**, p. 369, 1914.

[2] Battelli and Stern, *Journ. de Physiol. et de Path. gén.* 1907, p. 410; *Biochem. Zeit.* **21**, p. 487, 1909.

excited to abnormal metabolism by mechanical injuries, for the finer the state of division of a tissue the less is its respiratory power.[1] The respiratory processes did not cease altogether in an hour or so, but a smaller "accessory respiration" remained and might persist for some hours. The CO_2 output of this respiration was considerably smaller than the oxygen intake, and if the tissue had previously been heated to 70° it ceased altogether, though a moderate absorption of oxygen continued. This accessory respiration was due partly to soluble tissue constituents, for Warburg[2] found that if an aqueous extract of liver were filtered through a Berkefeld filter, it retained about 4 per cent. of the respiratory powers of the intact liver, whilst an unfiltered extract, containing minute particles in suspension, had about 20 per cent. the respiratory power. These extracts produced CO_2 in addition to absorbing oxygen, or exhibited a genuine respiratory process. Accessory respiration may continue for as long as three days in intact tissues, for the writer[3] found that small quantities of CO_2 continue to be evolved for this length of time by kidneys perfused with 1 per cent. NaF.

Freshly minced muscle, liver, and kidney are able to oxidise citric, fumaric, and malic acids, apparently to the $CO_2 + H_2O$ stage, but these powers appear to be bound up with the continued vitality of the cells, and cease when the "chief respiration" ceases.[4] This chief respiration is itself dependent to a considerable extent on soluble constituents of the tissues, for if freshly minced liver, kidney, or muscle is extracted with water it loses most of its respiratory powers, but it regains them on addition of the extract. These soluble constituents, called by Battelli and Stern[5] "pnein," probably consist for the most part of easily oxidisable organic acids. They are not destroyed by boiling, or by proteolytic enzymes.

It will be realised that the processes of tissue respiration which begin with the absorption of oxygen and end with the production of CO_2 and H_2O are of all degrees of complexity and possess

[1] Harden and Maclean, *Journ. Physiol.* **43**, p. 34, 1911.

[2] Warburg, *Arch. f. d. ges. Physiol.* **154**, p. 599, 1913; but cf. Harden and Maclean, *l.c.*

[3] Vernon, *Journ. Physiol.* **35**, p. 78, 1906.

[4] Battelli and Stern, *Biochem. Zeit.* **31**, p. 478, 1911.

[5] Battelli and Stern, *ibid.* **33**, p. 315, 1911.

many differences of character, and that it must be a very long time before we can hope to unravel all the intermediate stages. We have seen that under some conditions a considerable absorption of oxygen may occur without any contemporary production of CO_2, but in these experiments the minced tissue had lost its vitality. The writer[1] found that a loss of CO_2-producing power but continuance of oxygen absorption power can be induced in still living tissues. If intact kidneys be perfused for half an hour with saline containing 0·1 to 0·5 per cent. of formaldehyde, the oxygen absorption of the tissues is diminished to some extent, but the CO_2 production is diminished very much more, and for the first two hours or so after the poisoning respiratory quotients of 0·4 or less are obtained. Subsequently the kidneys gradually recover, and after ten hours' continuous perfusion may have nearly as great a CO_2 production and oxygen absorption as normal kidneys. Lactic and nitrous acids have an analogous though less marked effect, whilst alkalis likewise reduce the CO_2 production of the tissues more than their oxygen absorption, but do not permit of much recovery of respiration on subsequent perfusion. Other poisons, such as hydrocyanic acid, sodium fluoride, and acid sodium sulphite, reduce both the oxygen absorption and CO_2 production of the kidneys to the same extent, but after some hours' perfusion with fresh saline there may be complete recovery. Now all these substances are well known to have the power of entering into loose combinations with aldehyde groupings, so it seems probable that such groupings exist in living tissues, and that these poisons enter into temporary combination with them and so stop respiration processes.

From the foregoing brief account of the present state of our knowledge on the biochemistry of respiration, it will be seen that recent research points to its being in the main dependent on intracellular enzymes. In some organisms it is entirely a hydrolytic process, unaccompanied by any oxidation whatever, but in the great majority of organisms it is partly hydrolytic and partly oxidative. Whether the main processes of respiration in all organisms consist primarily of hydrolysis and only secondarily of oxidation cannot be definitely decided at present, but it seems very probable that in many of the

[1] Vernon, *Journ. Physiol.* 39, p. 149, 1909; 40, p. 295, 1910.

higher animals the hydrolytic changes which form such a marked feature in lower organisms have sunk largely into abeyance. The oxidation mechanisms have developed so enormously in the course of evolution and appear to have become so efficient that it is unnecessary for them to depend to any great extent on preceding hydrolyses. Just as hydrogen peroxide, acting *in vitro* in the presence of a catalyst, can oxidise fatty acids and other substances directly to CO_2 and water, so may intracellular oxidases effect similar direct oxidations within the cell. We have some experimental evidence in support of these direct oxidation processes, for Neuberg[1] found that animal tissues could oxidise glucose to glycuronic acid, and it seems probable that plants can oxidise it to malic and tartaric acids.[2]

[1] Neuberg, "Oppenheimer's Biochemie," *Ergänzungsband*, p. 569, 1913.
[2] Cf. Lesser, *Ergeb. d. Physiol.* **8**, p. 796, 1909.

SOME RECENT ADDITIONS TO OUR KNOWLEDGE OF THE GERM-CELL CYCLE IN ANIMALS

By ROBERT W. HEGNER, Ph.D.

Assistant Professor of Zoology in the University of Michigan, Ann Arbor, Michigan, U.S.A.

One of the most fruitful lines of research that has attracted the attention of biologists is the cytological study of the hereditary substance—the germ cells. Certain stages in the history of the germ cells have been especially emphasised, such as the maturation of the ova and spermatozoa. Other events in the germ-cell cycle have not, however, been slighted, although they have been rather overshadowed by the more popular study of the behaviour of the chromosomes. Ever since the publication of Weismann's studies on the germ-plasm theory there has been no lack of interest in the morphological continuity of the germ cells. A surprising number of important contributions have appeared within the past eight years dealing with the early segregation of the germ cells in embryological development, and it is this phase of the germ-cell cycle which I wish to discuss in the following pages.

According to the idea of the morphological continuity of the germ cells as first expressed by Jäger (1) and Nussbaum (2), and later elaborated by Weismann (3) and others, the germ cells are separated from the somatic cells at an early stage in the development of the egg, and they, and they only, are able to produce eggs and spermatozoa. These germ cells are protected, nourished, and transported by the body in which they lie; they later separate from the body and take part in the formation of new individuals, whereas the body dies. The origin of the germ cells has recently been definitely determined in a number of animals belonging to widely separated phyla, and while there are still some groups which have been carefully studied (such as the Cœlenterata and Porifera) in which an early segregation of the germ cells has not been demonstrated, there is no doubt concerning this process in certain insects, crustaceans, nematodes, and in *Sagitta*. Furthermore, in almost every thoroughly

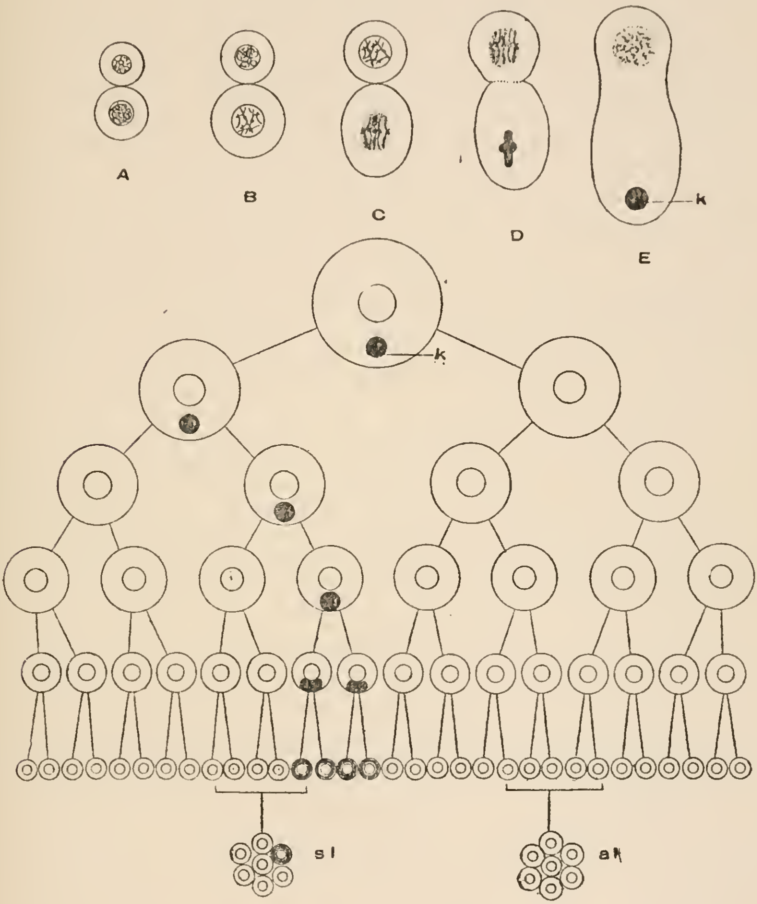


FIG. 1.—Diagrams illustrating the origin and history of the keimbahn-determinants in parasitic Hymenoptera.

A–E, the chromatin in the nucleus of the lower (older) oöcyte condenses to form keimbahn-chromatin (E, k); a spindle is organised in the upper oöcyte, but disintegrates to form a nucleus, and the two oöcytes unite (D and E), producing a single egg. (After Hegner.)

The diagram below indicates the history of the keimbahn-chromatin as described by Silvestri. Sexual larvæ (s.l.) develop from groups of cells, including one or more cells containing keimbahn-chromatin. Asexual larvæ (a.l.) arise from groups without any cells provided with keimbahn-chromatin. (After Silvestri.)

studied case inclusions in the cytoplasm enable us to identify the germ-cell substance in the undivided egg and to trace it until it becomes localised in the primordial germ cells. Such inclusions I have called "keimbahn-determinants," since they make it possible for us to determine the keimbahn with certainty during the early cleavage stages of the egg.

Although a large number of reports on the segregation of the germ cells were published during the latter part of the nineteenth century, including the well-known cases of *Ascaris* and *Cyclops*, we need not go back beyond the year 1906 for our illustrative material. In that year the first account of Silvestri's (4) researches on the development of hymenopterous parasites appeared. This was followed two years later by additional studies by the same investigator. The development of both monembryonic and polyembryonic species was worked out. In all cases, both in parthenogenetic and fertilised eggs, the ovum at the time of deposition contains near the anterior end a nucleus, and near the posterior end a body called by Silvestri the "nucleolo," and considered by him as a nucleolus (meta-nucleolus) which had escaped from the germinal vesicle. In the monembryonic species studied, early development proceeds as in the typical insect egg; the cleavage nuclei multiply rapidly and migrate toward the periphery, where they become cut off from one another by cell walls and form a single cellular layer. During most of this process the "nucleolo" remains intact at the posterior end. When the cleavage nuclei reach its vicinity, however, it disintegrates and becomes distributed in the cytoplasm of a few of the cells formed at the posterior pole. These, the primordial germ cells, were traced during subsequent development, and found to produce the eggs or spermatozoa of the succeeding generation. The cleavage processes are different in the polembryonic species studied. The first two cleavage nuclei are at once separated by cell walls, and the "nucleolo" is segregated in one of them. During several succeeding divisions the "nucleolo" is likewise limited to one cell. Finally, however, it disintegrates and becomes divided between two daughter cells, and later is distributed among four cells. These were not traced further, but it seems very probable that, as in the monembryonic species, the cells containing granules from the "nucleolo" may be considered as primordial germ cells which multiply and form part of the morula-like groups of cells from which the adult insects arise. Certain larvæ were found to lack several systems of organs, including reproductive organs, and were hence called sexless larvæ. The absence of germ cells within them is supposed to be due to the failure to obtain cells with material from the "nucleolo," *i.e.* germ cells.

The segregation of the germ cells in hymenopterous insects,

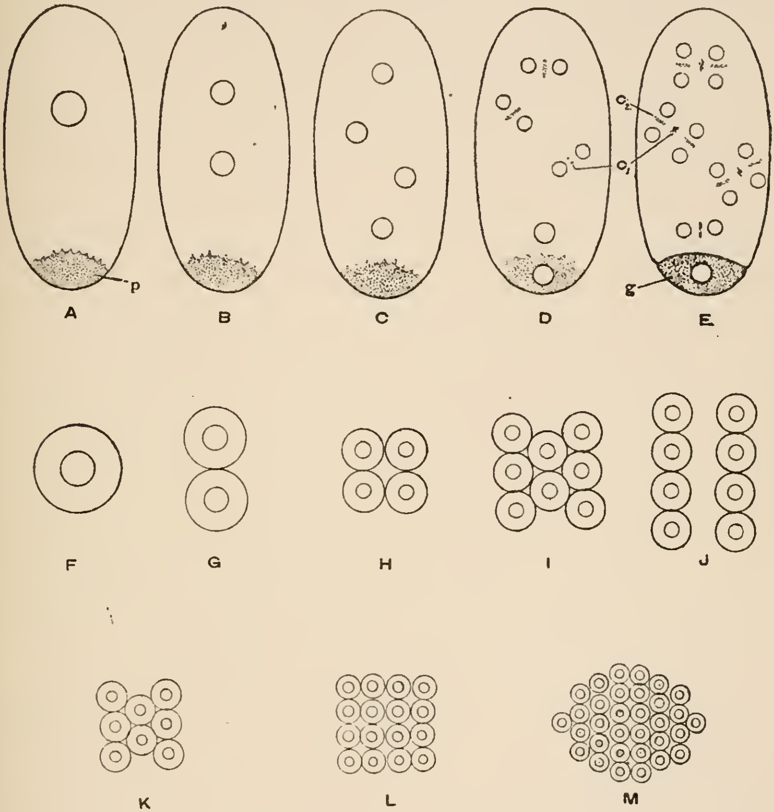


FIG. 2.—Diagrams illustrating the germ-cell cycle in Miastor.

A–E, stages in the formation of the primordial germ cell (g). F–M, stages in the history of the primordial germ cells. A, mature egg with cleavage nucleus and polar-plasm (p). B, two-cell stage. C, four-cell stage. D, eight-cell stage. C_1 = chromatin left behind in the cytoplasm after mitosis. E, fifteen cell stage. C_2 = cast-out chromatin after second diminution process. F, single primordial germ cell. G, two oögonia. H, four oögonia. I, eight oögonia in a single group. J, eight oögonia in two rows of four each. K, an ovary with eight oögonia. L, an ovary with sixteen oögonia. M, fully developed ovary with thirty-two oögonia. (After Kahle and Hegner.)

as just described, does not differ materially from that observed in many other insects, as we shall see later, but there are several points that need to be reinvestigated, particularly the distribution of the cells containing "nucleolar" material, the formation of the "sexless" larvæ, and the origin of the "nucleolo.'

The last-named problem has recently been solved by the writer (5). The "nucleolo" is not a nucleolus which has escaped from the germinal vesicle, but consists of chromatin. Because of its constitution and fate I have called it keimbahn-chromatin. All of the chromatin in certain young oöcytes becomes arranged as double rods on an asterless spindle. This spindle does not proceed to separate the chromosomes, but gradually condenses and shortens until the chromatin becomes a homogeneous mass in the form of a Greek cross. This cross later becomes approximately spherical, and usually occupies a position near the posterior end of the fully developed oöcyte. Oöcytes which possess keimbahn-chromatin are without a germinal vesicle. How the egg is provided with a nucleus appears to be as follows: Two oöcytes which chance to lie end to end in an ovarian tubule develop differently probably because of their relative positions; the spindle of the one posteriorly situated never completes its normal function, but disappears as the chromosomes condense to form the keimbahn-chromatin. The chromosomes on the spindle of the anterior member of the pair disintegrate and give rise to a nucleus. The two oöcytes then fuse end to end; one (posterior) furnishing the keimbahn-chromatin, the other (anterior) providing the nucleus. A number of stages were found in my material which indicate that such a process may occur. If further investigation proves this to be true, then every egg laid by these hymenopterous parasites consists of two oöcytes united end to end.

The development of the pædogenetic larvæ of *Miastor* furnishes us with the best example of the keimbahn in any animal. Kahle's (6) careful work on *Miastor metraloas* has been confirmed by myself (7) for *M. americana*. In the eggs of these species the keimbahn-determinants are present in the form of a deeply staining mass of substance (the "polares plasma" of Kahle) located at the posterior end, which becomes visible shortly before the maturation processes are initiated. One of the first eight cleavage nuclei fuses with the polar-plasm, and with it is cut off from the rest of the egg. This is the first cell formed, and is the primordial germ cell. It differs from all of the somatic cells in two important respects: (1) it contains all of the polar-plasm, and (2) its nucleus is provided with a complete amount of chromatin, whereas the nuclei of all of the somatic cells lose part of their chromatin during two diminution processes resembling those occurring in the blastomeres of *Ascaris*. Efforts made by the writer to determine the origin of the polar-plasm have thus far been fruitless. The primordial germ cell of *Miastor* passes through three divisions. The eight cells thus produced separate to form two rows of four each; these remain undivided for a long interval during embryonic development, but finally undergo a definite number of mitoses, namely three, resulting in two germ glands containing thirty-two oögonia each. There are thus in this genus a definite number of oögonial divisions (6), and it is no longer necessary for us to express our ignorance by stating that there are n divisions of the oögonia in the germ-cell cycle, since here $n = 6$. It is obvious that the original polar-plasm must be divided among the sixty-four daughter cells of the primordial germ-cell, each of which receives approximately one sixty-fourth of the original mass. During the growth period of these germ cells the quantity of the polar-plasm contained in the original mass is regained, *i.e.* the amount in the mature ovum equals sixty-four times that contained in the oögonium from which it developed. What causes this increase in amount, and where does the new substance come from? I (8) have suggested (1) that the polar-plasm may increase by autodivision of its particles during the growth period, or (2) that by its presence it may cause the production of new substances like itself; this activity ceases when an equilibrium point has been reached, as is known to occur in the case of enzymes, thus accounting for the rather definite quantity present in each generation.

An early segregation of the germ cells has been described in many other dipterous insects; of these perhaps the best account is that of Hasper (9) on *Chironomus*. Near the posterior end of the freshly laid egg of this midge is a layer of deeply staining granules, the "keimbahnplasma." One of the first four cleavage nuclei encounters these granules, becomes surrounded by them as with a halo, and is cut off from the rest of the egg; thus is formed the primordial germ cell. No chromatin diminution process was observed here such as occurs in *Miastor*. During the division of the primordial germ cell the granules of the keimbahnplasm appear to be equally distributed between the daughter cells. The origin of the granules was not determined.

The origin of the germ cells in certain chrysomelid beetles has been shown by the writer (10) to be similar in its main aspects. In several species of the genera *Calligrapha* and *Leptinotarsa* the posterior end of the egg differs from the anterior pole in the presence of a disc-shaped mass of granules which I first called the pole-disc, but later designated as germ-cell determinants, or keimbahn-determinants. The sixteen primordial germ cells (pole cells) originate at the time of blastoderm formation at the posterior end of the egg, and each becomes provided with a portion of the pole-disc.

These germ cells at first form a group outside of the embryo, but later migrate into the embryo and separate into two divisions, each of which becomes the basis of the germ glands on one side of the body. Experiments (11) have shown that if the posterior end of a freshly laid egg is killed with a hot needle, thus destroying the pole-disc and the substance in which it is suspended an embryo develops lacking sex cells. A similar operation after the primordial germ cells have been formed likewise results in embryos without sex cells. This is the earliest stage I believe in the development of any animal at which surgical castration has been performed.

Two groups of Crustacea are known to contain species which exhibit an early segregation of germ cells. Haecker's (12) investigations on *Cyclops* have recently been repeated by Amma (13) and extended to include species from the genera *Diaptomus*, *Canthocamptus*, and *Heterocope*. In these animals the stem cell, which at the thirty-two-cell stage becomes the single primordial germ cell, can be recognised by the presence within it of a mass of granules, the ectosomes, which appear at one end of the spindle during the mitotic division of the fertilised egg and at a similar stage in the four divisions of the stem cell. When the primordial germ cell divides, the granules are distributed apparently equally between the two daughter cells (oögonia or spermatogonia according to whether the egg would have developed into a male or female individual). In *Diaptomus cœruleus* the ectosomes appear in the egg before the pronuclei break down to form the first cleavage spindle.

Kühn's (14) researches on the summer egg of the Cladoceron, *Polyphemus pediculus*, show that in this crustacean the stem cell can be identified by the remains of one or several nurse cells. These nurse cells become embedded in the undivided egg, and later are always segregated in a particular blastomere until the sixteen-cell stage is reached. By this time the nurse cells have disintegrated into a mass of granules which become scattered throughout the cytoplasm of one cell, the primordial germ cell.

No recent morphological investigations of Nematodes have

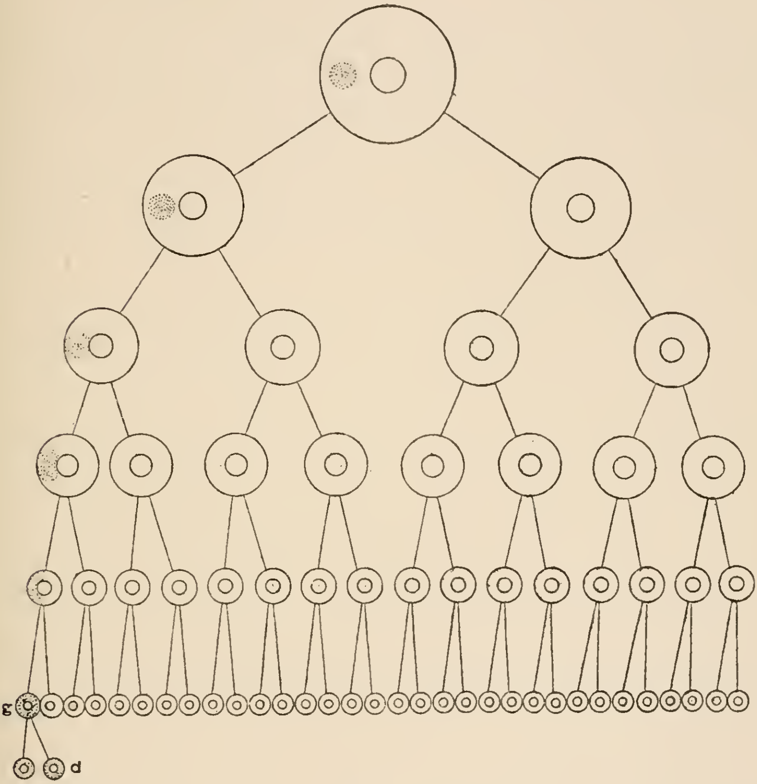


FIG. 3.—Diagram illustrating the segregation of the primordial germ cell in copepods. The egg contains a group of granules (ectosomes) which are gradually localised in one cell (g), the primordial germ cell. The descendants of the primordial germ cell (g) are apparently all provided with an equal amount of ectosomes. (After Haecker and Amma.)

added materially to our knowledge of the early history of the germ cells in this group, but the experimental results of Boveri (15) and Hogue (16) are of much interest. Studies of dispermic and centrifuged eggs have proved that the chromatin-diminution process in *Ascaris* eggs is not controlled by the nucleus, but by the other egg contents. It follows from this that the origin of the primordial germ cell, which always arises from the cell with the complete amount of chromatin, is also determined by the cytoplasm.

Because of the fact that it is hermaphroditic, the early history of the germ cells in the arrow worm, *Sagitta*, is of peculiar interest. In the mature egg of this animal, Elpatiewsky (17) discovered a peculiar body (the "besondere Körper") which becomes segregated in one cell (the stem cell) until the thirty-two-cell stage. The cell containing it may then be identified as the primordial germ cell. When this cell divides, the "besondere Körper" is apparently unequally distributed between the two daughter cells, one of which is considered an oögonium, the other a spermatogonium. Stevens (18) and Buchner (19) have confirmed Elpatiewsky's results, but whether or not the division of the primordial germ cell is differential has not yet been definitely determined.

Many recent reports have been published upon the germ cells of vertebrates, and for a time it seemed from the researches of Rubaschkin (20) and Tschaschkin (21) that the new methods devised for the purpose of staining the mitochondria might aid us in solving the problem of the origin of the germ cells in this group; but later investigators, such as von Berenberg-Gossler (22) and Swift (23), have been unable to discover any distinction between the mitochondria in the germ cells and those in the somatic cells of vertebrate embryos. Dodds (24) has found in the teleost, *Lophius*, an inclusion in the cytoplasm of the germ cells in young embryos which he thinks may be of nucleolar origin, and Swift has recorded an especially large attraction sphere in the earliest recognisable germ cells of chick embryos, so there is still ground for the hope that some method may yet be found for tracing these cells to their true origin in the vertebrates.

In the cases cited above, the stem cell, the primordial germ cell or cells, and the early oögonia and spermatogonia are visibly different from the other cells of the embryo because of the presence of inclusions in their cytoplasm. In only a few species do we know the genesis of these keimbahn-determinants. In the parasitic hymenopteron, *Copidosoma*, they consist of all of the chromatin from an oöcyte nucleus. In chrysomelid beetles they may consist of chromatin, or may be derived from the food stream. Haecker and Amma consider the ectosomes of the copepods to be by-products of metabolism, and Kühn has shown that in the Cladocera one or more nurse cells act as keimbahn-determinants. The writer (25) several years ago suggested two views as to the probable significance of these inclusions; first, they may actually represent the idioplasm, and second, they may consist simply of nutritive materials. Even now it is impossible to state definitely the rôle they play in the germ-cell cycle. We do know, however, that they are peculiar to the germ cells of a number of species belonging to groups that are diverse and widely separated in the animal series, that they are definitely localised in the egg and often in a certain blastomere (the stem cell), and that they disappear some time after the primordial germ cells have been segregated.

Most zoologists recognise the distinction between germ-plasm and somato-plasm and consider the germ-plasm to be situated within the germ cells. According to this view the primordial germ cell must contain all of the germ-plasm of the developing organism. The keimbahn-determinants are especially important, since they are in some way associated with the germ-plasm before this substance is segregated in the primordial germ cell. It thus becomes possible to determine the position of the germ-plasm in the stem cell and in the undivided egg, or, in the case of many insects, during the early cleavage stages before the blastoderm is formed. One fact that seems more and more certain as the results of investigations accumulate is that the cytoplasm cannot be ignored when the physical basis of inheritance is under consideration. My morphological and experimental investigations of chrysomelid beetles seems to prove that the nuclei of the developing eggs are all alike until the blastoderm is formed and that differentiation is controlled by the cytoplasm, the germ cells being produced at the posterior end where the pole-disc granules are situated. Similarly in copepods, Cladocera, and *Sagitta*, a substance definitely localised in the egg and stem cells and recognisable because of the presence of keimbahn-determinants seems to determine the genesis of the primordial germ cell. Boveri's experiments with *Ascaris* likewise furnish valuable data which indicate that the nucleus does not initiate the process which results in the formation of the primordial germ cell, but that chromatin-diminution is likewise controlled by the cytoplasm.

Literature

1. Jäger, G., 1877, Physiologische Briefe, *Kosmos*, Bd. 1.
2. Nussbaum, M., 1880, Zur Differenzierung des Geschlechts im Thierreich, *Arch. mikr. Anat.* Bd. 18.
3. Weismann, A., 1904, The Evolution Theory. London.
4. Silvestri, F., 1906 and 1908, Contribuzioni alla conoscenza biologica degli Imenotteri parassiti, I.–IV. *Boll. Scuola sup. Agric. Portici*, vols. i. and iii.
5. Hegner, R., 1914, Studies on Germ Cells, III. The Origin of the Keimbahn-Determinants in a Parasitic Hymenopteron, Copidosoma, *Anat. Anz.* Bd. 46.
6. Kahle, W., 1908, Die Pædogenese der Cecidomyiden, *Zoologica*, vol. 21.
7. Hegner, R., 1912, The History of the Germ Cells in the Pædogenetic Larva of Miastor, *Science*, vol. 36; 1914, Studies on Germ Cells, I. and II. *Journ. Morph.*, vol. 25.
8. —— 1914, Studies on Germ Cells, I. and II. *Journ. Morph.*, vol. 25.
9. Hasper, M., 1911, Zur Entwicklung der Geschlechtsorgane von Chironomus, *Zool. Jahrb.* Bd. 31.
10. Hegner, R., 1908, The Effects of Removing the Germ-Cell Determinants from the Eggs of Some Chrysomelid Beetles, *Biol. Bull.* vol. 16; 1909, The Origin and Early History of the Germ Cell in Some Chrysomelid Beetles, *Journ. Morph.* vol. 20.
11. —— 1911, Experiments with Chrysomelid Beetles, III. The Effects of Killing Parts of the Eggs of *Leptinotarsa decemlineata*, *Biol. Bull.* vol. 20.
12. Haecker, V., 1897, Die Keimbahn von Cyclops, *Arch. mikr. Anat.* Bd. 45.
13. Amma, K., 1911, Über die Differenzierung der Keimbahnzellen bei den Copepoden, *Arch. f. Zellforsch.* Bd. 6.
14. Kühn, A., 1913, Die Sonderung der Keimbezirke in der Entwickelung des Sommereier von *Polyphemus pediculus* De Geer, *Zool. Jahrb.* Bd. 35.
15. Boveri, T., 1910, Die Potenzen der Ascaris-Blastomeren bei abgeänderter Furchung, *Festschi. R. Hertwig.* Bd. 3.
16. Hogue, M., 1910, Über die Wirkung der Centrifugalkraft auf die Eier von Ascaris megalocephala, *Arch. Entwick.* Bd. 29.
17. Elpatiewsky, W., 1909, Die Urgeschlechtszellenbildung bei Sagitta, *Anat. Anz.* Bd. 35.
18. Stevens, N. M., 1910, Further Studies on Reproduction in Sagitta, *Journ. Morph.* vol. 21.
19. Buchner, P., 1910, Keimbahn und Ovogenese von Sagitta, *Anat. Anz.* Bd. 35.
20. Rubaschkin, W., 1910, Chondriosomen und Differenzierungsprocesse bei Sangetierembryonen, *Anat. Hefte*, Bd. 41.
21. Tschaschkin, S., 1910, Über die Chondriosomen der Urgeschlechtszellen bei Vögelembryonen, *Anat. Anz.* Bd. 37.
22. Berenberg-Gossler, H. von, 1912, Die Urgeschlechtszellen des Hühnerembryos, *Arch. mikr. Anat.* Bd. 81.
23. Swift, C. H., 1914, Origin and Early History of the Primordial Germ Cells in the Chick, *Am. Journ. Anat.* vol. 15.
24. Dodds, G. S., 1910, Segregation of the Germ Cells of the *Teleost*, *Lophius*, *Journ. Morph.* vol. 21.
25. Hegner, R. W., 1911, Germ-Cell Determinants and their Significance, *Amer. Nat.* vol. 45.

THE EXTINCT APES AND THEIR BEARING UPON THE ANTIQUITY OF THE HOMINIDÆ

By A. G. THACKER, A.R.C.Sc.
Curator, Public Museum, Gloucester

It is a matter of common knowledge that although the great discoveries of the last sixty years have swept away all the old conceptions of the recent origin of man, those discoveries have not thus far provided anything approaching a definite solution of the problem of the age of mankind. Indeed, the divergence of opinion among scientists is probably greater to-day than it has ever been. And my object in this paper is to approach the question from a point of view which appears to have been somewhat forgotten of late, although recent discoveries tend to emphasise its importance. I propose to deal with the antiquity of the stock from which mankind is believed to have arisen.

At the outset it should be noted that the phrase "the antiquity of man" is a highly ambiguous one; it may mean either of two very different things. On the one hand the expression may refer to the date of the origin of the existing species of man—true man—whilst on the other hand it may denote the length of time which is supposed to have elapsed since our ancestors ceased to be arboreal and became mainly ground-living creatures, with the consequent transformation of the hinder hands into feet; for the foot is the chief peculiarity of the Hominidæ, and hence any ape-like being who possessed feet could lay some claim to humanity. This distinction is germane to my present subject, because, firstly, the known antiquity of our own species is a very different thing from the known antiquity of the four fossil species of the human tribe, and because, also, we must recognise that whilst the existence of apes in any given period has virtually no bearing upon the antiquity of real man, it has a most important bearing upon the probable date of the appearance of those half-human creatures who were his forerunners.

It is of course obvious that on the Darwinian theory of continuous evolution there can have been no sudden appearance either of the Hominidæ or of true man. No doubt the theory of mutation, on the other hand, implies that the stages in the progression were fairly clearly marked off from each other. Widely different though these rival hypotheses are, yet for the purpose of the present discussion the differences are not of great importance. We have to suppose that the Hominidæ and the Simiidæ have originated from a common ancestor, which closely resembled both families in its anatomy. The much-discussed common ancestor was certainly biologically near both to man and to the chimpanzee, though not necessarily geologically near. Indeed, the creature in question would probably have been correctly included in the Simiidæ—which is not to say, of course, that any known member of the Simiidæ, fossil or otherwise, is ancestral to man.

Now it follows from this that the presence of Simiidæ in any period implies the possible existence of primitive Hominidæ slightly later. I say advisedly "slightly later." The differences between the Simiidæ and the Hominidæ are altogether trivial, compared with the vast range of mammalian structure. Hence, in terms of geological time, Hominidæ may have appeared very soon after the higher apes, even if the Darwinian theory be true. Whilst if the mutation theory be correct, the interval might be even shorter. For instance, a quarter or a third of the Pliocene would appear to be sufficient interval, whatever be the true theory of evolution.

As already stated, anthropologists differ very widely on the subject of human antiquity. The more cautious school give true man only a small fraction of the Pleistocene, limit themselves to paleolithic implements (ranging over about the latter two-thirds of the Pleistocene), and the most they will concede is that humanoid beings may possibly have existed as early as the Late Pliocene. The extreme school, including Keith, Reid Moir, Rutot, and others, trace *H. sapiens* far back into the Pleistocene, discover Pliocene and Miocene eoliths, and place the origin of the human tribe in the Miocene or even in the Oligocene. Rutot believes in Oligocene eoliths, and Keith[1] recently stated that the Javan ape-man was a type which

[1] *Bedrock*, January 1914.

probably came into existence "in the Miocene or even an earlier epoch."

Without discussing the direct evidence, I may perhaps say in passing that I happen to agree with those who are highly sceptical of eoliths, and that I think the alleged proofs of the great antiquity of *H. sapiens* are well-nigh valueless. I shall, therefore, not be accused of prejudice in favour of the extreme views. But when the extreme advocates of the opposing theory —Boyd Dawkins, the late W. H. Sutcliffe,[1] and others—set out to ridicule the attempt to find evidence of Miocene Hominidæ on the *a priori* ground that what is styled the highest mammal could not have existed when proboscidians had primitive teeth, deer only simple antlers, and horses three toes, it appears to be time to call a halt in the process of destructive criticism. If we had no more direct clue to the problem, the argument from the general evolution of the Mammalia would be legitimate, though singularly inconclusive; but since we have fossil apes to guide us, to discuss elephants, deer, or horses is illogical and misleading. The older school may or may not be right in thinking that there were no Hominidæ before the Late Pliocene, but they ought to base their conclusion either on the evolution of the Primates (which, however, lends no support to their doctrine), or else merely upon the absence of direct evidence of the man-tribe from earlier strata, and not upon the evolution of a totally different order of mammals. The point need not be laboured, for though it has been forgotten, it will not be disputed. It is true that a considerable amount of mammalian evolution has taken place since the Miocene, but such evolution is almost confined to the hoofed mammals and perhaps the seals, and it would of course be quite an error to suppose that the different groups of mammals have diverged from the common stock at the same speed. For instance, we believe that all the placental mammals have arisen from common ancestors, small insectivore-like quadrupeds, since the end of the Mesozoic era, and it is noteworthy that the two orders which have diverged most from the ancestral type, to wit, the bats and the whales, were already fully evolved before the end of the Eocene, and are therefore astonishingly ancient animals

[1] W. H. Sutcliffe, "A Criticism of some Modern Tendencies in Prehistoric Anthropology," in *Memoirs and Proceedings of the Manchester Literary and Philosophical Society*, vol. 57, part II.

when judged by equine or elephantine standards. I will revert to this matter shortly, in describing the diagram, but it is clear at once that the rate of evolution in one order is no criterion whatever of the rate of evolution in another.

Fossil remains of apes—I use the word "ape" in its strict confined sense, referring only to the Simiidæ—have been dug up from time to time during the last sixty years, but unfortunately the remains are of a very fragmentary character. It is necessary to insist upon this point. Simian relics have been found in various strata from the Oligocene to the Pleistocene, but the relics consist in most cases merely of lower jaws, and, indeed, two genera have been founded on the strength of a few teeth. It is important, therefore, to speak of the Oligocene and Miocene apes with all due caution. For aught we know to the contrary, these ancient creatures may have been nearer to their ancestors the Old-World monkeys (Cercopithecidæ), in various points of their anatomy, than are the Simiidæ living now. When all allowance is made, however, for this element of uncertainty, there remains at least a high degree of probability that the ape-like relics do really represent animals which were more ape-like than monkey-like.

The simian fossils may be briefly enumerated, without entering into great anatomical detail, which is unnecessary for the purpose in hand. Indeed, the phylogenetic significance of the morphological details has not thus far been worked out with any thoroughness, although a most interesting contribution to the subject was recently made by Dr. Smith Woodward.[1] The reader may be reminded that the lower apes, collectively known as gibbons, probably stand nearer to the common ancestor of the Simiidæ and the Hominidæ than do the higher apes—the chimpanzee and its kin, which seem to represent a more divergent twig of the phylogenetic tree. The notoriously gibbonoid characters of the lowest of the known Hominidæ, the Javan ape-man, are thus explicable. Perhaps we should call the common ancestor a gibbon if we could meet him in the flesh. We should therefore expect the gibbons to be more ancient than the higher apes, and this is now proved to be the case.

Nothing is known of the history of the gorilla, or of that

[1] See paper read to the Geological Society on April 29, 1914.

rare African ape described by Giraud Elliot as the pseudogorilla, but the remains of a species of chimpanzee and of an orang have been recovered from the Lower Pliocene of India. This fact alone is impressive enough, but the higher apes as a group are much older than the chimpanzee. Several great apes lived in Europe during the Middle and Late Miocene and during the Pliocene. The best-known genus is of course that famous animal *Dryopithecus*, which is known from jaws discovered in the Middle Miocene of France, and from another mandible, or rather one ramus of a mandible, found recently in the Upper Miocene, near Lerida, in Catalonia. This last specimen has just been described and figured by Prof. Vidal,[1] of Barcelona. These specimens are massive jaws, resembling those of the chimpanzee, but slightly more primitive in various details. Isolated teeth have also been found, proving that *Dryopithecus* survived until the Early Pliocene. An imperfect humerus, of slender form, was discovered in the same stratum as the French jaws, and has naturally been associated with *Dryopithecus*. The arm-bone is more slender than one would expect in the owner of the jaws. It is usual in the Primates to find that great bony development in one part implies a similar development in all parts—witness, the gorilla and Neandertal man.

Perhaps the most striking of all simian fossils is a solitary femur, found in the Lower Pliocene at Eppelsheim, in Hesse Darmstadt. This bone, which is wonderfully well preserved, is longer than the corresponding structure of the gorilla, but is much more slender and more man-like in form. This bone, too, has been attributed by some writers to *Dryopithecus*, but in all the circumstances it is safer to retain the distinct generic name, *Paidopithex*. The German femur is much longer than the French humerus, but no doubt that is because it belonged to a larger animal—whether really a species of *Dryopithecus* or not. *Paidopithex* was certainly a taller creature than the chimpanzee, and was much lighter and more man-like in build than any of the great apes now living. Two other genera, *Griphopithecus* and *Anthropodus*, have been established to include certain teeth found in the Miocene and Pliocene of Austria and Germany. These are the only known fossils of the greater

[1] "Nota sobre la presencia del Dryopithecus en el mioceno superior del Pirineo catalan." In the *Boletin de la Real Sociedad espanola de Historia natural* for December 1913.

apes, but they are sufficient to prove that the Simiidæ were virtually fully evolved by the Middle Miocene.

And when we pass to the gibbonoid group we find, as we should expect, that they are still more ancient animals. All the living gibbons are of course usually included in one genus, *Hylobates*. This same genus dates from the Miocene. Various jaws and teeth have been recovered from the Miocene and Pliocene of France, Switzerland, and Austria; and although the first specimen, which was discovered more than sixty years ago, was placed in a new genus, *Pliopithecus*, most writers agree now that the distinctions are too slight to justify a separation from *Hylobates*. There are, however, several fossil species, of which the best known is *H. antiquus*.

This is all that was known of the extinct gibbons until very recently. *Hylobates* could be traced back along with the greater apes to the Middle Miocene, but no farther. It was clear, however, that the primitive gibbonoid stock must be older than the great apes; it was to be inferred that primitive apes existed at least as far back as the Lower Miocene. And that such an inference would have been sound is now triumphantly demonstrated by the discovery of the two rami[1] of a small ape's jaw in the Oligocene of Fayum, Egypt. The two rami were broken away from each other by a fracture across the region of the symphysis, and all the incisors and one canine are missing, but the two halves clearly belong to the same jaw. The specimen has been described in great detail by Prof. Max Schlosser,[2] of Munich, the leading German authority on fossil apes. The jaw is closely similar to those of the so-called *Pliopithecus*, and hence the animal has been named *Propliopithecus* by Schlosser, a name which unfortunately becomes meaningless if we no longer retain the title *Pliopithecus* for the Miocene gibbons.

Propliopithecus chiefly differs from *Hylobates* in having extremely small canine teeth, which give the dentition a fortuitous resemblance to that of man. Schlosser hazarded the suggestion that the genus may be the common ancestor of all the Hominidæ and all the Simiidae. Without making that

[1] In the paper already referred to, Sutcliffe erroneously states that only one ramus was found.

[2] See " Beiträge zür Paläontologie und Geologie Oesterreich-Ungarns und des Orients, *Mitteilungen des Geologischen und Paläontologischen Institutes der Universität Wien*. Band xxiv. Heft 11, p. 52.

claim, the existence of this gibbonlike animal in the Oligocene is necessarily of great theoretic importance. Among other things, it implies still greater antiquity for the Old-World monkeys, although no older Cercopithecidæ have yet been discovered.[1]

It is not difficult to picture what these facts mean. The great apes are not new denizens of the jungles. When Europe was tropical and forests grew in Greenland, the immediate

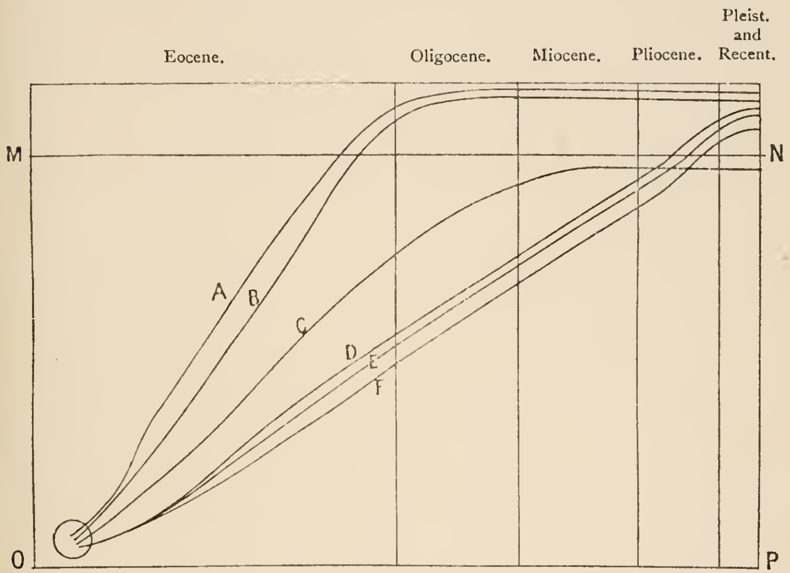


Diagram showing the evolution of certain groups of placental mammals.

A = line leading to whales, B = to bats, C = to Primates *minus* Hominidæ, D = to deer, E = to elephants, F = to horses, M–N = climax-line, O–P = base-line of placental evolution.

kindred of the chimpanzee were in existence. They listened to the trumpetings of the dinotherium and the mastodon, and they watched the three-toed hipparion browsing in the glades. Furthermore, the manlike mammals had virtually reached the point in their evolution now represented by the siamang at a time when the ancestors of the elephants had not even acquired their trunk, when the deer were quite hornless, and long before there were any animals that we should think of recognising as horses. Just as bats and whales antedate the apes by long ages,

[1] The oldest known Old-World monkey is *Parapithecus*, found in the same stratum with *Propliopithecus*.

so do the apes antedate by a great length of time many groups of our familiar ungulates. Apes, as apes, have witnessed the entire drama of elephantine and equine evolution.

This difference between the times at which different groups have reached, or virtually reached, the termination of their evolution may, I think, be brought out clearly in a diagram. The diagram is constructed upon a principle which is a variant of what I have seen used elsewhere in geology, and which is, moreover, almost identical with that applied by mathematicians in plotting a curve on squared paper. The vertical lines mark off the geological periods from one another, and the rising lines represent the progress of the several groups towards the termination of their evolution, which they virtually reach when they pass the second horizontal line of the diagram. I say "virtually" reach, because the evolution of characters of specific and generic value never ceases of course. There can of course be no pretence of mathematical accuracy. The method merely consists in attempting to visualise the evolution of each group as revealed by paleontology and then expressing the conception in a curved line. The extreme cases of the whales and horses illustrate the principle best, and it would be still more forcibly brought out by another diagram of the entire geological record showing the brachiopods and birds diverging from their common ancestor of Algonkian times, the brachiopods reaching their climax in the Cambrian and the birds continuing to evolve until the Cainozoic. The diagram has of course certain imperfections; it expresses only part of the truth. Thus, any kind of evolution, whether progressive or degenerative, is represented by ascension, and the height of the lines fails altogether to convey an impression of the relative distances which the various groups have travelled from their common ancestor. It should be noted, however, that these relative distances, if they could be shown, would have no connection with the points at which the climaxes are reached, since no mammals are so divergent as the whales. The varying widths of the five columns are intended to express the relative duration of the periods. This, it must be admitted, is scarcely better than guesswork, but I have followed Penck's estimates for the later periods. The ape-line does not pass above the climax-line because it is intended to represent the Primates *minus* the Hominidæ. In spite of its imperfections, I think a diagram constructed on this principle can express aspects of the truth that are completely masked by the ordinary geological diagram, such as that used by Sutcliffe in the same discussion.

Now what bearing have these facts of simian paleontology upon the vital question of the antiquity of the man-tribe? There is no indubitable inference, but there are certain probabilities. We know that by the Middle Miocene there had been time for one branch of the gibbonoid stem to grow far out in the direction of the chimpanzee. We suppose that man's latest arboreal ancestor was a somewhat gibbon-like creature, more intelligent, with shorter arms, and probably larger than the living gibbons. Such an animal may well have been contemporary with *Dryopithecus*. Or are we to believe that the branch which had so much the farther to grow, was also much the later to originate? It may be so. It is not inconceivable that those first expeditions to the ground, which were so pregnant with destiny, were not carried out until the Late Pliocene. But if that be so, there was a long pause in the upward evolution towards man: through the long ages of the Middle and Upper Miocene and the Lower Pliocene everything was ripe for the advent of the man-tribe, and yet none appeared.

Whatever weight attaches to the negative evidence—the fact that no remains of the Hominidæ have been found in the Lower Pliocene or in the Miocene—tells in favour of this view. But negative evidence is of course notoriously inconclusive in geology, and the possible proofs would be limited to bones, if so be that the ground-dwellers were slow in learning to chip stones with sufficient cunning to make their artificial character recognisable.

However this may be, the lesson of simian paleontology is clear. If there were no Hominidae in the Late Miocene, our ancestors tarried strangely long in the trees.

SCIENCE AND THE SUPPLY OF FINE COTTON

By W. LAWRENCE BALLS, M.A. (Cantab.)

Lately Botanist to the Khedivial Agricultural Society of Egypt and to the Egyptian Government

In the preface of a work on cotton published in 1835 may be read the following remarks: "The manufactory, the laboratory, and the study of the natural philosopher, are in close practical conjunction. Without the aid of science, the arts would be contemptible; without practical application, science would consist only of barren theories, which men would have no motive to pursue. These remarks apply . . . above all, to the Cotton Manufacture of England, which is the very creature of mechanical invention and chemical discovery, and which has, in its turn, rendered the most important service to science, as well as increased the wealth and power of the country."

We pass over an interval of nearly forty years, and from another standard work on cotton we extract a less optimistic view: "There is a great danger of resting satisfied with the present condition of things, especially when trade is pursued with the sole object of making money, and without the introduction of a spirit of pride in the attainment of better and higher results. That careful attention to detail which always marks the true workman must be carried by us into the recesses of all our manufacturing processes. Knowledge and science may enable up to do this without a material increase in the cost."

Lastly, from a recent communication by one of the first authorities in the ranks of the cotton-spinners, published in the present year of grace, we take the frank statement: "The spinner has not been accustomed to look for causes; all that he has cared for in the past has been the results. Would the cotton spin the counts he wanted? Did it give him the strength and cleanliness desired? Were these results achieved with the waste loss to which he was accustomed? . . . If cotton is to be developed in the future on scientific lines, some attempt must be made to define the characteristics required by spinners."

The sources of these quotations are respectively Baine's *History of the Cotton Manufacture*, Bowman's *Structure of the Cotton Fibre*, and an article "Some Thoughts on the Development of New Cottons," by Mr. J. W. McConnel, in the *Textile Mercury*, March 1914.

It is a noticeable feature of the three excerpts just made that their chronological order is exactly the reverse of what their statements ought to indicate. If we allow a great deal for the increased stringency of specialisation, it still remains that whereas the natural science of its day had a part to play in the cotton trade of the early nineteenth century, that part has steadily diminished in importance on the spinner's side of the trade, whatever its fate may have been in manufacturing.

The rapid extension of the trade, its increasing output, and consequent haste, have left little or no time or interest to spare for such refinements as were not capable of yielding direct profits. Some fifteen years ago the present limits of the existing sources of supply came into sight, and the development of new fields for cotton-growing became an urgent matter. The world has now been surveyed for this purpose, and the limits of productive area are fairly well defined. That which has not been defined is the productivity of these areas, and so long as a good field of cotton in Egypt can produce eight hundred pounds of lint to the acre, while the U.S.A. actually averages less than two hundred pounds, and India less than one hundred, there must be room for investigations leading to direct profit, in the many related branches of natural science and of economics which converge upon this colossal trade.

Nor is the yield alone implicated in this increasing pressure of events which are beginning to force the trade back into relationships with science. Of scarcely less importance is the question of quality in the raw product. The spinner is finding new demands and new markets opening before him, small and narrow at first, but steadily increasing. The coming fleets of the air will demand supplies of delicate yet trustworthy cotton fabrics in quantities which were scarcely dreamed of a generation ago. In proportion as the financial interests involved in these finer branches of the trade increase, so will the spinner begin to wonder more and more whether it is really necessary that he should have to remove the present amounts of waste fibre from the cotton he purchases. He will be driven to follow the lead of the thoughtful members who are already considering whether natural science cannot give them a better and more dependable supply, the extra cost of which would be covered by less expense in the preparatory stages of spinning.

The trend of affairs is to indicate therefore that natural science will before long be called upon—indeed, the call has already been made—to link together the widely separated ends of the cotton trade, the grower and the spinner. The task will not be an easy one, nor will it be done in a year. The two persons who ultimately typify the grower and spinner are respectively a skilful but uneducated native, working simply for the maximum profit with the minimum of trouble, and a highly skilled operative, cramped by trade unionism and working within a specialised series of operations by rule of thumb. When transport facilities were such that the employer of one could not reasonably expect ever to see the work of the other, much had to be taken for granted, or admitted as inevitable, which now demands explanation or reform.

We must not jump to the conclusion that all responsibility for the past half-century of uneventfulness lies with the spinner, though if circumstances had made it worth his while to formulate his demands in a form which bore some relation to the exigencies of cotton-growing, more progress might have been made quietly. The grower has also a large share of responsibility to bear, or—since he is personally low down the social and intellectual scale—his advisers must bear it for him. Bodies of various kinds, official and private, have come into existence of late years, as the realisation of the coming needs began to dawn, but their concern has naturally been to meet the immediate urgency for greater output on the one side, or to raise prices on the other. This urgency chimes with the interests of the countries concerned, who find in cotton, or in an increased output of it, a source of extra revenue. Many economic considerations render rapid expansion impossible, even in areas which are capable of growing good cotton, the chief of these being the need for cheap labour, and even the favoured areas will soon find it necessary to meet the increasing labour cast by producing cotton of higher value or of greater yielding-capacity. Increased production may in its turn depress the price, but a steadily increasing demand for fine cotton may be confidently expected.

The circumstances on both sides of the trade have thus been unfavourable to the development of intimate scientific investigations on cotton, as distinct from the immediate utilitarianism of economic work. While reserving some further remarks on general method until later in this article, those scientists who have been concerned with cotton investigations of late years, and the botanists in particular, may well ask themselves a few questions as to the drift of their work, especially as to whether our suggestions pay, or in other words, are our results sufficiently comprehensive to be simply and cheaply applied?

Much has been written and spoken about the improvement of cotton, but "improvement" is a very wide term. It will possibly be of some service to put forward a single definite object, which the most advanced investigations at all parts of the trade are settling upon as a prime necessity, for a hundred different reasons. This object is simply Uniformity.

An unopened bale of cotton has potentialities which are almost infinitely variable, according to the source from which it is derived. The length of its fibres may range from 15 to 50 mm., their breaking-strain from 1 to 10 grams, the diameter, the colour, the pitch of the twist, the lustre, flexibility, elasticity, and a number of other minor properties (which at present seem to be incapable of definition) all cover a wide range. Even within the best bale of any one kind there are great differences from hair to hair. Although many of these differences are not obvious to the untrained eye or hand, though some of them can only be differentiated by persons born with a natural aptitude for the task—and some are only capable of final resolution by the actual spinning-behaviour of the cotton—yet they are all real. An expert in Alexandria can often assign a lump of cotton to the village of Egypt in which it was grown.

Many of these characters by which the value of a sample are estimated are, nevertheless, merely indices, and not essentials. A remarkable example of this came under the writer's notice in connection with his work in Egypt on the isolation and propagation of pure strains of cotton. Some samples from the first year of propagation in bulk, grown under conditions which were not strictly those of field cultivation, but markedly inferior, were reported upon by the highest local authorities on commercial cotton. Their report was based on the handling of the lint, and stated that in the case of two out of three sets of ordinary cottons examined, the lint was perhaps rather inferior to that which the equivalent commercial varieties would produce. The samples, it should be noted, were "ugly," owing to the cultivation and treatment they had received. The same cottons were then sent by the writer to Lancashire, where they were put through spinning tests. The results of these tests surprised those who made them, the ugly ducklings turning out to be veritable swans, equal to high grades of the equivalent commercial varieties.

The reason is obvious. A pure strain gives a more uniform product, its only irregularities being those due to cultivation and fluctuation. When an impure commercial variety—and all commercial varieties are impure—is cultivated, as these samples were, its appearance is equally discounted, and its natural irregularity is intensified. Thus, an "ugly" sample of a pure strain is equal to a good-looking sample of commercial cotton, and judgments passed upon pure-strain cotton by its external appearance are necessarily far below the truth.

If a committee of the best experts can have their deductions so completely upset by merely substituting a pure strain for a commercial one, it follows that Uniformity is the first object for science to provide for the spinner. There is no need to elaborate the point, since nobody now denies the impurity of commercial varieties, nor has the spinner made any secret at any time of his desire for regular cotton; but it may be mentioned in passing that evidence is beginning to indicate that many other properties of the cotton lint which are separated under other names can be in part referred to this property of Uniformity.

For the rest of this article we can confine our attention to the main issue, as to the steps which science and the trade should take mutually in order to increase the yield and improve the uniformity of the product in those lands which can or do grow cotton crops.

These steps naturally classify themselves into three groups: improvements in the seed-supply, in the culture of the plant, and in the utilisation of the raw material. The spinners may be trusted to take care of the last, in the future as in the past, if only in self-interest. Seed-supply of pure strains would seem to have great possibilities, as we have just seen, and it is eminently suited to control the result of native labour. Without skilful cultivation, even the most perfect pure strain is useless, though not so useless as an impure variety, and for the supply of fine cotton, good cultivation is of equal importance with good seed.

Seed-supply

Some useful work has been contributed from several parts of the world on the facts of genetics in cotton, but so far as the fine-cotton supply is concerned, the matter has scarcely begun to take economic shape. Indirectly, Mendel's law of Gametic Segregation has been of immense use, allowing us to tread firmly where we should otherwise have been lost in uncertainties. The proof of extensive natural crossing in field crop may be cited as one result which could not have been obtained in pre-Mendelian days, and the isolation of pure strains is made straightforward through this law.

Work in genetics is primarily synthetic, however, and the two examples quoted are purely analytic. That synthesis of super-plants of *Gossypium* will be capable of prescription before long, no one can doubt; outside the fine-cotton sphere a beginning has been made by Mr. Leake. The matter is more complex when the effect of the plant-body upon minute characteristics of the lint-hairs has to be considered, still more complex when those characteristics are indefinable by the only persons who can see them, and practically hopeless when it becomes necessary—as it actually is—to grow several acres of any extracted strain, and handle it on a commercial scale, before its value can be determined.

The very magnitude of the trade with which the cotton-breeder deals is a hindrance rather than a help. The trade is so immense that it cannot afford to alter its methods, modify its machinery, and reorganise its market for the sake of a new cotton, however good and cheap that cotton might be, until its permanent supply is assured. The most which the breeder can do is to introduce cottons which typify the ideal of existing varieties. If such a cotton is introduced, it stands a very good chance of being disregarded through seeming the same as the existing varieties.

Apart from directly applicable research, there is great need for such investigation as would be conducted in academic circles, merely using cotton as the subject when practicable instead of some other plant. It is only by such steady effort that complete success can be obtained. Unfortunately, cotton is one of the least suitable plants for genetic investigation, and any university working with it would require heavy subsidies, which in their turn would demand results. When each plant requires a square metre of ground, when every flower used for seed has to be artificially prevented from crossing by bees, and when all the field work must be carried out in a sub-tropical climate, the genus cannot be regarded as a convenient one for the purpose, even if we disregard the trouble which ginning involves in the handling of pedigree seed.

The author had the privilege to experience several years of such work under the Khedivial Agricultural Society of Egypt, during which time his efforts were in part directed to analysing a few crosses between Upland and Egyptian cottons. The first few results gave the impression that the interpretation of cotton hybrids on Mendelian lines would be an easy task. Then, as the size of the families grew, and other crosses were examined, cases which had seemed simple showed themselves to be complex. Gametic reduplication, reversion on crossing, and similar phenomena were found at every turn, these being in no way abnormal or non-Mendelian, but each in itself raising matter for a separate piece of research. The task was evidently more than one worker could handle, and simpler crosses between Egyptian strains were made to elucidate the previous records; these in their turn frequently showed complexities of no mean order. The latter work was carried on, but in order to satisfy economic demands which had arisen in the meanwhile, a simpler attack, and application of data previously obtained, was made by the isolation of pure lines. At the end of nine years' work the first deliberate synthesis of a desired type of cotton had just been accomplished, in one family only.

It is difficult under these circumstances to see what possibility there is of rapid development in the study of Genetics in Cotton. The maintenance cost is too great for any university, in proportion to the results obtained; the time and skill required are too great to make such work a paying proposition for any commercial body; the immediate urgencies of the economic situation can be satisfied by the simpler work of preparing and testing pure strains from existing stocks by a refined rule of thumb.

The prospects of science in this direction are thus not very hopeful. Not because it will not be advantageous in the long run, nor because it will not ultimately be necessary that such knowledge shall be at the disposal of the trade, but because it will not pay at present.

A reservation might be made in the case of such genetic investigations as bear upon natural selection, and interweave with studies on the improvement of the environment. To effect analyses of the phenomena which are grouped together under such designations as "change of seed," "varietal deterioration," and so forth, will be of economic value.

Direct results may be expected from study and improvement of the environment, to which a longer discussion may be given.

Conditions of the Environment

When a dense population of plants is occupying a piece of land, they protect one another from extremes of climatic changes, and to some extent make their own climate.

It is partly owing to this that the cultivation of good cotton is possible in Egypt. The fellah plants his cotton at a density of population which appears absurd, but which is nevertheless exactly correct; a century of sub-conscious experiment has led him to customs which take up a position of delicate equilibrium between the ideal result and the practical limitations. Many of his customs which have been stigmatised as untidy, bad farming, and so forth, have been shown of late years to fulfil his crucial test; they pay him.

This brings us to the first essential to any work on the improvement of the environment; it must pay. In other words, generalisations on which such improvements are founded must be sufficiently sweeping to be simple of application. Little pieces of legislation are irritating and expensive to administrate.

Foremost among such improvements stands the provision of irrigation water, and this may be regarded as an ultimate essential. Some of the best cottons of to-day are grown on a rain supply of water, but fine uniform cotton in the quantities of which the world is beginning to find the need, can only be grown with a controlled supply of soil-water. It should be noted that a controlled supply of soil-water is not the same as a controlled supply of water; it implies not only irrigation, but drainage.

The remainder of this article will therefore be still further restricted in scope, not merely to the best cottons, but to cotton grown under irrigation.

The primary need which confronts any person concerned with the development of cotton-growing in a new district, is the need for knowing how the plants behave under optimum conditions. This statement is a very commonplace one, but the knowledge is practically unobtainable at present. We have first to ascertain the optimum conditions in general terms, and then to ascertain how the plants react to them.

A part of this question also concerns the grower of cotton in an established area. The optimum conditions have presumably been found there by trial and error, but economic botany has rarely effected such an analysis as shows exactly how the plants behave.

Lastly, there is the purely scientific inquiry into the causes of such behaviour. The economic investigator should carry his analysis of existing conditions down to the point at which they resolve into general problems. To study the physiology of growth on the lint-hair cells of cotton is not likely often to be practicable; it is quite practicable to study the growth of lint-hair cells under existing field conditions sufficiently minutely so that results obtained by the study of a fungus in another continent can be directly utilised to interpret the phenomena observed in cotton.

Taking the two economic issues only, viz. determination of the optimum conditions, and recognition of the reactions of the plant to those conditions, there is every prospect of a great and rapid advance in the application of science to cotton-growing.

As regards the determination of optimum conditions. The development of statistical methods, with the concurrent analysis of the errors to which experimental plots are subject, the devise of small plots and systematic scatter, and the use of graphic methods, have combined to make it practicable to determine the optimum conditions of cultivation for a given year on a small area, and at small cost. Whether repetition on the same scale is necessary for several years will depend partly on the climate of the area, and partly on the extent to which the causes of success or failure can be analysed by such methods as those to which we shall next advert. Such a set of small plots would include five of every kind, and would be sown at five different dates, with plants spaced in five different ways, and with water-supply or manures varied similarly according to the practical requirements of the district. Such work is worth doing even in an established area, on account of the insight it gives into local customs.

As regards the recognition of the reactions of plants to the environment, some simple methods devised by the writer make this much less difficult. A series of plants, varying in elaboration of plot-arrangement according to circumstances, are put under daily observation for such features as their rates of growth, flowering, and fruiting. Graphs are constructed from the data thus obtained, from which the causes affecting the yield can be deduced, by parallel plotting of all the known environmental changes. The continuous fruiting of cotton makes it a peculiarly difficult crop to analyse without some such method. The mere figures for yield mean very little, since the same final result may be reached in an infinite number of different ways. If we can not only ascertain exactly how the yield was produced, day by day, but also trace back the fruits to their origin as flowers, and the flowers to their origin as buds on the scaffolding of flowering branches, we have resolved the agricultural problem into components which the botanist can deal with.

It will be seen from the previous remarks that the writer's bent is towards intensive work, as promising to yield the best results in the future application of science to the cotton trade. This raises a very big discussion: whether the support given to scientific work on cotton shall be devoted to "survey" work, in which much is left to the intuition and subjectivity of the observer, and the integrated agricultural phenomena are observed as such; or whether intensive work, arriving at deductions by more minute methods, with detailed analysis of phenomena, shall be the accepted system.

There is certainly this much to be said for the intensive workers, that the survey work has had a long innings. Moreover, while intensive work can partially replace survey, by instructing the surveyor where to go and what to look for, the converse does not hold good.

On the other hand, intensive work may be held to be insufficiently related to practical needs, for while the behaviour of the plants on one particular experiment station may be interesting, the cultivator who is making his living out of his cotton crop wants to know what to do with his own particular field. The writer ventures to think that intensive work will of itself destroy this objection; in the event of such a query being made, accompanied by a set of daily observations of flowering and fruiting, it is now possible in Egypt to answer the query with very fair exactitude without moving from the desk.

The question of cost scarcely enters into the matter. The extra equipment which intensive investigation demands is covered by the lessened travelling expenses and by the smaller scale on which the work is carried out. The financial arrangements for intensive work should probably be more elastic than for survey, since expenses are bound to vary much more from year to year, according as the direction of research veers to minutiæ or to masses.

Lastly, although botany, in the form of plant physiology and genetics, is the science chiefly concerned in the study of cotton, it must not be forgotten that when dealing with a huge commercial product of this kind there are no definite limitations. By transitions which are not merely natural but inevitable, botanical investigations on the cotton crop find themselves stretching out into contact and collaboration with other branches of natural science. Physics and physical instrument-making, meteorology, human physiology, statistical mathematics, not to mention such obvious connections as geology and chemistry, all come into intimate relation with the botanical aspect of investigations. To make the most of opportunities requires collaboration, which is not likely to be available except in centres where intensive work is being pursued. The matter seems, in fact, to return again and again to the same suggestion, that such science as that for which commercial interests are now beginning to feel the need, is best to be obtained through a university organisation rather than through appointments in applied science.

The indications thus pointing to intensive work as the task of the future, it remains to be seen how this can be best applied to provide the uniformity of lint for which the spinners of cotton are beginning to ask. The study of fine cotton on irrigated land can be effected in two ways: primarily by study in the field, and in laboratories situated in the field; secondly, by investigations carried out on cotton material in laboratories situated in the temperate zone. To recommend the second method is frankly dangerous. Many of the errors which have been perpetuated in the scientific literature of cotton are due to conclusions drawn from the abnormal behaviour of plants grown in greenhouses. Nevertheless, provided always that the scope of such work was severely restricted to its proper functions, an immense amount of valuable research might be effected, and at a price which would be less than if the same work had to be done abroad, though the industry could afford to pay for it at a rate which would tend to raise the standard of payment for research in general.

Taking first the work which must of necessity be carried out in the cotton-fields, with suitable equipment, one essential must be fulfilled, if any real efficiency is desired, namely, residence of the workers on the spot. To fulfil this condition in the equipment of an experiment station may seem extravagant, but those who have attempted intensive research on crops in a sub-tropical or tropical climate will know that it is not only a prime necessity but also economy. Its fulfilment is not by any means easy in many cases, since land in such a position as to be readily in contact with a large town, and with the other scientific workers of the country, cannot always be obtained at a reasonable price. Nevertheless, the history of applied science contains so many examples of money, time, and labour thrown away by unsuitable localisation and equipment of Experiment Stations, that the two desiderata of residence and accessibility might with advantage be laid down in future as a part of the definition of such a station, without which it would be frankly entitled either a demonstration plot or a farm.

The first purpose of work done in such a station, beyond the studies in genetics and seed-supply with which we have already dealt, would be the precise recording of the existing crop. Such records should be made a piece of annual routine, and a portion of the work of the station should be organised to deal with them uninterruptedly. The aim of such records should be to eliminate all accidental influences, other than those which were common to the whole district in which the station was situated, and so to provide for each year a set of standard data. By linking up small observing-stations in outlying districts to the main one, with observers (not necessarily skilled) in charge of them, these records might be made to show the state of the crop over a whole continent. Such a presentment in numerical or graphic form, with defined precision, day by day, would in itself be of more than casual interest, but its value obviously does not remain at the level of mere crop-reporting. After a few years of such data had accumulated, and as the connection of their variations with conditions of the environment were worked out, they could be used for crop-forecasting. Much of the life-history of any plant is recapitulation of previous events, and this is remarkably so in the case of irrigated cotton, where practically the only uncontrollable variant in the environment is temperature. Whether the cost of organising such a system would offer a profitable return in the form of certainty and security remains to be seen. Much of the cost would be economised in the lessened need for travelling expert reporters on crop conditions, and the cost need not be enormous when administered as a branch of the normal activities of an experiment station.

The systematic records of existing crops having provided the investigators with definite information as to the reactions which the crop is making with the environment, the task of analysing these reactions becomes prominent. It may not always be practicable to attempt to withhold the incidence of some limiting factor throughout the season, under the conditions which obtain in the average fields of the country, but there would seem to be many cases—even in an old-established country like Egypt—where control can be exerted for a few days longer than would be practicable by rule of thumb.

This leads on to the classification and study of environmental factors in two fairly distinct groups: controllable factors and uncontrollable factors.

Of the controllable factors the chief are the soil and water, with insect pests, while atmospheric temperature and humidity may be modified to some extent by the spacing of the plants. In the matter of water-factors our views have undergone considerable revision since the deep draught of the cotton root was recognised, and the changes in water-content of the soil at depths down to three metres are now known to bear very direct relation to the production of good cotton. Insects and fungi may be classified as controllable or uncontrollable, according to their kind, and to the circumstances.

The factors of the environment which are uncontrollable are climatic. Some mitigation of their effects may be attained by methods of cultivation, or by choice of pure strains which suit the climate. When this has been done there remains a residuum of climatic effects which may modify the crop from year to year profoundly, but are beyond human control. The utility of crop-records and forecasting becomes apparent in this connection, since to be forewarned is nearly as good as to prevent. Once the physiological work has progressed sufficiently far to be used in the forecasting system, much of the objection to erratic climate will disappear, since the trade can be warned in advance by one to four months before the cotton reaches the quays of the port of export.

The connection between knowledge of the operation of environmental factors, and the production of uniform cotton may perhaps require some explanation, before passing on to consider such research as could be effected away from the cotton-fields.

Cotton is in many respects a somewhat exceptional plant, or at least it appears to be so at present. It is most successful commercially in circumstances where it is rendered almost inert during every afternoon of the season. Under these severe climatic conditions some forms of "self-poisoning" are very easily induced in its cells. We shall recur to these auto-toxic phenomena shortly, but meanwhile it may be noted that such actions as delayed irrigation may lead to enfeebled growth in all parts of the plant for some time afterwards, the length and thickness of the lint being affected incidentally, and spoiling the regularity of the cotton picked. So frequent are these phenomena that the writer has ventured to define the aim of a cotton cultivator as "a fight against self-poisoning." The plant has to be worked as nearly to the danger-points as is possible without transgressing them, if good results are required.

An accurate knowledge of what is dangerous, and what is not, will certainly be of use in guarding against these internal troubles. Moreover, it is not improbable that the whole specificity of such a character as the length of the fibre may be cognate with them, and that further study may enable us to produce uniform cotton of different kinds from the same pure strain by controlling its environment, so as to maintain a constant degree of ill-health.

Apart from these internal problems of growth there is also the obvious direct play of external limiting factors on the growth and functions of the plant. The aim in this connection of the scientist who would be a cultivator, is to maintain the growth-rates of the lint-hairs at the same mean daily velocity day in and day out, either by maintaining a uniform control through a single limiting factor of growth, or by controlling first one factor and then another, when it in its turn becomes limiting, so as still to keep the growth-rates constant.

The problem of the production of uniform cotton on a pure strain is essentially very simple. Flowers open day after day, set fruit, and the lint-hairs sprout on the seeds; these hairs grow up to their full length, provided that there is no internal poisoning, and subsequently thicken to their full strength, with the same reservation. The full strength and length revealed at the end of the seven weeks of maturation depend on the environmental conditions, acting directly, or indirectly through the nutrition of the plant on which the fruits are borne. If all the flowers opened and ripened simultaneously, the production of a uniform sample would be simple, but since they continue to open for a space of two months, the environmental conditions on any given day will affect the length in young fruits, and the strength in old ones, and each of these again in varying degree according to their age. If we confine our attention to the two characteristics of length and strength (as tested by breaking-strain of single fibres), and neglect all the minor features, it is still easy to prepare a most complicated sample of cotton. If, as is actually the case in good cultivation, the mean length swings steadily up and down about 5 per cent. in fortnightly oscillations, while the strength changes as much as 40 per cent. in the same way (but in fruits which are some three weeks older), the result cannot be a uniform series of fruits from day to day, even if plant-to-plant fluctuation is excluded. The aim of the grower is to smooth out these oscillations, so that the fruits of several successive days may all produce the same kind of lint, and it is in this respect that any physiological work will find a direct application.

The obvious answer to the spinner's request for more regular cotton is thus the recommendation to grow pure strains, pick the fields at short intervals, and sort the pickings according to class. The real answer is a counter-inquiry as to whether it will pay to do so. The extra labour involved in more frequent picking, the trouble and skilled advice required in classifying, go far to neutralise the advantages, and it may well be the case that it would cost less to continue to take out the short fibres by carding and combing. This is, nevertheless, a very practical and simple reform, but not so simple as some method which should enable any cultivator to watch some feature of his plants as the locomotive driver watches his pressure-gauge, and so keep the growth-processes constant within the ripening fruit.

This last may appear to be a somewhat far-fetched comparison, but so much precision has been imported into cotton affairs of late years, that such watching has actually become practicable as laboratory technique, and simplification may be confidently expected.

The reader may notice that this discussion has paid very little attention to the damage wrought by insects and fungi. This is partly because such damage is usually definite, and its effects on the plant are easily ascertained. Partly also because it is unnecessary to emphasise the economic importance of these plagues; much attention has been paid to them, and is still being paid. In the stress of the struggle against some pestilent insect one is apt to forget that it can only destroy that which the plant has constructed or would construct. All the damage done to the Egyptian cotton crop by the "boll-worms" in the worst year on record could be restored by the ripening of four additional fruits on each cotton-plant in the country.[1] That insects and fungi are important components of the environment, as "modifying" factors, no one would attempt to deny; that disproportionate amount of investigation—though by no means an excessive amount—has been devoted to them, as compared with the plants they act upon, is equally undeniable. The reason is very

[1] As this statement regarding the "boll-worm" may appear exaggerated, and as the general point at issue is rather important, the numerical basis may be stated. The average crop of Egypt is lately about 450 lb. of lint per acre. Land giving this yield, with the usual sowing of 12,000 holes per acre, or 24,000 plants, averages 16 bolls per plant (contents of each boll weighing 2 grams, or about 0·7 grams of lint). Total crop of Egypt is about $7\frac{1}{2}$ million kantars, thus produced by sixteen bolls per plant; four additional bolls would give nearly $9\frac{1}{2}$ million kantars; even liberal assessment of damage from boll-worm does not reach 2 million kantars.

simple; an insect devouring a plant is relatively easy to handle and study; molecular and colloidal changes in the protoplasm are not.

Investigations in Temperate Climates

We now turn to those branches of scientific work which could be better and more cheaply conducted in temperate climates, away from the cotton-fields, and in closer touch with the spinners on the one hand, and with learned societies on the other.

Such work would naturally fall into two categories, firstly, determination of constants for the reaction of various species of cotton to definite changes in a single environmental condition, and secondly, analysis of the technique of spinning in its relation to the raw cotton.

No very definite statements can be made as to the possibility of applicable results from the first category of researches. Indeed, such research should not be conducted with any immediate economic object. Many structural features may be recognised, involved, and eliminated in studying the growth of cotton, but there ultimately remain some phenomena which can only be ascribed to the protoplasm itself. This residuum seems to analyse up into the specific velocity of growth under conditions when temperature is the limiting factor, and into liability to senescence or auto-toxic effects, under conditions involving prolonged exposure to excessive heat, deficient aëration, or shortage of water-supply. To obtain any direct evidence upon only the first of these possibilities is not very easy; it involves the construction of special apparatus, and it demands continued series of observations and facilities for their conduct, such as are only obtainable in temperate zones. It is necessary for such research that the root itself should be exposed to high temperatures, but it is not necessary that the observer should also be exposed.

Whether any utilitarian advantages would accrue from an understanding of these temperature-growth phenomena or not, they are nevertheless of general interest, and would appear to be fairly fundamental. If it were possible, as it well may be, to prescribe the suitable temperature locality for a new pure strain by taking its Growth-Temperature Diagram, a great deal of tedious work might be economised through the elimination of field trials in those localities which were obviously unsuitable.

Apart from such specific problems, the modern study of the physiology of growth is in its infancy, and much may be expected of it. To carry out such work on cotton-seeds as a supplement to similar work on beans, sunflowers, etc., needs only such collaboration as would supply suitable seeds from known strains of cotton.

As regards the possibilities of investigation into the physical properties of the lint and their relation to spinning-processes, it is obvious that this can only be done in the spinning-mills and in laboratories annexed to them. In this respect the spinners may yet find it necessary to adopt the attitude of the brewers and develop the testing-room into a centre of technical control. It is not so obvious, or it does not seem to have been so obvious in the past, that such work needs to be co-ordinated with the grower also. A complete examination of a bale of commercial cotton, tracing the modifications which it undergoes in its passage through the mill, both in the properties of the average lint-hair, and in the relative proportions of the various hairs, would in itself be an interesting piece of work which—to the best of the writer's belief—has not yet been fully accomplished. To do the same thing with two bales of two different pure strains, or of the same pure strain grown in two different sites, would not merely be interesting, but would be a contribution towards the establishment of some generalisations as to the relation between yield and quality, as to the meaning of quality, and as to the direction which steps for the improvement of quality should take.

Such investigations, co-ordinated between spinner and grower, would follow some such directions as these: study of the internal structure of the wall of the lint-hair, linking up with chemical and physical researches already extant; invention of indirect methods for determining properties of the raw cotton with more ease and less tedium than is at present necessary, so that the time and labour required for precise determinations should not be so enormous as to make their employment ludicrous, when the expert grader can obtain fairly good results in a few seconds; from this one might reasonably hope to resolve the common properties of the raw material into some form of exact expression, and so to accumulate standards;

lastly, by following lint of known properties through the spinning-processes, and watching its modification thereby, some suggestions might arise whereby improvements in spinning might result, even in the present advanced state of the industry.

All work done away from the cotton-fields must nevertheless rest under a certain suspicion, and *unless* its limitations are acknowledged and realised, and a definite collaboration introduced as well, such work may well do more harm than good.

This brings us to the last aspect of the situation, namely, the need for collaboration.

We have spoken already of the ways in which the grower and spinner might colloborate, provided that their intermediary scientist were to provide them with a language in which they could exchange ideas. The extent of that colloboration would be defined by purely business considerations, and it may be objected that such considerations would render the suggested relationships with the scientist impracticable. If it were a question of some technical detail on which the scientist were employed, capable of protection by patent law, and of exploitation by financiers, such objection would certainly be valid, and the position of the scientist would be that of an employee engaged as a speculative investment. We decided, however, in the early part of this article, that the need which is coming upon the cotton trade is rather one for wide generalisation, clear understanding, and a view of the whole trade from the native grower to the mill-hand as a single organisation. That this is not a matter of mere opinion may be seen by the existence and work of the International Federation of Master Cotton Spinners, whose object is to provide for the whole trade a means of information and understanding which can only be effective when it embraces, like this Federation, nearly every spinner of cotton in the Old World. Our view of the function of natural science in the cotton trade is akin to this; there is room for fundamental work which will be public and accessible, not private and secret, which will be of immediate benefit to nobody, but of general utility to all; overlap and confusion could be reduced, and while the chances of any individual in relation to his fellows would not be affected, much time, trouble, and money would be economised. The work of the existing bodies dealing specifically with cotton has been confined to the economic and financial aspects of the trade, or else to the development of cotton-growing in new districts, intensive scientific research being outside their province in either case. The organisations already existing form, however, material upon which and through which the spinner and the grower are coming into closer contact, with the consequent discovery that only natural science can give them a common language. Some may object that science, far from giving them a common language, has merely wrapped up the meaning of its results in technical jargon. This is, however, inevitable as a stage in constructive work. For the interchange of ideas between investigators of many lands, terms of precise meaning are an unpleasant necessity; for the individual student they serve as nuclei for the crystallisation of his ideas; it is only in the finished work that they can be translated into common language. It would be illogical to conclude that the amount of technicality in the literature of a subject is an exact index to the activity of research therein, but it is not altogether without significance that the use of technicalities is extremely infrequent in writings which deal with cotton.

THEORIES OF DYEING

By E. A. FISHER, M.A. (Oxon.)

Research Department, South-Eastern Agricultural College, Wye, Kent

From the intensity of their colours many naturally occurring colouring matters would attract very early the attention of primitive man, and among the first signs of intelligence in man would probably be his utilisation of naturally occurring dye-stuffs for purposes of personal adornment. From the application of pigment colours to the human body to the dyeing of fabrics by means of solutions of colouring matters was a step that occurred quite early in human history, and probably no very great period elapsed before dyes were seen to be of two kinds—those which would dye directly (substantive dyes), and those which could be fixed to the fibre only by means of a mordant (adjective dyes). Dyeing is thus very probably the oldest technological process known. It is therefore somewhat surprising that though the process of dyeing was brought very early to a high state of perfection, yet it attracted so little attention among scientific workers, that until the latter part of last century no real explanation of the process was put forward. During the last thirty years, however, the whole problem seems to have come suddenly into prominence, and many different theories have been brought forward to explain the phenomena; each of them supported by evidence that at first sight seems to be perfectly sound, and each of them quickly overthrown by evidence of an equally incontrovertible nature.

The whole problem, until recently, was in a perfectly chaotic condition, and it is extremely difficult to find a way through, or to bring order into, the vast mass of experimental data that has accumulated during the last few years. The confusion is increased by the fact that the adherents of the various theories seem never seriously to have attempted a rational compromise between their divergent opinions; one always striving to prove the other theory unsatisfactory or untenable. Such a course could only be justified if it could be proved that all the phenomena manifested in the process of dyeing various fibres with dyes of different classes belong to one order only of chemical or physical phenomena. But we know that the behaviour of the different fibres towards the same dye shows great differences, and so also does the behaviour of one and the same fibre towards different dyes.

The first theory put forward was the mechanical theory, according to which the molecules of the dye leave the bath and deposit themselves between the molecules of the fibre. This theory, however, failed to explain the fact that only some dyes have the power of acting substantively, and these behave differently with different fibres.

Perhaps the first observation that contributed towards a real theory of dyeing was that there is an undeniable connection between the differences in behaviour of the various dyes, and of the chemical nature of those dyes and of the textile materials themselves. And especially is it well established that a coloured substance, in order to be applicable as a textile dye, must of necessity possess the properties and constitution of either an acid or a base. Thus indigo cannot strictly be called a dye, but should be described rather as a pigment colour; while on the other hand indigo carmine is a true dye. There exists no exception to this rule, which is perhaps the strongest evidence there is in favour of a chemical theory of dyeing. The first experimental work in favour of such a theory was done in the 'eighties by *Knecht*. He showed that combination in definite proportions does occur between wool fibre and such dyes as the nitro-phenols, although such combination cannot be proved for the great majority of dyes. He also showed that when silk or wool is dyed with basic colouring matters the dye is decomposed, the base combining with the fibre, and the acid remaining in solution; *e.g.*, magenta, methyl-violet, and chrysoidine are hydrochlorides of the colour bases, and on dyeing wool or silk with these the colour base is taken up, and the hydrochloric acid remains quantitatively in the bath. He concluded that silk and wool are amino-acids, and have therefore the power of combining with both basic and acidic dyes.

Knecht extended this theory by suggesting that many cases of substantive dyeing are really cases of adjective dyeing, and that in these cases the colouring matter combines with the lanuginic acid of the wool, which is slowly formed on boiling the fibre with water, and which then acts as a mordant, being itself contained in the wool fibre in a state of solid solution.

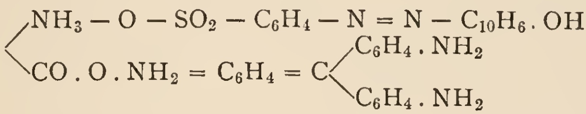
Vignon next worked on the problem and found that by introducing an amino-group into the cellulose molecule by treating with strong ammonia at a high temperature, and under a high pressure, the fibre readily took up acid dyes. This result is interesting, and although it is only what might have been expected, it shows that the chemical theory may possibly apply to the dyeing of silk or wool, as both of these fibres contain nitrogen. Such a theory, however, could hardly apply to the dyeing of cotton, since cellulose contains no nitrogen.

This chemical theory of dyeing received its most precise expression in the hands of *Weber* about 1894. Wool has the greatest affinity for colouring matters of all kinds, and is capable of decomposing the basic dyes into free acid and colour base; the latter then combining with the wool. It is also capable of combining with the colour-acids of sulphonated dyes. In all these cases the wool is dyed in shades identical with those of the salts, or rather the "lakes," of these dyes. This double function of the wool was shown by Knecht to be due to the amino-acidic nature of the wool fibre, or to the lanuginic acid contained in it. The process of dyeing can thus be expressed in the form of an equation, *e.g.*:

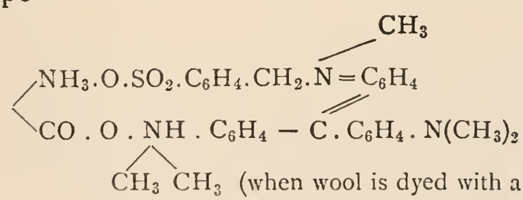
$$\left\langle\begin{matrix}NH_2\\ COOH\end{matrix}\right. + \begin{matrix}\text{naphthol}\\ \text{orange}\end{matrix} \rightarrow \left\langle\begin{matrix}NH_3.O.SO_2.C_6H_4.N{=}N.C_{10}H_6.OH\\ COOH;\end{matrix}\right.$$

$$\begin{matrix}\text{or with}\\ \text{magenta}\end{matrix} \rightarrow \left\langle\begin{matrix}NH_2\\ CO.O-NH_2=C_6H_4=C\left\langle\begin{matrix}C_6H_4.NH_2\\ C_6H_4.NH_2\end{matrix}\right.\end{matrix}\right.$$

If this is so, then in the first example there is still a free COOH group, and this ought to be able to combine with a basic colouring matter in the same manner and degree as the undyed wool. This is actually found to be the case. A skein of wool dyed with "scarlet R" was washed and immersed in a bath of magenta. At the same time a white skein of the same weight was immersed in an exactly similar bath of magenta of the same strength. It was found that both skeins absorbed the same quantity of magenta. It may be urged against this that solutions of all acid colouring matters precipitate solutions of all basic colouring matters forming "lakes," which may therefore be described as salts of colour-bases with colour-acids. It might be suggested that the magenta in the above experiment did not combine with the COOH group of the wool at all, but rather formed a "lake" with the acid dye upon the fibre; in which case the wool would simply be dyed with a kind of pigment, *viz.* a magenta-scarlet lake. Lakes of this kind, however, are easily soluble in alcohol. On treating the skein dyed with scarlet and magenta with alcohol only a very small quantity of magenta and not the faintest trace of scarlet was dissolved. This proves indisputably that on the skein the scarlet and magenta occur quite independently of each other; the wool, in fact, dyed in this manner may be represented as



Again, if a strong colour-base contain a sulphonic and an amino group, it can form a lake by means of the amino-group as well as by means of the sulphonic-group. Thus "acid-green" forms a lake with $BaCl_2$ in the sulphonic-group, and this again readily combines with tannic acid, forming a lake by means of the amino-group. This is quite general: the same thing occurs in the conversion of every amino-sulphonic dye into a lake, always providing, of course, that the NH_2 group of the unsulphonated dye be capable of lake-formation. If we dye wool with one of these colouring matters, and treat it afterwards in a bath with either $BaCl_2$ or tannic acid, not the slightest alteration is observable. This certainly seems to indicate that dyed wool is as much a lake as the compounds of alizarin with alumina or of magenta with tannic acid. That is, it must be a compound of the type



(when wool is dyed with acid-violet).

These results indicate that the reason why certain dyes will dye wool substantively is simply that the requisite lake-forming basic and acidic groups form part and parcel of the molecule of the wool substance. Many of these same dyes will dye cotton only in the presence of a mordant. This is because cotton (cellulose) does not possess any acidic (COOH) groups to act as lake formers. Similarly, if we could convert cellulose into amino-cellulose (as was actually done by *Vignon*) the latter would behave towards acid dyestuffs very much like wool. Indeed, in fixing tannic acid upon cotton we produce upon the cotton simply that lake-producing group—the COOH group—which, by being naturally present in the molecule of the wool substance, enables it to fix, *i.e.* to form lakes with, basic dyes.

In opposition to this chemical theory of dyeing, *O. N. Witt*, in 1891, put forward his very ingenious and plausible "solid solution" theory. Witt pointed out first that the older theory was quite unable to explain the substantive dyeing of cotton and silk, since these fibres do not contain basic and acidic groups and are not altered by boiling, thus differing from wool. Another objection was put forward by Witt: magenta on silk is "fast" even in the presence of strong soap solution; this seems at first sight to support the chemical theory, but on dipping the dyed fabric into absolute alcohol all the dye is removed from the silk. No affinity exists between alcohol and magenta—the former is merely a solvent for the latter. Again, on mixing the alcoholic solution and the fabric with water, whether the dye returns to the silk or stays in solution depends only on the amount of alcohol present. Such considerations as these are quite general—it is frequently found quite impossible to exhaust the dye-bath, although according to the theory the fabric precipitates the dye-stuff. It is strange that a large excess of fibre will not precipitate all the dye-stuff, for, since the product is insoluble, there is no similarity between this incomplete action and the ordinary case of incomplete precipitation in chemical reactions, for the latter is due to partial solution of the precipitate.

In support of his solid solution theory *Witt* pointed out that dyed materials show the colour, not of the solid dye-stuff, but of the dye-stuff *in solution*, when there is a difference of colour between the two states. Thus, solid fuchsine is bronze-green, its aqueous solutions are red, and so also are materials dyed with it. The dye-stuff rhodamine in the solid state exhibits no fluorescence ; in solution it does, and silk dyed with rhodamine is fluorescent likewise. According to the mechanical theory the colour of the dyed fabric ought to be that of the solid dye. On the other hand, there is an undoubted instance of mechanical dyeing in the case of indigo, and in dark shades of this colour the bronzy hue of dry indigo is readily seen. Witt therefore suggested that in dyed fabrics the dye-stuff was present in the form of a solid solution. This theory affords a clear explanation of the difference between substantive dyeing and adjective dyeing. All dyes which are soluble in water are more or less soluble in the three typical fibre-principles—fibroïn, keratin, and cellulose ; and the differences between substantive and adjective dyes are due to the relative solubility of the same in the fibre and in the water. Substantive dyes may in fact be defined as those which are more soluble in the fibre principles than in water, and are consequently extracted by the former from their solution in the latter. The operation of substantive dyeing is thus analogous to the extraction of substances from aqueous solution by means of a solvent which is non-miscible with water ; *e.g.* extraction of resorcinol from water by shaking up with ether. In the case of dyeing, too, the fibre is a colloid, and so allows the dye solution to penetrate through its molecular interstices. Again, if the solvent power of the dye-bath be increased by adding alcohol, or that of the fibre decreased by adding tannin matters, dyeing does not take place. According to this theory the chemical nature of the fibre is only of importance in so far as it affects the solvent capacity of the same. Thus fibroïn dyes better than other fibres on account of its superior solvent power ; keratin again has for most dyes a greater solvent power than cellulose ; the solvent power of cellulose is indeed so little that there are very few dyes which it is capable of extracting from water, and with some of these, *e.g.* the stillbene dyes, it is found advantageous to decrease the solubility of the dye in water by adding common salt.

The same theory applies to adjective dyeing. Solution takes place first between the fibre and the mordant. When once dissolved by the fibre the mordant acts as a precipitating agent and fixes the dye-stuff which comes in contact with it. An analogous case is met with in an instance of ordinary solution : benzene is incapable of extracting resorcinol from its aqueous solution, but if benzoyl chloride or acetic anhydride is added to the benzene the resorcinol is readily taken up by it, being converted into an ester; the benzoyl chloride here plays the part of a mordant.

The problem was next worked upon by *Dreaper* in 1894. He investigated the action of the colouring matters derived from p-toluidine which are sent out under the name of "primuline." If such a dye is diazotised and combined with an amine or a phenol or other suitable compound, azo-dyes of various colours are produced. These colours can be produced on the fibres, and are then remarkably resistent to the action of soap solution, although when formed away from the fibre they are easily soluble in alkaline or soap solutions. Again, on preparing some of the azo-dyes outside the fibre and then dyeing direct the colour is no longer "fast" to soap. Some other explanation than solid solution must obviously be found. Dreaper noticed, too, that a difference in tint occurred according to whether the process of dyeing was "direct" or "ingrain," thus indicating that the compounds formed in the two cases are not identical. Also, on "boiling out" the colour with standard soap solutions, the curves obtained were not identical. The results are strikingly at variance with any simple theory of solid solution; for in that case the curves from the "direct" dyeing method should coincide with those obtained from the "ingrain" dyed material. This is not the case, and it is particularly noticeable that while the "direct" dyes are fairly easily removed, the "ingrain" dyes are very resistant to the action of soap or alkalies. Also some of these azo-dyes cannot be developed *on* silk at all—this must be due to the ability of the diazo-compound to form a compound with silk which the developer—amine or phenol—is unable to decompose. Dreaper therefore suggested that when silk is dyed in the first place with primuline a combination takes place between the primuline base and the fibroin, then on diazotising the primuline is diazotised and probably partly combines with the silk again, and then the developer, if powerful enough, will form the dye. Dreaper also suggested that osmosis very probably plays an important part in the process, inasmuch as we can only imagine that the dyes are introduced into the interior of the fibre by osmotic action.

A searching examination of these three theories was carried out by *von Georgevics* in 1895–6. He repeated and confirmed most of Knecht's experimental work in support of the chemical theory, but he noticed that exactly the same quantitative results were obtained if, instead of dyeing fibres, he used broken porous porcelain or glass beads, or even glass beakers, *i.e.* the colour base was taken up and the HCl left behind in the bath. The chemical theory cannot hold here, and he therefore rejected Knecht's explanation of the process and sought for another. Obviously two alternatives present themselves, either the dye is dissociated by the material of the glass or porcelain (which is not likely since these two substances are chemically inert), or else the dye is hydrolysed before it comes in contact with the glass, *i.e.* magenta, methyl-violet, and chrysoidine are hydrolysed in aqueous solution. In support of this v. Georgevics dipped one end of a tube containing a roll of filter paper in a solution of magenta and allowed it to stand for some time; then the amounts of colouring matter and of HCl in the paper above the surface of the liquid were estimated. If hydrolysis had occurred the two constituents would rise according to their velocities of diffusion. As a matter of fact, the molecular proportion of chlorine found was twenty times that of the magenta. He suggested therefore that on dissolving in water the dye-stuff is hydrolysed, and if a substance for which the colour base has an adhesive attraction be brought into this solution, the base is taken up and a dyed body is obtained.

V. Georgevics next examined Witt's solution theory, and came to the conclusion that, when all the properties of the dyed fabric are taken into account, this theory is quite inadequate to explain the facts, since not only the substance but the structure of the fibre plays an important part in the process. Thus he found that if magenta be powdered and mixed with chalk or other white substance, the original green colour changes at once to red. Similarly magenta or methyl-violet, when rubbed between ground-glass plates, exhibits no longer the green colour characteristic of the crystals, but are now red and violet respectively. The colour of magenta and other crystals is not the colour of the solid substance, but is due to abnormal dispersion of the light at their surface; such dispersion only taking place with relatively thick layers. Also, if wool is dyed with a very concentrated solution of magenta, the fabric shows the same surface colours as the crystals—an effect known to dyers as "bronzing"; when dyed in the ordinary way the magenta is so finely divided in the fabric that it now exhibits its proper colours.

Again, fluorescence is not restricted to solutions for many solids, *e.g.* fluorspar and barium-platino-cyanide show this property to a marked degree. Silk dyed with fluorescein fluoresces, wool does not; therefore, according to Witt's theory, the dye-stuff exists in the former in solution, in the latter in the free state. Silk, the surface of which has been injured by mechanical treatment, no longer exhibits fluorescence when dyed with these colouring matters; and jute, finely bleached before weaving, and fine Angora wool, exhibit fluorescence when dyed with fluorescein, but on weaving lose this property. The appearance of fluorescence on a dyed fabric is dependent therefore, not on the state of aggregation of the colouring matter, but on the condition of the surface or lustre of the textile material.

Again, if dyeing is a case of solution it should be a reversible process, *i.e.* on treatment with water a dyed material should give up its colour, which with many dye-stuffs is well known not to be the case. Thus wool takes up the colour best at 100° C., and does not absorb it in the cold, *i.e.* according to Witt the colour is more soluble in wool at 100° C. than it is in water. Consequently wool dyed at 100° C. should give up its colour to water at the ordinary temperature, which is contrary to all experience.

Georgevics favoured a mechanical view of the process, and maintained that Vignon's experiments, although compatible with a chemical explanation of the process of dyeing, did not prove it, since many physical phenomena, such as capillarity and surface tension, are influenced by the chemical character of the active substance. The chemical theory must therefore be rejected until stoichiometrical relationships are definitely shown to exist between the various factors of the dyeing process.

Perhaps the strongest evidence against both the chemical and more particularly the solid solution theory is that afforded by an investigation of the partition co-efficients of various dyes between water and the fibre employed. In the case of a solution, when the molecular complexity of the solute is the same in both solvents, the solute so distributes itself between them that at any given temperature there is a definite ratio between the concentrations of the two solutions when equilibrium is attained, this ratio being independent of the amounts of solute and solvents originally taken, *i.e.*:

$$\frac{C_f}{C_w} = K.$$

When the molecular complexity of the solute is not identical in the two solvents, there is no constant ratio of distribution, the ratios of the concentrations varying with the original quantities present. There is, however, a somewhat more complex function which in such cases takes the place of the simple distribution ratio. If the molecular weight of the solute in one is *n* times as great as its molecular weight in the other solvent, then, at equilibrium, the *n*th root of the concentration in the first solvent will bear a constant ratio to the concentration in the second solvent, *i.e.*:

$$\frac{\sqrt[n]{C_f}}{C_w} = K.$$

This equation was first applied to dye solutions by Georgevics, who studied the manner in which indigo-carmine distributed itself between the dyed fabric and the dye-bath under different concentrations. Silk was dyed in a bath containing indigo-carmine and sulphuric acid. The amounts of silk, dye-stuff, sulphuric acid, and water were varied separately and in pairs, and the amount of dye-stuff remaining in the bath after the process was estimated by careful colorimetric comparison with the original solution. The difference gave the amount taken up by the fibre. He found that

$$\frac{C_f}{\sqrt[2]{C_w}} = K$$

in which C_w = amount of dye-stuff in 100 c.c. of dye liquor after the process, and C_f = the amount of dye taken up by 100 grms. of silk. He further found that this ratio increases slightly with increasing concentration, and drew the obvious inference that, according to Witt's solid solution theory, these results show that the molecules of dye in the dye-liquor must be more complex than those in the fibre!

Later *Georgevics* and *Löwy* (1896) tried to show whether the value of K depended at all on the chemical nature and physical structure of the dyed material. They studied the behaviour of cellulose in its two forms of fibre and powder. The latter they obtained by precipitating a solution of cellulose in ammoniacal copper-hydrate by means of an acid. So obtained it contains hydro-cellulose which has a greater affinity, or absorbing power, for basic dyes than cotton has, so to render them more alike in behaviour both were mercerised. The work was carried out at a uniform temperature of 14 – 17° C. and it was found that

$$\frac{C_f}{\sqrt[3]{C_w}} = K,$$

thus showing that the general partition law holds for other fibres than silk.

The actual value, however, for

$$\frac{C_f}{\sqrt[3]{C_w}} = K$$

is less for mercerised cotton than for mercerised precipitated cellulose, *i.e.* the latter appears to take up more dye. This, however, is dependent on the temperature, for at 100° C. the reverse is the case. The increased attraction of the powdered cellulose is probably due to its greater surface area, but owing to the same cause the powder offers less resistance to the increased solvent action of the water at high temperatures and thus loses colour more readily than the fibre. This again points to "surface effects" or "adsorption phenomena" as being the true cause of dyeing.

Schmidt in 1894 from experiments on the absorption of picric acid by cellulose and of malachite green and eosine by silk could obtain no constant ratio of the form given above, so *Walker* and *Appleyard* in 1896 re-investigated this point. They studied the absorption of picric acid by silk, estimating the acid volumetrically with $\frac{N}{20}$ KOH with lacmoid as an indicator. If the solid solution theory applies, it is a matter of indifference whether the picric acid is originally all in the silk, all in the water, or distributed between these two substances in any ratio whatever. The final equilibrium must always be the same, being determined only by a certain ratio of the concentrations in the water and in the silk. If the original concentration in the silk is too great, it will lose picric acid to the water. This was confirmed by Walker, but he pointed out that such a result is equally in accordance with any theory which involves equilibrium whether chemical or physical. Walker found that the relation

$$\frac{C_f}{\sqrt[x]{C_w}} = K$$

holds for this distribution, but in this particular instance $x = 2{\cdot}7$ and $K = 35{\cdot}5$; that is

$$\frac{C_f}{\sqrt[2\cdot7]{C_w}} = K = 35{\cdot}5;$$

i.e. if the solid solution theory holds, this equation means that the molecule of picric acid in aqueous solution is, on the average, 2·7 times as great as the molecule of picric acid dissolved in silk. This cannot be so, since a consideration of freezing points and electrical conductivity of aqueous solutions of picric acid shows that the molecule in solution in water is the simplest possible, *i.e.* it is not only *not* greater than $C_6H_2(NO_2)_3 \cdot OH$, but is much less than this owing to high electrolytic dissociation. By a simple transformation we get

$$\frac{C_f}{\sqrt[2\cdot7]{C_w}} = 35{\cdot}5$$

$$\log C_f = \log 35{\cdot}5 + \frac{1}{2{\cdot}7} \cdot \log C_w$$

$$\therefore \frac{dC_f}{C_f} = \frac{1}{2{\cdot}7} \cdot \frac{dC_w}{C_w}.$$

In this form the equation states that a slight proportionate change in the concentration of picric acid in the water is always accompanied by a corresponding proportionate change in the concentration of the acid in the silk. Thus if the concentration in the water increases by 1 per cent. of its value, no matter what that value is, the concentration in the silk will increase by $\frac{1}{2{\cdot}7}$ per cent. of its own value.

Formulæ of this kind apply to very many cases of absorption, *e.g. Schmidt* found a similar one to apply to the absorption of iodine and various acids from solution by animal charcoal, and *Küster* for the distribution of iodine between water and starch solution. But it does not, of course, follow from the identity of the formulæ valid in these cases that the phenomena themselves are identical in nature.

Walker and *Appleyard* next turned their attention to the chemical theory. If the union of the dye with the fibre is one of chemical combination, and if any finite amount of the fibre is incapable of completely exhausting the dye-bath, *i.e.* if the action is reversible, then the theory of mass action enables us to predict the nature of the equilibrium. The active masses of the solid fibre, the solid dyed fibre and of the water remain practically constant, so that in the case of the action

$$\underset{\text{(constant)}}{\text{fibre}} + \text{(dye and water)} = \underset{\text{(constant)}}{\text{dyed fibre}} + \underset{\text{(constant)}}{\text{water}}$$

we have if n = the active mass of the dye in aqueous solution—*i.e.* its concentration—

$$c \times \text{a constant} \times n = c_1 \times \text{a constant} \times \text{a constant},$$

c and c_1 being the velocity constants of the opposed reactions. For equilibrium therefore at a given temperature n must be constant; in other words, the dyed and undyed fibre (these together forming the partially dyed fabric) can only exist in contact with the aqueous solution of the dye when that solution has a certain fixed concentration, n. This is at variance with all dyeing experience. No known fibre presents such an undoubted and simple chemical union with any dye. Walker and Appleyard proved the correctness of the above reasoning by making use of what may be described as an artificial fibre. Di-phenylamine and picric acid form a deep chocolate-coloured addition compound; both this and the amine itself being practically insoluble in water. Here the ϕ_2NH represents the fibre, ϕ_2NH-picrate the fibre dyed to saturation, any mixture of the two corresponds to the partially dyed fibre. When the picrate is treated with water it is partially decomposed, some of the picric acid dissolving and the ϕ_2NH remaining behind. n, the concentration of the picric acid in the water, was found to be constant and equal to 13·6 m.grms per c.c. When n was made equal to 14 and ϕ_2NH was added the ϕ_2NH was stained a deep brown; when n was made equal to 13 no colour at all appeared in the ϕ_2NH or only a very slight stain after shaking for a very long time. No case of actual dyeing corresponds to this—the weakest dye-bath will always colour the fibre, and no discontinuity exists at a certain concentration, so that above it dyeing can take place and below it not.

What was probably the first step towards a real solution of the problem was made by *Zacharias* in 1901 in what he called a "new chemical theory of dyeing." According to this the operation of dyeing cannot be considered as a homogeneous process at all, but must take place in two stages: (1) the absorption of the dyestuff, and (2) the fixation and development of the colour.

1. *Absorption of the Dyestuff.*—According to Zacharias the dissolved dyestuff diffuses from the solution into the fibre and no attraction of it by the fibre need be postulated, for any body held in solution by the water can pass by free diffusion into the fibre. Chemical combination between dyestuff and fibre is, in fact, very unlikely, for the textile fibres are chemically inert substances and undoubtedly of very high molecular weight. They are also colloidal and hygroscopic. Every body which possesses similar absorbent powers can be dyed according to the same general laws. Among them are amorphous carbon, coagulable albumen, and certain colloidal metallic oxides such as alumina. Such substances can remove from solution all those dyestuffs which dye textile fibres direct. All colloidal substances absorb according to the partition law

$$\frac{C_1}{C_2^v} = K$$

in which v is a constant and may be equal to, greater than, or less than unity. It is true that a similar law holds for the distribution of a substance between two immiscible solvents, but to deduce from this that the substance absorbed by the fibre is dissolved in it can lead only, as Walker and Appleyard showed, to absurdities. Such a deduction is gratuitous and is moreover not in accordance with facts.

2. *Fixation and Development of the Colour.*—The dyestuff which has passed into the fibre by diffusion must now be fixed, *i.e.* it must be transformed into an insoluble dye incapable of being washed out again. This may take place either by chemical precipitation as is the case with adjective dyes such as chrome yellow and indigo blue, or by colloidal precipitation as is the case with direct or substantive dyes. The solutions of many dyestuffs and solutions of colour "lakes" in acids are colloidal, and the precipitation of insoluble colloidal substances from them is accelerated by the presence of the fibre, which, by virtue of its structure, or more probably by the nature and

extent of its surface, exercises a catalytic action. Zacharias thus distinguishes explicitly between dyestuff and dye on the fibre. In the case of acid and basic dyestuffs, which are true salts, the dyes formed are the free colloidal colour-acids or bases. With the direct cotton dyestuffs of high molecular weights or pronounced colloidal character, the dyes on the fibre do not differ in chemical constitution from the dyestuff in solution. These dyes are not fast to washing, since they are readily transformed back to the soluble condition. Dyes which are fast to washing are always produced by some chemical alteration of the dyestuff.

On this theory the various dyestuffs can be divided into crystalline and colloidal, but the division between these two groups is not sharp. Picric acid for example is crystalloidal, but has weak colloidal properties; it dyes wool quickly, but does not give a fast colour and it is incapable of dyeing vegetable fibres. Magenta is colloidal, but not sufficiently so to be capable of dyeing cotton direct. Every substance which assists the colloidal separation of the dye is a useful addition to the dye-bath. For this purpose acids are often added and dyeing seldom takes place in an alkaline bath, since alkalies tend to bring colloids into solution. Such addition to the dye-bath will of course affect both K and v in the distribution equation, but will have no effect on the nature of the process.

This theory of Zacharias has much to recommend it, and it seems to be a distinct advance towards a real solution of the problem of dyeing. At the same time it is the logical outcome or extension of the solid solution theory of Witt: it merely assumes that the absorption is always accompanied by secondary chemical changes. Many cases have been investigated that support this, although some cases are known in which the ratio of the concentrations in the liquid and in the fibre is constant, *i.e.*

$$\frac{C_f}{C_w} = K,$$

and is independent of concentration within the limits examined. This may be attributed purely to simple absorption without any accompanying polymerisation or any chemical change, *i.e.* the results in these particular cases clearly support Witt's theory. Nevertheless such a theory cannot hold good in general for the non-reversibility of the process in most cases speaks volumes in favour of the view that a chemical or physical change takes place during the fixing.

The experimental side of the theory of Zacharias was developed in 1905 by *Linder* and *Picton*, who attempted to prove experimentally that dyeing was a phase of the coagulation of colloids. They showed that when a colloidal solution of ferric hydroxide is exactly coagulated by a dilute solution of Am_2SO_4, the whole of the iron and sulphuric acid separate as an insoluble hydroxy-sulphate, whilst an equivalent amount of AmCl remains in solution. If in place of Am_2SO_4, we use a solution of "soluble blue" or "Nicholson's blue" the result is precisely similar: at a certain critical point a red coagulum separates which contains the whole of the iron and the whole of the sulphonic acid added, an equivalent amount of NaCl remaining in solution. If, in place of such acidic dyes, we use an equivalent amount of a basic dye, such as methyl-violet, no coagulation is observed. In this case we are dealing with a chloride, and chlorides coagulate ferric hydroxide only in concentrated solution. With As_2S_3 the behaviour of dyes is reversed; methyl-violet readily coagulates As_2S_3 with formation of a dye, a hydrosulphide derivative, with liberation of HCl, whilst acid dyes have no such power. Further, if we continue the addition of Am_2SO_4 to a solution of ferric hydroxide beyond the point at which the coagulum separates, the excess remains in solution. With many dyes, however, this is not the case: the coagulum which separates continues to take up dye with avidity, withdrawing from solution in this way an amount four or five times as great as that required to coagulate the hydroxide before the excess of dye added begins to colour the solution. Up to this point, the supernatant liquid remains clear and colourless, and, what is equally important, no trace of alkali can be detected—the dye is therefore taken up as a whole, *not* as a sulphonic acid. Similar results are obtained if methyl-violet is added to As_2S_3. On the other hand, if ferric hydroxide coagula are treated with methyl-violet or As_2S_3 with aniline blue, no appreciable amounts of the dyes are taken up. Ferric hydroxide has a selective affinity for the acidic dyes, As_2S_3 for the basic dyes.

This line of investigation was further pursued by *Biltz* in a series of papers published between the years 1904 and 1906. It is well known that a colloid when converted into the "gel" condition has the power of taking up another colloid from its solution with the formation of "adsorption compounds." Biltz replaced the gel which serves to take up the dissolved colloid by animal and vegetable fibres and found that inorganic as well as organic colloids were taken up by the fibres. A coloured colloid was fixed with its characteristic colour. Coloured inorganic substances when converted into the colloidal condition thus acquire the capacity of being "adsorbed." Experiments were made with colloidal solutions of selenium, tellurium, gold, vanadium pentoxide, molybdenum-blue, tungsten-blue, cadmium sulphide, arsenious sulphide, antimony disulphide (SbS_2), copper ferrocyanide, mercury, stannous sulphide, cupric hydroxide, and molybdenum tungsten purple. In the case of selenium, tellurium, and gold the fibre is dyed and the solution is more or less completely exhausted. In the other cases also the fibre is dyed, but the colour is not extracted from the solution to so great an extent. A colloidal solution of gold prepared by Zsigmondy's method will not dye silk; a constituent of the latter (called by Biltz a "protective colloid") passes into solution and prevents the gold from being deposited. The colloidal gold solution after being boiled with silk can no longer be precipitated by electrolytes. In several cases, too, the dyeing of the fibre by inorganic colloids was found to be favoured by the addition of salt just as ordinary dyeing is. There is no essential difference between the dyeing properties of coloured inorganic colloidal substances and organic dyestuffs, the partition law holding good in both cases. In further experiments the hydrogel of alumina was used in place of the organic fibre, and here again it was found that the substitution of the inorganic colloid for the organic fibre had no effect on the quantitative relations observed in the dyeing process. Again, solutions of such dyestuffs as the Immedial sulphur dyestuffs in alkaline sulphides were submitted to dialysis, clear colloidal solutions being thus obtained free from alkali, and it was found that these were coagulated by electrolytes in a similar manner to other colloidal solutions, *e.g.* the coagulating power of the electrolytes on the dyestuff solutions increased with the valency of the cation; and the process was in every way identical with the coagulation of ordinary colloids.

Finally Biltz turned his attention to another disputed point and tried to show whether the "lakes" which adjective organic dyestuffs form with metallic oxides (mordants) are chemical compounds in the ordinary sense, or solid solutions or "adsorption compounds." The point was investigated by agitating metallic oxides or hydroxides with solutions of mordant dyestuffs, both in the cold and at the boiling temperature of the solvent, and noting the effect of varying the concentrations of the dyestuff solutions when the amount of metallic oxide was kept constant. The amount of dyestuff left in solution was determined colorimetrically after removal of all suspended matter by filtration and curves were then constructed showing the concentrations of the dyestuffs in the lakes as ordinates and concentration of the dyestuff in the final solution as abscissæ. In the case of the reaction of Fe_2O_3 on a solution of alizarin in 1 per cent. NaOH solution in the cold the results obtained showed that a chemical compound of the formula Fe_2O_3, $3C_{14}H_8O_4$ is formed. On the other hand the action of boiling aqueous solutions of alizarin red on chromium hydroxide led to the production of adsorption compounds, since in this case the results agreed fairly well with the formula

$$\frac{C^n \text{ oxide}}{C \text{ dye-bath}} = K, \ n \text{ being equal to } 3 \text{ and } K \text{ to } 2{\cdot}1.$$

Further experiments, in which the action of ammoniacal and alcoholic solutions of alizarin and aqueous solution of alizarin-blue on Fe_2O_3 and of alcoholic solutions of gallein on Al_2O_3 were investigated, did not lead to definite results. The curves obtained end in an almost horizontal portion, but the distance of this from the abscissa does not agree with any simple stoichiometric formula. Moreover, by comparing these results with those obtained with Fe_2O_3 and alcoholic solutions of alizarin yellow, cloth, red, etc., the amounts of dyestuff taken up by the same weight of Fe_2O_3 were not found to stand in any stoichiometric relation with one another. It was also found that in all the cases investigated the exact physical condition of the ferric hydroxide used greatly influenced the results.

The work of Biltz seemed to show, in so far as pure experimentation alone could do so, that the process of dyeing really was a phase in the coagulation of colloids and belonged to that class of reaction known generally as "adsorption phenomena."

The matter was taken up in 1907 by *Freundlich*, who attacked the problem from the standpoint of Willard Gibbs's theory of surface concentration.

The great majority of substances, including the common dyestuffs, cause a decrease in the surface tension of a solvent in which they are dissolved. *Willard Gibbs* showed from thermodynamical considerations that in such cases the solute will tend to accumulate in the surface layer of the solvent. In a system such as the colloidal solution of a dye in the presence of a solid colloid, such as a textile fibre, the amount of surface involved is really enormous, and in consequence we should expect to find changes of concentration in one phase (the liquid) at its boundary surface with the other phase (the solid). It is in fact such changes of concentration on a surface, especially a solid colloidal surface in contact with the solution, due to a tendency of the surface energy to attain a minimum that are designated collectively as "adsorption phenomena." The absorption of gases and of iodine by animal charcoal and the clarifying action of certain colloid-gels, such as alumina, on organic solutions belong to this type of phenomena. Investigations by Freundlich and others have shown that in all such cases an equilibrium is attained more or less quickly and that the relative concentrations in the two phases can be expressed by a simple exponential formula. If a is the original total amount of solute (*e.g.*, dye) before adsorption, a/v is the original concentration of the solution. If x is the amount of dye adsorbed when m grms. of solid (*e.g.*, textile fibre) are employed, x/m is the amount adsorbed per grm. of solid. $a-x$ is therefore the amount left unadsorbed, and hence

$$\frac{a-x}{v} = c$$

is the final concentration of the solution.

Then

$$\frac{x}{m} = \beta c^{\frac{l}{p}} = \beta \left(\frac{a-x}{v}\right)^{\frac{l}{p}};$$

where β and p are constants depending on the nature of the solutions and the adsorbent. It is obvious that this equation is merely an extension of Henry's law with one side raised to a power $\frac{l}{p}$; that it is in fact a general statement applicable to various kinds of equilibria, and of which the partition law mentioned above is a particular case. In all adsorption phenomena the value of p is found to lie somewhere between 2 and 10.

This equation is a purely empirical one and has no theoretical basis although it has been found to fit the facts with a fair degree of accuracy; moreover, it shows that many types of equilibria of very different nature can be described by similar formulæ. Willard Gibbs, however, by thermodynamical methods deduced another more comprehensive formula to describe adsorption phenomena; if Γ is the adsorption coefficient, *i.e.* the mass of solute adsorbed per unit area of surface, c the concentration of solute in the mass of solution, and σ the surface energy per unit of surface, then

$$\Gamma = -\frac{c}{RT} \cdot \frac{d\sigma}{dc}.$$

This formula contains the differential coefficient of the function connecting surface energy and concentration (*i.e.*, $\frac{d\sigma}{dc}$), and this will be positive if both change in the same sense, but negative if they change in opposite senses. This, taken in conjunction with the minus sign on the right-hand side of the equation, shows that, if the surface energy increases with increasing concentration, there will be a diminished concentration in the surface, *i.e.* a negative adsorption. On the other hand, if, as is commonly the case, the surface energy decreases with increasing concentration, there will be an increased concentration in the surface, *i.e.* ordinary positive adsorption will occur.

This equation was investigated experimentally by *Lewis* in 1908, and applied by him with some ingenuity and success to the equilibrium involved in dyeing. He determined the alteration of surface tension at the interface between two immiscible liquids, this alteration being due to the surface adsorption by one liquid of a solute contained in another. It was found that in all cases the experimental value of Γ was many times—anything up to 80—the theoretical value. This discrepancy between the theoretical and observed values of Γ has been confirmed by other workers, and so may be taken as well established; but nevertheless it does not affect the value of the theory for comparative purposes. In applying the theory to dyes it is evident that if the theory holds, the dyestuffs in solution should possess the property of lowering the interfacial tension. This, of course, cannot be determined in ordinary dyeing owing to the solid nature of the fibre, but considerable support would be given to the adsorption theory of dyeing if it could be shown that dye solutions actually do lower the tension at the interface between the solution and an inert liquid. This liquid must of course be such that it excludes both chemical combination and solution. For this purpose a hydrocarbon oil was used as the substance to be dyed, and aqueous solutions of various concentrations of Congo-red and methyl-orange were prepared. In each case it was found that the interfacial tension was lowered, and that the lowering was proportional to the concentration up to about 0·1 per cent. solutions, at which point the tension remained practically constant. On applying the equation it was found to hold not in its theoretical form but with the anomaly it shows in cases of ordinary "adsorption." This seems to show that substantive dyeing must be more or less an adsorptive process. In all probability the actual mechanism of dyeing consists first in adsorption and afterwards fixation brought about probably by "colloidal precipitation." In some cases there may even be some degree of chemical combination which varies with the nature of the dye, but whatever the nature of this fixation, it is in a high degree probable that the first step in the process of dyeing is an adsorption of dyestuff by the fibre in accordance with Gibbs's theory of surface concentration.

SMOKE ABATEMENT

NOTES ON THE PROGRESS OF THE MOVEMENT TO SECURE A CLEANER AND PURER ATMOSPHERE

By JOHN B. C. KERSHAW, F.I.C.

Member of the London and Hamburg Smoke Abatement Societies, and of the Committee for the Investigation of Atmospheric Pollution

Introduction

The progress of thought and achievement that has marked all branches of scientific knowledge during the past fourteen years has not been without its influence upon the efforts to secure a cleaner and purer atmosphere. In the United Kingdom, in Germany, and in America, the apathy which marked the closing years of the nineteenth-century, with regard to the problems of *smoke*, has given place to keen interest and activity, and in all three countries much good work has been accomplished in arousing public attention to the enormous losses and evils that result from the dust and dirt suspended in the atmosphere. For if, as the majority of those who have studied the problem assert, *smoke is a sign of inefficiency and loss*, then the smoke-abatement movement is in reality one for increasing the efficiency and economy of all power-plants, and for lengthening the life of the world's resources of solid and liquid fuel.

No new or remarkable discoveries relating to combustion or to the utilisation of fuel have marked the progress of the last fourteen years.[1] The correct principles of combustion, and the conditions required for the smokeless combustion of bituminous fuel in boiler-furnaces, were enunciated by C. Wye Williams (a Liverpool engineer) seventy-five years ago, and a perusal of the book which he published in 1839, under the title of *The Combustion of Coal*, will prove that for three-quarters of a century sound scientific teaching has been available on this subject, for those who cared to make use of it.

Williams pointed out clearly that the combustion of

[1] Bone's system of flameless combustion is based on an application of a very old principle discovered by Sir Humphry Davy in 1816.

bituminous fuel involved two distinct things, namely, the liberation and combustion of the volatile gases in a suitable chamber, and the combustion of the solid coke on the bars of the grate. In one place he described how he proposed to put into practice these principles of fuel combustion, and showed drawings of boiler-furnaces with properly designed and scientific arrangements for providing a secondary and heated air-supply behind the bridge. He was very sarcastic at the expense of inventors, who, even in his day, were patenting devices for "consuming smoke," instead of attempting to show how to prevent its original formation.

Williams's book was reprinted in 1854 and 1886, but its teaching appears to have had little effect upon the practice of boiler-engineers, who still persisted in designing and erecting boilers with wholly inadequate combustion space for the class of coal that was burned.

The results of trials carried out by the Manchester Association for the Prevention of Steam-Boiler Explosions in 1867-68 were similarily ignored by boiler-makers and their users, for here again it was proved conclusively that the bituminous fuel of the Wigan coalfields could be burned with high evaporative efficiency and *without smoke emission* under steam-boilers, if proper attention were given to the design of the furnace, and to the control of the firemen and of the draught. The methods of attaining comparatively smokeless combustion and an evaporative efficiency of 9½ lb. of water per 1 lb. of fuel, in these Wigan trials, were practically those recommended by Williams twenty-nine years earlier, and the chief novelty of Mr. Fletcher's report was the insistance upon the need for good stoking. In his opinion stoking was an art, and should be treated as such, "and not as a slap-dash random process, which any untaught labourer could accomplish." [1]

The progress that has occurred during the past fourteen years therefore has been chiefly educational—that is, factory owners, manufacturers, and fuel-users generally, are beginning to accept, and to put into practice, the scientific principles that have been known, but ignored, for so many years. Although a strong case for smoke abatement can be made out on hygienic grounds, it will always be the economic argument that is the most convincing to the man who is actually producing the smoke in his works or factory. Prove to him that a cleaner chimney-top means a smaller coal bill, and he will become an ardent and willing helper in the campaign against black smoke;

[1] From a paper by J. B. C. Kershaw, read before the *Society of Arts*, March 20, 1907.

whereas, in the absence of this proof, he will remain (as in the past) an indifferent witness, or energetic opposer, of all your schemes of improvement.

The aim of those actively working for the creation of a cleaner and purer atmosphere must therefore be directed towards the securing and presentation *of positive proof that it pays to suppress black smoke.* The more varied the experience and facts upon which this proof rests, and the more clearly these can be driven home into the minds of the fuel-consumer, the more certain and real will be the progress made towards the day when smoke will be banished from our towns; and our great centres of population and industry will no longer be distinguishable, miles away, by the canopy of haze and smoke that screens them from the sun.

It is the purpose of the writer in the present article to present a few brief notes on the latest phases of the smoke-abatement movement in the United Kingdom, Germany, and America. No details will be given of smoke-abatement appliances, since these merely apply in different ways the well-known scientific principles of combustion.

The United Kingdom

The introduction of a new Smoke Abatement Bill into the House of Lords early in the present year, and the fact that the Government only secured its withdrawal by appointing a Departmental Commission to inquire into the whole subject, is proof that the public authorities in England are awakening to the great importance of preventing any further pollution of the air-supply of their towns and cities. If other proof were required, it would be found in the efforts which are now being made by the health authorities of twenty-two of the most important towns and cities of the United Kingdom to obtain reliable and systematic records of the amount of soot and dust suspended in the atmosphere.

I. Details of the New Smoke Bill.—The Smoke Abatement Bill which was introduced into the House of Commons on April 30, 1913, and into the House of Lords early in the present year, was backed by a group of independent members, and had for its object the extension of the powers of the local authorities and of the Local Government Board, in relation to the nuisances caused by the improper emission of smoke.

By Section 1 of the new Act, it is made incumbent upon the owner, occupier, or user of a furnace to have such furnace constructed so that it will consume its own smoke; and the emission of smoke or grit from such furnace or its chimney is constituted a nuisance. At the same time the Local Government Board are given power to make special exemptions in those cases in which it is impracticable to avoid making smoke in carrying on a business, but such power to exempt is limited to ten years from the passing of the Act, and an exemption itself must be renewed every two years.

Section 2 lays down the penalties for offending against the Act, and also the procedure of prosecution.

By Section 3 the Local Government Board are authorised to set up local smoke-abatement authorities in those areas in which the local sanitary authorities are failing to carry out their duties with regard to smoke abatement. In London, the London County Council is made the local smoke-abatement authority.

Under Section 4 local smoke-inspectors may be appointed by the local authority, subject to certain requirements as to experience, and to the control of the Local Government Board.

By Section 5 the Local Government Board are given the right to inspect the records of offences under the Act, in order to see if the local authority is properly carrying out its duties, and, subject to such obligation being satisfied, the Local Government Board may pay not more than half the salary of local smoke-inspectors, by way of grant in aid.

The same Section requires the central authority themselves to appoint Government smoke-inspectors, and authorises such inspectors to prosecute in cases where the local authority has failed to do so.

The manner of providing for the expense of local smoke-abatement authorities is dealt with in Section 6, together with a power of allocation by the Local Government Board.

By Section 7 existing powers as to the abatement of smoke nuisances are expressly preserved.

The chief differences between the provisions of this Bill and those of the Acts now in force, concern the relationship of the Local Government Board to the local authorities as regards smoke emission. At present many local authorities will not enforce the law, because they do not wish to offend the big manufacturers who provide the largest share of the rates and might leave the neighbourhood, if the laws regarding smoke emission were very strictly enforced. The power of the Local Government Board over these defaulting local authorities at present is feeble, and the Bill seeks to strengthen this in two ways.

In the first place by securing uniformity of the law in all localities and centres of manufacturing industry, the migration of an industry or manufacture from one district to another would be checked; and in the second place, by the appointment of smoke-inspectors under the Local Government Board, similar in rank and education to the inspectors under the Alkali Acts, the best technical and scientific advice as to the methods of securing smoke abatement could be offered, with more probability than at present that it would be accepted and acted upon.

The provision by which the smoke-inspectors appointed under the local authority are to be paid in part by the Local Government Board, is also a useful one, since it will secure greater uniformity and efficiency in the whole service.

As already stated, the Bill has been shelved for the time by the appointment of a strong Departmental Committee to report on the best method of attacking the admitted evils of black smoke, and of strengthening the laws against excessive smoke emission. This Committee has held many meetings during the present summer, and there is no doubt that the Bill will be reintroduced into both Houses of Parliament at an early date, with the modifications suggested by the report of the Committee.

II. Soot and Dust Fall Observations in Cities and Towns.—At the International Exhibition of Smoke Abatement Appliances and Conference on the Black Smoke Problem, held in London in March 1912, a committee was appointed to draw up a specification for a standard apparatus and method for measuring the extent and character of the air pollution in cities and towns. Numerous meetings of the members of this committee were held in London in 1912–13, under the presidency of Dr. Norman Shaw, F.R.S., Chief of the Meteorological Office, and eventually a type of apparatus and method were selected, based on that used for the *Lancet* observations in London in 1910–11, with certain modifications suggested by experience. Circular letters were sent out by the committee to the greater number of local authorities in the country, asking for their co-operation and support in these new observations, and as a result of this propaganda, the public health authorities of twenty-one English and Scotch towns, also of several of the London boroughs, have agreed to join in this work of recording the character and amount of atmospheric pollution, and thirty-eight gauges are now (July) in use.

These observations were to have commenced on October 1, 1913, but considerable delay occurred in obtaining a supply of the large number of soot and dust gauges (of a standard size and pattern) required for the work, and the commencement of the observations was delayed in consequence until March 1 of the present year. The health authorities of the following twenty-five cities and boroughs are joining in the movement: Aberdeen, Ayr, Birmingham, Coatbridge, Exeter, Glasgow, Greenock, Hull, Leicester, Leith, Liverpool, London (3), Malvern, Manchester, Newcastle-on-Tyne, Oldham, Paisley, Plymouth, Sheffield, Stirling, Wishaw, and York. Hamburg is also commencing similar observations by the standard method, the very active Smoke Abatement Society of that city having agreed to take charge of these observations.

The principle of the apparatus and method that have been selected by the committee, after careful examination of all that have been tried for the purpose, is that of collecting the soot and dust and other impurities that fall by their own weight, or are carried down by the rainfall in a given area, in one month, in a large collecting gauge of enamelled iron.

This method does not give, it is true, the amount of solid matter suspended in the air, at any given moment of time. There is, however, a close relationship between the amount of solid matter that falls, or is carried down by the rain, in a stated period, and the amount in suspension—and the former may be taken without serious error as an index and measure of the latter. The apparatus used is therefore simply an enlarged rain-gauge, the catchment area being increased to four square feet, and several large bottles, connected by syphons, being provided to hold the collected solid and liquid matter. The gauges are placed on the ground level, in open spaces free from abnormal dust. The bottles containing the water and deposit are removed on the last day of each month, and are replaced by thoroughly cleaned empty bottles. Before removing the bottles the gauge vessel is washed down with some of the collected water, a brush being used to remove any adherent matter. A chemical analysis of the water and deposit is then made by a standard method, details of which have been settled by the committee. The analysis will reveal the relative proportions of carbonaceous matters and of tarry matters in the solid deposit, and will enable the committee to judge how far the domestic fire and chimney have helped to contribute to the smoke problem in each locality.

As the observations with the standard gauge were only commenced in March, no figures covering twelve months are yet available, but tests made three years ago by somewhat similar methods of observation in *London, Glasgow, and Leeds* have yielded astonishing figures for the annual soot and dust fall, and have proved the importance of obtaining more accurate and comparative data on the subject.

London.—For the *Lancet* observations, carried out in London in 1910–11, at three stations in the London area, the following constituents were determined: total solids, insoluble deposit, soluble deposit, soluble volatile solids, soluble fixed solids, sulphates, ammonia, chlorides.

The total fall of solid matter in the twelve months ranged from 195 tons per square mile at Sutton to 650 tons in the East End of London, and if worked out on the basis of an average of 441 tons for the whole of the Metropolitan area (of 117 square miles), it represents an annual soot and dust fall of 51,597 tons over the Administrative County of London.

Leeds.—Prof. Cohen's observations at Leeds, as reported in the proceedings of the Smoke Abatement Conference, held at Manchester in November 1911, gave the following figures:

TABLE I. THE CHARACTER AND AMOUNT OF AIR-POLLUTION IN LEEDS AND SUBURBS. (COHEN.)

(*Tons per square mile per annum.*)

Station.		Suspended Matter.			
		Carbon.	Tar.	Ash.	Total.
Industrial.	Leeds Forge	189·6	31·4	318·0	539
	Hunslet	241·2	19·7	187·2	448
	Beeston Hill	87·1	42·6	202·5	332
Town.	Philosophical Hall.	99·7	22·3	120·6	242
Residential.	Headingley	100·2	12·3	56·9	188
	Armley	98·0	9·7	61·7	169
	Woodhouse Moor	63·2	9·1	41·7	114
	Kirkstall	52·3	8·0	40·3	100
	Westwood Lane	19·2	7·4	15·4	42
	Roundhay	7·7	4·0	14·0	25

The fall was largest, therefore, at Leeds Forge, in the heart of the manufacturing district, and smallest at Roundhay, an agricultural district situated on the west side of the city. The fall in the centre of the city was 242 tons.

Glasgow and other Scotch Towns.—The average amount of air pollution in several towns and cities of Scotland has been investigated by Chief Sanitary Inspector Fyfe, of Glasgow, and the results of his investigations are recorded in an interesting paper contributed to the Manchester Conference referred to above. The observations were only continued for the two winter months of the years 1910–11, and therefore they cannot be compared with observations extending over twelve months, as the winter soot and dust fall is always much heavier than the summer one, especially in thickly populated districts. Assuming that the fall during the two winter months, when Inspector Fyfe's observations were made, was twice as heavy as in June and July, the total *annual* fall of soot and dust in Scotch towns, as determined by these observations, would be as follows: Inverness 47 tons, Stirling 80 tons, Port Glasgow 160 tons, Govan 240 tons, Falkirk 314 tons, Glasgow 665 tons, Coatbridge 969 tons.

The results of the English and Scotch observations, however, are not comparable one with the other, as they were made at different times, with different apparatus, and under different conditions—and it is for the purpose of overcoming this defect that the Committee for the Investigation of Atmospheric Pollution have insisted on the use of a standard form of gauge, and of a standard method of examining the deposit, in the observations now being carried on.

The figures for the first three months' observations are not yet complete, but so far as they are available they are given in Table II.

It will be observed that the totals in column *b* require multiplying by 12 to give the total fall in tons per square mile per annum, and that when this factor has been applied, the totals vary from 54 tons at Malvern up to 504 tons at Liverpool and Paisley, and 564 tons at Newcastle-on-Tyne. The winter months will, of course, provide far higher totals than these, but the average for the twelve months will no doubt be somewhere about this figure. These are staggering totals when one remembers that this amount of impurity is suspended under

TABLE II. SOOT- AND DUST-FALL IN TONS PER SQUARE MILE PER MONTH, FOR SOME ENGLISH AND SCOTCH TOWNS, BY STANDARD GAUGE AND METHOD (UNCORRECTED).

	Total Solids collected.		Month of Observation.
	Grams.	Tons per sq. mile.	
Newcastle-on-Tyne	6·907	47	April
Liverpool	6·261	43	"
Paisley	6·193	42	"
Meteorological Office, London	4·680	31	"
Ayr	4·294	29	"
Kingston-upon-Hull	4·290	29	"
Greenock	3·751	24	"
Exeter	3·656	24	"
York	3·157	21	"
London (Golden Lane)	2·770	17	"
Leith	2·739	17	"
Malvern	0·684	4½	"

The Liverpool totals, since the observations have been started, were as follows:

	Total Solids collected.	
	Grams.	Tons per sq. mile.
March	10·797	74
April	6·261	43
May	5·182	35
June	6·386	44
July	5·619	39

normal conditions in the atmosphere, and that only after heavy and long-continued rain can the air we breathe be considered clean and free from solid impurities. The committee in charge of the observations is trusting, therefore, that the information gained as to the character and amount of atmospheric pollution in the various cities and towns where gauges are installed will arouse great interest on the subject, and will lead to greater efforts on the part of the local authorities to abate smoke and to secure a cleaner and more healthy atmosphere.

GERMANY

The most notable work in furtherance of smoke abatement in Germany is being carried on by the "*Verein für Feüerüngs-*

betrieb und Räuchbekämpfung" of Hamburg, a Society which was founded in 1902 by a group of Hamburg manufacturers, who were convinced that some better system of control of their boiler and heating plants was desirable, and possible, than that in use up to that date. Since its formation, twelve years ago, this Society has achieved an undoubted success, and the tenth annual report, which has appeared this year, states that the Society on April 1, 1914, had 500 members and 1,744 boilers or other heating appliances on its register. When one notes that the Society started in 1902 with only a few Hamburg manufacturers behind it, and that in 1911 a daughter-society was founded in Helsingfors, and withdrew many members from the parent body in Hamburg, this success is the more striking. It proves that when worked on right lines, co-operation in the scientific supervision of boilers and heating-plant can be made beneficial to both the individual factory-owner and to the general community—for the one obtains higher efficiency and a smaller coal-bill, and the other gains by less smoke and a cleaner atmosphere.

The objects of the Hamburg Society, as set forth in its rules, are the attainment of the highest possible efficiency from the heating and boiler plants of its members, with the least possible emission of smoke. To this end, regular examination of these plants and of the methods of working them is undertaken by the expert staff of the Society, and suggestions are made for improvements when such are required. The education and control of the firemen in the proper performance of their duties are also undertaken by the firemen-instructors on the staff of the Society. Comparative tests of fuel, and tests of smoke prevention and other appliances of a similar character are also carried out by the expert staff, and the results are circulated amongst the members of the Society. Each boiler or heating plant, when brought under the control of the expert staff, is tested at the earliest possible date, and a written report upon the results of the examination is submitted to the owner. Should the firing have proved inefficient, one of the firemen-instructors is sent to the works to give practical instruction to the firemen employed there, and tests of the plant are made at intervals until this fault is remedied. Defects in design are similarly dealt with. The annual subscription to the Society for members, without any boilers or heating-plant

("Fordernde Mitglieder"), is 20 marks (£1). Members having boilers or furnaces which they desire to place under the control of the experts of the Society, pay a further 20 marks annually for each boiler or furnace. The extra charges for tests and reports are based upon the time spent upon them and the number of experts employed. Engineers are charged for at the rate of 20 marks per day, and firemen-instructors at 5 marks per day. Special reports upon patented appliances are charged for at the customary rates, but members receive a special discount of 30 per cent. on these as compared with outsiders.

The work of the Society is controlled by a committee of seven members, elected annually. The technical and scientific work is undertaken by the staff of experts retained for this special work. At the date of the last report this staff consisted of a chief engineer, three assistant engineers, five instructors for firemen, and three clerks; while for steam-raising and other trials, additional assistance had been employed. The chief engineer attends the committee meetings and takes part in the discussions relating to the work of the Society. The funds of the Society are drawn from three sources: from the annual subscriptions of its members; from payments for special work and reports for its members; from payments for outside work. The Society is thus entirely self-supporting, and its success is dependent upon the value of the return it makes to its members, for their contributions and fees.

Turning now to a consideration of the practical results of the work carried on, as recorded in the past annual reports of the Society, we find that great stress is laid upon the improvements in efficiency, due to the training of the stokers in the proper performance of their duties; and figures are given in every report, showing gains in efficiency varying from 10 per cent. to 20 per cent., *from this one cause alone.* As examples of the gains which have followed from the Society's control of boiler-plants, the following two diagrams (taken from the 1912 report) are interesting, since they show in graphic form the results of tests on the same boilers, before and after the engineers of the Society had taken charge of the respective plants. The "heat-loss" in chimney gases, ashes, etc., is here seen to have varied between 35 and 53 per cent. when the boilers were working under the normal conditions (*i.e.* in charge of their usual stokers), while after the Society's expert staff had taken control

23

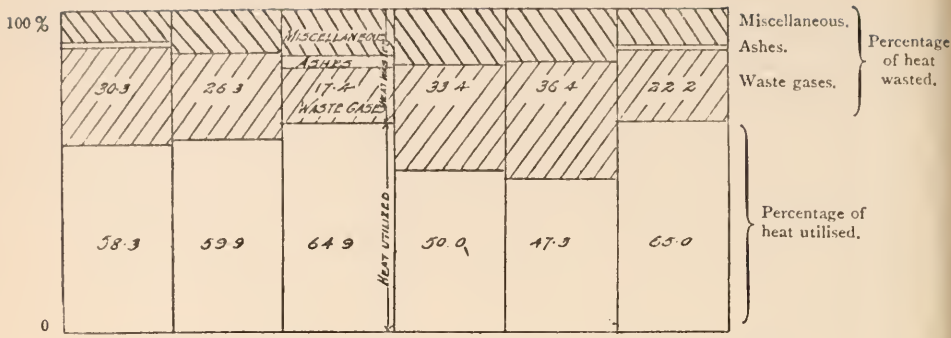


FIG. 1.—Results of the first trials before special training of stoker.

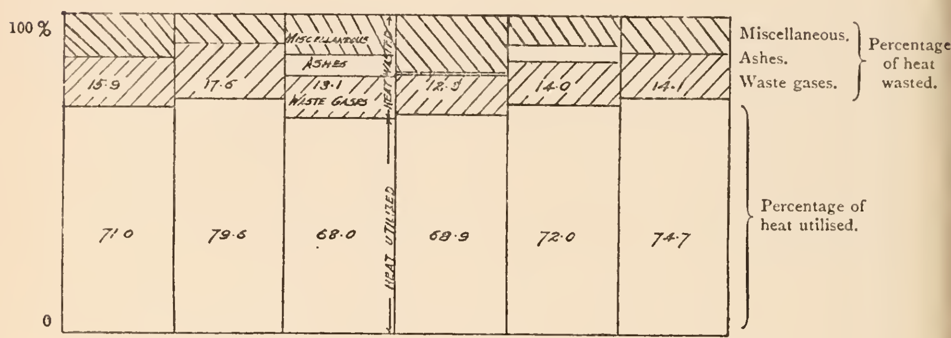


FIG. 2.—Results of the first trials after special training of stoker.

the total heat-losses were reduced to between 21 and 32 per cent. The best efficiency recorded in these diagrams is that of No. 2 installation, with 79·6 per cent. of the heat-value of the fuel utilised in steam-raising, compared with only 59·9 per cent. utilised before the transfer of the boiler plant to the Society's charge. This represents a gain of 19·7 per cent. in efficiency, and a saving of 20 per cent. on the coal-bill.

The municipal authorities of Hamburg are so convinced of the value of the work that the *Verein für Feüerüngsbetrieb und Räuchbekämpfung* is accomplishing in the city, that they have placed all their own steam-raising and heating plant under the charge of the Society's staff. The Hamburg Society has also been consulted by many outside authorities and bodies respecting smoke-abatement appliances, and inventions relating to fuel economy. The engineering staff of the Society is now devoting some attention to the question of smoke emission from marine-boilers, and is carrying on an investigation of the conditions obtaining in the boiler-rooms of the steamers that frequent the port of Hamburg, with a view to the application of the same methods of supervision and control that have proved so successful on land.

United States

The black smoke problem in America, though less difficult of solution than in the United Kingdom, owing to the more general use of closed stoves and of anthracite coal, or coke, for domestic heating purposes, is still a serious one. This is especially the case in such cities as Pittsburg and Cleveland, which are centres of the iron and steel industries, and in these and other American cities, where soft bituminous coal is burned in large quantities, the smoke-abatement movement has attained considerable force and momentum.

A decided impetus has been given to the movement in America in recent years, by the smoke investigation that has been carried on by the Mellon Institute of Industrial Research attached to the University of Pittsburg. The funds for this investigation were provided in 1911 by a generous Pittsburg citizen who wished to remain anonymous. The inquiry has engaged the attention of a staff of twenty-eight university graduates since early in 1912, and as regards some of the branches of investigation is not yet completed. When one states that the commission has eight physicians and five architects in addition to several chemists and engineers, and a botanist, a meteorologist, and a bacteriologist on its staff, one will understand that it is attempting to cover the ground very thoroughly, and that the data obtained ought to be of exceptional interest and value.

The investigation has been divided into two parts—the first being analytical or, to use an American term, "diagnostic," and the second constructive or remedial. Under the first head are grouped the meteorological, botanical, chemical, physical, architectural, hygienic and medical, and economic researches. Under the second are grouped the experimental and educational work of the staff, as well as the more important phases of the engineering investigation and of the legal administrative investigation.

Apart from these two main divisions of the work, provision was made for a brief history of the smoke nuisance in Europe and in the United States, and for an exhaustive bibliography of the subject, surpassing in scope and thoroughness anything hitherto published.

Eight Bulletins have so far been published, giving the results arrived at in particular branches of this investigation, and of these No. 3 on the *Psychological Aspects of the Smoke Problem*, No. 4 on the *Economic Cost of Pittsburg Smoke*, No. 5 on the *Meteorological Aspect of the Smoke Problem*, and No. 7 on *The Effect of Soot in Smoke on Vegetation*, will be found most interesting by readers of this journal. *Professor Wallace Wallin*, Ph.D., Director of the Psychological Clinic of the University, is the compiler and author of Bulletin No. 3, and his statements:

1. That sunshine is an important bio-dynamic agent;
2. That it promotes anabolism, transpiration, and perspiration, and increases the percentage of hæmoglobin;
3. That the blue and ultra-violet rays of sunshine exert a bactericidal effect on pathogenic bacteria, and a tonic, vitalising influence upon the human organism;
4. That sunshine exerts an exuberant influence on the feelings;

will be endorsed no doubt by the majority of scientific men in this country.

Mr. J. J. O'Connor, the statistician who is responsible for the figures given in Bulletin No. 4, estimates Pittsburg's loss by smoke and dirt at *$9,944,740*, equal to *£1,988,950 per annum*, or £5 13*s*. per head of the population.

Prof. Kimball, of the United States Weather Bureau, who has contributed Bulletin No. 5 to the series of publications, states that the chemical action of daylight in Pittsburg is only 60 per cent. of what it is in Sewickley, a neighbouring small residential town, and that, in general, the chemical intensity of sunlight in cities is 25 per cent. less than it is in small towns.

Mr. J. F. Clevenger, the compiler of Bulletin No. 7, gives details of the experimental observations made by him in certain of the public parks of Chicago, in Pittsburg and its vicinity, and also along the railroad between Pittsburg and Tyrone. Specific observations were made also at State College and in its vicinity. The conclusions at which he arrives are, that soot is poisonous to vegetation, especially to pines, and that the injury caused by it is due, chiefly if not entirely, to the ash, tar, and gases which it contains.

In connection with the Pittsburg investigation, measurements were commenced in April 1912 of the soot and dust fall in various parts of the city by a method of observation somewhat similar in principle to that now being used by the English Committee for the Investigation of Atmospheric Pollution. Twelve stations were selected, and samples of the soot fall were obtained by exposing glass jars measuring 4 in. × 10 in., for a month at a time, on the roofs of buildings, situated in the selected areas. Table III. gives the results obtained in these observations, which extended over twelve months, and the figures certainly seem to justify Pittsburg's claim to be considered the dirtiest and smokiest city in the world.

TABLE III. SOOT-FALL IN TONS PER SQUARE MILE PER YEAR IN PITTSBURG, U.S.A. (BREMNER.)

Station No.	Composition of Soot.				
	Tar.	Fixed Carbon.	Ash.	Fe_2O.	Total.
	Tons.	Tons.	Tons.	Tons.	Tons.
1	13	131	306	145	595
2	16	713	809	412	1,950
3	10	202	189	269	670
4	19	417	844	380	1,660
5	21	357	704	548	1,630
6	4	322	343	309	978
7	8	300	309	195	812
8	7	286	419	210	922
9	5	211	344	188	748
10	3	274	336	108	721
11	8	391	378	218	995
12	7	200	333	153	693

From article by John O'Connor, Jr., in *Metallurgical and Chemical Engineering*, April 1914.

It is interesting to note that at the Convention of the International Association for the Prevention of Smoke, held in Pittsburg in September 1913, a committee was appointed, on the suggestion of Bailie Smith, of Glasgow, to co-operate with the English committee in the introduction of the standard method of soot and dust measurement into America. One may endorse the hope expressed by Mr. O'Connor, that this committee will be successful in its work, and will obtain the co-operation of the public health authorities in America. In the near future, therefore, we may have the more progressive cities of U.S.A. competing with those of the United Kingdom and Germany, in their efforts to show a low soot-fall and clean atmosphere, as measured by the standard form of apparatus.

When one sees and notes the great attention given to, and the immense sums of money spent in these days upon, the provision of pure supplies of water and food for our city populations, it is amazing to find that the no less important factor of *pure air* has been neglected to so great an extent.

Pure water, pure food, and pure air are, in fact, the three most essential aids in the preservation of good health, and it is satisfactory to find that the importance of the third factor is at last being recognised.

TORNADOES AND TALL BUILDINGS

By JAMES HUNEKER, New York

About twenty years ago there was a lively discussion in the American press provoked by an article that had appeared in a now defunct daily newspaper. The writer, evidently a meteorological humorist, described New York in the throes of a tornado visitation. With saffron realism the advent was pictured of a live Western tornado, a "twister" of the Kansas variety. Across the New Jersey meadows came this horrid monster, dangling a smoky funnel like the trunk of an elephant. The noise was indescribable, a cross between a thunderclap and the booming of a million railway trains racing full tilt over loose steel bars. Of course, the sky was green, livid, and lurid. When, according to the lively fancy of the romancer, the tornado struck the river, it dragged up the clouds in a watery embrace—or was it windy?—a wall of water. The tall Battery buildings disappeared in a twinkling; where the Post Office once stood was a hole full of mud and débris, and naturally the Brooklyn Bridge was racked to its foundations, its harp-strings snapped, and when last seen was going seaward on the pinions of the storm. What became later of the inky column we do not remember. But the damage had been done. A doubt had been insinuated in the minds of many godfearing citizens that perhaps a whirlwind in New York city might play hob with its skyscrapers. Finally, architectural authorities being invoked the idea was pooh-poohed off the map. For instance the Singer Building weighs 90,000 tons, rests on caissons and concrete. It has been estimated that the wind pressure is 128,000 foot-tons. To guard against the tendency to lift on the windward side a set of big steel rods are run down into the concrete 50 feet, thus anchoring the building to the foundation. This building is 49 stories high. It seems tornado-proof.

But only to return at intervals. Every spring and summer brings its crops of Western and Southern tornadoes. As a rule they occur on the outskirts of cities or wreak their fury among small-frame houses. We are often asked if these storms are on the increase. The answer is simple: they are not. As the western and south-western states become thickly settled casualties may seem more frequent, but the storms are of normal number. Losses to life and property, while not being greatly exaggerated—for the old-fashioned tornado "scare" headline has gone out of fashion in Western journalism—are by no means out of proportion to the general average of living risks. This is proved by tornado insurance, a thriving branch of the business. As Prof. Cleveland Abbe wrote after the disastrous tornado of Kirksville, Miss., April 27, 1899: "In a few states, such as Illinois, Indiana, Iowa, Kansas, Maryland, Massachusetts, Missouri, and New Jersey, the probability that a given spot one mile square will be struck by a tornado is about once in a thousand years." Lightning and fire are much more dangerous to mankind. Another fact important for residents of New York is that it has never suffered from a tornado, severe as have been some storms in its vicinity.

Wallingford city in New Haven County, Conn., is not so far away. In August, 1878, there occurred a veritable "twister" of the veritable pattern; 34 persons were killed, 70 wounded, damage $200,000 (or about £40,000). Camden, New Jersey, a city opposite Philadelphia, suffered from a small tornado in August, 1885; 6 persons were killed, 100 injured, 500 houses razed, loss $500,000. I visited the scene of disaster the next day, and some of the buildings were levelled as if they had undergone a bombardment; yet across the Delaware river in Philadelphia there was no untoward disturbance—just a thunderstorm of average intensity. January, 1889, a most unusual season for "twisters," Brooklyn enjoyed a visit from a genuine whirlwind, funnel and all. It took place at 7.30 p.m. Its width was 500 to 600 feet, length 2 miles, whirl from right to left, roar heard 10 to 15 minutes before, loss $300,000. This was getting very near home. Chicago had a fright in 1876; the storm-cloud left the sky and bounded like a ball. St. Louis in 1890 caught a tartar, and May 25, 1896, it was bombarded by a tornado that played havoc, even shifting the massive new bridge at one end. Louisville, Kentucky, was the heaviest sufferer of all when, March 27, 1890, a cloud traversed part of the city, killing 76, injuring 200, and damaging $2,250,000 worth of property. And this cloud, described as turnip-shaped, did not touch the ground;

Lake Gervais cyclone as seen from St. Paul, July 13, 1890.

if it had, the destruction would have been epical. As for such tornadoes as those that wiped out Grinnell, Iowa, Marshfield, Missouri, and Wellington, Kansas (1893 the latter), they were all of the classic sort, coming late in the afternoon and giving plenty of warning because of the atmospheric conditions—the alternate streaks of chilly and sultry winds, and the peculiarly appalling appearance of the sky. For the experienced a rush to the tornado cellars or caves is the custom. In the public schools of certain cities a tornado drill is one of the rules.

The most vivid description of an approaching tornado was given by Mr. John R. Musick, in a lengthy article which appeared some years ago in the *Century Magazine.* A few lines may be quoted from the story of this eye-witness, a fair example of all such storms. "About 6.30 p.m. April 27, 1899," writes Mr. Musick, "I left my house in Kirksville, Missouri, to post some letters. The day had been rather remarkable, alternating between a suffocating heat and the chilliness of early spring. Dense black clouds occasionally rolled across the saffron sky, and showers of rain alternated with bursts of sunshine followed by a dead calm. As I stepped from the door a continuous roaring off to the south-west burst on my ears. In the sky hung a lowering thundercloud, from which peals of thunder issued. Just below the cloud, seeming to rest upon the earth, was a whirling monster of vapour, dust, and smoke, coming apparently towards me, with an incessant and steadily increasing roar. The first appearance was that of a huge locomotive emitting black smoke and steam, and coming at a tremendous speed. The tornado suddenly tore itself loose from the black storm-cloud and advanced at increased speed, rotating from right to left . . . the great funnel-shaped cloud expanding and extending up into the vault of heaven, spread over the entire eastern horizon. It was a dark, steamy cloud, from which were emitted evanescent flashes of electric light." Luckily the cloud turned into another street before it reached the house of the narrator. He tells of the devastation it caused and the freaks it played, among others carrying high in the air a young woman who, finding herself a neighbour to a flying white horse, was in much concern lest its kicking would hurt her. She was dropped, but the horse was carried for two miles and not the worse for its aerial experience. These experiences are commonplaces of tornadic storms like the stripping of fowls of their feathers, the twisting of great trees as if by a monster corkscrew, the bursting apart of houses on the vacuum side of the funnel and even the drawing of corks from bottles. I have seen trees into which were driven straws, so mighty is the impact of the wind.

Now, what would happen if a Kansas "twister" should caper across the North river and squarely hit New York city where it lives highest? If one talks to the architects it will soon be discovered that the fiercest wind ever forged in the caverns of Æolus would retire rebuffed by the battlements of steel and stone. But would it? Prof. Abbe does not think so. H. H. Hazen, once of the United States Signal Office, does not believe the affair would pass off so lightly. Prof. Hazen is an authority on the subject, and his study, *The Tornado*, is a standard book of reference. Moreover, Hazen is an iconoclast in his theories. He does not altogether subscribe to the inferences made by Espy and others as to the nature of tornadic storms. He even questions whether they always whirl counter clockwise; whether the column encloses a vacuum; whether the primal causes are entirely such as Espy believed them to be. Finally, he points to the disposition of the débris as a disconcerting evidence that the whirl may not be always in one direction. However, it may be conceded that there is an uprush in the focus of the storm, and that its origin is electrical. The thunderstorm and the tornado are first cousins in the kingdom of our sky. There is an overwhelming generation of heat in the cloud, which, as Abbe asserts without fear of contradiction, represents "a display of force beside which 10,000 great steam-engines shrink into insignificance."

It is the possibilities of American towering buildings that interest one. The higher the gale the greater the problem of strain and resistance; 70, 80, perhaps 100 miles an hour some of the new structures might successfully encounter, but a straight wind? The tornado is not a straight wind, but a circular one; it twirls, it grinds, it bores and lashes whatever it touches. It has several movements. It moves at the rate of from 30 to 80 miles an hour; what the velocity of its whirl is no man may say, though attempts have been made to compute it; 1,000 miles an hour is no doubt an over-estimate; about 250 miles an hour is nearer the truth. When the Wallingford tornado "blew off monuments in a cemetery without chipping either the upper or lower stone," it was calculated that a revolving wind of 260 miles an hour would be required to accomplish the fact. But estimates of this sort are apt to lead us astray. It is the incalculable force that strips fowls of their feathers and drives straws through railway ties that sets us to wondering if the tornado will not always be a puzzle to scientists. Its "eccentricity" manifests itself in the manner in which it rebounds from the earth or swings from side to side on its axis. Hazen declares that no two are alike in appearance or behaviour. Last summer when in London I was much interested in the accounts of a funnel-shaped storm that wrought disaster in Wales, coming through a gap in the hills and displaying all the indices of a full-fledged tornado. But such visitors are rare in Great Britain, though not in France, Austria, and certain regions of Germany. The waterspout, a wet brother of the tornado, is not missing in the Western world. The mountains and forests are the best safeguards against tornadoes; nevertheless, it has been written: "Nothing erected by the hand of man could withstand a tornado." And on this rather pessimistic note let us close.

NOTES

The Principle of Relativity

At the Annual General Meeting of the Aristotelian Society, held on July 13, the Rt. Hon. A. J. Balfour was elected President for the ensuing session. After the election of the officers and committee and the transaction of other business, Dr. H. Wildon Carr opened a discussion on the "Principle of Relativity and its importance for Philosophy."

Dr. Carr gave a short account of the scientific aspect of the subject, asserting that "the Principle of Relativity" affirms that neither space, time, matter, nor ether (if there is ether) is absolute, no one of these is one and the same identical reality for every observer, but that each is particular to the observer." He contended that this assertion was of value to philosophy in many ways. It showed that continuity was not in anything that was observed but in the observer. Continuity was a psychical principle. "Science can not only no longer find, but can no longer believe that it is possible there can be, a basis of absolute reference in any fact observed, whether it be formal or material, and is driven by exclusion to find the continuity necessary to its existence in the psychological principle of an observer in a system of reference. The problem of philosophy is thereby raised to a new and higher level."

Dr. T. P. Nunn contended that the importance of the Principle of Relativity was greatly overestimated. There was nothing particularly strange or startling for those who were acquainted with the works of Mach and Ostwald. Poincaré, notwithstanding his scientific and mathematical eminence, was hardly sound on the philosophical side, and was prone to make sensational statements, without adequate foundation. Anyone who had got rid of the concept of mass as a quantity of stuff would find no philosophical difficulty in non-Newtonian mechanics.

Mr. Shelton agreed that the philosophical importance of the principle was overestimated, but for a different reason. He contended that the metaphysics began much sooner than the relativist knew. All the mathematics proved was that it was possible to interchange time and a dimension of space in certain equations, a fact interesting mathematically but unimportant philosophically. The experiments were capable of a variety of explanations.

Mr. Worsley said that Einstein's argument concerning the change of time with velocity was a paradox easily solved, if not a mathematical error.

Dr. A. Wolf said that the relativists were continually confusing the metaphysical question of absolute space and time with the purely physical one of the possibility of an absolute measurement of space and time.

Mr. Benecke thought that the displacement of spectroscopic lines with the velocity of the source was a fact that the relativists would have some difficulty in explaining on their hypothesis.

Dr. Dawes Hicks agreed with Dr. Wolf on the confusion involved in the principle of relativity. He said that actual time measurement involved great experimental difficulties.

After some further discussion, Dr. Carr replied.

Action of the British Science Guild

We are very glad to see that at the July Meeting of the Executive Committee of the British Science Guild, Sir Norman Lockyer in the Chair, a special committee was appointed "to consider and report upon various matters arising in connection with Science and the State and the encouragement of discovery referred to in an address delivered by Sir Ronald Ross at the Annual Meeting of the Guild at the Mansion House on May 22 last." The Committee consists of the Right Honourable Sir William Mather (President of the Guild), Sir Norman Lockyer (Chairman), Lieutenant-Colonel Sir Charles Bedford, Honourable Sir John Cockburn, Professor Meldola, Major O'Meara, Sir Boverton Redwood, Major Sir Ronald Ross, and Professor Sylvanus P. Thompson.

This Committee has before it a very great field of work which has not been taken up by any other body in the way which, in our opinion, has been demanded by the needs of science; and we wish it every success. Those who desire to support possible movements for the betterment of science would do well to join the Guild. The Office is at 199, Piccadilly, London, W.

Undergraduates and the Betterment of Science

There is at present evidence of much intellectual stagnation in this country, probably induced by old mental adhesions and ossifications. Some might ascribe this to mental paralysis, even to senile paralysis; because it is probable that countries like individuals tend to become older. Fortunately countries possess a constant growth of new cells, which, according to some physiologists, is not found in the brain of individuals. The new cells of the community are the young people, and with them the future lies. It is therefore with peculiar gratification that we note the following plain speaking in the columns of our contemporary *The Undergraduate*, and we are glad to see the support it gives to the campaign in which we are engaged :—

"What is the complaint? It is this, that society will not remunerate the thinking, that seeks to observe and establish principles. Effective work in other directions obtains lavish recognition. An organiser who provides good and reliable meals for the people, near their work, is hailed a captain of industry, and achieves an enormous fortune. The maker of a slightly superior soap, the distributor of tea, sugar, and butter, on a system which permits their cost to the poor consumer to be ever so slightly reduced, secures a like reward. The provider of luxuries, the manipulator of shares on the Stock Exchange, the bookmaker, the brewer, in return for their small contributions to the common weal, are enabled to retire in affluence.

The scientific worker and thinker is outside the network of channels in which the streams of money flow. If he attempts to tap the channels, as Kelvin did, and as Ehrlich does, there is a murmur. Why should there be? Why should he be debarred access to the means by which he, too, can marry, can educate his children, and secure his own old age? If his work is to be at the disposal of all, as in many cases it must be . . . then it is the duty of the State to recognise the work, not by empty honours, not by posthumous favours through the Civil List, but by definite, clear, and appropriate payment.

If society will not do this voluntarily, if it is so far apathetic to its own material interests, the time cannot be far distant, when definite and unequivocal steps will be taken, to protect scientific speculation and enterprise. Science has many sympathisers who are capable of organising it; and there are within 'the limits of the constitution,' innumerable expedients by which the results of research and patient scientific enquiry can be withheld from society, until society pays."

CORRESPONDENCE

LE CHATELIER'S LAW AND INTRA-ATOMIC CHANGE

MR. HOLMES' criticisms of my theories of solar heat and of the origin of radioelements[1] call for some comment and reply. He asserts that my theories are inconsistent with Le Chatelier's reaction law, accuses me of creating a difficulty that does not exist, and is good enough not only to make erroneous criticisms on the theories as published, but to give a still more erroneous account of what I am supposed to mean but do not say. To this I can only make the reply that my theories are not inconsistent with any valid chemical law, that the difficulty is entirely one of Mr. Holmes' own making, and that what is meant by my articles is precisely what is said, not what Mr. Holmes thinks I ought to mean.

For simplicity we will consider the theory of the origin of the radioelements first. My suggestion is thoroughly in accord with Le Chatelier's law and, moreover, this is the case where, if applicable at all to intra-atomic change, it would apply. In the interior of the Earth we can assume the existence of matter under conditions of approximate mechanical and thermal equilibrium. Should any cause whatever—mechanical stress, increased pressure or temperature, or both, tidal friction—occasion a concentration of energy over and above a certain limit, it is suggested that the energy is stored in the formation of radioactive compounds. As the radioactive matter gradually works its way to the surface, the stresses are removed, temperature and pressure are reduced, and the energy is slowly evolved once more in radioactive decay.

The theories of solar energy are on a different basis. In this case to bring in Le Chatelier's law of reaction, or any similar consideration, is not allowable. Rules of this class apply only to chemical systems in equilibrium, and the hypothesis I have put forward is the progressive evolution of energy of a metastable system assuming a condition of greater stability. If Mr. Holmes requires a chemical analogy the reaction between hydrogen and chlorine in diffused daylight is perhaps as good as any. A certain amount and quality of energy must be supplied externally, and then the reaction proceeds with *evolution* of energy. Without the initial energy (in the case of hydrogen and chlorine in the dark) the reaction will not work. The temperature, or other stellar conditions, are compared by me to the light energy necessary to the hydrochloric acid reaction. I therefore do not assume a formation of a stabler form with *absorption* of energy. This is Mr. Holmes' idea and appears to me to be unsound. If Mr. Holmes will try, bulk for bulk, to calculate the temperature which would correspond with the energy evolved radioactively, he will see that his idea is not very probable.

[1] This Journal, July, 1914, p. 23.

Certainly it is not mine. My idea is that the radioelements and the nebular elements are both less stable than those commonly found in the crust of the Earth, and than those the lines of which are found in the spectra of stars approaching extinction. Mr. Holmes will find greater detail in the articles to which he refers, and would do well to read them with the presupposition that (with the exception of three lines obviously recast by the printer by the light of nature) they are intended to mean what they say. I would add also that if Mr. Holmes attributes to the experiments of Boltwood on lead ratios, which he has repeated and extended, any validity whatever, he is bound to adopt some hypothesis of the kind to account for the duration of solar heat. Certainly the hypothesis, *as stated by me*, whatever criticisms may be made, is not contrary to any recognised and valid law of chemical reaction.

The present note provides an opportunity for me to modify a statement made in my last article on geologic time.[1] In that article I stated that Prof. F. W. Clarke had expressed doubts concerning the validity of Prof. Joly's theories of geologic time based on the amount of sodium in the sea. That statement is strictly accurate.[2] I have just discovered that, more recently, he has practically identified himself with Prof. Joly's views,[3] by a strange coincidence, at a time when Prof. Joly shows signs of abandoning them. I, therefore, make the correction. The argument of the article, however, stands, and I shall be greatly interested to discover that either Prof. Clarke, or Prof. Joly, or any one else can answer it. Prof. Clarke seemingly places no value whatever on radioactive methods of investigation.

[1] This Journal, July 1914, p. 57.
[2] *Data of Chemistry*, 1st edition, p. 110.
[3] *Ib.* 2nd edition, p. 138 and elsewhere.

H. S. SHELTON.

REVIEWS

Perception, Physics, and Reality: An Enquiry into the Information that Physical Science can Supply about the Real. By C. D. BROAD, M.A. [Pp. xii + 388.] (Cambridge: University Press, 1914. Price 10*s*. net.)

THE volume under review is a slight expansion of a thesis submitted for Fellowships at Trinity College, Cambridge. Mr. Broad was duly awarded a Fellowship, and it is interesting to note that the University of Cambridge encourages those of its own graduates whose work shows exceptional merit. This is noteworthy in the present instance, because the intellectual standard of the volume is superior to that of the ordinary student's thesis. Moreover, Mr. Broad shows scant respect for constituted authority, past or present.

It must not be inferred from the above that the volume contains anything specially original or startling. While it can hardly be described as a work of genius, it is a solid, balanced, well-argued essay in metaphysics. Its main thesis is strongly reminiscent of Mr. Bradley's epigram: "Metaphysics is the finding of bad reasons for what we believe on instinct, but to find those reasons is no less an instinct." We quote the epigram with the proviso that Mr. Broad's reasons are somewhat better than usual. Mr. Broad is one of the new realists. The natural assumption of the average man is that of naïf realism.

"Common sense is naïvely realistic wherever it does not think that there is some positive reason why it should cease to be so. And this is so in the vast majority of its perceptions. When we see a tree we think that it is really green and really waving about in precisely the same way as it appears to be. . . .

"But, as every one knows, we do not stay in this happy condition of innocence for long. We have perceptions which are believed to be illusory, by which it is meant that their objects only exist when they are perceived. . . ." (p. 1.)

Mr. Broad's conclusion is not quite so simple, but is as near as philosophic reasoning will permit. The assumption underlying the whole argument is that, if we reason deeply enough and go far enough, the truth about existence is very much that which the average common-sense man unconsciously assumes. The book might well be entitled a refutation of idealism, had not that title already been used by another recently published work.

So far so good; but the special relevance of physics to the discussion is not yet quite clear. Mr. Broad starts with an advantage shared by few in the philosophical world. Without being a specialist in any branch of science, and without (so far as we are aware) having added anything to scientific knowledge and research, he has studied some branches thoroughly enough to speak of matters scientific from direct personal acquaintance. We can thus be confident that his work will be free from the blunders and misunderstandings to which philosophers are so liable when they write about science without a sound groundwork of detailed knowledge. It must be said, however, that the science and the philosophy rarely come into direct contact. The ostensible object of the volume is "to attempt to discover how much natural science can actually tell us about the nature of reality, and what kind of assumptions it has to make before it tells us anything." It is difficult to find in the volume conclusions which can be described as an approximate and provisional answer to the questions raised.

There are, nevertheless, a large number of points of interest. In some quarters there seems to be a vague impression that there is likely to be founded a new philosophy based on science which will displace philosophy as commonly understood. Ostwald and Mach are the principal writers who commonly appeal to this school. To such, Mr. Broad's volume, and especially the chapter on phenomenalism, can be recommended as an antidote. Although Mr. Broad is in sympathy with the scientific point of view, and acknowledges the philosophic importance of scientific research, he is opposed to a narrow scientific view of philosophy. On the other hand, he emphatically asserts the probability that scientific constructions and hypotheses (such as atoms and electrons) may possess a greater degree of reality than the school of Mach will allow. While such information about reality is not certain, there is no sound philosophic reason for asserting that such constructions are merely conceptual shorthand. In matters like these, Mr. Broad is an admirable advocate of the common-sense view of science.

It seems inevitable that works dealing with science in its relation to philosophy should devote considerable space to the philosophy of cause. The volume under review is no exception to the rule. The chapters on causation and on the causal theory of perception occupy together more than half the book, and it would be hopeless to attempt to summarise them in a brief review. Nor do the conclusions on the laws of mechanics admit of brief and simple statement.

The appendix is of more than usual interest. Certain aspects of the Principle of Relativity are criticised, and the author thinks that some mental confusion is involved in this interpretation of experimental data. When robbed of the element of confusion the residuum need worry no one.

The volume is of considerable merit. It should be mentioned also that the dullness of philosophic disquisition is enlivened with occasional epigram and joke.

H. S. SHELTON.

X-Rays: An Introduction to the Study of Röntgen Rays. By G. W. C. KAYE, M.A., D.Sc., Head of the Radium Testing Department at the National Physical Laboratory. [Pp. xix + 252.] (London: Longmans, Green & Co. Price 5*s*. net.)

THE X-rays appeal to so many different classes of workers that a book which gives a full and at the same time not too abstruse account of all that is known about them at the present time is sure to find a ready welcome. Dr. Kaye's book contains in a very compact compass an immense amount of valuable information, written in a clear and lucid manner, mainly from the physical point of view. It avoids the purely technical and also the more mathematical aspects and brings together a great number of widely scattered important physical researches which have not before been collected in book form. On this account it will find a useful place on the shelves of all those who are trying to keep abreast of modern physical researches. But it should also appeal to medical men and medical students, especially those about to take up the subject for the first time and anxious not to depend for their information on purely trade sources. It is obvious that finality is very far from having been reached even in the methods of X-ray production. It is probable that in the future instead of using the same means, an induction coil, for producing the electrons from the metal cathode as for accelerating them sufficiently through the vacuum to generate X-rays by impact upon the target, the operation may with advantage be done in stages, and more appropriate means devised for the two stages separately than for both together. Thus the Coolidge X-ray tube, described fully in one of the appendices, which so far has hardly been tried in this country, generates its electrons from an incandescent coil of tungsten wire, and operates therefore in an absolute vacuum. Whether it justifies itself or not in practice, it is clear that some knowledge of the theory of electrons and X-rays, beyond that which sufficed for the earlier pioneers, is likely to prove very useful and interesting to the large and increasing number of people who use X-rays in their daily work.

Judged by the following sentence in the introduction, which it is only fair to say is not representative of the quality of the book, the author appears to take a somewhat narrow and partial view of the recent profound changes which have overtaken physical science, for it would be difficult to give an adequate historical justification for the statements it contains. We read : "The discovery of electrons provided us with the present accepted theory of the constitution of matter ; it paved the way for a ready recognition of the properties of the radioactive elements, then on the point of discovery ; and it led to a new school of physics which accepted as a creed the transmutation of the elements, an idea utterly repugnant to the orthodox chemist who had been taught to regard the elements as fundamental and immutable. . . ." Whether any one is so foolish as to accept transmutation as a creed now or not, it is some eleven years since it was established as a fact in the domain of the radio-elements by the researches of chemists equally with those of physicists. The suggestion that the electron or the electrical theory of the constitution of matter played any part at all in the establishment of the fact is unfounded. If that theory is really accepted at the present time it must be rather as a creed than as established by experiment, for it fails to account for more than one two-thousandth part of the mass of matter. Though as a first step it has proved itself extremely suggestive and fruitful in almost every direction, and doubtless in time will assist in the still unsolved problem of artificial transmutation.

The time is just ripe for the appearance of a good book on the X-rays. The work of Laue and his colleagues on the reflection of X-rays from crystal surfaces, has shed light on many problems, and most of all on the nature of the X-rays themselves, the name of which is now a misnomer after eighteen years of appropriateness. These researches and the corresponding ones of the Braggs, Moseley, and others in this country, although so recent, occupy an important place in the present volume. In an appendix, Sir James Mackenzie Davidson, one of the earliest pioneers in the medical uses of the X-rays in this country and the inventor of stereoscopic methods of localisation, contributes an interesting interview with Prof. Röntgen at Wurzburg soon after the famous discovery.

F. S.

An Introduction to the Study of Organic Chemistry. By H. T. Clarke, D.Sc., F.I.C. [Pp. viii + 484, with diagrams.] (London : Longmans, Green & Co., 1914. Price 6*s.* 6*d.*)

In compiling this book the author's intention has been to display the orderly principles and structural unity of organic chemistry rather than to direct attention to details, and descriptions of practical methods are accordingly omitted.

The first chapter, which is devoted to an exposition of the simplest features of the chemistry of carbon compounds, explains in a lucid manner the relations of the hydrocarbons and their simple halogen and hydroxyl derivatives, and should be readily intelligible to the beginner. It is, however, doubtful whether the same can be said of the second chapter, in which, as the author admits, the ground is traversed with extreme rapidity, the object being to familiarise the student with the structural formulæ of the chief types of aliphatic compounds; such a rapid survey of the subject, as is contained in the twenty pages of this chapter, might be useful to the student later on in his studies, but it is more than likely to frighten and confuse the beginner. The third chapter deals with methods of purification and analysis, and the remaining thirty-four chapters cover the ground required by the new syllabus of the lower examinations in organic chemistry in the Board of Education Examinations in Science and Technology; they are, for the most part, clearly and attractively written. The chapter devoted to carbohydrates is, however, somewhat disappointing; it is surely a pity that a newly written text-book on organic chemistry should contain no mention of the γ-lactone formulæ for α- and β-glucose and their bearing upon the phenomenon of mutarotation. This is the more surprising as the γ-lactone formula for methylglucoside is given, although its connection with the two forms of glucose and the behaviour of the two methylglucosides towards enzymes is not even mentioned. For some unexplained reason the author avoids using the word polysaccharide, the substances ordinarily included under this heading being described as higher carbohydrates. The statement on page 278 that fructose, on careful oxidation, yields glycollic and trihydroxyglutaric acids requires correction; and to say that maltose on fermentation with yeast is like glucose converted into alcohol and carbon dioxide, without mentioning the enzyme maltase, is rather misleading.

Nucleic Acids: Their Chemical Properties and Physiological Conduct. By WALTER JONES, Ph.D. [Pp. viii + 118.] (London: Longmans, Green & Co., 1914. Price 3*s.* 6*d.* net.)

THE preface to this monograph contains the encouraging statement that the nucleic acids constitute what is possibly the best understood field of physiological chemistry, but the average reader is hardly likely to find that it is a very easily understood field. The chemistry of the nucleic acids so far as it is known is very complicated, and the first three chapters, which are devoted to the chemical properties of these substances, will be found to make considerable demands on the reader's chemical knowledge.

The discovery of the nucleic acids is due to Miescher, who, working with salmon milt, found that the spermatozoa heads, which may be regarded as metamorphosed nuclei, were made up almost exclusively of a single chemical individual, namely, the salt of an organic nitrogen base or protamine with an organic acid termed nucleic acid. Subsequent workers have from time to time described a number of different nucleic acids from various sources, but the opinion is gradually gaining ground that all these substances are identical with either thymus nucleic acid obtained from animals or yeast nucleic acid occurring in plants. The chief difference between these two substances is the fact that while the former yields on hydrolysis phosphoric acid, guanine, adenine, cytosine, thymine, and lævulinic acid, the latter gives rise to phosphoric acid, guanine, adenine, cytosine, uracil, and a pentose. The formation of lævulinic acid from thymus nucleic acid points to the presence of a hexose complex, whereas the carbohydrate obtained from plant nucleic acids is invariably found to be the pentose *d*-ribose. Partial hydrolysis of these nucleic acids gives rise to substances known as nucleotides, in which a carbohydrate group links together phosphoric acid with a purine or pyrimidine base, while ferment action produces from nucleic acid the so-called nucleosides which are composed of one of these nitrogenous ring compounds coupled to a carbohydrate complex. These facts have led to the conclusion that the nucleic acids are in reality tetra-nucleotides, that is to say are compounds produced by linking together four different nucleotides containing as their nitrogen bases either guanine, adenine, cytosine and thymine or guanine, adenine, cytosine and uracil, according as they are of animal or plant origin. The author's views concerning the vexed question of the origin of urinary uric acid may be gleaned from the following quotation: "If one is inclined to believe that uric acid is not formed in the body from nucleic acid, he should at least note that the organism is equipped with a mechanism that can effect all of the transformations necessary to its formation." Much useful information concerning the preparation of the two nucleic acids and their derivatives and the demonstration of the purine ferments is contained in the appendix.

The author has succeeded in reviewing this most difficult subject in a very able manner, applying his criticisms in an impartial, though at times humorously caustic manner.

P. H.

The Simpler Natural Bases. By GEORGE BARGER, M.A., D.Sc. [Pp. viii + 216.] (London: Longmans, Green & Co., 1914. Price 6*s*. net.)

With the discovery of the physiological action of extracts of putrid meat upon the blood pressure and the isolation by Barger and Walpole of the active bases isoamylamine and p-hydroxyphenylethylamine from these extracts, the investigation of the chemistry of putrefaction received a great stimulus. Subsequent investigation proved that these bases were produced by decarboxylation of amino acids derived from protein, and a systematic search was therefore undertaken for the amines corresponding to the other fission products of proteins; it was thus found that the activity of ergot was due principally to p-hydroxyphenylethylamine and β-imidazolyl-ethylamine, the amine corresponding to histidine. This discovery has since been put to practical use inasmuch as β-iminazolyl-ethylamine is at the present time being manufactured commercially by bacterial decarboxylation of histidine.

In view of the extensive literature which has grown up around the substances within the last few years a summary of our present knowledge of this subject is to be welcomed. The title chosen for the monograph having no precise chemical significance, the author has been compelled to use his own judgment in determining what substances should be included under this heading, with the result that the purine bases have been omitted. This decision, in some ways regrettable, was no doubt wise in view of the fact that the monograph has already attained considerable proportions. The subject-matter has been divided into seven chapters, the first three of which deal with bases derived by slight modifications from the constituent units of protein, while the remaining chapters deal successively with choline and allied substances, creatine, creatinine and guanidine, etc., adrenaline and a group of bases of unknown constitution. The enormous mass of literature on creatine and creatinine and also on adrenaline has been very skilfully summarised and made into a more or less connected story, the value of which is considerably enhanced by the addition of a very complete bibliography. Perhaps the most valuable chapter, however, is the last, which deals with the methods employed in the separation and isolation of the various substances described; it contains a great mass of information in a very compact form. The book can be strongly recommended to chemists of all shades of opinion whether specially interested in biological problems or not, as it deals with a number of substances not as yet described in the text-books, but none the less of such importance that even the pure chemist cannot afford to ignore them.

P. H.

Principles of Metallurgy. By ARTHUR H. HIORNS. Second Edition. [Pp. xiv + 389; 157 illustrations.] (London: Macmillan & Co., Ltd., 1914. Price 6*s.*)

THE first edition of this useful summary appeared in 1895, and has proved to be of value to teachers and students in technical schools and classes. It was time that something was done to bring the book up-to-date, but Mr. Hiorns, who was until lately the Head of the Metallurgy Department, Birmingham Municipal Technical School, can hardly be congratulated on the thoroughness of his revision. The changes in the text are few and unimportant, and the result is that the book may be called a very fair presentation of the state of knowledge in metallurgy in the late Victorian period. Those who already possess the first edition need not consider the desirability of obtaining the second, but it is otherwise in the case of new students who have neither, and especially with those who have a taste for the historical side of their subject.

The Quaternary Ice Age. By W. B. WRIGHT. [Pp. xxiv + 464, with numerous illustrations.] (London: Macmillan & Co., 1914. Price 17*s.*)

THE dedication by Penck and Brückner of "Die Alpen im Eiszeitalter" to the author of "The Great Ice Age," is sufficient testimony to the impression made by James Geikie upon European thought. Since, however, the issue of the last edition of the Edinburgh professor's comprehensive work, researches on glacial phenomena have multiplied in every continent. Mr. W. B. Wright has now brought together the results of these investigations, in a book that in size and excellence of production will stand fitly beside that of his predecessor. His choice of illustrations, including many due to his own work in the field, would in itself commend this treatise.

In some respects the book is almost too modern, and gives but a slight impression of the long struggle between the supporters of the view that continental ice-sheets once spread across vast surfaces of land, and those who, on the other hand, urged the efficacy of ice-rafts and running water. Hall and Playfair, the extreme but triumphant Agassiz, Lyell, erring on the side of caution, Andrew Ramsay, and Goodchild, a pioneer in various branches of geology, alike find no mention in the index. T. F. Jamieson, as is indeed just, meets with full and honourable recognition. This aloofness from the historic aspect has the merit of freeing the book from controversy. The author's personal views are simply stated, and his advocacy of isostasy as an explanation of recent changes of sea-level finds much justification in his own researches and is by no means forced upon the reader.

We may feel that Mr. Wright, in adopting the view that eskers are mostly formed in water near ice-margins, overlooks the possibility of their development under motionless stagnating ice. In referring the walls of cirques to frost-action, the conclusions of Matthes on "nivation," as a first cause of the cirque-hollow, should be cited, especially since his observations are so easy to repeat in Spitsbergen. The description of drumlins is excellent; but we fancy that too much stress is laid on the hypothetical "ground moraine" (p. 29), said to be formed beneath the ice, and too little on the accumulation of everything that gets into the ice, whether by plucking or gravitation, towards the lower layers of the sheet. In the field it appears that this englacial material is the true source of boulder-clay.

But these are details foreign to the main purpose of the book. It presents an admirable view of the features left by the last ice-age, and its title implies a recognition, so often lacked by glacialists, of previous and probably very similar ice-ages which also affected the whole globe. A mention of these in the chapter on theories of the Ice-age may be useful in the next edition. Scandinavia, as a well-worked and typical district, is dealt with in special detail, and serves, with the Great Lake region of North America, as a basis for the isostatic theory of shore-lines. Mr. Wright has given us the fruits of a wide range of reading, and his very lucid and attractive style is an additional claim upon our gratitude. We note extremely few misprints, such as "Rundhockorn" on p. 31. The use of the spelling "isostacy" is evidently intentional. G. A. J. C.

Geological Excursions round London. By G. MacDonald Davies, B Sc. F.G.S. [Pp. viii + 156.] (London: T. Murby & Co., 1914. Price 3*s*. 6*d*. net.)

London, in addition to its great academic schools of geology, includes also the headquarters of the Geologists' Association, a body whose activities seem to grow, from year to year. Mr. Davies's book will thus recall to generations of students their first instruction from actual sections and from land-forms viewed from the Chilterns or the Surrey hills. It assures us that there are still people in our great cities who move about occasionally on foot, and who love the quiet of the Chalk downs, flecked with cloud-shadows, or the crisp heather of the Greensand ranges, remote for a time from the dust and hooting of the highways. In their excursions, with the help of this pocket guide-book, they may learn a great deal about the rocks that form the surface, from the Jurassic period onwards, and also about the unseen Armorican range, against and across which these successive strata were laid down.

Mr. Davies's illustrations revive our first enthusiasms. Some of us, under the care of Prof. Judd, were drawing sections of Tilburstow Hill and Croham Hurst nearly forty years ago. The author does not fail to point out where fine views may be obtained, and he even leads us to tea-houses in delightful places. To those who do not know the London Basin, the extent of rural landscape and unbroken woodland within thirty miles of St. Paul's will be amazing. The first ten miles should discourage no one who has an object in the country at the other end; and Mr. Davies's photographs of quarries and descriptions of field-routes provide us with every inducement to go out and observe. A very good geological map in colours is given as a frontispiece. G. A. J. C.

Lehrbuch der Anthropologie, in systematischer Darstellung. By Dr. Rudolf Martin. [Pp. xvi + 1181. With 3 plates, 460 figures in the text, and 2 observation-forms.] (Jena: Gustav Fischer. Price 35 marks.)

Anthropology is certainly not the least important of the pure sciences, and might even put in a claim to be regarded as the most essential of them all. It forms the connecting link between zoology and sociology and between zoology and medicine. It is the means whereby the application of biological discoveries to all the phenomena of human life may be made patent. Yet this science, "the natural history of the Hominidæ," as Professor Martin aptly calls it, is given a status in our educational system which is very inferior to that of zoology or botany. There are at present no professorships of anthropology in the majority of universities and university-colleges in the British Isles, and the same condition obtains in most of the universities of Germany and Austria. As has often been pointed out before, this is an especially serious deficiency in Great Britain, a country which has undertaken to govern three hundred millions of people belonging to all manner of alien races. Even zoology, however, is scarcely a century old (as a serious science), and optimists may hope that before long a B.Sc. in Anthropology will be deemed an essential qualification for an Indian or Colonial administrator—aye, and even for a Member of the Imperial Parliament.

It is from this point of view that we approach Dr. Martin's work, for if university-courses in anthropology are conspicuously few, university text-books are virtually non-existent. The book deals only with physical anthropology, there being no reference to the social or cultural side of the subject. This is no doubt an advantage, and is indeed unavoidable, since the two branches of the science are very distinct, and a student of cultural anthropology requires a somewhat different training. The book is intended especially for the enlightenment of medical students and prospective explorers, and has been written with particular reference to the methods of making accurate observations. The author lays great stress upon this point, as he believes very rightly that much confusion and even actual waste of valuable material have been occasioned by the diverse modes of making and recording observations adopted by different pioneers. Dr. Martin's laboratory and teaching experience in Zurich University enables him to speak with authority on this subject, and not only are there detailed descriptions of the apparatus and of the correct methods of using the various instruments, but the numerous figures assist the student in understanding this somewhat difficult technique. Moreover, the study of groups, as distinct from the study of individuals, is not forgotten, the new statistical methods developed by the biometricians being fully described. This part of the book is very valuable, and so far from Dr. Martin's labour being as he modestly says "a thankless task," we think that for this reason alone his handsome work ought to obtain a hearty welcome in Britain and America, as well as in Switzerland and Germany. We know of no other book, English or German, which deals nearly as thoroughly with this essential part of an anthropologist's training. The two observation-forms are included in order that the student may be able to apply the methods for himself and tabulate his results in a systematic manner.

It is unfortunate that the work does not cover the whole field of physical anthropology. It does not deal with the anatomy of any of the soft parts, except such as can be studied during life ; and there is no treatment of philosophic principles, nothing about the factors or manner of human evolution. This, it must be admitted, unfits the book to serve as a complete text-book for the advanced student of the science, since the subjects omitted are of course vitally important. The study of the brain is the link between physical and social anthropology, and the application of the principles of evolution to the Hominidæ may be legitimately described as the chief *raison d'être* of the zoologist who has specialised in the higher Primates. This, however, does not detract of course from the value of the book as a partial treatise.

The work is divided into four parts, the first dealing with certain general matters, the second with somatology (*i.e.*, with the anatomy of the soft parts that can be studied in the living subject), the third with craniology, and the last with the skeleton other than the skull. An admirable feature in Parts III and IV is the full treatment of fossil skeletons, both of *Homo sapiens* and of *Homo neandertalensis* It is most gratifying to find full details of the osteology of both living and fossil races in the same volume. We infer that the bulk of the book had gone to press before the Piltdown discovery was made known to the world, for there are only brief references to *Eoanthropus*. We think it should have been possible to append notes on this skull. There are constant references to the unimportant Galley Hill skeleton, but the more interesting " Ipswich man " is not discussed, which is regrettable, because we should have liked to have known Prof. Martin's views upon that specimen's singular tibia.

Another excellent feature is the constant linking up of the details of man's structure with the corresponding points in the anatomy of the living apes, and even of the living monkeys and lemurs. The author fails, however, to see the importance of the fossil apes and largely ignores them. Although only fragments of the fossil Simiidæ are known, those fragments are of the greatest evolutionary significance. For instance, the remarkable femur found in the Pliocene at Eppelsheim, and variously described as *Paidopithex* or *Dryopithecus*, ought to have been included in the table on page 1018, especially as its form contrasts sharply with that found in the living higher apes. Even where the author does refer to the fossil apes, he is apt to blunder. Thus in his table of the Anthropoidea on pages 10 and 11 he includes *Propliopithecus* and *Pliopithecus* with the higher apes, and not in the Hylobatidæ, where they really belong, a mistake which is apparently due to carelessness, since the position of these apes is correctly stated in another place. *Parapithecus*, which Schlosser makes the type of a new family, is omitted from the table altogether, though it is mentioned subsequently in a footnote.

There is a good summary of what is known of prehistoric man in Part I. Prof. Martin thinks, with many French paleontologists, that *Homo sapiens* did not appear until after the Würmian (or fourth) Ice-Age, but in a text-book intended for students he ought to have explained the alternative paleolithic time-table, which places the apparition of *H. sapiens* in the Third Inter-glacial Epoch. This alternative scheme may or may not be right, but it is more generally accepted than the one set out on page 14, and a student, as we say, should be taught both.

There is an immense bibliography, covering 100 pages, but the index is very inadequate.

A. G. THACKER.

Untersuchungen über Chlorophyll Methoden und Ergebnisse. By R. WILLSTÄTTER and A. STOLL. [Pp. 419, 16 text figures, and 11 plates.] (Berlin : Julius Springer, 1913. Price 20*s*. 6*d*.)

IT is difficult to express in brief terms the contents of a book like this, which gives a comprehensive statement of the methods and results of the classical series of experiments that have been carried out by Willstätter and his co-workers during the last ten years. Probably no finer example could be found of the light that chemistry can throw upon complex biological problems or of the methods by which success can be achieved in difficult fields of work. Every chapter illustrates the care with which the work has been carried out ; seldom probably has a greater piece of work in biochemistry been carried out in which speculation has been so rigidly restrained and made to wait upon the careful establishment of chemical facts. The complex problems that the chemistry of chlorophyll presents are stated and to a remarkable extent solved, and yet hypotheses as to the function of the pigment in the plant are neither advanced nor discussed, with the exception of a few very suggestive paragraphs in the first chapter (see pp. 24 and 25).

After the first chapter, in which the bearing of this new work upon the constitution of chlorophyll is considered, chapters are devoted to the methods of extraction and separation of the various pigments of the plant chloroplast. It is not too much to say that these chapters, which contain a considerable account of previously unpublished methods, will go far to revolutionise the crude methods of extraction at present in use in botanical laboratories.

It is very interesting to learn that with their new methods Willstätter and Stoll are able to obtain preparations of pure chlorophyll from some kilogrammes of dried leaves within so short a space of time as one day and with a yield of some 6½ grammes of chlorophyll per kilogramme of dried leaf powder, a yield which probably represents 75 per cent. of the available pigment.

The genesis of the new methods seems to lie in the fact that the dried leaf material does not readily yield its pigment to pure organic solvents, but that extraction is rapid in, for instance, acetone containing some 15 per cent. water, The water seems to alter the colloidal nature of the matrix in which the chlorophyll is retained and it is then readily obtained in solution. Attention may be specially drawn to an interesting feature of the book contained in Chapter II. This consists of descriptions of a series of experiments upon a conveniently reduced scale, by means of which the reader may acquaint himself with the general principles of the quantitative methods to be described in detail in subsequent chapters. The majority of these experiments should subsequently find their way into laboratory courses in plant physiology.

The isolation of the two chlorophylls *a* and *b* is fully described, and full details given of the long and difficult processes by which quantitative separation is effected ; the ratios of these two components in different chlorophyll extracts is compared. and the fact brought out that they occur in surprisingly constant proportions in different plants. The separation and isolation of the two yellow pigments xanthophyll and carotin is also described and their quantitative distribution compared. The constant occurrence and relatively constant distribution of these two pairs of pigments is of great physiological interest, particularly as the two chlorophylls, like the two yellow pigments, differ in their oxygen content.

But there are innumerable facts of interest in this book ; it must suffice here to state briefly certain other problems to which space is prominently devoted.

The relation of the crystallin chlorophyll to the amorphous pigment and its formation through the agency of the enzyme chlorophyllase are fully described.

The pigments of the Brown Algæ are fully discussed, the view confirmed that phycophæin has no existence in fresh material, the fact established that chlorophyll *a* is practically the only one of the two normal chlorophylls present, and three nitrogen free pigments fully described, carotin, xanthophyll, and fucoxanthin.

Then follows a long series of chapters dealing with the extensive researches that have been carried out upon the various derivatives of chlorophyll, the long series of magnesium containing derivatives, the magnesium free derivatives which give rise to the various porphyrins, bodies allied to the hæmatin derivatives, the alcohol phytol which enters into the composition of amorphous chlorophyll, etc. It is impossible in a brief review to deal adequately with the great amount of valuable matter that is contained in the book, but sufficient has probably been said to make it clear that this is a book which will need to be in every botanical library, while at the same time its interest for the organic chemist and bio-chemist is very great indeed.

J. H. P.

Heredity and Sex. Columbia University Lectures. By THOMAS HUNT MORGAN, Ph.D. [Pp. ix + 282.] (Columbia University Press, and Humphrey Milford, London, E.C., 1913. Price 7*s.* 6*d.* net.)

PROF. MORGAN, like many other biologists, has been impressed by the great advances that have been made in recent years along two lines of research. The study of the cell has been pursued by a multitude of observers : errors of observation and interpretation have been corrected, and a clear and tolerably coherent picture is being gained as to the nature of nuclear changes, the normal chromosome contents of different types of cells, and the events of maturation, formation of polar bodies, and fertilisation in the case of the sexual cells. The experimental study of inheritance has occupied even a larger number of acute observers, with the result that a most important body of knowledge has been gained as to the particulate nature of inheritance, the fashion in which the units are transferred, combined, remarshalled, segregated, or linked. The former set of observations shows the existence of an elaborate machinery for the segregation, distribution, and reassembling of the physical substance which most of us now believe to be the hereditary material. The latter set of observations would seem theoretically to demand the existence of a complex and particulate machinery. Prof. Morgan thinks that "a failure to recognise the close bond between these two modern lines of advance can no longer be interpreted as a wise or cautious scepticism." The task that he has set himself is to show in as much detail as possible that there is a close and suggestive correspondence between the two sets of results. Unfortunately most of the points that he discusses can be interpreted in different ways, and we are not prepared to go farther than to commend his book as stimulating and useful.

Artificial Parthenogenesis and Fertilisation. By JACQUES LOEB. [Pp. x + 312.] (The University Press of Chicago; The Cambridge University Press, London, 1913. Price 10*s.* net.)

THIS volume was originally translated from the German edition, but has been supplemented and revised by the author and now gives a clear and connected account of the extraordinarily novel work that Professor Loeb has been carrying out. When it first became known that he had succeeded in fertilising eggs by chemical agencies, it seemed as if the discovery were too remote from what was known to be true. In the Introduction, however, he gives an explanation which brings the results of his experiments, however surprising, into line with other work. The spermatazoon has a double function. It transmits paternal characters to the developing embryo and this function cannot be replaced by physical agents. But it also excites the development of the egg. The latter action is not specific, for the eggs of the sea-urchin can be fertilised with the spermatazoa of quite different genera and species, *e.g.* starfish, brittlestars, holothurians, crinoids, and even molluscs. In these cases the developing embryo reproduces only the maternal characters, and it is this action that can be replaced by physical stimuli.

Both the spermatazoon and the physical agents with which it may be replaced have a double action in inducing the development of the egg. Professor Loeb and the other investigators whose results he correlates with his own, have gone a long way towards establishing the nature of these processes. First of all the so-called fertilisation or vitelline membrane is formed. This was long thought to be a phenomenon of minor importance, but has now been proved to be a necessary part of the process. Various artificial agents, monobasic acids, glucosides, soaps, lysins, and serums, introduced into the egg in certain cases, allowed to diffuse into it in the more common cases, induce the formation of the membrane. This having been formed, the eggs proceed to segment, but, except in rare instances, the segmentation is speedily replaced by cytolosis. The egg and its contents break down and die. The action of these agents is not specific, and Professor Loeb thinks that their effect is to overthrow the stability of the emulsions of which the surface of the egg is composed, and in fact is a first stage in the cytolitic death of the contents.

The formation of the membrane appears to lead to segmentation chiefly by accelerating the process of oxidation, and in fact transforming the egg from being anærobic to being ærobic. Complete removal of oxygen from the medium containing the eggs arrests any further development whether in the case of normal fertilisation, natural or artificial parthenogenesis.

The second factor is an agent which arrests the normal cytolysis and allows the eggs after the formation of the membrane to proceed to full development. In artificial fertilisation, the second factor also is not specific, but the result is produced almost by any hypertonic agent, that is to say any agent which raises the concentration of the water, salt or sugar being used successfully. Loeb has found that the hypertonic treatment is effective even if it is applied before the formation of the membrane.

We have given a very short summary of Loeb's more important results. In the volume before us they are set forth with full detail and there are important discussions of the various chemical details involved. From a general point of view, Loeb's work falls in with advances in knowledge of the chemistry of proteids, leading towards knowledge of the constitution of protoplasm, and with the work of Bütschli and others on the structure of protoplasm. It may be the case that even when all the chemical and physical process of living matter have been tracked out and understood, there may remain something different from chemical and physical action, something that is peculiar to life and not to be resolved into inorganic agents. But the fact remains that step by step inorganic forces and agencies are being proved to explain a greater and greater amount of vital action.

Controlled Natural Selection and Value Marking. By J. C. MOTTRAM, M.B. [Pp. vii + 130.] (London: Longmans, Green & Co., 1914. Price 3*s*. 6*d*. net.)

MR. MOTTRAM has propounded an interesting theory with lucidity and succinctness, and the fashion in which he offers it as a working hypothesis to be tested by observation, rather than as a complete solution, is attractive and scientific. Every one knows that Darwin's theory of sexual selection presents many difficulties and is far from having convinced either those who stood in the forefront of the battle for natural selection or the general body of later naturalists. The view that the exuberances of sexual habit, structure, and coloration are the mere efflorescence of exalted vitality, and the Mendelian attempts to associate them with the presence or absence of factors unevenly distributed in inheritance do not satisfy our craving for causality. No doubt the origin of sexually differentiated qualities may be physiological, and the mechanism of distribution may be in some such fashion as Mendelian analysis suggests, but it would be comfortable to our intelligence if we could associate the control and elaboration of secondary sexual characters with some utilitarian principle.

Mr. Mottram points out that individuals of a species are not of equal value; the female as she often is pregnant, or may become pregnant, can secure the maintenance of the species, but any single male is never independently the vehicle of species-maintenance; young animals, as they have more life in front of them, are more valuable than old animals. Next, he points out that individuals form themselves into societies such as pairs, families, and herds. Next, that individuals vary in structure, male from female, young from old. He correlates these three sets of facts, the presence of unequal value with difference of structure, and of both with the formation of societies. His argument follows that natural selection must appreciate differences in structure, that it must treat associations as units, bring about diversity of structure in them, and bring it about that the less valuable individuals are more liable to destruction than the more valuable individuals.

Mr. Mottram's theory or working hypothesis must be put to the test by a study of conspicuousness in nature, and he has paved the way for this by giving many illustrative examples. Conspicuousness, he points out, may be of use as a signal to friends, a form of utility which does not bear on his argument, or a signal to enemies, and in this latter case, it may serve either to warn them off or to attract them. Attracting conspicuousness may serve to draw off the attack of the enemy from the more valuable to the less valuable individuals of a species or members of a society, and if this, in fact, happen to any great degree, then natural selection would encourage differences in structure and habit, distinguishing the less valuable from the more valuable. Innumerable examples can be produced which are harmonious with Mr. Mottram's view. Conspicuously coloured birds make themselves even more obvious to an enemy by movements which concentrate attention on themselves and divert it from the concealed and protectively coloured young squatting in the turf. Cock birds sing perched in the open or high in the air, loudly advertising their presence, whilst the hen covers the eggs or the young in a secretly placed nest. A cloud of bright males, the destruction of any of which brings no harm to the species, mob the more precious female, with the result that the chances of her destruction by an enemy are very small.

It would not be difficult to allege cases of difficulty, and no doubt this will be done, but until the large inquiry for which Mr. Mottram asks has been carried out, it would be impossible to guess on which side the balance lies, and we can certainly congratulate the author on the clearness and value of his statements.

Evolution by Co-operation: A study in Bio-Economics. By H. Reinheimer. [Pp. xiii + 199.] (London: Kegan Paul, 1913.)

Unfortunately it is easy to be unjust to Mr. Reinheimer's treatise, as it is written in a diffuse fashion with a good deal of repetition and an unprepossessing acceptance of popular and expert writers as equivalent authorities. The background of the argument, which from time to time surges up into the foreground of the picture, is an ardent desire to interpret evolution as a moral process. It seems intolerable to Mr. Reinheimer that what he regards as virtue should go unrewarded or what he deems vice unpunished in the organic world. Advance in organisation is the prize in the school of evolution, and he therefore desires to prove that successful organisms are organisms of higher moral value.

He has much to say about the web of life and the interdependence of organisms. Green plants build up nutrient substances far in excess of their own wants, and the surplus is used in the first place as an altruistic "love-store," the elaborate chemical compounds that are to form the food of the next generation, and partly as a source of food for animals. Green plants in fact are successful in so far as they are altruistic, and the selfish parasites, moulds, and fungi not only do not attain so high a grade of chemical perfection, but become degenerate in structure. Vegetarian, or as Mr. Reinheimer calls them, "out-feeding" animals co-operate with green plants in various ways and are rewarded by attaining a higher grade of organisation, the vegetarian monkeys, for instance, culminate in man, but "in-feeders" or carnivorous animals do little good in the world and offer examples of lop-sided, blind-alley development. It is hardly necessary to allude to what Mr. Reinheimer has to say about animal parasites ; possibly it might be compared with the views of an extreme Calvinist as to the prospects of a baby dead before it had been baptized.

We do not happen to agree with what we infer to be Mr. Reinheimer's conception of morality or with his system of applying it to animals and plants. That, however, is an unimportant side-issue. What is obvious is that to command due attention to his thesis, the author must develop it on different lines. He must take animals and plants, family by family, genus by genus, species by species, and endeavour to show that in fact there is some connection between place in the scale of organisation and altruistic or selfish habit. Let him begin with a single group. The carnivora, for instance, present almost every variety of diet ; they range from carrion feeders, lithe and aggressive killers of the living, to purely frugivorous and vegetarian forms. A very great deal is known as to their systematic relations, structure, reproductive habits, and general success or failure in holding their own in the struggle for life. If Mr. Reinheimer could work out some correlation in this case between what he thinks ought to happen on his theory and what seems to have happened, he would at least secure a patient and attentive hearing.

The Principles of Biology. By J. I. HAMAKER, Ph.D. [Pp. x + 459, with 267 illustrations.] (Philadelphia : P. Blakiston's Son & Co., 1913. Price $1.50.)

THE plan of this book is certainly attractive. Part I deals with plants and has an appendix on classification, Part II treats in the same way but more fully of animals, and Part III is devoted to the consideration of general principles. The printing, most of the illustrations, and the general "get-up" are creditable to the publishers.

Preceding Parts I and II are series of laboratory exercises. However useful these may be to students attending the author's courses they are of practically no value to the ordinary reader. Indeed, the exercises under Vertebrata do not appear to be arranged in any order whatsoever and follow neither the ordinal arrangement of the types nor the classification of the organs into systems that is given on p. 139. As an introduction to vertebrate symmetry why take *Perca* in which the pelvic fins have such an atypical position below or even slightly in front of the pectoral fins ?

Questions of classification will always form ground for discussion, and one does not look for perfection in an elementary book which must of necessity aim at simplicity, but it is extraordinary to group Hydrozoa (*i.e.* Cœlenterata) and Porifera together in one Phylum Cœlenterata. Again the inclusion of the Rotifers and Nematodes as orders of the one class Aschelminthes is hardly justifiable.

There is a looseness about the whole book that is undesirable. Thus on p. 208 the reader is told that the thoracic duct opens into the *left* sub-clavian vein and on the preceding page it is carefully figured opening into the precaval vein on the *right*. The student is instructed to examine living Entomostraca, yet the term Entomostraca is not referred to in the index nor used in the classification.

A similar looseness is also shown in expression and perhaps culminates in the almost grotesque statement that "The locomotion of the animal has to do largely with obtaining food, and this probably determines that the anterior end is located near the mouth." The author has a remarkable way of varying the spelling of words: sometimes it is crustacea and other times crustaceæ. Hermaphroditic on p. 222 becomes hermaphrodytic on p. 279 and hermaphrodyte on p. 353, but never does it reach the form we are accustomed to in this country. Osmose is not a suitable substitute for osmosis.

It is unnecessary to weary the reader with further examples of the above faults when other more serious ones are present. On p. 216 we are told that the "green glands" of the crayfish, the nephridia of the worm, and the uriniferous tubule of the vertebrate are homologous. This is either a misuse of the word homologous or a misstatement of fact. In dealing with the Marsupials it is asserted that they have no placenta, but such a statement was shown to be untrue some years ago.

The book contains very little that is new either in subject matter or presentation and is disappointing. In view of the number of elementary biological text-books already available it is hard to see why it should have been printed at all save for private circulation.

C. H. O'D.

Modern Problems of Biology. Lectures delivered at the University of Jena. By CHARLES SEDGWICK MINOT, LL.D., D.SC. [Pp. viii + 124. With 53 illustrations.] (Philadelphia: P. Blakiston's Son & Co., 1913.)

IN 1912, C. S. Minot of Harvard was exchange Professor at Berlin, and as Prof. Eucken of Jena had taken his place he was also invited to lecture in Jena. The result was a series of six lectures which were published originally in German, but have been translated in the present volume. Although those on Immortality, Death and the Notion of Life suggest philosophical or even theological speculation it is hardly necessary to add that they are approached from the purely biological standpoint.

All the subjects dealt with, especially the determination of sex, have been investigated by a large number of American zoologists, among whom Minot himself is by no means least. The author purposely drew mainly on the results of this American school for his data and consequently the literature list, while not pretending to completeness, is a valuable source of reference.

Minot's ideas on cytomorphosis and the evolution of death are already familiar to English readers. Cytomorphosis is the term proposed to include the whole of the series of changes undergone by cells from their primitive, undifferentiated, embryonic condition up to the time when, after becoming fully specialised, they degenerate and die. The undifferentiated cell is capable of performing all the vital functions including reproduction by division. With the increase of specialisation all other functions are lost save only the one. Hence in the adult state of the higher animals we have myriads of highly modified cells all capable of performing one particular function with remarkable efficiency, but nearly all of which have lost the power of reproduction. After the supply of primitive cells has been exhausted the body cells gradually complete their cytomorphosis and degenerate. When a sufficient number of cells or a group of such as are of vital importance die, the death of the organism as a whole necessarily follows, as there are no undifferentiated cells to make good the loss, and the others, highly specialised already, cannot take on another function. This is assuredly a much more acceptable hypothesis than Metschnikoff's theory of disharmonies.

The chapter on the doctrine of immortality is practically a review of what we have been accustomed to call the continuity of the germ plasm in the light of recent research.

The author is very cautious in dealing with the conception of life and apparently reaches no definite conclusion. On the one hand he admits that "the mechanistic explanation is stringently sufficient for most vital processes," but on the other, he points out that three phenomena, viz. organisation, the teleological mechanism, and consciousness, are not yet satisfactorily explained by the mechanistic theory.

Two or three printers' errors, *e.g.* "Crustacia" for Crustacea (p. 29), "works" for worms (p. 38), "whech" for which (p. 100), and "Weissman" for Weisman in several places, might well have been avoided in so small a book. The author himself also makes use of one or two words in a somewhat unusual sense—"defines" instead of designates (p. 29), "critic" instead of critique (p. 76), and "designate with" instead of designate by (p. 4).

The whole book is written in a moderate manner, no rash conclusions are set out, and the views of other writers are treated with consideration. It is remarkably readable and stimulating and, if the system of exchange professors leads to series of lectures such as these, the sooner it is adopted in our universities the better.

C. H. O'D.

Animal Life by the Sea-shore. By G. A. Boulenger, LL.D., D.Sc., F.R.S., and C. L. Boulenger, M.A., D.Sc. [Pp. xii + 83, with 91 illustrations.] (London: Country Life, Ltd. Price 5*s*. net.)

Few pursuits yield more pleasure than the study of the many animal forms inhabiting the sea-shore, and few are more easily followed. The amateur, however, is constantly encountering difficulties, and needs some book for further guidance. It is to supply such a need on the part of the sea-shore naturalist that the present book has been written.

The names of the authors are a sufficient guarantee of the soundness of the zoology of the book; but something more than this is needed to appeal to the layman, and we must confess to a slight feeling of disappointment on reading it. The charm and enthusiasm that one meets in the pages of the old favourite Philip Gosse are missing, and the present book is perhaps more for reference than for reading. From this point of view the work is an admirable one, and contains a great deal of the right sort of information within its all too few pages. The illustrations are on the whole very good, though the photograph reproduced in fig. 27 (wrongly numbered fig. 21), and fig. 73 are not up to the standard of the rest. By the aid of these and the descriptions in the text, the naturalist should have little difficulty in recognising almost any animal ordinarily found on the beach, and in this respect Chapter I., on Fishes, is exceptionally good.

The book is remarkably free from typographical errors, and will, doubtless, prove useful to a wide circle of readers.

C. H. O'D.

Our Common Sea-birds. By Percy R. Lowe, B.A., M.B., B.C. [Pp. xvi + 310, with 246 illustrations.] (London : Country Life, Ltd. Price 15*s*. net.)

It is certainly a venture to launch a volume of bird photographs upon a market that is already wellnigh flooded, but let us add from the outset that in the present case the proceeding is amply justified. The present book deals, as its sub-title states, with Cormorants, Terns, Gulls, Skuas, Petrels, and Auks. Most people are aware that sea-birds are bound to the land during their breeding season, but it is not generally appreciated that species seemingly quite unfettered are in reality just as closely tied to land, although indirectly, throughout the remaining part of the year. The introductory chapter brings this home in a clear and striking manner by pointing out the dependence of such birds on the plancton and benthos and of the last named on the distribution of the land masses. The two chapters on the flight of birds make interesting reading, and the author of the second (Bently Beetham) certainly makes out a very good case for the view that at the commencement of flight the unfolding of the units of the wing takes place from within outwards, and supports it by some striking photographs.

Apart from certain chapters by experts the majority of the book is due to Dr. Lowe, a well-known ornithologist, and is very well written. The author is haunted by a ghost, "The doctrine which blindly teaches that every specific character, however insignificant, must be either advantageous or disadvantageous to any given species," which he lays quite an unnecessary number of times. On p. 256 " Schizopoda (*Mysis*, *Euphansia*) " implies that Mysis and Euphansia belong to the Stomatopoda, an inference that is far from true. Although much is known about our sea-birds many enigmas still remain to be solved by the persevering observer and these problems are indicated in the many chapters dealing with the various species.

Without any disparagement of the text, it may be said that the illustrations constitute the chief feature of the book. These are reproductions of photographs taken by a number of skilful bird photographers illustrating in a very complete manner the whole life of the bird, and are remarkable for their clearness and beauty. They represent an expenditure of time and patience that can only be fully appreciated by one who has tried this fascinating way of studying nature.

Naturalists will find much to learn from its pages and illustrations, and the general reader will obtain an insight into bird life that should add an interest to his visits to the sea.

Both author and publishers are to be congratulated on the production of this book, which is one of outstanding merit, and the second volume, promised in the preface, will be awaited with interest, and should be assured of a wide welcome.

C. H. O'D.

Antarctic Penguins: A Study of Their Social Habits. By Dr. G. MURRAY LEVICK, R.N., Zoologist to the British Antarctic Expedition (1910–1913). [Pp. x + 140, with 63 illustrations.] (London: William Heinemann. Price 6*s*. net.)

THIS excellent little book records the habits of the Adélie penguins during the breeding season as observed at Cape Adare during one of the British Antarctic Expeditions, and does so in an extremely interesting and instructive manner. It will be remembered that these penguins winter somewhere (it is not known exactly where) on the drift ice to the north, but that in the early Antarctic spring they migrate southwards towards the breeding rookeries. Not being able to fly, they perform this migration partly by swimming and partly by walking on their short legs and tobogganning across the snow and ice fields. The rookeries are comparatively scarce and small areas, because they must consist of bare gravel as free as possible of snow in order to enable the birds to make their nests of stones. Dr. Levick records that the first penguins began to arrive in single-file processions about the middle of October. In a few days the Cape Adare rookery contained many thousands of them. His description of the habits of these very interesting birds is excellent, and is illustrated by a large number of good photographs. The hens settle themselves in scoops, and the courtly combats of the cocks are very interesting and amusing, but seem to consist chiefly in pushing away rivals without resorting to killing them. If two hens have made their scoops too close together, they engage in a war of recriminations and peckings. As the rookeries are some distance from the sea, in which only the birds can obtain food, both sexes remain fasting for from four to six weeks. The eggs are laid about the middle of November, and then apparently the cocks are released for about a fortnight, when they walk down in large parties to bathe and feed in the sea. Dr. Levick's study of these parties is very interesting, and suggests that the birds have a high degree of intelligence. They seem to enjoy life on these occasions just as do parties of human beings at the seaside, and would appear to indulge in games, tricks, and even pleasure trips on drifting masses of ice.

Owing to the small fear of human beings shown by these birds, they are peculiarly suitable for a study of the comparative psychology of animals, and we suggest that on another occasion a special investigator for the purpose might add much to our knowledge of this neglected branch of science. Dr. Levick records many observations and some experiments. Thus he found that the cocks preferred to take brightly red-painted stones, and also stole them from neighbouring nests. On one occasion, one of them seemed to desire to make friends with one of the human party by laying a stone at his feet; and we should have liked to have read of more attempts to cultivate such friendships. On the other hand the birds are occasionally very stupid, as for instance when they cannot pass a rope slung low across the ground. It is also remarkable that they make no efforts to destroy the nests of Skua gulls which injure their eggs and young very frequently. There can be little doubt that penguins are able to communicate ideas to one another, and some of Dr. Levick's evidence as to this is both instructive and amusing. They undertake an extraordinary kind of drill before starting for the north again at the end of the Antarctic summer. There are also some notes regarding Skua gulls and Emperor Penguins.

A Text-book of Medical Entomology. By WALTER SCOTT PATTON, M.B., I.M.S., and FRANCIS WILLIAM CRAGG, M.D., I.M.S. [Pp. xxxiii + 768, pls. 89.] (Christian Literature Society for India, London, Madras, and Calcutta, 1913. Price 21*s.*)

AT first sight the contents of this somewhat massive volume would appear to be rather more than are necessary for the average text-book. As the author's preface informs us, however, it is intended to serve as "a guide to the study of the relations between arthropods and disease, rather than a text-book on entomology," and as such it well fulfils its object. After a brief introduction in which, *inter alia*, attention is drawn to the zoological position of the blood-sucking arthropoda, a lengthy chapter, covering 143 pages, deals with the anatomy and physiology of the blood-sucking diptera. This subject, which is also meant to act as an introduction to insect morphology in general, is divided into two sections; the first relating to general structure and the second to internal structure. In the former a considerable amount of care has been devoted to the elucidation of the structure and mechanism of the mouth parts of these insects, and especially is this the case with the more important blood-sucking forms.

Chapters III.–X. deal in a systematic manner with the various orders and groups connected with this branch of the applied science. Considerable space and two chapters (III. and IV.) are devoted to the Diptera, and then follow the Siphonaptera or fleas (V.), the Rhynchota or bugs (VI.), and the Anoplura or lice (VII.).

Chapters VIII. and IX. are reserved for the Acarina, the former dealing with the ticks, and Chapter X. (divided into two sections) for the Linguatulidæ or tongue worms and Cyclops. As far as possible a regular system has been adhered to in connection with the arrangement of the matter in these chapters. After drawing attention to the general features of the group in question and its relation to disease and natural parasites, the external anatomy is dealt with. Following an authority in each group, the classification is next considered, numerous keys to genera and species (compiled, or adapted from those compiled by various specialists) being given, together with short descriptions of certain of the more important species. Bionomics and breeding habits are then discussed, particular attention being given to the methods of breeding and laboratory manipulation. A considerable amount of original work in regard to internal anatomy has been performed and this part of the subject, together with the notes on methods of dissection, should prove of much utility. A list of the more important works relating to each group concludes the chapter. The last chapters of the volume (XI. and XII.) deal respectively with Laboratory Technique and the Relations of Arthropoda to their Parasites, and the whole concludes with a well-arranged index.

The work is well presented and profusely illustrated, the plates on the whole being very well executed. There are comparatively few misprints, but unfortunately numerous errors occur in regard to the text-references to the figures. Especially is this the case with the first few plates, and in those relating to Plate VI. it is very noticeable. The explanations of the plates are for the most part correct, but it would be well to note that Plate I., Fig. 6, depicts the head of a *female* tabanid—not a male. Also that Plate XXXIII., Fig. 2, represents the female, and Fig. 4 the male of *Phlebotomus papatasii*—not *vice versa* as stated.

This volume, however, is a valuable addition to the literature of the subject and will undoubtedly be of great service to all who are interested. Especially will it be of use to those workers in the tropics who frequently have considerable difficulty in procuring the necessary and widely scattered literature.

H. F. C.

Some Minute Animal Parasites or Unseen Foes in the Animal World. By H. B. FANTHAM, D.Sc., B.A., A.R.C.S., F.Z.S., Lecturer in Parasitology, Liverpool School of Tropical Medicine, and ANNE PORTER, D.Sc., F.L.S. [Pp. xi + 319. With frontispiece and fifty-six text figures.] (London: Methuen & Co. Price 5*s*. net.)

THE aim of this book is said to be to give a readable and popular but accurate account of the life-histories of some microscopical protozoal organisms which produce disease in higher animals, including man—principally sleeping-sickness, malaria, dysentery, and kala-azar in man, and tse-tse fly disease of cattle and some diseases of fish and bees. The book is meant, not only as a preliminary work for students of science, but for colonists, sportsmen, poultry breeders, fishermen, etc. The work, though small, fulfils this programme admirably. The style is excellent and the book shows throughout how interesting the life-histories of the various parasities can be made. More than that, it will be distinctly useful to medical men and indeed to students of tropical medicine, who will here find an admirable ummary of general information which they should possess. In fact it affords a good opening for the more technical monographs which they will ultimately have to study. At the same time it will be useful to all residents in the tropics, as it shows automatically how many of the most important diseases of man and animals may be prevented. As a matter of fact the information given ought now to lie within the general knowledge of every educated person in warm countries, since it is certain to be more useful to such than many of the facts which are so laboriously instilled into us all in youth. The passages on evolution in parasitism, the diseases of grouse and bees, and of the parasities of dysentery are specially within the province of Dr. Fantham to write about. In can be safely said of the book—what cannot be said of all such works—that it is both learned and extremely interesting.

The Internal Secretory Organs: their Physiology and Pathology. By Prof. Dr. ARTUR BIEDL. English Translation by LINDA FORSTER. [Pp. viii + 606.] (London: John Bale, Sons & Danielsson, Ltd., 1913. Price 21*s*. net.)

PROF. Biedl's book is remarkable not alone for the store of information contained in it, but especially for the masterly way in which it is arranged. By reference to some five thousand original papers the author gives a wide and detailed survey of his subject. Again and again one is forced to admire the skill with which discrepancies between the results of different workers are shown to be explained by researches carried on in another direction. The development of our knowledge of internal secretions illustrates in a striking way the reciprocal relations between physiology and pathology, clinical medicine and comparative anatomy.

And a perusal of Dr. Biedl's book makes one realise that a new limb of the nascent science comparative physiology is coming to light in the study of the various effects produced by removal of homologous organs in different species of animals. By argument chiefly from the author's experiments on elasmobranch fishes, a strong case is made out for the vital importance of the cortical portion of the suprarenal glands. Biedl's treatment of this obscure subject should stimulate further research on the "inter-renal" tissue.

Throughout the book stress is laid on the peculiar correlation which appears to exist between the several organs of internal secretion. If the function of one ductless gland be exaggerated or suppressed, the abnormal composition of the blood which results may modify the behaviour not alone of the nervous or muscular systems, but also, and sometimes especially, that of other tissues producing hormones. Thus the effects of thyroidectomy are not all due directly to the removal of thyroid secretion from the circulation, but in part to the deranged metabolism of the genital glands which results. It would be easy to multiply instances of such secondary relations between the hormone produced in one tissue and the body generally; the conception is of the highest importance.

English readers are under an obligation to Miss Forster for the way in which she has carried out a difficult task. One hopes that she may before long place before them a translation of the second German edition of Dr. Biedl's monograph. Since the publication of the first edition much work of fundamental importance has appeared, in particular the nervous control of the adrenal organs has been exhaustively dealt with by Elliott and by Cannon, with the startling result that a new avenue from physiology to psychology has been opened up.

G. R. MINES.

Physiological Plant Anatomy. By PROF. G. HABERLANDT. Translated from the fourth German edition by Montagu Drummond, B.A., F.L.S. [Pp. xv + 777. With 291 figures in the text.] (London: Macmillan & Co., 1914. Price 25*s*. net.)

WHEN the first (German) edition of Professor Haberlandt's book was published in 1884, it came as an exposition of the results obtained during the first decade after the appearance of Schwendener's classical work on the application of mechanical principles to the structure of the higher plants, which represented the first systematic attempt to consider the details of plant structure in the light of adaptation to function. The earliest pioneers of plant anatomy—Malpighi and Grew in particular—fell into the very natural error of speculating on the functions of the structures they unravelled, at a time when physics and chemistry and the knowledge of the general economy of plant life were in a very rudimentary stage, the result being the promulgation of grotesque theories. Some of these persisted among untrained observers and popular writers, and indeed it is no uncommon thing to come across them even now in popular works, while one is continually reminded of them by the retention of terms which had a very different meaning when first applied from that they now bear—as for instance, the term "tracheæ" for the water-conducting vessels in the wood, which were supposed by Malpighi and Grew, in applying the term to plant anatomy, to have the same air-conveying function as the similarly named organs in animals. The ground was prepared for the Schwendenerian school, of which Haberlandt was and remains the most brilliant pupil by the labours of Hugo von Mohl and his successors, von Mohl rigidly confining himself to descriptive work, and, by bringing the subject back to objective reality, well and truly laying the foundation of modern plant anatomy, while Nægeli, Sanio, Hanstein and others took as their guiding principle the idea that the history of development gives the true key to the interpretation of structure. However, even the historical or phylogenetic method tends to become barren and formal without constant reference to function, and it is only in a combination of the two points of view, the historical and the physiological, that we find a logically satisfactory method, which has been so thoroughly applied in detail by Haberlandt. In applying this method it is constantly borne in mind that though a plant, like an animal, is a machine, it is not a brand-new one constructed to fulfil its present functions, but has been built up and modified step by step according to the slowly changing needs of its ancestors.

Ever since the original publication of Haberlandt's book, to which his own research has in almost every part greatly contributed, it has been the standard account of physiological plant anatomy. It is perhaps, after all, not much to be regretted that English readers have had to wait so long for the present edition, which sets forth the author's matured views, with much additional matter not given in the previous German editions. On some controverted points he is still somewhat inclined to dogmatise, laying too great stress upon his own explanations and either barely mentioning or altogether ignoring results which lead to quite different views or which show that the question dealt with is still a very open one. The principle of adaptation is apt to be badly overworked in a book of this kind, with the result that ingenious explanations are given currency without the hall-mark of absolutely rigid experimental test. Indeed, it is hardly unfair to say that the book is marked by a pronounced and all-pervading teleology which is somewhat at variance with the general trend of botanical philosophy. However, the logical division of the subject, the lucid style, and the tolerably successful attempt on the author's part to distinguish clearly between well-ascertained facts and speculative suggestions, combine to render it a model text-book; while even if this attempt is not always carried out as completely as might be desired, the book does for the study of plant histology what Goebel's well-known "Organography of Plants" does for that of general plant morphology—that is to say, the author has brought together a vast amount of scattered information and presented it in a most attractive style, so that whether or not one agrees with all the explanations and suggestions presented, one has nothing but praise for the brilliant manner in which the familiar facts of plant structure are correlated with function and with the history of the various organs and tissues. On the whole, Haberlandt's book, which occupies a unique position in botanical literature, represents the most successful attempt that has been made to correlate the two distinct aspects of plant life which are comprehended in its title, and our admiration for his work is not seriously lessened by the reflection that there is need for the development of much sounder and more cautious methods than have hitherto been available, before physiological plant anatomy can be regarded as fulfilling adequately its important function in assisting the advance of morphological plant anatomy on one hand and that of physiology and ecology on the other.

After an introduction setting forth the objects and principles of the work, the author discusses the general character of cells and tissues and gives his well-known anatomico-physiological classification of the latter, dealing with the various tissue systems (dermal, mechanical, absorbing, photosynthetic, vascular, storage, aërating, secretory, motor, sensory, and stimulus transmitting) in the succeeding chapters. The last three of these made their first appearance in the third German edition (1904) and it is in connection with them that Haberlandt's original work is best known. His brilliant discoveries in connection with sense organs for gravitational and light stimuli—the "statoliths" or falling starch-grains for the former and the "epidermis lenses" in leaves for the latter—have found their way into most recent botanical text-books and even into the newspaper press.

English readers, perhaps in particular teachers of botany, to whom this book will afford a well-nigh inexhaustible mine of facts as well as of attractive and stimulating points of view, owe a debt of gratitude to Mr. Montagu Drummond for presenting them with this excellent translation, and we trust its reception will be such as to repay him for having undertaken the laborious task which he has performed so ably.

F. CAVERS.

Impurities of Agricultural Seed. BY S. T. PARKINSON, B.SC. [Pp. 105, with 152 illustrations.] (London: Headley Brothers. Price 3*s*. net.)

ALTHOUGH the importance of using clean and high-grade seed is generally recognised by agriculturists, very little is done in this country to help the farmer to avoid the troubles certain to arise from sowing inferior grades of seed which he may unsuspectingly have purchased. Most of the well-known firms now give a guarantee of purity with their seeds; but many very inferior grades, often of foreign origin, are on the market to-day. The temptation to make a bargain cannot be resisted, and the farmer learns, too late, that such impure samples are dear at any price.

The authors of this useful little book endeavour to show those who are practically interested in this question how they may examine their seeds themselves, and how to identify the weed seeds occurring in the samples offered to them. As there is no official seed-testing station in this country, many British seedsmen send their samples abroad, especially to the Control Station at Zurich, which has a world-wide reputation for seed-testing. It is time that this unsatisfactory state of affairs was ended by the establishment of a National Seed-Testing Station for Great Britain. All the important European countries, the United States, and many of our Colonies already possess such stations.

Many weeds, because of their poisonous properties, or because of the difficulty of eradicating them when once established, or for other reasons, are classed as "noxious." So countries have resorted to special legislation designed to protect the farmer from the damage due to these noxious weeds and to improve the grade of seed generally. Thus, Canada schedules twenty-six species of plants as "noxious," and samples containing more than one of these weed seeds in every pound of the pure cereal, or more than one weed seed in every 1,500 of smaller type seeds, must be labelled to that effect. For seeds of the Red Clover and Lucerne type four standard grades are recognised according to the proportion of noxious or other weeds they contain. The authors are of the opinion that such legislation is not always an unmixed blessing; but some of the countries concerned pronounce themselves satisfied with the working of their Seed Control Acts.

The greater part of the book is devoted to a very complete descriptive list of the commonly occurring weed seeds. The descriptions are very clearly expressed in simple, non-technical language, while the accompanying illustrations from photographs of 150 weed seeds are remarkably well reproduced. The plates are very conveniently arranged with regard to the text. By means of the "key" provided, it is comparatively easy for any one possessing no special knowledge of the subject to identify the various species. A practical test has shown that the observer is most likely to go wrong in the early stages of classification, where he has to decide on the nature of the surface before assigning the seed to its particular group. Possibly line drawings rather than the half-tone blocks used for the plates would have been better in some cases.

A guide of this kind was obviously wanted, and the authors may be congratulated on producing a book that more than fulfils the modest claims put forward in the preface.

Studies in Water Supply. By A. C. HOUSTON, M.B., D.Sc., Director of Water Examination, Metropolitan Water Board. [Pp. ix + 203.] (London: Macmillan & Co., 1913. Price 5*s*. net.)

IN his preface, Dr. Houston explains that he has often been urged to write a text-book on Water Supply, but as he has not been able to find the time for such a large task, he has here contented himself with writing a monograph dealing with his own personal experiences and investigations. His monograph will probably be, if anything, more valuable than a text-book, as it is full of just the kind of information which is required. In order to indicate the scope of the work we may note that the successive chapters deal with the Sources of Water Supply, Researches justifying Rivers as Sources of Water Supply, the Question of Abstraction, Supplementary Processes of Water Purification, the Sterilisation Processes with Special Reference to the "Excess-Lime Method," Storage in Relation to Purification, Water and Disease, the Financial Value of a Pure Water Supply, Bacteriological Routine Methods, Bacteriological Special Methods, and Miscellaneous Information dealing with the Weather of London, Statistics, and so on, and some of the Chief Reports of the Metropolitan Water Supply. In addition to several very interesting theoretical discussions, there is much technical information based upon the practice of the Metropolitan Water Board. This excellent monograph is very properly dedicated by Dr. Houston to his Staff.

Modern Methods of Water Purification. By JOHN DON, F.I.C., A.M.I. Mech.E., and JOHN CHISHOLM, A.M.I. Mech.E., Engineer and Manager of the Artdree, Coatbridge, and District Waterworks. [Pp. xvii + 398, with 106 illustrations. Second, revised and enlarged edition.] (London: Edward Arnold, 1913. Price 15*s*. net.)

THIS work on a cognate subject is more of the nature of a definite text-book, and we are glad to see a second and revised edition, called for by the necessity of describing processes which have come into prominence during the two years which have elapsed since the publication of the first edition. The work purports to be, and is, a general account of the theory and practice of water purification, that is to say of a subject which is of importance to every one, and especially to our innumerable Water Authorities and Medical Officers of Health. The successive chapters deal with Sources of Supply, Storage, the Construction of Reservoirs and Care of Filtered Water, Sand-Filtration, the Management of Sand-Filters, Mechanical Filters, Purification of Ozone, Water Softening and Household Applicances, the Testing of Water, the Problems of Distribution, and Recent Advances in Sterilisation, with an appendix of Useful Appliances and Data relating to Water Filtration and Measurements. There is also a concise and very useful bibliography of the subject of Water Purification. The description of the biological contents of waters is perhaps rather short to please biologists, but this is in its entirety too large a subject for text-books. There are copious illustrations and diagrams of machinery and of other matters.

Kinship and Social Organisation. By W. H. R. RIVERS, M.D., F.R.S. [Pp. iv + 96.] (London: Constable & Co., 1914. Price 2*s*. 6*d*. net.)

THIS little book is one of the series of Studies in Economic and Political Science edited by the Hon. W. Pember Reeves, Director of the London School of Economics and Political Science. It consists of three lectures delivered by the author at the school. Its object is "to demonstrate the dependence of the terminology of relationship upon social conditions." There has hitherto been so much misconception and confusion, arising from the premature formulation by anthropologists of an earlier day of theories, for which the facts had as yet been imperfectly collected and classified, that it was time for a sane and careful review of the results of the inquiries of the last twenty years. During those years British Anthropologists, led in the first instance by Dr. Haddon, and American anthropologists, under the direction of the Bureau of Ethnology at Washington, have by minute and scientific investigations very different from the haphazard observations of previous generations accumulated a store of information on the social conditions and organisation of peoples in the lower culture. Dr. Rivers's wide experience in the field, his reputation for cautious and accurate reasoning, and his authority on questions relating to social organisation, have rendered him peculiarly fit for the task of exhibiting in a small compass the conclusions to which our more recent and trustworthy information points.

He has achieved the task; and these lectures must for a long time be a text-book of method and a model of exposition on the subject. It has always seemed incredible to most students that the terms of kinship used by savages and others could be mere terms of address. Even as polite formalities they must have had some basis of social fact. Dr. Rivers, by a series of well-chosen examples, drawn partly from his own personal inquiries in the field, has victoriously demonstrated this, and has shown that however strange, or even absurd, the relationships that emerge may appear, they notwithstanding actually exist, or at least existed at a period not very remote.

His lucidity is admirable. His criticism of the terminology hitherto in scientific use is excellent. "Classificatory" and "descriptive" are quite meaningless as applied to the systems of kinship employed in the lower and higher cultures respectively. It would have been well, however, not to take for granted that all his readers would know what "cross-cousins" are or what are the distinguishing characters of the Hawaiian, or as Morgan, the distinguished American anthropologist called it, the Malayan system of kinship.

Attention may be called to one interesting suggestion. Dr. Rivers refers (p. 90) to Mr. Blunt's account, in the Report of the Indian Census of 1911, of the terms in use in the United Provinces. They present several remarkable features, explained by Mr. Blunt as arising from the use by younger members of the family of the same terms as are used by older members to describe their relationship with the persons spoken of. The existence of the joint or extended family, where it obtains, would cause close contact, which would render such an explanation probable. But Dr. Rivers, while admitting this, points to the possibility of an alternative explanation in the case of our term, *bahu*, used alike for the son's wife, for the wife, and for the mother. He suggests that it points to a form of polyandry in which a man and his son have a wife in common. No such form is found now, though of course it may once have existed. If we can rightly explain one of these terms by itself without reference to the others (which perhaps is doubtful), it is interesting to note that Cæsar attributes exactly this form of polyandry to the ancient Britons, and it would be worth while to inquire whether any terms can be found in the Celtic or in the Teutonic languages explicable by such a custom. Dr. Rivers finds traces of the extended family in these languages. If he could trace also this special form of polyandry, his suggestion as to the origin of the term *bahu* would begin to look somewhat more than possible.

E. SIDNEY HARTLAND.

The Yearbook of the Universities of the Empire, 1914. Edited by W. D. Dawson, I.C.S., and published for the Universities Bureau of the British Empire. [Pp. xii + 606.] (London: Herbert Jenkyns, Ltd. Price 7*s.* 6*d.* net.)

A book which will be very welcome to all staffs of Universities, and heads of scientific institutions. The various institutions are dealt with in alphabetical order, and much useful information is given under each heading, including the personnel under subjects, changes in staff, and general information regarding the faculties, matriculation, academic terms, degrees, libraries, museums, and laboratories, benefactions received, number of students, honorary degrees conferred, associated schools, and other matters. More than seventy institutions are dealt with.

BOOKS RECEIVED

(*Publishers are requested to notify prices*)

Nature and Nurture in Mental Development. By F. W. Mott, M.D., F.R.S., F.R.C.P., LL.D. Edin., Pathologist to the London County Asylums, Consulting Physician to Charing Cross Hospital and the Queen Alexandra Military Hospital, formerly Fullerian Professor of Physiology Royal Institution. With Diagrams. London: John Murray, Albemarle Street, W., 1914. (Pp. xii + 151.) Price 3*s.* 6*d.* net.

The Theory of Heat Radition. By Dr. Max Planck, Professor of Theoretical Physics in the University of Berlin. Authorised Translation by Morton Masius, M.A., Ph.D. Leipzig, Instructor in Physics in the Worcester Polytechnic Institute. With 7 Illustrations. Philadelphia: P. Blakiston's Son & Co., 1012 Walnut Street. (Pp. xiv + 225.)

A Manual of Practical Physical Chemistry. By Francis W. Gray, M.A., D.Sc., Lecturer in Charge of the Physical Chemistry Department, Aberdeen University. Macmillan & Co., Ltd., St. Martin's Street, London, 1914. (Pp. xvi + 211.) Price 4*s.* 6*d.* net.

An Introduction to the Study of Plants. By F. E. Fritsch, D.Sc., Ph.D., F.L.S., Professor of Botany, East London College, University of London, and E. J. Salisbury, D.Sc., F.L.S., Lecturer in Botany, East London College, University of London. With 8 Plates and 222 Figures in the Text. London: G. Bell & Sons, Ltd., 1914. (Pp. viii + 397.) Price 4*s.* 6*d.* net.

An Elementary Treatment of the Theory of Spinning Tops and Gyroscopic Motion. By Harold Crabtree, M.A., formerly Scholar of Pembroke College, Cambridge, Assistant Master at Charterhouse. With Illustrations. Second Edition. Longmans, Green & Co., 39, Paternoster Row, London; Fourth Avenue and 30th Street, New York; Bombay, Calcutta, and Madras, 1914. (Pp. xv + 193.) Price 7*s.* 6*d.* net.

A Miscellany. Presented to John Macdonald Mackay, LL.D., July 1914, Liverpool: At the University Press. London: Constable & Co., 1914. Illustrated. (Pp. xvi + 403.)

A New University. By J. M. Mackay, M.A., LL.D., Professor of History, University of Liverpool. At the University Press of Liverpool, 1914. (Pp. 36.) Price 6*d.*

Sound and Signs: A Criticism of the Alphabet. With Suggestion for Reform. By Archer Wilde. London: Constable & Co., Ltd., 1914. (Pp. 180.)

The Theory of Relativity. By L. Silberstein, Ph.D., Lecturer in Natural Philosophy at the University of Rome. Macmillan & Co., Ltd., St. Martin's Street, London, 1914. (Pp. viii + 295.) Price 10*s.* net.

Photo-Electricity. By Arthur Llewelyn Hughes, D.Sc., B.A., Assistant Professor of Physics in the Rice Institute, Houston, Texas. Cambridge: At the University Press, 1914. (Pp. viii + 142.) Price 6*s.* net.

Robert Boyle. A Biography by Flora Masson. London: Constable & Co., Ltd., 1914. (Pp. ix + 323.) Price 7*s.* 6*d.* net.

Immanuel Kant. A Study and a Comparison with Goethe, Leonardo da Vinci, Bruno, Plato, and Descartes. By Houston Stewart Chamberlain. Authorised Translation from the German by Lord Redesdale, G.C.V.O., K.C.B. With an Introduction by the Translator. In two volumes. With 8 Portraits. London: John Lane, The Bodley Head. New York: John Lane Company. Toronto: Bell & Cockburn, 1914. (Pp. xvii + 436 Vol. I., and 518 Vol. II.) Price 25*s.* net.

ANNOUNCEMENTS

MEETINGS OF SOCIETIES

Royal Society. Ordinary Meetings, 4.30 p.m., November 5, 12, 19, and 26. General Meeting, 4 p.m., November 26.

Royal Astronomical Society. Meetings, 5 p.m., November 13, December 11, and January 8.

Royal Meteorological Society. Meetings, 7.30 p.m., November 18, December 16. Annual General Meeting, January 20.

Geologists' Association. Meeting, November 6, 8 p.m.

NOTICE

THE EMOLUMENTS OF SCIENTIFIC WORKERS

It is proposed to undertake an inquiry regarding the pay, position, tenure of appointments, and pensions of scientific workers and teachers in this country and the Colonies. The Editor will therefore be much obliged if all workers and teachers who hold such appointments, temporary or permanent, paid or unpaid, will give him the necessary information suggested below. The figures will be published only in a collective form, and without reference to the names of correspondents, unless they expressly wish their names to be published. The Editor reserves the right not to publish any facts communicated to him. Workers who are conducting unpaid private investigations must not be included. The required information should be sent as soon as possible, and should be placed under the following headings:

(1) Full name
(2) Date of birth. Whether married. Number of family living
(3) Qualifications, diplomas, and degrees
(4) Titles and honorary degrees
(5) Appointments held in the past
(6) Appointments now held, with actual salary, allowances, fees, and expected rises, if any. Whether work is whole time or not
(7) Body under which appointment is held
(8) Conditions and length of tenure
(9) Pension, if any, with conditions
(10) Insurance against injury, if any, paid by employers
(11) Family pensions, if any
(12) Remarks

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury.

MILITARISM AND PARTY-POLITICS

The appalling catastrophe of the present almost universal war has suddenly fallen upon us just when man, in the pride of his ever-increasing knowledge and the humility of his ever-increasing morality, was beginning to think that he had outgrown the possibility of such crimes. Within a few months the highest civilisation ever reached by humanity has fallen back toward a condition of barbarism which has scarcely existed since the time of the Huns. And the greater the height the worse the fall; for deeds which may be forgiven in wild beasts and savages who performed them in ignorance or out of the impulsions of their nature become iniquitous in those who should know better. Is it for this that the great men of the past have taught and led us by their thoughts, their deeds, their example, and their martyrdoms? We have called ourselves the heirs of all the ages; but how have we not squandered our patrimony? Is it for this that the great men of science have laid stone upon stone to build the temple of knowledge which we all entered into when we were born; or the great poets have figured the wonders of the world; or the musicians have created their exquisite art of pure emotion; or explorers have visited every part of the earth; or Buddha, Bruno, and Galilei have suffered, or Christ and Socrates have died? How after this shall the spirit of humanity, now proved so ignoble, dare to face those spirits of their great benefactors? One of them said to us, "By this shall all men know that ye are My disciples, if ye have love one to another." We all call ourselves His disciples, but we are destroying one another like wolves. And in this antithesis the mass of humanity must face two alternative accusations—that it possesses either the heart of the tiger or the brain of the fool. For the average mind-point of humanity lies between its extremes—midway between the god and the brute.

To what madness was the catastrophe due? War is a phenomenon of nature, which, like other phenomena, should be studied by men of science in the impartial way which they find is the only way that enables them to draw her secrets from her clenched hand. As in time of perfect peace and equilibrium the grain of dust is said to precipitate a tornado, or a small vibration to start a violent reaction in a previously torpid chemical mixture, so may war flare up in a moment from a single spark. What then are the reasons for this—and until we can find the cause of a disease it is hopeless to talk about prevention? The present war is a favourable case for the inquiry: because it is not a war in which some superior civilisation imposes itself upon an inferior one, as in many of the wars of Rome, Spain, and Britain; and it is not a war in which virile barbarians sweep away the effete dregs of a decayed past, as at the end of Rome; but it is a war between nations which are for the most part equal in civilisation and strength—belonging to very similar races, having nearly equal opportunities for agriculture, manufactures, trades, arts and sciences, and for the most part obeying, or pretending to obey, the same great moral code. Under these circumstances, what could one of these nations expect to gain by flinging itself at the throat of others; what then would compensate for the dreadful tragedies which were sure to ensue; what praise of humanity could, under these circumstances, ever be bestowed upon the victor; or what god would be ever likely to bless such a deed? Yet in a moment the tragedy has befallen us.

Let us recount the story briefly. It begins from the date of the last great world-cyclone and the crushing of Napoleon. Then it seemed that a general peace was likely to fall upon the earth for generations; and indeed the peace was broken only by smaller and shorter struggles between two or three peoples quarrelling over some poor fleshless bone of territory. For years, however, France, who had not yet found herself in popular government, was the storm-centre. Napoleon III. arose and threatened the surrounding peoples, and pretended to military hegemony. Slowly, however, another nation, Prussia, was seated quietly in the north, nursing a similar ambition, and organising the arts by which she proposed to achieve it. War, she said, is war; its end is victory; and its means are justified by its end. Nor was her philosophy at that time without excuse. She was surrounded by the great states of Russia, Austria, and France, then her superiors in population and wealth. She had often been attacked by all; and had been crushed under the genius-bolts of Napoleon. She then had the right of the wronged—she was justified in doing her all in order to guard herself in the future from what she had suffered in the past. She adopted the great moral axiom that every able-bodied man is bound in duty to prepare himself to defend his country from attack. So far, this is a just axiom, and her perception of it gave her immense force. At the same time her kings, soldiers, and statesmen perceived another but less moral truth, that the ensurance of victory in war is preparation in peace—preparation by every possible means, good and bad. Opportunities to test her doctrines soon came to her. In a moment she struck down Denmark, and in another moment paralysed her former great adversary, Austria; but a third enterprise awaited her. Napoleon III. then dominated Europe, and she determined to wrest this domination from him. Secretly prepared for any issue, she found the auspicious moment; she misled the world as to the justice of her cause by a false statement regarding her adversary, and struck him down in a few months. As the result of each victory she acquired territory and huge indemnities, and the small and scattered German states, which had previously suffered so much from their disunion, now placed themselves under the ægis of Prussia, and the modern German Empire was established.

In its broad outlines this history was not a new one—it was like that of Macedon and of Rome; but their times are not ours, and what was justifiable in small semi-barbarous tribes is not always commendable in the great nations of to-day. How was Germany going to use her great victories? Would she now, inspired by a lofty magnanimity, sheath the sword, make friends with the surrounding nations, and devote herself to the beneficent arts of peace; or would she still demand more and more from her fallen enemies and attempt more and more to dominate her unfallen ones? Her conquest of France occurred forty-three years ago, and since that time Germany as well as the whole world has progressed immensely in population and prosperity. The sense of triumph which her victories gave her, and the spirit of science and discipline which enabled her to achieve them, soon led her to the front in the arts of peace. In manufactures, in shipbuilding, and in trade she began to rival the nation which was then preëminent in all, namely Great Britain. In science her numerous laboratories bestowed great benefits upon mankind;

the rapid increase of her population overflowed into the New World; and it was apparent that without any further war she was already taking that place in the sun which she desired and deserved. She was secure in her own boundaries, not only by her continued practice of universal service, but by her alliance with two other great states; and there appeared to be no cause why she should not be content.

Unfortunately she was not content. There remained on either side of her two great nations with which she had not been recently at war, Russia and Britain. War with the former was not advised by her statesmen, and would promise too little benefit, even in the case of victory. But it was othewise with us. Britain still remained mistress of the seas; her enormous colonies, commerce, and prosperity always led to envy, and perhaps to unworthy ambitions; and it was soon seen by every one that Germany began to point her sword in this direction.

It was also evident that Britain did not, and perhaps would not, perceive the truth. The British, proud of their prosperity, had acquired the carelessness of prosperity. Guarded by the seas, they scarcely conceived the possibility of attack from an inland state. They had obtained their own empire, not by the military arts of Macedon, Rome, and Germany, but mostly by peaceful colonisation and commerce, enforced only when necessary by war. It was we who had chiefly created the great modern developments of industry and transport by machinery, and who had invented most of the great weapons of to-day and those immense floating machines of war of which our navy is composed. But the British were generous in all things. If they had wished it they might then have seized the colonies of almost every other nation; but they coveted none of them. Everywhere Germans were welcomed in British territory, were allowed to trade under our flag, were shown the secrets of our industries and even of our armaments, were allowed to acquire wealth, titles, and influence in Britain itself. For centuries we had remained the friends of our relatives the Germans. We had not opposed them in their ambitions. We raised no tariff barriers against them. We made no war upon their commerce, but gave to them and to all an open entry and an equal chance. There was therefore no reason based upon racial animosity or past disfavours to urge Germany to attack us. But what really happened? In

1890 one of our politicians was so foolish as to give Germany a British island not far from our own coast. This she fortified, and with the aid of the Kiel Canal which she had constructed in territory conquered from the Danes she established an immensely powerful naval base facing us. At the same time she began to build her navy, ship by ship, evidently in emulation of ours; and every year witnessed its greater growth. Not content with the military hegemony of Europe, Germany was evidently going to challenge the naval hegemony of Britain—her people gloried in the idea and even spoke openly of the great victory which was about to come to them. These things cannot be denied. Germany was within her rights to build a navy; but it was a right which could be exercised only to our danger. She denied that she was threatening us; but of what use to us was that denial when we saw the pistol pointed at our head? To us our navy was vital; to her her navy was a toy. But the toy was loaded and might be used at any moment. There are times when we do wrong if we do things which we have every right to do; and that is when our right threatens another's. Thus Germany had chosen her way. She rejected the magnanimous way and adopted the other. She was not content with the triumphs of peace, but would wrest their goods from others by war. She made her choice—and it was the choice, not of the warrior, but of the brigand.

Here we must draw an obvious distinction. There is a good militarism and a bad militarism. The one seeks by every possible precaution to ensure an honourable defence: the other seeks by every possible trick to achieve a dishonourable offence. The one is the attitude of the strong man armed in his own house; the other is the attitude of the strong man armed in another's house. Germany has produced many false philosophies; but the most evil of these has been the philosophy which attempts to justify in us the spirit of the bandit—the heart of the tiger. To rush upon her unprepared neighbours, to seize their goods, to demand enormous indemnities from them, and to hold them under while they suffer, have been her false creed. And the evil has been heightened by the innumerable tricks of the robber. She made treaties which she had no intention of keeping—treaties with other nations and conventions regarding the rules of war. She utilised her own citizens who were living in foreign countries to abuse the hospitality shown to them by spying on their hosts; and, it is said, even arranged to make gun-emplacements and stations for wireless-telegraphy in the territories of friendly peoples by whom her citizens had been always welcomed and often enriched. There is clear evidence that she had fixed the hour for the present outbreak long before it occurred and that she used the murder of the Austrian Archduke merely as a plausible excuse. Like a bandit she prepared the secret dagger while she avowed friendship. It is a false statement that nations, unlike individuals, cannot be indicted for evil deeds—and in fact there is a nation now existing which has suffered from such an indictment for centuries; but the Germans have been so stupid as not to perceive the stigma which their actions have placed and will place upon their race for a century to come. And, however evil their intentions were, their deeds have been still more so. Their first action in the war was to infringe the neutrality of two small states—which they themselves had guaranteed; and still worse than that, when one of these states resisted, they crushed it by unspeakable barbarities. The whole picture is one which offends every notion of virtue, among individuals and between states alike.

But have not any of the other states been to blame? There are sins of omission as well as those of commission. The secret preparations of Germany have been perceived for several decades by every one capable of seeing and thinking. Every soldier agrees that if Britain had possessed an army of a million trained soldiers, or even half a million, which she could have dispatched to the Continent within a few weeks after the outbreak of hostilities, the present war would probably never have occurred (at least as regards Britain, because Germany would scarcely have dared to bring us upon her back while she was engaged with France and Russia). Certainly she would not have dared to infringe the neutrality of Belgium, if we could have thrown such a force into it or upon the neighbouring shores of France. But our statesmen refused to maintain such an army, though they did not hesitate to guarantee the independence of Belgium without preparing the means by which they could support that guarantee. They were like a man who backs a friend's bill, but does not possess the money to enable him to do so—a dishonest action. When war broke out, all we could do was to dispatch less than a hundred thousand men to stay the German torrent, and it was swept back like a broken branch in a spate, leaving brave Belgium to her fate. When history comes to tell the whole story it will not exonerate us. Certainly the chief offender was the aggressive one; but the rich man who remains unarmed in the presence of thieves cannot pass altogether unblamed. As our soldiers pointed out over and over again, the international equilibrium was rendered unstable by the excessive weakness of any one nation. Especially if that nation was the richest of all did she invite attack if unprepared. Every possible means was attempted to persuade our politicians of our military weakness. Leagues were formed for the purpose, innumerable meetings were held, and our most distinguished soldier led the way in his Cassandra-like prophecies. They all failed; our politicians pinned their faith upon the navy; but they gave guarantees and formed tacit alliances without having sufficient means to meet their obligations; and they will be judged accordingly.

Just as there is a good and a bad militarism, so there is a good and a bad popular government. Aggressive militarism is a disease of aristocratic government and party-politics is a disease of democracy. Neither is essential to the form of government concerned. Democracy is government by free discussion; but free discussion does not necessarily imply party discussion—on the contrary it excludes it. As every man of science knows, in order to reach the truth free discussion must first consider all the related facts and then form an unbiassed judgment. But the very nature of party-politics is that the final judgment should be trammelled by the exigencies of the party. Thus party-politicians seldom judge honestly and therefore seldom reach the truth. It is absolutely allowable that two parties may form themselves in the discussion of a single question; but in the discussion of two independent questions, there should therefore be four parties, and in the discussion of three independent questions there should be eight parties. How comes it then that, however many independent questions there may be before the country, only two parties exist? Because the politicians throw over some of their convictions in order to keep well with their side. Thus truth is never reached, and the utterance of a party-politician is utterly worthless on any question which is touched by his politics. We cannot trust him, for we never know whether his professed opinion is genuine. It is useless for him to declare that his allegiance to his party cannot compel him to vote against his own opinion in great matters; for if he is false in small matters, he may also be false in great ones. The very nature of party means that the partisan shall be false in some matters; how, therefore, do we know that he will not be false in all?

The party-politicians refused to take the advice of the best experts. They adopted the absurd hypothesis that without any power of striking effectively they could maintain the position of the empire in the midst of other fully-armed nations. Many experts have even doubted whether they could maintain an entirely safe defensive with a navy alone—but they would not listen. The great moral law that every male citizen is bound to train himself for the defence of his country did not appeal to them Their partisans who, if this evident law had been accepted by them, would have themselves been obliged to undertake such training, perhaps to their own personal loss and discomfort, invented every possible sophistry and false statement to discredit the law. It was not proper, they said, that every man should do his duty; it was better that the dutiful should die for the undutiful! But even though they rejected universal training they might at least have ensured the possession of a sufficient army of volunteers—ensured it by adequate payment and other advantages, by sufficient training, by the provision of means for supplying enough armaments and clothing, artillery and officers, for a larger army in case of need. They should have mastered the principles of war as the Germans had done, and should have made long beforehand every possible arrangement in the case of an outbreak. Almost all of these they neglected—at least as far as our military forces were concerned. That the whole existence of the empire was endangered by this neglect did not move them—that, in the case of any war, successful or not, the State would be put to enormous inconvenience and expense, that our sailors and soldiers would be destroyed by hundreds of thousands, and that the whole of civilisation might be set back for years. Their excuse was invariably that of expense, and they even reduced our small standing army on that account—though they did not hesitate to give themselves from a quarter to a half a million pounds a year, and squandered enormous sums on policies which have too often proved to be of little advantage to any one. They scoffed at the military experts and even at the great soldier who led them. And indeed it is not only military experts whom they ignore—for we have heard politicians who constantly ridicule experts of all kinds. The fact is that the party-politicians feel an instinctive dislike for experts and indeed for all knowledge. They do not thrive by studying matters, but by pretending to the intellectual dregs of the population that they know everything. They are quacks crying their wares in the market-place—patentees of commercial medicines to cure all ailments of the State. Secretly they dread knowledge, for if the public were to possess more of it they themselves could not exist. But the matter is still worse than this, for in order to save their personal positions they dare not impose disagreeable legislation on the mob however necessary such legislation may be for the safety or the welfare of the State. If they do venture upon any disagreeable legislation, it is legislation which is disagreeable only to the classes which have few votes. They are all alike in this respect, and, however much they may pretend to the contrary, are really guided in the first place by their own interests and those of their party rather than by those of the nation and of the world. Just as militarism is the Spirit of Force so is party-politics the Spirit of Falsehood. These are the evil spirits which have destroyed the woods and villages of Belgium; which send our young men by the thousand like sheep to the slaughter; which fill the world with widows and orphans, and which devastate the prosperity of whole nations.

Let us hope that this war may have not one, but two effects; that it will not only diminish militarism, but the still meaner and viler Spirit of Self-Service. After all, even aggressive militarism, wicked as it is, gives the advantages of science, work, and discipline to the nation which believes in it; but the other imposes upon its advocates the worship of lower things—of the false statement, the false argument, the avoidance of toil and of obligations, and the acquisition of wealth without labour and distinction without merit. In this sense, but for her aggressiveness, Germany would have shown a truly noble spirit—that which impels all her citizens to die as they have done, perhaps by millions, for their fatherland. It is difficult to find so much of this spirit here, except only in the noble men who have volunteered to do that for their country which every able-bodied man should be proud to do without the asking. In this matter our politicians have been lying to us, and we have been lying to ourselves. If we win the victory, it will be due not to the nation as a whole, but to the individuals who have fought for us. On the other hand, Germany can at least boast that what she has done she has done at the personal risk of nearly all her people. Her fault has been great, but she has carried herself bravely in spite of it. And fortunately we too can boast that, though our fault has been great, we have done much to retrieve it.

Neither aggressive militarism nor party-politics are found to the same extent throughout the world as in Germany and Britain, and any one who is capable of independent thought must be convinced that they are both pathological manifestations—bad habits of nations like alcoholism and sloth among individuals. Neither is essential, either for autocratic or for popular government. They exist among our two allied races owing to a certain hebetude which attaches to us as peoples; and the average German and British minds tend to differ in the following way. The German is industrious in reasoning as in all things, and, being fond of it, is apt to reach his conclusions prematurely, before he has acquired enough facts to reason upon. On the other hand, the Briton is perhaps not too laborious, and hates above all the arduous process of impartial analysis, unless compelled to it by his own business; but this very dislike of thought allows his mind to lie fallow to the reception of many more data, so that when he is forced to come to a conclusion it is more likely to be a right one. Generally, however, he is intellectually so lazy as to prefer to mould his opinions upon those of others; and this explains his love for parties, sects, dogmas, and fads. Out of pride, both like to adhere to their conclusions when once formed; and militarism flatters the vanity of the newman not infrequently found east of the Rhine, while party-politics flatters the love which all Britons have for games. Indeed, our party-politics is a gigantic sport like our football, cricket, and boatraces; and the whole nation watches with interest while the politicians play it before them, using organised falsification for a battledore and the feather-brained voter as a shuttlecock. But our tendencies are generally more injurious to our own institutions, our administration, and ourselves, than to other nations—though, as several French writers have maintained, they have largely been responsible for the present disaster.

On both sides the error lies in inadequate reasoning. We persistently refuse to look on matters in the true scientific spirit, which seeks only for the truth. The mass of men and of their governments still live in the obsession of dogmas, the scorn of knowledge, the worship of images, and the hatred of God which the prophets of old denounced. So too, as of old, aye in the self-same lands where during untold æons our naked ancestors ran fighting and shrieking under the dripping forests, there we, the children of the Light which our great men gave us, still run shrieking and fighting and blowing each other into fragments by means of the science which they created. We have witnessed the greatest crime ever perpetrated upon humanity. It is due in the first place to the wickedness or incompetence of those by whom the mass of men allow themselves to be ruled—the prince who pretends to possess the mandate of God, or the politicians who pretend to possess the mandate of the people; and secondly to the fact that, however far civilisation has progressed, the mass of men still remain intellectually in but little better condition than they were in when they smote each other with sticks and hammered each other to death with stones.

THE CURVES OF LIFE: A CRITICISM[1]

BY H. G. PLIMMER, F.R.S.

HE who will build a new house has undertaken a hard task. He has to fetch the bricks and the planks from afar, and when the house is built a crack may appear in the walls, an arch may sink, so that it may be years before it is fit to live in and to be a joy to himself and to others. Mr. Cook has here attempted to build in the kingdom of thought a temple to Proportion (under various guises), and he has had to collect his bricks, his planks, his materials from every part of that kingdom, from zoologists, artists, architects, mathematicians, botanists and anthropologists. It has been a difficult task, but it has been extremely well done. The linking together of life and art by a common factor is as important as it is interesting, and this common factor, which is a new mathematical conception of far-reaching applicability, is a key opening unexpected relationships to many natural and artistic phenomena which are set forth in Mr. Cook's book.

This book, which contains the results of twenty years of observation and research, deals with a subject that has been for centuries in the minds of those men of science and of those artists who were capably appreciative of each other's field of work. It deals with the question of Proportion, in the largest sense; of the relations of growth and form, of function and form, of the relations of form to the various arts, and it gives us a mathematical constant which can be applied to these various cases; which constant is a quite new solution of a very old difficulty, and it would seem to be of almost universal applicability. Besides this, the book contains many interesting episodical pages on the work and "life-laboured utterance of passionate thought" of Leonardo da Vinci, whose spirit, in quotations and drawings, hangs over the whole content, and inspires it.

[1] *The Curves of Life*, being an account of spiral formations and their application to growth in Nature, to Science, and to Art, with special reference to the manuscripts of Leonardo da Vinci. By Theodore Andrea Cook, M.A., F.S.A. Pp. xxx + 478, with 11 plates and 415 illustrations. (Constable & Co., Ltd., 1914. Price 12*s*. 6*d*. net.)

The equiangular or logarithmic spiral, upon which hangs so much of Mr. Cook's theories, is itself connected with an older proportion, to which the names of *Sectio aurea*, *Sectio divina*, were given in the Middle Ages. The question of proportion in relation to beauty of form had then been long in men's minds. Apparently Polyclitus, as mentioned by Vitruvius, Galen, and Pliny, was the first who recognised that there were certain proportions in the beautiful human form, to which he gave expression in his statues; so that the people of the time said "Polyclitus has created Art." He determined for each part of the body a certain size, but the human body refused to allow itself to be expressed in lifeless numbers, and others after him set up new systems to show that his normal was not absolute. The *Sectio aurea* is only a name for the extreme and mean proportion of the geometricians, which may be stated thus: "*The first part is to the second part as the second is to the whole or sum of the two parts*": *i.e.*,

A ————————————|—————————— B
C

BC : AC : : AC : AB.

The Greeks knew this ratio, for we find it in Euclid (300 B.C.); and in 1509 Fra Luca Pacioli, a friend of Leonardo's, gave the name of *Divina proportione* to his book on these proportions, since which time a number of books have been written on the subject without producing any definite result.

Up to the present time it had not been found possible to express this ratio arithmetically, with any accuracy. A famous series, called the Fibonacci series from its inventor, which is well known in connection with work on phyllotaxis, comes near to it, but, as will be seen, it is not accurate. This additive series is that obtained by the summation of $1 + 2 = 3$, $2 + 3 = 5$, $3 + 5 = 8$, etc., giving the following series: 1, 2, 3, 5, 8, 13, 21, 34, 55, etc. If we place any of these numbers in the above proportion the result is not perfect; there is a difference. For instance,

$$3 : 5 :: 5 : 8$$
$$8 \times 3 = 24 - 5 \times 5 = 25.$$

Now Mr. Cook has been so fortunate as to have Mr. Mark Barr and Mr. William Schooling as his collaborators on the mathematical side of his work, and they discovered that if, in a geometrical progression, the sum of any two terms is to equal the next term, the value of the ratio must be $\frac{1 \pm \sqrt{5}}{2} = 1{\cdot}6180$, or $-0{\cdot}6180$. This is the only geometrical progression in which the successive terms can be got by addition as well as by multiplication by the common ratio—that is, they have extended this proportion concerning two magnitudes into an infinite series. Its discoverers have suggested that this ratio be indicated by ϕ, which symbol was chosen partly because it is the first letter of the name of Pheidias (in whose works this proportion is constant) and partly, as Mr. Cook says, "because it has a familiar sound to those who wrestle constantly with π."

This ϕ ratio has very important properties, and it seems probable that these may reach much farther than their present confines, for the ratio can be expressed with binomial coefficients, and it can also be used to simplify enormously the computation of logarithms.

In the ϕ series the ratio of any two successive numbers is exact, and the constant ratio is $\phi = 1{\cdot}618034$. With regard to the logarithmic spiral, which plays such a large part in Mr. Cook's researches, the radii vectores, when separated by equal angles, are in ϕ proportion, and moreover the sum of the distances between two successive curves of the spiral is equal to the distance along the same radius to the succeeding curve.

That large things spring from very little ones we all know. Did not the Milky Way, with all its spirals, spring from one drop of milk from Juno's breast? Mr. Cook's twenty years of work on spirals was started by the late Charles Stewart, F.R.S., who, when shown a photograph of the central column of the open staircase in the Château of Blois, recognised the identity of the curves on the central column with those on the shell *Voluta vespertilio.* From this incident Mr. Cook's very manifold and extremely careful observations have grown, and are recorded for us in beautiful English and in an admirable style in the present work. His idea is not that the extraordinary spiral forms in Nature which he describes are evidences of conscious design, but that they indicate "a community of process imposed by the operation of universal laws." He suggests that as Newton postulated Perfect Motion, and from this explained the working of the solar system, so it might be possible to postulate Perfect Growth (by means of a logarithmic spiral), and from this to deduce some law ruling the forms of organic objects. He deals with spirals in general, from those of the smallest spirillum to those of the starry nebulæ, touching on the way whirlwinds, water-spouts, crystals, shells, trees, plants, human organs and parts, horns, and also the decorative use of the spiral; and some of these are dealt with at great length and with much detail and necessary diffuseness. But although the spiral is one of Nature's commonest and most widely diffused ciphers, one has hitherto looked in vain for a key to this wondrous cipher-writing.

Mr. Cook quotes the following sentence of Leonardo's: "In this the eye surpasses Nature, inasmuch as the works of Nature are finite, while the things which can be accomplished by the handiwork, at the command of the eye, are infinite"; which he would explain by saying "that the logarithmic spiral (for instance) can never be reached in Nature," for "Nature is finite, while the logarithmic spiral is infinite, and goes on for ever, as no living organism does." Apparently the logarithmic spiral is the nearest mathematical expression we can use for the relation of form to growth: the other factor which would enable us to express the whole accurately is the life of the object itself. So, also, in a work of art the "baffling factor . . . is its beauty." There is something more than the mere mathematical statement, some variation, some inflection which is life in the one case and beauty in the other. "All beautiful lines," said Ruskin, "are drawn under mathematical laws organically *transgressed*."

Mr. Cook is very careful to insist that the ϕ ratio does not, of course, provide a recipe by which any modern mathematician could produce sculpture like the Greek, or painting like that of the great masters; but it does show that both Beauty and Growth are "visibly expressed to us in terms of the same fundamental principles."

The first examples given are those of upright and flat spiral formation in shells, of which Mr. Cook gives a number of illustrations, many of which are well known, for it is easier to examine spiral formation in shells than in any other natural object. Mr. Cook's explanation of spiral growth in shells on physical grounds alone seems to us hardly sufficient. He says it is in need of "a complete scientific explanation," but surely no physical agency alone can account for the diversity of forms in nature, unless this agency be unduly tortured for the sake of the argument. There must be some physiological force, such, for instance, as Hering's idea of inherited memory at work, which implies, of course, that memory is a function of living matter. Newton showed that if attraction had varied as the inverse cube, instead of as the inverse square of the distance, the heavenly bodies would revolve, not in ellipses, but in logarithmic spirals. Mr. Cook quotes Goodsir in this connection, who suggested that if the law of the square is the law of attraction, the law of the cube (that is, of the cell) is the law of production; and that the logarithmic spiral is a manifestation of the law which is at work in the increase and growth of organic bodies. This extremely interesting suggestion is followed more closely as the work proceeds.

A very detailed account is given, with many examples and illustrations, of the spiral in connection with plant life and growth, with especial reference to the admirable work of Mr. A. H. Church on phyllotaxis. Mr. Church suggested (in 1901) that theories of spiral growth must be founded on logarithmic spirals: and it had been found previously that plants express their leaf-arrangement more or less in terms of the Fibonacci series before mentioned. It is now found that the ϕ ratio expresses the exact arrangement, but it must be remembered that growth is never continuous, but is carried on by accretion. Two examples are given of the functional use of the spiral. In *Valisneria* and *Cyclamen* the fertilised seeds are drawn down under the water in the one case, and into the earth in the other, by a spiral formation of their stalk, so as to enable each to develop properly and in safety. Mr. Church's hypothesis that logarithmic spirals are the sole curves of uniform growth has received confirmation, but it must be based on the form of a spiral on a plane surface, and not on that of a helix around a cylinder, which is the older idea. A great advantage of this theory is that it gives at once a standard of reference for the comparison of phenomena seen on any given shoot of a plant, and it can show how and where the natural growth, which is never uniform, differs from the perfect mathematical line. These plant spirals are either right-handed or left-handed, and it seems probable that they are divided about equally: but the reason for this diversity is not apparent. There are some parts of plants, such as the antherozoids of certain algæ and mosses, which are entirely spiral, and these, although extremely minute, are of very regular and beautiful formation. In another very interesting section details are given of the extraordinary phenomena exhibited by climbing plants, which twine spirally round any support, preferably upright ones. In this connection Mr. Cook discusses plant-intelligence, and quotes a passage from Sir Francis Darwin, who states that "a plant has memory in Hering's and S. Butler's sense of the word, according to which memory and inheritance are different aspects of the same quality of living things. Thus, in the movements of plants . . . the individual acts by that unconscious memory we call inheritance." This view, which has been mentioned above in connection with shell-formation, seems to be the only one which throws any light on these special forms and growths of living things.

Mr. Cook has given a great deal of attention to the spiral formation in the horns of animals, and has collected some remarkable facts, but this subject does not seem to have any close connection with the immediate subject and object of the book. The spirals of horns require to be reduced to plane spirals before one can compare their characters, as their general form is too diverse and irregular.

Of the spiral formations in the body a somewhat short account is given, and Prof. Dixon's beautiful work on the bones is laid under contribution. It is a pity that the work on the extremely complex spiral arrangement of the trabeculæ in the femur and long bones is not more fully treated; but measurements, or anything but general statements, must be very difficult or impossible in such a case. The cochlea is mentioned in such close connection with the femur that it would be better to point out that the function of the spiral arrangement here is quite different to that in the femur, this ensuring strength in the one case and extension in the other. Many other parts of the animal body show a spiral arrangement, such as the umbilical cord, the cystic duct, sweat ducts, muscle-fibres around arterioles, the muscular fibres of the heart, wings of birds and insects, and intestines of certain animals and birds. Some of these are very complicated, and it must be a long time before they can be classified and compared. In some of these cases the spiral arrangement is of advantage from a mechanical or functional point of view, but in many instances its significance is uncertain or unknown.

There is a very suggestive chapter on right- and left-handedness, which has no near bearing on the main thesis, but which serves as a means of introducing the largest episode in the book, which is an account of Leonardo da Vinci and his work. This is so entertaining, and is done with such sympathy, that it might perhaps be one day removed to a book of its own, which would leave the present one stronger and more concentrated. Part of the Leonardo episode is concerned with a very difficult, important, and exciting question as to whether Leonardo did or did not design the very beautiful open staircase in the Château of Blois. This will interest all those who have seen this wonderful piece of work, and all those, also, who are interested in the work of one of the greatest men who have ever lived. It will no doubt excite further research, and by the time it comes to a book of its own something more definite may have been found, so as to confirm Mr. Cook's already convincing view that it was the work of Leonardo.

Connected with the Leonardo episode are some very careful and detailed chapters on the connection of spirals with architecture, beginning with the development of spiral staircases and leading up to the finest example of this kind of work in the Château of Blois; and in regard to this it is desirable to remember how much the spiral is concerned in all beautiful architecture and ornament, and to consider how much of the pleasure taken both in ornament and in natural forms is founded elementarily on groups of spiral lines. As Ruskin has pointed out, the older architects made the spiral "eloquent with endless symbolism." No doubt the twisted pillar was used first (in Lombard Gothic) as a pleasant variety of form, but later it was used constructively (as by Giotto). It is very curious (as shown by Mr. Banister Fletcher) that it is possible to describe an Ionic volute by the unwinding of a string from the fossil shell *Fusus antiquus*, the volute also showing the same divergence from mathematical exactitude as the shell. Mr. Cook gives, as an instance of the artistic use of the spiral, the head of a violin, many of which are finished off in this way. He might have added, which is of interest, that the proportions of many of the older violins—examples by Stradivarius and Amati have been measured—are in ϕ ratio. Mr. Cook has much to say of Greek architecture, and of its deviations and inflections, but there is nothing particularly new in this part of the book, and it is probable that too much already has been written about these deviations; that it is very easy to overdo them practically can be seen in the cathedral at Tours.

And now, where has this collection of examples of spiral formation in nature and art brought us to? Albrecht Dürer, who also studied spirals and proportion (Jean Goujon said of him, "No one has thoroughly understood the true theory of this volute except Albrecht Dürer"), wrote in the third of his Four Books on Human Proportion as follows: "Therefore regard Nature diligently, order thyself thereby, and depart not from her in thy opinions, neither think that thou canst invent better of thyself, else thou shalt be led astray. For truly Art standeth firmly fixed in Nature, and only he who can tear her forth possesseth her. If thou vanquish her, she will remove many faults for thee from thy work. . . . But the closer thy work is to life in its form so much the better will thy work appear. And this is true: therefore never more imagine that thou either canst or wilt make anything better than God hath given power to his creatures to do, for thy power is impotent as compared with God's creative power. Therefore it is ordained that no man can ever create a beautiful figure out of his own thoughts unless he hath well stored his mind by study." This seems to express the great artist's feeling of the relation of his work to nature, and it implies that if without proper study and observation an artist can achieve nothing, yet there is something required in the artist that no study or knowledge can give him. In music, for example, many fugues written as degree exercises are as accurate as any of J. S. Bach's, but the latter have something additional, which enables them to live in men's ears and hearts; but what that something is we have no words for, whose connotation is exact. But in this book, apart from the Leonardo episode, many things are grouped together, but are not fused into a whole. There is the main thesis of spirals, and of their comparison with an ideal spiral of certain properties, with which is connected the very remarkable discovery of Mr. Mark Barr and Mr. Schooling of the ϕ ratio. Then there are questions of proportion and its mathematical expression. In considering those parts of the book which deal with proportion it is necessary to carefully keep clear in one's mind the difference between symmetry and proportion, remembering that the latter is the connection of unequal quantities with each other, just as the former is the opposition of equal quantities to each other.

The book is so suggestive in each of its directions that one could wish they could have been treated more as separate entities, but by doing this no doubt the book would have lost much of its charm, which is rather suggestiveness in many directions than proof in any one direction.

The large collection of facts recorded in the book are, as Mr. Cook modestly claims, spadework, but excellent spadework. The ϕ ratio around which the facts are placed is a very brilliant piece of work, and it should have a wide extension of use. The most difficult parts of the book to form a judgment on are those which attempt to correlate the facts and the ϕ ratio. Are the approximations to the logarithmic spiral in Nature accidental, or have they any meaning, and, if so, what meaning? Are the ϕ ratios which are found in Nature and in many forms of Art accidental, or have they any relation to the impression of beauty received by us when looking at a beautiful thing? Is the power of an artist to produce beautiful works of art, is his feeling for beauty, nothing else than the unconscious use of these proportions? These are a few questions which arise, which will require much more work yet to settle. Is it possible that, having a preconceived idea, you can fit such a thing as a ratio to anything? For instance, if you take a large photograph of Leonardo's Last Supper, it is a remarkable fact that the space proportions can be planned out according to this ratio. The twelve Apostles are divided into four groups, and in each group the heads are so arranged as to correspond to the extreme and mean proportion; also the distances of the two groups on the right and those on the left from the central figure are in the same proportion; and even the width of the two parts of the tablecloth.

Mr. Cook gives the Venus of Botticelli as an example which shows the ϕ ratio in its proportions. It is curious that these proportions should so nearly fit this type of the beautiful consumptive girl; and also that this type should have persisted in art as far, at any rate, as Burne-Jones. The model of the Venus is said to have been Simonetta Catanea, who died of tuberculosis in 1476, at the age of twenty-three.

It may be that the discovery of this ϕ ratio will so stimulate work on these fascinating problems of proportion in Nature and Art that their real meaning may be discovered. At present it seems impossible to speak with any certainty upon the results of these inquiries. That this ratio appears so constantly in Nature and Art is certain, but it is not yet possible to give it the dignity of a law. The present work has brought together a larger mass of facts bearing on the subject than has ever been collected before, which facts are essential to all future endeavours to solve the biological and artistic meaning of spiral formations and of this ratio which is connected with them. It may certainly be said of this ϕ ratio that it is the ideal to which all additive series have now, but not till now, attained: it gives exact expression to them all; and, moreover, it does express certain tendencies of growth in natural objects, and certain pleasing proportions of form in artistic objects and is an expression of that truth of form which we call beauty. It is therefore of very profound significance, and should stimulate to further work, so that its relations to Nature and Art can be made clear and explicit. Mr. Cook's idea is that the logarithmic spiral and this ratio are standards which are never quite reached in Nature or Art, and that it is this divergence, slight as it is, that predicates life in the one case and beauty in the other. This is perhaps the safest view to take at the present time.

The present writer once made a series of measurements of models and statues, from the point of view of the *Sectio aurea*, and he found that the statues approached much more nearly to this proportion than the models. Bacon, it will be remembered, treats with ridicule the idea of confining proportions by rigid rules. He says: "There is no excellent beauty that hath not some strangeness in the proportion. . . . Not but I think a painter may make a better face than ever was; but he must do it by a kind of felicity (as a musician that maketh an excellent air in music), and not by rule." This "felicity" is the word we were trying to find before; it is like Keats's "magic hand of chance," and Ovid's "Ars casum simulet": it is "the little more and how much it is" which Mr. Cook insists upon.

But it may be found, when much more work has been done, that this remarkable ratio stands in a nearer relation to Nature and to Art than we can at present imagine: then Mr. Cook's dream, that the biologist shall lie down with the artist and the mathematician with the botanist, will be fulfilled.

A REPLY TO SOME CHARGES AGAINST LOGIC

By Miss L. S. STEBBING

In the October number of Science Progress Dr. Mercier continues the sweeping attack upon "traditional" Logic begun in his *New Logic* in the hope that the shorter article may reach those who are unacquainted with the former work. He now begins by formulating three charges against Logic: (1) logicians do not use the forms which they say are the only possible modes of reasoning; (2) Logic has many professors because the bounty of past ages has endowed chairs of Logic, but it has few students because there is a general appreciation of its uselessness; (3) logicians are not better but worse reasoners than other men, and he adds, "so wanting are their statements in clearness that the two professors of Logic who are most followed at the present time, and who are credited with the greatest profundity of original thought, write so abominably that they are always difficult to understand, and sometimes completely unintelligible" (p. 210)—a statement which, like the similar attack upon Dr. Bosanquet in the April number of *Mind*, reminds one of the device of abusing the plaintiff's attorney.

Dr. Mercier's general charge is, then, that traditional Logic is as much an imposture as Christian Science or Astrology, and he asserts that he has written a large volume to show up the absurdities of Logic, and that "no logician has ventured to deny that I have achieved his impossibilities" (p. 212). For the sake of brevity he here repeats his attack upon one logical "absurdity" only—the doctrine of the syllogism—and examines one by one its rules, each of which he claims that he can violate with ease and thus achieve what the logician has declared to be impossible. For the same reason the present article must be confined to this one doctrine.

The corner stone of Dr. Mercier's attack is the acceptance of the two statements that syllogism is the exclusive form of all argument and that a syllogism must consist of three and only three terms, and of three and only three propositions. But it is a commonplace among present-day logicians that the excessive claims made on behalf of the Aristotelian syllogism by, *e.g.*, Whately,[1] and no less by J. S. Mill,[2] must be rejected.[3] The adduction of arguments which do not conform to syllogistic rules is not, therefore, either so startling or so novel as Dr. Mercier seems to suppose. Nevertheless, many of the arguments which he does adduce are really syllogisms in disguise—that is to say, they are arguments only if there be *assumed* premisses the production of which suffices to turn the argument into a correct syllogism.

Let us examine some of these examples.

(1) Argument with only two propositions:

> If The bed contains nothing but geraniums and violas,
> then It contains no asters.

This argument is valid because, and only because, neither geraniums nor violas are also asters. It could be thrown into the form of a syllogism, *e.g.*:

> All the flowers in the bed are geraniums and violas,
> No geranium or viola is an aster;
> Therefore No flower in the bed is an aster.

(2) Argument without a middle term:

> If His hands were tied behind him,
> then He could not wipe his nose.

This argument is clearly elliptical and only appears to be immediately self-evident because of its triviality and great familiarity. The unexpressed middle term which is the necessary link of connection between the premisses is the necessity of having a free use of his hands. A similar explanation must be applied to the other arguments on p. 215, designed to illustrate the achievement of this impossibility.

(3) Argument with an undistributed middle term:

> If Hannibal crossed the Alps,
> and The part of the Alps that he crossed is impassable for elephants;
> then He took no elephants across with him.

Here Dr. Mercier argues that "the Alps" is not distributed because it is the predicate of an affirmative premiss, and consequently "although we do in fact refer to the whole class of

[1] *Logic*, p. 12. [2] *Logic*, ii. 2, 1. [3] See Keynes, *Formal Logic*, p. 387.

the Alps, the convention of Logic requires us to suppose that we do not" (p. 215).

To the "traditional" logician it is clear that the middle term is distributed in *both* premisses, for "The part of the Alps" refers to the *whole* of that part, and so too does "the Alps" in the first premiss.

(4) Argument in which the middle term appears in the conclusion:

If Cherries will grow on calcareous soil,
and Rhododendrons will not grow on calcareous soil;
then Calcareous soil is not equally suitable for all plants.

This argument is not expressed in strictly syllogistic form, since the syllogism requires a movement of thought which consists in eliminating the middle term in order to bring out the connection between the two extreme terms. In the given argument the advance consists in the fact that soil suitable for cherries is seen not to be suitable for rhododendrons. It can be expressed in the form:

Calcareous soil is suitable soil for cherries,
Calcareous soil is not suitable soil for rhododendrons;
Therefore Soil suitable for cherries is not soil suitable for rhododendrons,

and it is this conclusion which brings out the force of the argument.

(5) Argument with a term distributed in the conclusion, but not in either of the premisses:

If Some of the crew manned the jolly boat,
and Others of the crew manned the long boat;
then The whole of the crew were enough to man both these boats.

This argument is clearly not stated in syllogistic form, yet, nevertheless, it obeys the rule it professes to break, for the term in the conclusion "The whole of the crew" is a summation of the two terms in the two premisses, viz. "Some of the crew" and "Others of the crew," which together distribute the term used in the conclusion.

(6) Argument with a conclusion drawn from two negative premisses:

If No one ever reasons by "logical" methods,
and No one always reasons erroneously;
then It is quite possible to reason correctly by non-logical methods.

Here again we have an argument not expressed syllogistically, but it may be so expressed, and then it will be found to consist of two affirmative propositions which bring out the real force of the statements far better than the negative form into which they have been forced.

All reasoning is by non-logical methods,
Some reasoning is correct reasoning;
Therefore Some correct reasoning is by non-logical methods.

A similar analysis applies to the other arguments given to illustrate this point.

Dr. Mercier brings forward several arguments to illustrate the point that logicians exclude signs of quantity other than *all*, *no*, *some*, but that, nevertheless, perfectly valid arguments can be drawn when other signs of quantity are used. The accusation is entirely false. Not to mention logicians such as Dr. Bosanquet, who certainly would not adhere to the rule, formal logicians such as De Morgan and Dr. Keynes [1] explicitly admit other signs of quantity. De Morgan, indeed, works out at length a treatment of the numerically definite syllogism which is based upon the recognition of other signs of quantity.[2]

To illustrate Dr. Mercier's treatment of this point we may examine an argument which he triumphantly concludes "is in flat violation of seven out of the eight rules of the syllogism," and yet "is undeniably and incontestably valid."

If Some of them are infantry
and Others are cavalry,
and Others are artillery,
and The rest are naval officers;
then None of them is a civilian.

Now with regard to the claim, first that this argument "contains no middle term," and secondly that "no term in the premisses is distributed," it is clear that the middle term is "*them*" about some of whom various predications are made in the successive premisses, and that this term is distributed since the last premiss in combination with any of the others is sufficient to give *ultra total* distribution of the middle term.[3] "The rest" refers to all those not mentioned in the other premiss, or premisses, and therefore ensures that the *whole* of

[1] See De Morgan, *Formal Logic*, p. 141 *seq.*; Keynes, *op. cit.* p. 377.

[2] *Op. cit.*, ch. viii.

[3] For recognition of this see Hamilton, *Logic*, ii. p. 362, and De Morgan, *op. cit.* p. 127, and Keynes § 327.

the class has been indicated. It may at once be granted that the argument is *not*, as stated, a syllogism, and *therefore* need not contain only three propositions and only three terms. That it draws a negative conclusion from affirmative premisses is more apparent than real, for it requires the assumption of the premiss "none of these (ranks) are civilian," and this at once gives us a negative premiss as well as a distributed term.

Dr. Mercier in his attack on the syllogism has committed two mistakes: (1) he assumes that the traditional syllogistic rules are intended to apply to *all* possible reasonings, and (2) he considers that the middle term is not essential for valid deductive reasoning. The second is much the more serious mistake, and must be dealt with at greater length. With regard to the first it is sufficient to point out that rules formulated with reference to a specific form of reasoning do not necessarily apply to forms outside it. If the validity of obversion be admitted it is clear that the premisses of every valid syllogism may be expressed as negatives from which the affirmative conclusion will still follow.[1] But the reasoning has then ceased to be syllogistic as traditionally defined.[2]

In spite of all Dr. Mercier's efforts to dispense with the middle term, there is in all his arguments a true middle term—that is, an element of *identical reference*, which, whether explicitly stated or not, is the essential condition upon which the validity of the argument depends. Without such an element of identical reference it would be impossible to draw any conclusion from the bringing of a special case under a general principle. But all reasoning consists in the application of general rules to particular cases, and hence always involves a middle term.[3]

Moreover, although it may be admitted that the syllogism as traditionally treated is not the exclusive form of reasoning,

[1] On this point cf. Keynes, *op. cit.* §§ 205–206.

[2] Dr. Mercier seems not to know that Aristotle—to whose evil influence he attributes all the errors of Logic—defined the syllogism as: *λόγος ἐν ᾧ τεθέντων τινῶν ἕτερόν τι τῶν κειμένον ἐξ ἀνάγκης συμβαίνει τῷ ταῦτα εἶναι*, although it is true that in his investigation of the *forms* of the syllogism he interpreted it much more narrowly (see *Anal. Pri.*, i. $24^{b}18$).

[3] In the case of arguments, in the Third Figure, where the middle term is a singular name, this is not immediately apparent, but, since any syllogistic argument can be restated in the First Figure, its essential structure can always be made evident.

since, for example, it leaves no room for Relative Arguments, yet the essential nature of the syllogism is capable of restatement in such form as to make it co-extensive with every form of argument. Dr. Keynes, for instance, sums up the valid moods of fig. 1 of the traditional syllogism as

Rule
Case
Result,

and he shows how figs. 2 and 3 may be similarly treated. But this will, on examination, be found to be the common form of all argument.

To Dr. Mercier's contention that no logician ever uses syllogistic argument in his own reasoning, it may be replied that certainly no logician—or any other writer—states a sustained piece of reasoning in a series of *complete* syllogisms. But, however complicated the reasoning may be, in so far as it is reasoning and not mere rhetorical embellishment, it can always be broken up into a series of syllogisms, each step in the argument depending for its validity upon the element of identical reference that constitutes the middle term. This syllogistic structure may be no more evident than the anatomical structure of the human body painted by an artist. Yet, just as in the latter case the representation would be faulty if it did not conform to the principles of human anatomy, so the argument is faulty if it does not conform to the conditions of *logical* reasoning, whether these conditions are explicitly stated or not.

Dr. Mercier concludes his examination of the syllogism with the remark: "In *A New Logic* I have surveyed the whole field of Logic, have examined every one of its doctrines, and have shown that every one of them *prima facie* requires justification as much as the doctrine of the syllogism." Space does not permit the separate examination here of the charges made against these other doctrines, but I hope at a future date to show, by a detailed examination of these charges, that they are equally unfounded.

There is no doubt that at the present time Logic is undergoing a process of reconstruction under the influence of the modern tendency to apply to all science and philosophy the touchstone of practical use. Continuous development in the light of criticism is the sign of a progressive science, and Logic will undoubtedly benefit by being brought more into touch with practical life—that is, in being shaped with a view to its *application* to the concrete arguments of science and every-day life. In this respect much of the criticism urged by Mr. Alfred Sidgwick in his logical works, and more especially in his *Application of Logic*, is extremely useful in developing the Science of Logic. Had Dr. Mercier confined himself to such criticism he would no doubt have done something to aid in this development. Unfortunately, however, Logic, as distinguished from his own logic, is regarded by him merely as an inexhaustible field for the exercise of his facetiousness. But an excessive sense of humour, no less than the complete lack of it, is apt to destroy one's sense of proportion.

A SURVEY OF THE PROBLEM OF VITALISM

BY HUGH ELLIOT

THE vitalistic problem has been so much discussed in the course of the last few years that the time seems now to have arrived for a general survey of the results of the controversy and of the character of the arguments used on either side. We know from experience that the usual effects of a controversy upon those who take part in it is to fortify the disputants in their original beliefs. Though they themselves are not likely to be converted, yet the public who come to the problem without bias are likely to be influenced by the greater strength of the arguments on one side than the other; and as time goes on, the more weakly supported view declines in popularity, while the opposite view if persistently and repeatedly maintained in public attention comes to occupy the entire field. In the present paper I do not propose to enter at any length into controversial details: I propose to survey the ground already traversed, to examine the type of arguments employed, and to estimate the position now reached.

A survey of this kind should naturally begin with a statement as to what the problem is. From the very earliest times, philosophers have discussed the special characteristics of living bodies, and have endeavoured to establish a fundamental differentiation between organic and inorganic. They have likewise attempted to draw sharp lines between human beings and the rest of the animal world. They regarded the universe as graded in successive and distinct orders of priority: man, animals and plants, minerals. Between each of these classes there was supposed to be complete discontinuity. The theory of evolution broke down one of the great divisions—that namely between men and animals; and the progress of science threatens speedily to break down the other great division—that between the organic and the inorganic.

It has, however, been very widely felt that complete continuity never could be established, on account of the factor called mind. It may be admitted that there is complete *material* continuity in evolution from the most primitive forms of inorganic matter to the most advanced forms of organic matter; and from the properties of the most elementary substances to the manifestations of the highest forms of life (for under this view the manifestations of life are considered as simply the physical and chemical properties of the highly complex substances composing living matter). It is alleged on the other hand that this continuity is only a material continuity: that living bodies manifest a mental or spiritual life, of which no counterpart exists in inorganic bodies: that this new spiritual factor appears for the first time somewhere in the evolutional chain, and that at the point of its appearance there must be a true discontinuity, which abruptly and fundamentally severs those bodies which have it from those which do not have it. Opinion has differed widely as to the precise point at which this supposed transition has occurred. Descartes placed it between men and other animals. He regarded all animals except man as soulless machines, devoid of sensation or any kind of feeling. Lamarck placed it between his classes of worms and insects, where he imagined the earliest traces of a nervous system appeared. Modern philosophers are inclined to place it at a still earlier stage, and in fact to mark it as the dividing line between organic and inorganic.

The whole problem is nevertheless a survival of mediæval modes of thought, possessing no greater reality than the cognate problem as to the site of the soul. It rests upon a totally false conception of the relation between mind and matter. It is based upon the assumption that the universe exhibits *two* agencies, by which all events are brought to pass. The one agency is that dealt with in our mechanics, physics, and chemistry: it works by an absolutely uniform and invariable procedure, which has been to some extent analysed, crystallised, and formulated as the laws of science. The other supposed agency is the spiritual: it works in a wholly capricious and arbitrary manner, displays no kind of uniformity, and is by its very nature incapable of reduction to any scientific laws.

At the outset of civilisation, nearly all events appeared to be of this fickle and disconnected character. Few uniformities of sequences—few laws of science, that is—had been recognised. Occurrences were largely haphazard and unaccountable, unrelated to any visible antecedent cause. They fell therefore within the purview of the spiritualistic world : and a very large proportion of the ordinary vicissitudes of life were regarded as under the immediate control of irresponsible and omnipotent spirits, with very human passions and interests.

As centuries passed on, many classes of events, previously unaccountable and arbitrary, fell into their place in the expanding system of scientific laws. They were perceived not to be haphazard after all, but to follow strictly uniform sequences, so that in simple cases their occurrence could with entire confidence be prophesied beforehand. For these classes of events there was no further occasion for the assumption of spirits, which accordingly were discarded as a means of explanation. But many other classes of events remained, not so easily correlated with any recognisable laws; and for the explanation of these, spiritual influence was still retained. As these more complex events continued to yield before increasing knowledge, the conception ultimately arose that *all* events are in reality the product of natural law, that the uniformity of nature is complete, and that the arbitrary and unaccountable character of certain kinds of events is simply an appearance—that such events are, like all others, due to some particular constellation of material *causes*, though too numerous and intricate to be immediately unravelled.

No sooner was this conception reached than it was applied to the most extreme case of arbitrary events—the activities and conduct of man. It had always been extremely difficult to believe that human thought and behaviour were controlled by the ordinary material laws of cause and effect, and in short that all human activities were simply a special manifestation of physical and chemical processes, which, although of incredible intricacy, yet worked out their effects with the same fatal and absolute necessity that characterises the most elementary phenomena of the inorganic world. Such a belief, moreover, seemed to be directly contradicted by introspection ; for are we not conscious of possessing a mind, altogether separate from our material bodies ? Do we not know that our actions are controlled largely by mental processes, and cannot therefore be a necessary product of our material organisation ? Does not such a belief involve fatalism, and is it not destructive of all theories of moral responsibility? And how can the conception of "purpose" arise out of the blind interaction of physical forces? Notwithstanding these truly formidable difficulties, philosophers and men of science boldly affirmed that all manifestations of life were subject to the same uniformities and inevitable sequences, which had received the name of natural laws. They asserted that all conduct or acts are the necessary outcome of our material organisation, and that they are not in the smallest degree affected by mind or any similar entity, in so far as any such entity is a separate thing from our bodily functions. Thus there arose two schools of thinkers, one of whom affirmed that all the manifestations of life are physico-chemical in character, while the other alleged that a mental or spiritual force acts in co-operation with the material forces known to science, which forces alone are inadequate to furnish an explanation of the observed phenomena. The latter school are now commonly known as vitalists, while the former have often been described as mechanists.

It is evident that this controversy is no other than the ancient problem of free-will and determinism. Determinism was asserted by the ancient Greeks in the philosophy of Democritus. It was powerfully supported by Lucretius. It died out during the darkness of the Middle Ages, to revive with vigour at the time of the Renaissance. Descartes applied the most rigid determinism to the activities of all animals save man, who was not then regarded as an animal. In the succeeding century La Mettrie in *L'homme machine* included man within a revised Cartesian theory. But in the main the defence of determinism has fallen upon Scotch and English philosophers, and the so-called materialist view has remained specially characteristic of the thinkers of our own country.

The whole subject has now been carried to a far higher level of discussion by the advance of physiology, and the question has become definitely a physiological problem. It is obvious indeed that the agencies at work in human activity can only be properly investigated by the science which deals with the nature of organic functions. If we want to know by what process a man performs a certain act, the proper scientific method is to look inside him and see; that is, to carry out an experiment upon him or upon some other animal which may furnish us by observation with the answer to our inquiry. We no longer investigate such questions by abstract logic: and God preserve us from the aid of metaphysics!

It is, however, unfortunately the case that we are not yet able to settle the question by immediate observation. Cerebral processes are so immeasurably complex that it may still be some time before physiology can entirely analyse them. We are therefore thrown back upon a number of other considerations, by which a solution to our problem may be approached.

The mechanist begins by pointing out that the whole course of science has led to the adoption of material forces alone, and the regular and uninterrupted substitution of material agencies for the spiritual agencies so copiously invoked by uncivilised races. Whereas in former times every kind of natural event, from the movements of the planets to the blowing of a wind, were attributed to spiritual agency, the progress of science has invariably contradicted that opinion and set up a material agency in its place. Whatever the universe may be in its ultimate character—and that is a question which does not concern us—the isolated events occurring in it hang together on strictly materialistic lines. The universality of cause and effect is broken in not one single instance.

In so far as the functions of living beings have been brought within the range of observation and experiment, they are found to conform with the most absolute rigour to the uniformity of law which holds good in the inorganic world. The older vitalists used to urge that an organism is a centre of activity, a perpetual fountain of energy, that it *creates* mechanical power, which outside the organic world can neither be created nor destroyed. They knew this by introspection: we can raise our arm by an effort of will—there is the spiritual cause, followed by the material creation of energy. But the answer was obvious. It is not the will that moves the muscle, but the nerves running to it. It is not even the will that stimulates the nerves. They are stimulated by other nervous processes within the brain, and with these processes the spiritual will has no more to do than an inert and accompanying shadow. The nervous processes are the counterpart of the will, and indistinguishable from it. When we say that the will moves the arm, the true facts are that the cerebral processes associated with the will effect the movement. The organism thus presents no exception to the law of conservation of energy. It is found by actual experiment that the quantity of energy emanating from the organism is precisely equal to that absorbed into the organism mainly in the form of chemical energy in the food. The organism as a whole is proved to be a machine for the transformation of energy, in which the food is the fuel.

The argument of the mechanist is based, therefore, mainly on the fact that spiritual intervention is a factor unknown to science. The universe consists of a sum-total of matter and energy undergoing redistribution in conformity with unalterable laws. The organism is wholly subordinate to those laws, in respect of all those functions which are capable of experimental investigation. But organic functions, and especially cerebral functions, with which mental processes are specially bound up, are exceedingly complex by nature, and many of them have not yet been brought within the range of experimental investigation. The mechanist does not doubt that, when they are so subjected, they will be found to conform to the same uniformities of sequence that characterise every other class of known phenomena. Summing up his point of view, the mechanist insists that in the past there has been an unconquerable tendency to ascribe to spiritual initiative all classes of events the causes of which were wholly obscure; that the invasion of materialist explanations has always been triumphant; that at length the spiritual initiative is no longer invoked in the inorganic world, but is confined to the more inaccessible class of organic events; that in every instance where the rising physiology has investigated organic events—as for instance in reflex action, or in energy output—the materialist explanations have been triumphantly established; that the spiritual agency which was invoked for explaining reflex action, etc., has given way only in quite recent years, only when confronted with a certainty, and then with a very bad grace; that spiritualistic initiative has now been driven to entrench in its final stronghold, namely those immensely complex cerebral processes which are not yet amenable to experimental methods. Thus, argues the mechanist, if, in view of the two conflicting systems of nature, one has in every battle from the beginning of history to the present time invariably been successful, while the other has invariably been defeated, if the materialistic system has now as as a result entered into possession of the whole inorganic world, and of every part of the organic world that has been brought within the range of observation, what right have you to affirm that the part still remaining outside the sphere of observation is managed on a totally and unutterably different system from anything that we have ever met with in nature? Further, he continues, supposing it to be true that physico-chemical forces do not control the higher organic activities, what are your grounds for alleging that they are controlled by a vital force, or by any spiritual agency? When we invoke physical or chemical forces, we are dealing in things we understand and can investigate at leisure: we know what we are talking about. But when you invoke a spiritual or vital force, you are dragging in a new and unknown conception, of which you have not the slightest knowledge, nor the slenderest rag of evidence for its existence. If you succeeded in proving that physico-chemical forces were not the active agency in mental processes, you ought to be satisfied with that and say no more. But you go farther: you are prepared to tell us what is the active agency: you speak of a force which really is meaningless to us; and notwithstanding the considerable fortress of words with which you strengthen your theory, that theory remains outside the range of human intellect, and we are no wiser than we should be if you confined yourself to denying the all-sufficiency of mechanical forces.

Let us, however, limit ourselves, as a few—very few—vitalists have done, to this more modest proposition; and let us recognise its implications. Modern researches in the physiology of the nervous system indicate that the reflex arc is the functional unit of that system; and indeed that the system has been built up in the course of evolution by the multiplication of reflex arcs, and their superimposition upon one another to a degree of almost infinite number and complexity. In the simple, typical reflex arc (which, by the way, is an abstraction nowhere found in nature, though none the less a useful conception) a stimulus at one end of the arc is conveyed down an afferent nerve to the central ganglion, whence proceeds a further impulse along an efferent nerve to (say) a muscle, which thereupon undergoes contraction. The contraction of the muscle is dependent upon the original stimulus, and follows necessarily and fatally upon that stimulus by means of some nervous process of a physico-chemical nature. Given this reflex-arc preparation in a fit functional condition, the effect is bound to follow the cause: and the whole process works with the same inevitable certainty as the law of gravitation. This fact is no longer questioned; nor is it questioned that so far as the developed nervous system has been brought within the range of observation, the impulses propagated through it are likewise a physico-chemical manifestation, the precise nature of which is still a subject of discussion. Now under the vitalist hypothesis, there occur at certain points in the nervous system sudden interferences with the normal processes; as a result of which a cause no longer produces its effect, and physical laws or universally founded uniformities are suspended. Let us attempt to visualise the process.

In order to obtain a conception of a nerve impulse, let us take a large number of billiard balls and arrange them in a straight line at a few inches' distance from each other. Let us now propel one of the balls at the end of the line against the centre of the next ball. What happens? The end ball gives up its entire motion to the second ball [1]: the end ball comes to a dead stop, while the second ball carries on the motion to the third. In this way the original impulse travels right down the line: each ball in turn takes up the motion from the one behind it, and passes it on to the one in front, immediately coming to rest itself. At the end of the experiment all the balls will remain in the same straight line and at the same distances from one another as at first, except, of course, for the last ball of all, which will travel away with precisely the same velocity that was originally impressed upon the first ball.

Now this, of course, is a very rough representation of the nervous impulse. It symbolises the fact, however, that, in the nervous impulse, *something* is passed on from molecule to molecule. That something is not motion, indeed; it appears to be some kind of electromotive change: but whatever it is, the molecule or other unit of the nerve-substance passes it on, and then immediately reverts to its former quiescence.

Now the vitalistic conception requires and affirms that at certain points in the propagation of impulses a vital or spiritual force intervenes and causes a diversion of the current from the channel into which the material forces would by themselves have guided it. With the help of our analogy of the

[1] I assume that there is no friction, and therefore that the balls do not roll.

billiard balls we may visualise the process. Each ball passes on its motion to its next neighbour as described. All at once, in the midst of the series, one of the balls, instead of travelling forwards in the direction conferred upon it, moves off at a totally new angle—at a right angle to the line, for instance—and carries off the impulse perhaps to some other series of balls in the neighbourhood. Or we may suppose that a ball, before it has been struck, moves off of its own accord and begins hitting other balls, thus conferring upon them a motion which had no material origin.

Were such an event to occur on a billiard table, we should at once assume some peculiarity in the table or in the make of the ball, to account for the phenomenon. But by hypothesis all *physical* explanations are ruled out. We are in the presence of a miracle: ghosts are at work—genuine ghosts which no investigation can ever convert into rats—good, honest ghosts which cannot be precipitated by any known chemical reagent, with the possible exception of holy water!

Now this is the event which the vitalists allege to occur. They ascribe it, no doubt, not to billiard balls, but to processes in the brain which cannot be seen, but that makes no difference. Their main contention is that the physical sequences are hung up and diverted; and that events pursue a course which is contrary to the *material* nature of the particles concerned.

I do not for a moment suggest that the mechanists regard such an analogy as destructive to vitalism. It is indeed only cited in order that we may have a clear idea of the implications of the vitalistic theory: to see vitalism at work, in short. The mechanist very often has no *à priori* objection to the possibility that such things may happen. He will perhaps not even ask for proof: but he *does* ask, with some insistence, for evidence pointing in that direction; and at the moment of writing no evidence of any kind whatsoever has been produced. I proceed therefore to detail the main character of the arguments by which he is met.

In the first place there are arguments of an ethical character. *If* the mechanistic theory is true, then (it is said) there can be no such thing as moral responsibility, and we are landed in a doctrine of fatalism. To this it is replied, firstly, that moral responsibility is not in the slightest degree affected by the theory; secondly, that fatalism is not found by experience to flow from mechanistic beliefs, but, on the other hand, that it is found to flow from the intensely spiritualistic systems of various Eastern races; thirdly, that even if both accusations were correct instead of being incorrect, they would still remain altogether irrelevant to the point at issue. A true theory is not falsified by having results which we deplore. A fact is none the less a fact, however much it offends our moral sentiments. Our wishes in the matter have no relation to the actual character of the facts. This line of argument, if it were correct, could prove no more than that a knowledge of mechanism is injurious: it does not touch the question of the truth of mechanism itself.

In the second place, past vitalists have cited direct introspection as evidence of their theory. This contention is now almost wholly abandoned, and is recognised to be based upon a misunderstanding. When, by an act of will, we move an arm we are conscious of two things, the act of will and the motion of the arm: no flight of introspection can disclose the processes intervening between these events, and it is just these processes that are the subject of discussion. We are simply animated by a desire, or a determination, to raise an arm, and behold! it is raised. But we could not explain *how* we did it: and if, in spite of a determination to raise our arm, the limb remained motionless, no amount of introspection would ever induce it to move; for the mechanism is not a matter known by instinct, but has to be laboriously worked out by the physiologist.

The third line of argument employed by vitalists is the only one that is seriously advanced at the present day. It proceeds by a recitation of diverse illustrations of the astonishing complexity of mental events, and insists upon the impossibility that phenomena of this nature could arise out of any mere mechanism, however complete it might be. Arguments of this character lie at the foundation of the whole criticisms of Driesch, Bergson, McDougall, and even of Dr. Johnstone, whose work on the Philosophy of Biology constitutes the most recent attempt to discredit mechanism. Driesch gave the name of *per exclusionem* to this argument. His method is to take some particular organic phenomenon, to consider in turn all the possible ways in which it might be explained mechanistically, to refute or *exclude* each of these ways one after the other, and finally to fall back upon the vitalistic hypothesis as the only remaining alternative, and therefore necessarily the true explanation.

This argument is met in a variety of ways. Many physiologists, such as Loeb and Schäfer, have endeavoured to show that the events alleged to be inexplicable by mechanistic means are not in the least inexplicable. In the next place it has been urged that the logic of *per exclusionem* is erroneous: the whole method is impugned. It is pointed out that it is impossible to know and to marshal all conceivable mechanistic explanations; that such a proceeding demands an enormously greater knowledge of nervous physiology than we possess: further, that even though we should succeed in exhausting the list of mechanistic possibilities, the attempt to disprove them is a failure. It is the less necessary for me to dilate upon this subject, inasmuch as I dealt with it at length in this Review two years ago. The opinion that my criticism of vitalistic logic was unanswerable has been borne out by the fact that neither Driesch nor any of his disciples have ever attempted to answer it. More recently I have pointed out that the *per exclusionem* logic was used by Lamarck and many others to prove the existence of a subtle nervous fluid which raced in canals up and down the nervous system. I took the further opportunity of issuing a challenge to vitalists to defend the logic of their position against the heavy preponderance of adverse arguments and opinions [1]; but I think it may safely be assumed that they will not venture to take it up.

Surely under any principle of *per exclusionem* the vitalistic hypothesis would be the first to be excluded. Take, for instance, the famous instance of Driesch: "My brother is seriously ill": "Mon frère est sévèrement malade": "Mein Bruder ist ernstlich erkrankt." The utterance of any of these sentences produces very different stimuli to the auditory nerve of a listener. Yet his resulting activity will be the same. If the whole brain and nervous system is a mere piece of mechanism, how can such widely different stimuli set up identical effects? Supposing, however, the sentence was "My mother is seriously ill," the stimulus would be very nearly the same as in the first case, but the effects would be wholly different. Let us refer once again to the analogy of the billiard balls.

[1] *Vide* my Introduction to Lamarck's *Zoological Philosophy* (Macmillan).

Suppose this time that a large number of them are scattered at hazard over the table. The stimulus may be represented by a new ball which comes into the system, and transfers its motion to one of the balls lying at rest. This ball takes up the energy, and travels forward to collide with one or more other balls, which continue in their turn to pass the motion along. Ultimately the motion may be supposed to reach some distant ball on the outskirts of the system, which ball will travel off in a direction depending upon the impacts which have gone before.[1] This final motion represents the ultimate effect produced by the original stimulus.

Now, interpreting Driesch's example in the terms of this analogy, we see that the case he cites is one in which the stimulus, or new ball, enters the system from three different directions; yet the resultant motion of the final ball is the same in each case. On the other hand, he names two cases in which the new balls enter by closely similar paths, and yet the resultant motion is widely dissimilar. This, as Driesch correctly states, is the supposition of mechanism. But does he correctly state that such an event is impossible, is inconceivable, and that it furnishes a permanent refutation of the entire scientific position? What is the alternative which he suggests? That some particular ball in the middle of the system miraculously acquires a motion which was not conferred upon it by any impact or by any physical or material cause; that this created energy of motion causes the ball to move in a specific direction and collide with another ball, which again passes on the motion, etc.; and that the direction of this self-created motion is such that its ultimate effect, when combined with the ultimate effects of the motion produced by the original stimulus, gives rise to the required resultant effect. In short, the problem is this: *Data:* Incoming balls (constituting the stimulus) may deliver their motion into the group at any of two or three different points, and this motion in each case has an identical exit from the group. *Mechanistic explanation:* There is no theoretical difficulty whatever in supposing that this occurrence is due to the normal effects of the impacts under the ordinary laws of

[1] In actual practice, of course, the motion would be dissipated throughout the system. To improve the analogy we must assume a number of cushions, by which the direction of motion may be changed, and the energy remain concentrated in one ball at a time.

mechanics. It would, even in practice, be quite easy to arrange the balls in such positions that stimuli arriving from three different named directions should all produce the same result, whereas two stimuli arriving in closely similar directions should produce widely different results. There is nothing at all inconceivable about it, either in theory or practice; it is simply a question of the prior arrangement of the balls, or *autrement dit* a question of the elaboration and complexity of the machinery interposed between the stimulus and its ultimate effect. *Vitalistic explanation:* The incoming ball doubtless sets up a normal series of impacts which without interference would proceed to a normal result. But in order to explain the identity of result from a varying stimulus, we assume that one, or more, balls begin to move by a process of spiritual inspiration, and so nicely do they calculate the effects of their motion that after a series of impacts the new motion will become added to the pre-existing normal motion to produce just the precise result expected.

Now, if it be alleged that the mechanistic proposition is *difficult* to imagine, it may be replied that the vitalistic proposition is *impossible* to imagine. For the mechanistic proposition, the only requirement is a machine of enormous elaboration and complexity, just such a machine as the developed brain appears morphologically to be. For the vitalistic proposition, no machine is necessary at all, and the machine which we actually find for the transmission of impulses appears to be a magnificent redundancy. The effect is achieved by immediate creation of spontaneous motion, the precise direction of which is calculated to a nicety. *This* is a conception unknown to science: it has about it a strange ring of mediæval mythology: it belongs to that spiritual type of "explanation" which has ever receded before the advance of science, and whose long series of defeats has never in history been broken by a single victory.

The billiard-ball analogy errs, of course, in its immeasurable simplicity when compared with the structure of the brain. Supposing I were to cover this page with little dots of ink, and supposing the page itself were magnified and extended until it covered the entire surface of the earth, still blackened with innumerable dots running in long strands or meeting in complex reticulations, we should have a somewhat more accurate analogy with the nervous system and its component physiological units.

We have to regard the stimulus as delivered upon certain dots at one end, and transmitted to a further group of dots at the remote end. Is it more intelligible that a stimulus, entering by different doors and departing by the same door, should be transmitted by ordinary mechanical principles, *or* that certain dots in the middle should suddenly assume a ghostly agitation, and carry the diverging mechanical impulses to a common exit? Vitalism affirms the negation of law; a thing unknown in the experience and history of man. Vitalism, affirming that the belief of its opponents is inconceivable, sets up an alternative that is far more inconceivable.

It has not, of course, been possible to mention all the main arguments of both sides in the present article. It is indeed only desired to give an indication of the nature of the arguments used. Vitalistic writers are far more numerous than mechanistic writers; for vitalism has a strong natural appeal to the average mind. Mechanism is almost limited to biologists, and is overwhelmingly supported only by physiologists, with a few stray philosophers. Consequently there are many shades of opinion and argument in the vitalist camp; there are many vitalisms, but only one mechanism. Dr. C. A. Mercier has endeavoured to defend an agnostic view, affirming that the problem is insoluble; but since he presents a number of arguments suggesting a vitalistic solution, he must be reckoned as inclined at all events to recede from the absoluteness of his agnosticism. Reverting once more to the billiard balls, it is quite clear that either some new motion is created out of nothing (in which case vitalism would be true), or it is not (in which case mechanism would be true). To affirm that we can never know the truth is to set limits on the advance of nervous physiology, and to deny that we shall ever be able to penetrate into the nature of nervous processes. Seeing how far we have already gone towards the penetration of such processes, especially during the last few years, and how rapid the progress has been, there seems no more justification for saying that we have at length reached a limit than there would be for making the same statement about the construction of naval armaments.

The philosopher regards science as an expanding sphere of light set in the midst of infinite darkness. Wherever there is light we find all reduced to law and uniformity; wherever there is darkness the popular imagination has at all times peopled the universe with spirits and ghosts which account for all classes of unexplained events. These spirits play with special energy around the edges of the growing sphere. Time after time, with the advance of knowledge, it has been loudly proclaimed that the next increment to the sphere will disclose the spirits to the view of the most hardened sceptic. Time after time the increment has been added, and not a single spirit was to be seen. All things happened with the same order and uniformity that we had become accustomed to. At length our ball of light had expanded until it attained the region of living organisms, the neighbourhood in which spirits revelled in their greatest power and intensity. Biology became a science, this teeming world of ghosts was reclaimed for the light; and behold! they had vanished away, without leaving so much as a shadow to indicate their former habitats. Again the sphere of light has grown; it borders now on the most intricate processes of the nervous system. Once more mankind are gazing into a half-lit land, and appear to see the dancing phantoms and spirits which shall soon be illuminated and established by the pale light of science. Yet the philosopher can hold out no hope that they are more substantial than their predecessors; they are, indeed, but little specks of dust in the eye of the observer. When their territory is engulfed by the rolling tide of science, they will merely recede a little farther. To the scientific historian, the problem of mechanism *v.* vitalism is no more than an incident in a mighty and world-long struggle; and its solution is to him a certainty, founded on innumerable cases of the past.

CAPILLARY CONSTANTS AND THEIR MEASUREMENT

By ALLAN FERGUSON, B.Sc.(Lond.)
Assistant Lecturer in Physics in the University College of North Wales, Bangor

1. Classification of Methods for measuring Surface-tensions
2. Critical Discussion of Principal Methods
3. Discussion of Methods for measuring Contact-angles
4. Conditions for Accurate Comparison of Capillary Constants of Related Liquids
5. Surface-tension and Temperature

The importance of an accurate knowledge of any physical constant need not here be emphasised—the "fourth decimal place" has from time to time proclaimed its weight with no uncertain voice, and in a manner altogether disproportioned to its relative magnitude.

Amongst the physical constants of a substance in the liquid phase, the surface-tension of a liquid is by no means the least important. *Inter alia*, the information which it gives concerning the state of molecular aggregation, and to a less degree the chemical constitution of substances in the liquid state, renders its accurate determination a matter of some moment alike to the chemist and to the physicist.

Nevertheless, in spite of the labour that has been spent on these determinations and the many methods that have been evolved, it cannot be said with certainty that the surface-tension of, say, water is known with an accuracy of even 1 per cent. If we restrict ourselves to those determinations which have been carried out within the last twenty years, we find values given for the surface-tension of water at 15° C. ranging between the extreme limits of 71 and 77 dynes per centimetre, the majority of the results grouping themselves round the values 73 to 75 dynes per centimetre.

Whilst much of this variation is undoubtedly due to the difficulties inherent in the formation of a perfectly uncontaminated surface, it is no less true that the methods used are very variable in quality, and it is the main object of this paper to discuss these methods critically, and to attempt to obtain some criterion which will serve to discriminate between them.

In attempting to describe the various methods that have been used from time to time for the determination of surface-tensions it is convenient to adopt some scheme of classification such as is given in the table on p. 430.

The table explains itself, and, whilst not assuming to be complete, it contains, I believe, all the methods which have been used at all extensively.[1]

In seeking for a criterion to discriminate between these various methods we first note that the primary conditions to be fulfilled by any method which can be used successfully and widely must be those of a high order of accuracy, reasonable rapidity in performance, perfect and easy control of temperature conditions, and unimpeachable rigour in mathematical details.

And it therefore follows that all the methods tabulated under the heading "Dependent on contact-angle" stand at once condemned.

All these methods give only T cos θ (where T is the surface tension, and θ the angle of contact between the liquid and—usually—glass), or some other function of T and the angle of contact, and therefore a separate knowledge of θ is required before T can be obtained. It will be seen that included in this list are two methods, the "capillary-rise" method and Wilhelmy's method, which may almost be considered classic.

The capillary-rise method, in particular, is very widely used in physico-chemical research work, and is almost the only method described at any length in the great majority of physico-chemical text-books. In the practice of the method, surface-tensions are determined by measurement of the height h to which the liquid rises in a tube of small radius r. As a simple calculation shows, neglecting minor corrections, T is given by the equation

$$T = \frac{rh\rho g}{2 \cos \theta},$$

where ρ is the density of the liquid, and g the acceleration due to gravity. If we know the value of the contact-angle for the liquid under examination the method can be considered a fairly reliable one. But as a matter of fact it is too readily assumed

[1] With one exception, which will be referred to afterwards.

TABLE I.

METHODS FOR MEASURING SURFACE-TENSION.

- (A) Independent of contact-angle.
  - Vibrations.
    - (1) Vibrations of drop.
    - (2) Waves on jet.
    - (3) Ripples.
  - Balance methods.
    - (4) Pull on ring.
    - (5) Tension in film.
  - Bubbles and Drops.
    - (6) Pressure in pendent drop.
    - (7) Large bubbles and drops.
    - (8) Bubbles and drops (any size).
  - Capillary tubes.
    - (9) Sentis' method.
    - (10) Jaeger's method.
- (B) Dependent on contact-angle.
  - Capillary-rise.
    - (11) Capillary tubes.
    - (12) Capillary plates.
    - (13) Single plate.
    - (14) Large cylinders.
  - Balance methods.
    - (15) Wilhelmy's method.
    - (16) Pull on sphere.
    - (17) Pull on disk.
    - (18) Buoyancy method.
  - Bubbles and Drops.
    - (19) Total depth of large bubbles and drops.

in the case of many liquids that their contact-angles are zero if they "wet glass." What is meant by the phrase "wetting glass" is not usually defined, nor is it easy to frame a clear definition. The mere spreading of the liquid is a very shaky criterion, and there is nothing so markedly different between the behaviour of benzene and of ether when poured on to horizontal sheets of clean glass as to indicate that the one has a zero contact-angle, the other a contact-angle of 16°.

In fact, it is almost more satisfactory to reverse the definition, and to *define* "wetting glass" as a property possessed by liquids having zero contact-angles, rather than to define zero contact-angle as the angle of contact of a liquid which wets glass. (This assumes of course, which is quite practicable, that we have measured the angle of contact of the liquid by some quite independent method.)

The capillary-rise method has been used for the determination of the surface-tensions of a very large number of liquids, organic and inorganic, and almost all the results are of doubtful value inasmuch as, in the great majority of cases, the question of the contact-angle has been passed over in silence, or at best the angle has been assumed to be zero on the strength of more or less obscure "glass-wetting" phenomena. In some cases the surface-tensions and molecular complexities of such liquids as fused salts have been determined by the capillary-rise method without the slightest apparent appreciation of the fact that such liquids might have contact-angles large enough to render the calculated results quite valueless.

This is all the more unfortunate, since there are several methods, as we shall see later, which are equally rapid and easy to handle, and which are quite independent of any knowledge of contact-angles.

But even if we assume that the contact-angle is known, there exist other defects which are inherent in the method. The surface-tension of the liquid under examination is, neglecting small corrections, proportional to rh. Hence if one arranges matters so that h is large, and can therefore be measured with considerable percentage accuracy, r must be small, and the difficulties avoided in the measurement of h are merely transferred to the measurement of r. As a matter of fact, it is usual, where tubes of very small radius are employed, to determine the radius of the tube indirectly by measuring the capillary elevation therein of a liquid of known surface-tension. But this method of procedure at once raises the question of the validity of whatever method has been used for the determination of the surface-tension of the standard liquid.

Further, the thorough cleaning and drying of very narrow capillary tubes are matters of no small difficulty, whilst, if the measurements be carried out over any very wide range of temperature, the steadily decreasing value of the rise—which becomes zero at the critical temperature—subjects the measured value of h to a constantly increasing percentage error.

These latter defects are quite avoided in Wilhelmy's method (No. 15 in Table I), in which surface-tensions are determined by hanging a number of plates beneath the pan of a balance with their planes vertical and their lower edges in a horizontal plane. The liquid under examination is then adjusted until its surface just touches the lower edges of the plates, when the downward pull on the plates due to surface-tension may be estimated by adding weights (mg) to the other pan of the balance. If p be the perimeter of the plates, the surface-tension of the liquid is given by $mg = pT$, if we assume the contact-angle to be zero. The general equation, assuming a finite contact-angle, is $mg = pT \cos \theta$.

This method again, in common with the capillary-rise method, suffers from the fatal defect that a knowledge of the contact-angle θ is necessary before T can be determined. But, apart from this, it seems strange that the method should not have been more generally used in physico-chemical researches. For, depending as it does on a weight-determination, it can be made far more sensitive than the capillary-rise method, which depends on the exact estimation of a length, whilst, for liquids of very small surface-tension, its sensitiveness can be increased to any extent desired by increasing the perimeter of the plates touching the liquid surface. Further, the plates can be cleaned much more easily than can a glass tube of capillary bore.

The other methods classified in section B need not be reviewed in detail. Whilst, in the laboratory, they possess great educational value both as providing experimental illustrations of points in capillary theory, and as giving useful exercises for laboratory practice, considered as instruments of research they all possess the inherent defect that they demand a knowledge of the contact-angle before they can be used to give reliable values for T. Many of them, of course, can be used with advantage as research methods when the specific object of the research is the determination of contact-angles, but that is not at present the point under consideration. Those who desire further knowledge of the practical details of these methods will find them under the references in the bibliography appended to this article.

We now proceed briefly to discuss the various methods which do not require a knowledge of the contact-angle.

Concerning the three methods grouped under the heading "Vibrations," it may be said of all that they are experimentally of such complexity that more rapid methods which possess the same order of accuracy are to be preferred. Method (1) makes use of the fact that the period of oscillation about its equilibrium figure of a small falling drop is a function of its surface-tension, and this period may be determined by taking a rapid series of instantaneous photographs of a falling drop. Method (2) depends on the measurement of the wave-length of the standing waves formed on the surface of a jet of liquid issuing from a small elliptical orifice, and is, both in theory and in practice, of such complexity, as to be prohibitive as a practical method.

Method (3) depends on the measurement of the wave-length of the ripples generated on the surface of a liquid by a dipper vibrating with known frequency. Whilst it is more simple in practice than the other methods, an accurate determination of T demands considerable care in the measurement of the wave-length, for it unfortunately happens that the most important term in the equation which gives T contains the cube of the wave-length, and therefore a 1-per-cent. error in the determination of the wave-length burdens the corresponding value of T with a 3-per-cent. error.

Moreover, in each of the three above methods temperature measurements are somewhat difficult. And it is to be remembered that, in discussing questions of molecular complexity, and of the relation between surface-tension and chemical composition and constitution, the determination of the exact value of the temperature-coefficient of surface-tension is quite as important as that of the surface-tension itself. It is therefore absolutely essential that the apparatus used shall be capable of being placed in some form of thermostat which shall maintain the liquid at definite and accurately-measurable temperatures.

And although this difficulty is not insurmountable, especially in the case of the ripple-method, some of the methods afterwards to be discussed are more accurately statical and permit of greater compactness of arrangement of the liquid under examination, and hence allow of more accurate observation and control of the temperature.

Turning now to the weighing methods, (4) determines surface-tensions in terms of the mechanical pull on a ring formed from a piece of thin rod bent into a circle of large radius. If the anchor ring thus formed be suspended underneath the pan of a balance with its plane horizontal, be allowed to touch the surface of the liquid under observation, and then be slowly withdrawn, a distinct maximum pull is observable as the ring is raised from the liquid surface. From a knowledge of the dimensions of the ring and of this maximum pull the surface-tension of the liquid can be calculated with considerable accuracy, if the dimensions of the ring be properly chosen. It will readily be seen that the principles employed in the approximate integration of the differential equation to the capillary surface which give the working formulæ for this problem are of much the same type as those which govern the formulæ obtained in the case—to be discussed immediately —of large bubbles and drops; and when it is said that the anchor ring must be of large radius, the word "large" is used in exactly the same sense in both cases. As far as exactness is concerned, the method leaves little to be desired, and the only factor that has prevented it from coming into wider use is, perhaps, the difficulty of hitting off the maximum position exactly, and some lack of appreciation of the exact conditions under which the approximate formulæ used in the computations may be applied.

In the practice of method (5) a light frame is made by bending a piece of thin glass rod so as to form three sides of a rectangle. The frame is suspended from the arm of a balance, dipped into the liquid under examination, and raised therefrom until a film is formed which extends from the horizontal bar to the liquid surface. The balancing weight is now read off, and again after the film is broken, when the difference of the two weights divided by twice the width of the film gives the required surface-tension. Subject to one or two small corrections, which are easily made, this method is both sensitive and convenient, but its usefulness is seriously limited by the fact that with many liquids it is difficult, indeed impossible, to form true films which shall be sufficiently permanent to enable the weighings to be carried out. In this method, as in the one last described, a distinct maximum pull is observable just before the true film is formed, and in this case also the surface-tension may be calculated in terms of this maximum pull. Indeed, this method is, analytically, simply a particular case of the former one, and the equation which gives the surface-tension may be deduced from the equation of the anchor-ring method by supposing the radius of the anchor-ring to become infinite. But the ring-method is, on the whole, preferable, since it avoids troublesome end-corrections.

The first of the methods classified under the heading "Bubbles and Drops," is fairly simple both in theory and in practice. If one limb of an inverted U-tube be plunged into a beaker full of liquid, by suitably raising or lowering the beaker a pendent drop may be formed at the end of the other limb. The pressure-excess at the vertex of this drop may be determined by measuring the distance from the drop-vertex to the horizontal level of the liquid in the beaker. But this pressure-excess is equal to $\frac{2T}{R}$, where R is the principal radius of curvature of the vertex of the drop. R may be determined by photographing the drop on a large scale, and measuring the co-ordinates of a few points in the neighbourhood of the vertex taken as origin. If one assumes, what is very nearly the case, that the outline of the drop in the neighbourhood of the vertex is parabolic, R may be calculated from the values of the co-ordinates. The method is very suitable for certain special cases, but considered as a general method it is not of a sufficiently high order of accuracy, as it is somewhat difficult to determine R with great exactness.

Methods (7) and (8) depend for their accuracy primarily on the closeness with which one can obtain integrals of the differential equation to the capillary surface. As is well known, this equation, in its most general form, is not susceptible to exact integration; and even if we restrict ourselves to those cases where the capillary surface is one of revolution about a vertical axis, the difficulties in the way of an exact integration are insurmountable. Approximate integrals can, however, be obtained in certain special cases—broadly speaking, those cases in which the maximum horizontal radius is very large, or very small, in comparison with the capillary constant a_1.[1] If due regard be paid to the conditions under which these approximate integrals are obtained, the formulæ developed have a higher accuracy than can be obtained experimentally, and so can be used with perfect confidence. Thus if a "large" bubble be formed in a liquid under a plane sheet of glass, and if the radius of the greatest horizontal section of the bubble be r, and the vertical distance from the plane of greatest horizontal section to the vertex of the bubble be q, it can be shown that

$$a_1^2 = q^2 - \frac{a_1^3}{3r}(2\sqrt{2} - 1),$$

an equation which can easily be solved for a_1^2 (and therefore for T) by successive approximations. The second term on the right-hand side is *ex hypothesi* a small term, and r should be so large that the third approximation has very little effect on the value obtained for a_1^2 by the second approximation. If this condition be fulfilled, experimenters can be assured that they are well on the safe side in assuming that the dimensions of their bubbles are in accordance with the hypotheses made in solving the differential equation to the surface.

Although it has not been very widely used in the past, this seems to be one of the best of the various methods which are independent of the contact-angle. Its use has probably been limited by two factors. First, experimenters have too hastily assumed that if the bubble formed be so large as to be plane at the vertex, it is then large enough to satisfy the requirements of the above equation. This is by no means the case, and the application of such equations to bubbles which, while plane at the vertex, are not more than two or three centimetres in *diameter*, is certainly unjustifiable, leads to inconsistent results, and by putting on to its equations a strain they were never intended to bear, only results in making the method unpopular. Again, it is not very easy to make direct determinations of q on the bubble itself. Apart from the difficulty of obtaining accurately the position of the plane of greatest horizontal section, q is never a very large quantity. As the above equation shows, q is approximately equal to a_1, which for most liquids has a value of

[1] a_1 is defined by the relation $a_1^2 = \frac{2T}{g\rho}$, so that a_1 has the dimensions of a length. a_1^2 is usually called the "specific cohesion."

some two or three millimetres. But these difficulties may be overcome by taking a magnified photograph of the bubble illuminated by means of a pencil of light the rays of which are accurately horizontal. From measurements of this photograph the values of q and of r may be determined with a very high percentage accuracy. The method possesses two other advantages—it is accurately statical, and there is very little risk of contamination of the surface of the bubble.

Method (8), which depends on the photographic measurement of bubbles or drops of any size, is suited to various special cases, e.g. the determination of the surface-tensions of molten metals. Suppose that a photograph of a bubble or drop has been taken and that the co-ordinates of a number of points on the outline have been determined. The equation of equilibrium of any given portion of the bubble or drop can be written down exactly in a form which contains the integrals

$$\int x\,dy, \int x^2 dy, \text{ and } \int xy\,dy$$

taken between the appropriate limits. As we do not know x as a function of y, the above integrals cannot be evaluated algebraically. But by plotting out three curves between x and y, x^2 and y, and xy and y respectively on squared paper, the values of the integrals can be determined with considerable accuracy either by the planimeter or by square-counting. These values, substituted in the original equation, then enable us to determine T.

Two methods of a high order of exactness are tabulated under the heading "capillary tubes." In the very ingenious method due to M. Sentis, a pendent drop of the liquid is formed at the end of a vertical capillary tube. The position of the meniscus formed by the liquid in the tube is then noted, and the maximum radius (r) of the pendent drop is measured. A beaker of the same liquid is then placed on the head of a spherometer, which is raised until the liquid in the beaker just touches the vertex of the drop. The spherometer is now further raised until the liquid in the capillary reaches its original level. If the difference between the spherometer readings in the two positions be h, then T is given by the equation

$$\mathrm{T} = \frac{g\rho}{2}\left(rh - \frac{r^2}{3}\right).$$

The method is simple and very exact, and the only factor which prevents its wider use seems to be the difficulty of measuring exactly and varying arbitrarily the temperature of the drop.

In the well-known method associated with the name of Jaeger, T is determined from observations of the maximum pressure required to release a bubble of air from the end of a capillary plunged vertically into the liquid under examination. Neglecting minor corrections, and supposing that the end of the capillary tube (of radius r) is just touching the surface of the liquid, we have simply

$$2T = rh\rho g \quad . \quad . \quad . \quad . \qquad \text{(iii)},$$

where ρ is the density, and h the difference of level of the surfaces of the liquid in the pressure-gauge. Taking into account general convenience, rapidity, accuracy, and ease of temperature-control, this appears to be one of the best of the methods passed under review. It will be seen from the above equation that if the liquid in the manometer be the same as that examined, the difference of level observed in the manometer is equal to the height to which the liquid would rise in a capillary of radius r; and therefore, apart from the fact that it is independent of the contact-angle, the Jaeger method possesses another advantage over the capillary-rise method in that, by using a very light liquid in the manometer, this height may be correspondingly magnified, and therefore may be read off with a percentage accuracy considerably higher than that of the measurement of the corresponding height in the capillary-rise experiment.

The only serious criticism that can be brought against the method is that in the formation of equation (iii) statical principles are brought to bear on what is really a dynamical problem. But experience shows that the maximum pressure observed is only a function of the rate of release of the bubbles for comparatively high speeds. If the rate of liberation be slow enough—one bubble every two or three seconds, or slower—the maximum pressure observed is quite independent of the rate of extrusion of the bubble, and may therefore safely be taken to represent the true maximum.

It will doubtless have been noticed that one method which has lately come into prominence has not been included in the table given—the so-called "drop-weight" method. If a drop of liquid be allowed to form on and to fall slowly from a properly constructed tip, it is known that, *cæteris paribus*, the weight of the drop is proportional to the surface-tension of the liquid, and therefore if the constant of the tip be known, the surface-tension may be determined. Of late years the practical details of the method—the most compact form of the apparatus, the proper shape of the tip, the accurate control of temperature and of the conditions governing the fall of the drop—have been most carefully studied and worked out.[1] It possesses several advantages over the capillary-rise method—for, if no counterbalancing disadvantages exist, a weighing method is more sensitive than one which depends on the estimation of a length, and at high temperatures the percentage error is not increased, inasmuch as one can allow more drops to fall, so compensating for the diminished weight of each drop.

But it is not quite certain that the method is independent of the contact-angle, for, in the words of the experimenter who has made a special study of the subject: "the weight of a falling drop of liquid is thus found to be *strictly proportional to its surface-tension, determined by capillary-rise or any other accurate method.*"[2]

If this be so, then, equally with the capillary-rise method, the drop-weight method depends on contact-angles, and its value as an instrument of research is correspondingly minimised; it is true that the elementary theory of the falling drop gives an equation which is independent of the angle of contact, but it is well known that the theory as usually given is only a rough approximation to the truth, and the point, which is one of some importance, would be best settled by experimental evidence.

The elementary theory of this method is so uniformly misstated in treatises and text-books on physical chemistry that no apology is needed for discussing it here in detail. In the great majority of text-books[3] it is stated that the weight (mg) of the falling drop is related to the surface-tension of the liquid by the equation

$$\mathrm{mg} = 2\pi \mathrm{rT} \quad . \quad . \quad . \quad . \quad . \quad \text{(iv)}.$$

As a matter of fact any experimenter who used this equation would soon be convinced of his error by finding that his results for T would have about half their true value. The error arises

[1] See various papers by Morgan and colleagues in the *Journal of the American Chemical Society* for 1909 and later years.

[2] Morgan, l.c. xxxii, 349, 1911. (Italics mine.)

[3] *Dolus latet in universalibus*, but where so many books go wrong, it would be invidious to select any particular treatise for special mention.

from the fact that the pressure-excess in the drop due to its curvature has been neglected. If we assume that the drop is cylindrical round its circle of contact with the tube this pressure-excess will be equal to $\frac{T}{r}$, and the equation of equilibrium will become

$$mg = \pi rT \quad . \quad . \quad . \quad . \quad . \quad (v).$$

The error in equation (iv) was pointed out so long ago, I believe, as 1881 by Mr. A. M. Worthington; nevertheless, generation after generation of writers and researchers have, up to the present day, repeated the erroneous statement with a persistence worthy of a better cause.

Equation (v) represents only a first approximation to the truth, as in reality the phenomena attendant upon the detachment of a falling drop are too complex to be amenable to so simple a statical treatment. The equation, as given by Lord Rayleigh, which best represents the results of experiment is

$$mg = 3{\cdot}8rT \quad . \quad . \quad . \quad . \quad . \quad (vi),$$

but this fact does not make the neglect of elementary dynamical principles involved in the writing down of equation (iv) any less culpable.

In the actual practice of the method it is usually assumed that with any given tube

$$mg = kT,$$

and the constant k is determined, once for all, from observations of the drop-weight of a liquid of known surface-tension. It is for this reason that an erroneously stated equation leads to correct results.

It must be confessed that the whole treatment of the principles of capillarity, as given in many physico-chemical treatises, is dotted with errors. Thus, in a text-book published in 1911 for the use of pass and honours students in the Universities, the following truly astonishing definition is to be found: "The surface-tension is defined as the force which acts *at right angles to the surface of a liquid*, along a line of unit length." Comment is needless.

In the matter of the statement of dimensions of physical quantities there is much room for improvement. Surface-tensions are sometimes expressed in dynes, sometimes in dynes per centimetre. On one page of a recently-published authoritative treatise, the well-known quantity "molecular surface energy" is expressed in *dynes per centimetre*! Two pages later it is expressed in ergs; and in recently-published research papers, the differential coefficient with respect to temperature of this same quantity is also given in ergs!

Moreover, in the discussion of the drop-weight experiment, it is not uncommon to find a statement such as the following: "The drop falls when its weight just exceeds the surface-tension." This statement is probably a somewhat slipshod attempt to expand into longhand the erroneous equation (iv), and as such should read, "The drop falls when its weight just exceeds the product of the surface-tension and the maximum horizontal perimeter of the drop," but if English has any meaning it means that the equation of equilibrium of the drop just before detachment is given by $mg = T$, and it cannot be too strongly emphasised that a surface-tension is a force-per-unit-length, and is no more a force than a velocity is a length. Such a statement, therefore, involves a breach of the elementary theory of dimensions which would, in most examinations, bring down a candidate's marks dangerously near to the limiting value zero.

We have now reviewed rapidly some twenty methods for the determination of surface-tensions. Whilst many of the methods are specially suited to special cases, three methods seem to stand out, in point of convenience and accuracy, as most suitable for general use. These are Jaeger's method, the method which depends on the estimation of the maximum pull upon an anchor ring, and that which depends on the photographic measurement of the partial depth of large bubbles. If one could be quite sure that the drop-weight method would give results which would be in accordance with those obtained by these three methods, and not in general in accordance with those obtained from capillary-rise, as its exponents claim, this also would take its place as one of the most accurate methods yet devised for the determination of capillary constants; but, as pointed out above, this question requires to be definitely settled by experimental evidence.

It is not necessary to discuss in any great detail the methods used for the determination of the other important capillary constant—the angle of contact—as comparatively little work has been done thereon. Direct methods, such as that of Traube, who measured the height of the meniscus in capillary tubes and deduced a finite contact-angle from the fact that the height of the meniscus was always less than the radius of the tube, or that of Sentis, who attempted to determine the contact-angle from the observation of refraction phenomena at the line separating solid from liquid, are not very satisfactory; the most promising method of investigation is that which consists in determining the surface-tension of the same sample of liquid by two methods, one method being independent of, the other dependent on a knowledge of the contact-angle. The contact-angle is then assumed to be zero, and the surface-tension is calculated in each case. If the two results agree within the limits of experimental error, the assumption is justified. If the results are not in agreement—and the criterion of this is that the mean values shall differ by more than the sum of the probable errors—the knowledge of the value of the surface-tension given by the first method enables one, from the equation of condition of the second method, to calculate a value for the angle of contact. Experiments of this kind have been successfully carried out by Magie, who compared the values of the surface-tension given by measurements of the partial and total depths of large bubbles of air blown in the liquid and imprisoned beneath a concave lens; the present writer is now engaged on a comparison of the results given by the capillary-rise method and by Jaeger's method.

The importance of surface-tension determinations in exhibiting the relation between capillary constants and chemical constitution warrants a discussion of one or two points of interest concerning, especially, the proper conditions under which results should be compared. Broadly speaking, the results of past researches have shown that surface-tension, considered from the physico-chemical point of view, is almost entirely additive in character. Constitutive influences are, however, to a minor degree exhibited. For example, the surface-tensions of ortho-, meta-, and para-xylene are given by Feustel as 3·21, 3·08, and 3·03 respectively (measured in milligrams-weight per millimetre, at 19°·2 C.). But it is clear that, in discussing the relations between surface-tension and chemical composition and constitution, especially with respect to the constitutive influence, the conditions for comparison must be carefully chosen and specified. The surface-tension of a liquid is affected by the amount of dissolved gas, by the nature of the gas in contact with the liquid surface, by pressure, by the state of molecular aggregation of the liquid, and above all by temperature. In the earlier researches on the subject, comparisons were made at the same temperature, but it was recognised by Schiff that surface-tensions should be compared at corresponding temperatures—that is, at temperatures which are equal fractions of the critical temperatures of the liquids under comparison. Unfortunately the critical temperatures of comparatively few organic compounds have been directly determined, and it was supposed that these conditions were fulfilled at the boiling-points (under atmospheric pressure) of the liquids examined. If this be the case, the ratio of the boiling-point to the critical temperature of all liquids should be the same when temperatures are measured on the absolute scale. The degree of exactness with which this condition is fulfilled is exhibited in the table given below, which shows the value of this ratio calculated for a number of substances of very diverse boiling-points.

TABLE II.[1]

Substance.	Crit. Temp. (cent.)	Crit. Temp. (abs.)	B. Pt. (cent.)	B. Pt. (abs.)	$\frac{\text{B. Pt.}}{\text{Crit. Temp.}}$
Oxygen	−118	155	−183	90	·581
Water	365	638	100	373	·584
Bromine	302	575	63	336	·584
Sulphur dioxide	155	428	−10	263	·615
Stannic chloride	319	592	114	387	·654
Carbon dioxide	31	304	−78	195	·641
Carbon tetrachloride	283	556	77	350	·630
Ethyl alcohol	243	516	78	351	·680
Ether	197	470	35	308	·655
Chloroform	260	533	61	334	·627
Aniline	426	699	184	457	·654
Propyl alcohol (n)	264	537	97	370	·689
Ethyl acetate	250	523	77	350	·669
Pentane (n)	197	470	36	309	·658
Methyl alcohol	240	513	65	338	·659
Acetic acid	322	595	119	392	·659
Benzene	288	561	80	353	·629
Methyl acetate	234	507	57	330	·651
Propyl acetate	276	549	102	375	·683

It seems, therefore, that the condition is approximately fulfilled, and, calculating from the carbon compounds only, the relation

[1] The data for the calculations in this table have been taken from Kaye and Laby's *Tables of Physical and Chemical Constants*, 1911.

between the boiling-point and the critical temperature of a liquid is given by

$$\text{Boiling-point} = \cdot 656 \times \text{Critical Temperature},$$

when temperatures are measured on the absolute scale; so that from the boiling-point one can calculate the critical temperature of a liquid (subject to an error of some 4 or 5 per cent.),[1] and knowing the critical temperature one can compare surface-tensions, not only at the boiling-points, but at other corresponding temperatures which are different fractions of the critical temperature. Such a process is only approximate, but it is much better than having no guide at all.

An empirical relation from which critical temperatures can be indirectly obtained has been given by Walden. Over limited ranges of temperature one can write with sufficient accuracy

$$T = T_o(1 - \alpha\theta) \text{ and } a^2 = a_o^2(1 - \beta\theta)$$

for the variation with temperature of the surface-tension and specific cohesion respectively. But these formulæ will not bear extrapolation; and in particular if, knowing that the surface-tension vanishes at the critical temperature, we attempt to calculate this temperature from either of the relations

$$\theta_c' = \frac{1}{\alpha} \text{ or } \theta_c'' = \frac{1}{\beta},$$

we obtain results which agree neither with each other nor with the observed value for the critical temperature. But it so happens that in many cases the variation of θ'_c in one direction from the true value is compensated by the opposite variation of θ'_c in the other direction, so that the true value of the critical temperature is proportional to $\frac{1}{\alpha} + \frac{1}{\beta}$, and so, as Walden pointed out, can be calculated from a knowledge of these temperature-coefficients.

It would, however, be distinctly advantageous if one possessed a general formula connecting surface-tension and temperature which could be extrapolated with confidence up to the critical temperature. Given such a relation, one could then deduce the value of the critical temperature from observations over a limited range of temperature of the surface-tension alone.

[1] This error may be considerably reduced if the effect of constitution on the value of this "constant" be taken into account.

As mentioned above, the ordinary linear relation only holds over very restricted ranges of temperature, whilst the equation

$$T = T_0 (1 - \alpha\theta - \beta\theta^2),$$

which represents with considerable accuracy the relation of surface-tension to temperature over a fairly wide range, does not give results of even a rough degree of accuracy when extrapolated far beyond the range of observation. In several cases which the writer has examined, it would seem that the equation

$$T = T_0 (1 - b\theta)^n \quad . \quad . \quad . \quad . \quad \text{(vii)},$$

can be extrapolated over a wide range of temperature so as to give the critical temperature—which is, of course obtained from the relation $\theta_c = \frac{1}{b}$—correct to two or three degrees.

Thus, the first two columns of Table III below show the relation between the temperature and surface-tension of benzene as given by the experiments of Ramsay and Shields.

TABLE III.

Temp.	T (observed)	T (calculated)
80	20·28	20·35
100	18·0	18·0
120	15·71	15·70
140	13·45	13·45
170	10·20	10·20
190	8·16	8·13
210	6·20	6·20
230	4·32	4·28
250	2·56	2·54
270	·99	1·00

As the table only begins at 80° C., 100° was selected as an arbitrary zero, in which case equation (vii) may be written

$$T = T_{100} [1 - b(\theta - 100)]^n.$$

From values of T given in the table and taken between the limits 100° and 200° the constants b and n were calculated, giving

$$T = 18 [1 - \cdot00534 (\theta - 100)]^{1\cdot213} \quad . \quad . \quad \text{(viii)}.$$

The third column in the above table shows the values of T calculated from this equation, and it will be seen that the agreement between the observed and calculated values is fairly close.

The point of greatest importance is—can the formula be extrapolated with any confidence over more than a few degrees below 100° and above 200°? The critical temperature of benzene from the above equation is

$$\theta_c = 100 + \frac{1}{\cdot 00534} = 287\cdot 3,$$

and the value as determined from direct experiment is 288°·5—so that the agreement is very close. Extrapolating in the other direction, we have for the surface-tension of benzene at 0°,

$$T_0 = 18\,[1 + \cdot 00534 \times 100]^{1\cdot 213} = 30\cdot 3 \text{ dynes per cm.}$$

Unfortunately T_0 is not given in the above table. The experiments of Renard and Guye, extrapolated from temperatures in the neighbourhood of 20° by means of the linear relation, give $T_0 = 30\cdot 1$, and those of Ramsay and Aston[1] similarly treated give $T_0 = 30\cdot 7$.

Again, in the case of ethyl acetate, taking 20° C. as an arbitrary zero, we find from the observed values between 100° and 180°,

$$T = 23\cdot 6\,[1 - \cdot 00423(\theta - 20)]^{1\cdot 267},$$

giving 256° as the critical temperature. The directly observed value is 250°.

In the case of ether, calculating the constants from values given between 0° and 130°, we find that

$$T = 18\cdot 9\,(1 - \cdot 00513\theta)^{1\cdot 229},$$

giving 195° as the critical temperature, whilst the observed value is 194°—197°.

It seems, therefore, that this formula in the cases examined gives the critical temperature very closely indeed even when extrapolated over a range approaching 100°. If this should prove to be generally true, it would be possible to calculate the critical temperature of a liquid with some closeness from capillary observations alone, and the problem of obtaining the "corresponding temperatures" at which to make comparisons would be greatly simplified.

One word in conclusion. In the comparison and collation

[1] It should be noted that the results of Ramsay and Shields and of Ramsay and Aston are for benzene in contact with its own vapour, those of Renard and Guye for benzene in contact with air.

of a large number of papers dealing with this subject, the writer has found the multitude of units in which surface-tensions are given a constant source of trouble. Dynes per centimetre, milligrams per millimetre, grams per centimetre, and grains per inch are a few of the units employed. It would greatly lessen the labour of those engaged in comparing the accuracy of different methods and in searching for general results connecting capillarity and constitution if capillary constants were invariably expressed in C.G.S. units—surface-tensions in dynes per centimetre and specific cohesions in *square* centimetres.

NOTE.—The numbers in brackets immediately following the names in the brief bibliography appended show that the paper refers to the method classified under that number in TABLE I.

BOHR (2), *Phil. Trans.* A., p. 209 (1909).
CANTOR (10, 4), *Wied. Ann.* **47**, p. 399 (1892).
FERGUSON (6, 7, 8, 10, 16, 17, 18, 19), *Phil. Mag.*, March 1912, November 1912, March 1913, November 1913, July 1914, September 1914.
FEUSTEL (10), *Ann. d. Phys.* **16**, p. 61 (1905).
HALL (5), *Phil. Mag.*, November 1893. (Contains a full bibliography of earlier experiments.)
LENARD (1), *Wied. Ann.* **30**, p. 209 (1887).
MAGIE (7, 19, and contact angles), *Phil. Mag.*, August 1888.
J. L. R. MORGAN and colleagues (Drop-weight Method). Various papers in the *Journal of the American Chemical Society*, from 1908 onwards.
RAMSAY and SHIELDS (11), *Phil. Trans.* A., p. 647 (1893).
RAYLEIGH (3), *Phil. Mag.*, October 1890.
SENTIS (9 and contact-angles), *Jour. de Phys.* 1887—1897.
SMILES, *Chemical Constitution and some Physical Properties* (Longmans 1910), chap. "Capillarity" gives a detailed account of, and full bibliography concerning the relation of surface-tension to chemical constitution.
TIMBERG (4), *Wied. Ann.* **30**, p. 545 (1887).
VOLKMANN (11), *Wied. Ann.* **17**, p. 361 (1882).
WORTHINGTON (6) *Proc. Roy Soc.* **32**, p. 362 (1881).
WORTHINGTON (15) *Phil. Mag.*, January 1885, p. 43.

THE FORMATION OF OZONE IN THE UPPER ATMOSPHERE, AND ITS INFLUENCE ON THE OPTICAL PROPERTIES OF THE SKY

By J. N. PRING, D.Sc., *University, Manchester*

Part 1. Some Factors which determine the Optical Properties of the Atmosphere
Part 2. The Chemical Estimation and Distinction of some Constituents of the Atmosphere
Part 3. The Action of Ultra-violet Light on Air
Part 4. The Estimation of Ozone in the Atmosphere at High Altitudes
Part 5. The Influence of Ozone on the Nature of Light from the Sky

Part 1. Some Factors which determine the Optical Properties of the Atmosphere

The importance of the question of the presence of ozone in the air is due to the large influence which would be exerted by its occurrence, though only in small amounts, on the physical and chemical properties of the atmosphere. From the chemical standpoint the importance of ozone centres in its powerful oxidising properties, in virtue of which the gas, even when diluted, quickly reacts with all organic matter, and acts as a strong bactericide. In this way ozone would be expected to take an important part in the purification of the atmosphere and in determining the salubrity of the climate. From a physical standpoint, its presence is mainly of interest on account of the influence it would exert on the transmission of light radiated from the sun. The absorption of light by ozone is particularly marked in the ultra-violet region of the spectrum. It has indeed been found by photo-electric measurements that in a column of gas 16 cm. long, a quantity of ozone amounting to only 0·001 per cent. can be detected by measuring the intensity of light transmitted. The particular wave-length for which this absorption is a maximum has the value 258 $\mu\mu$, while the band extends from about 200 to 300 $\mu\mu$. These values attain an important significance when it is considered that the solar spectrum ceases suddenly at 293 $\mu\mu$, indicating the probability that light of shorter wave-length is absorbed in the atmosphere. As the absorption of light by oxygen is not appreciable for wave-lengths greater than 200 $\mu\mu$, the above phenomenon gives evidence of the presence of ozone in the higher atmosphere. In addition to this behaviour of ozone with respect to ultra-violet light, spectroscopic measurements show that this gas gives two well-defined bands in the red part of the spectrum. It is on account of this last absorption that the gas possesses a marked blue colour by transmitted light.

Bearing of Ozone on the Colour of the Sky.—The view has several times been put forward by chemists that ozone is present in the upper atmosphere in sufficiently large amounts to account for the normal blue colour of the sky. This idea has not up to the present time been at all generally accepted, but, on the contrary, in nearly all physical researches on the optical properties of the atmosphere the presence of ozone has been ignored. This omission has arisen on account of the absence, until recently, of any definite quantitative measurements of the amount of this gas in the air, and more especially on account of the larger developments of the purely physical theories which, on quite other lines, have established some of the main factors which determine the nature of sky light.

On this physical basis it has been demonstrated by Tyndall, and deduced from dynamical principles by Rayleigh, that one factor which contributes to this colour is the presence of ultra-miscroscopic particles of dust, which are present throughout the atmosphere, and probably of meteoric and volcanic origin. These particles, when of the same order of magnitude as the wave-length of the light, exert a selective influence on the light, causing the short waves which compose the blue light to be reflected, while the longer waves, or red light, pass on. This phenomenon has also been shown to operate in the production of the greenish blue colour of glacier water and certain lakes. The atmosphere is thus to be considered as a turbid medium; but this admission does not necessarily exclude other factors which might contribute to the colour.

After the development of the above theory to account for the scattering of light, Lord Rayleigh drew attention to the fact that, in addition to the part played by minute dust particles in this connection, the actual molecules of air act in a similar manner, and cause a selective refraction of the light. In this way it was considered that even in the absence of larger particles of matter, the observed properties of sky light could be accounted for.

Light which has been reflected or scattered by these minute dust particles in the atmosphere or by molecules shows the following properties. The rays which are reflected at an angle of 90° to the incident light are completely polarised, and the blue light is reflected much more completely than the red. It is calculated from theoretical considerations that with different wave-lengths the ratio of the reflected to the transmitted light varies inversely as the fourth power of the wave-length. According to this, blue light should be reflected to about eight times the extent of the red.

By measuring the intensity of the light which proceeds directly from the sun, Rayleigh's theory enables a calculation to be made of the composition of light proceeding from the sky, if controlled entirely by this principle of scattering. Experiments made with an artificially produced turbid medium, such as a steam jet or a fine precipitate suspended in water, show that this relation is very closely followed. Similar measurements on sky light can readily be made by means of a spectrophotometer. This instrument is first sighted directly on to sunlight, and a measurement made of the intensities of light in the different parts of the spectrum. These values are then compared with measurements made on light proceeding from the sky when viewed in a direction at right angles to the direction of the sun. According, then, to this theoretical relation deduced by Rayleigh, the intensity is highest in the violet, and falls rapidly towards the red in proportion to the inverse fourth power of the wave-length.

For wave-lengths still shorter than those in the violet the intensity would be expected to fall again rapidly, on account of the known absorption of the ultra-violet light by the atmosphere. This absorption, however, in presence of the admitted constituents of a clear atmosphere should have no appreciable influence in the region of the visible spectrum, so that the relation between intensity and wave-length should be capable of being plotted by means of an even curve.

Spectro-photometric measurements have been made by a number of workers under different conditions, such as varying altitude and latitude, with clear and overcast skies, and at different hours of the day and times of the year. Recent measurements have clearly shown that in some cases the curve of distribution of intensity follows the general direction demanded by Rayleigh's formula, but that as a rule the intensity in the violet and blue part of the spectrum exceeds the theoretical value in a very variable manner, and in some cases amounts to about double the normal value. Measurements made by E. L. Nichols[1] in Switzerland and the Tyrol showed that in the early morning curves agreeing closely with the theoretical values were given, but later in the day a comparatively large upward peak developed in that part of the curve representing the blue region of the spectrum. This peak reached a maximum in the early afternoon, and then diminished ; the effect was particularly marked at high altitudes. The nature of this peak in the curve has the appearance of an emission band superimposed on the normal spectrum of the sky, and lends support to a view which has been put forward that ozone, which is known to be a fluorescent gas, plays a part in the illumination of the sky in virtue of this property of fluorescence. An alternative view is that this comparative increase in the intensity of the violet is due to the selective absorption of the atmosphere for ultra-violet on one side and green on the other, an effect which also harmonises with the presence of ozone. Measurements which have been made to show the relation between wave-length and intensity of sky light, as reflected at an angle of 90° to the incident light, have shown that the exponent of 4, which is demanded by Rayleigh's formula, does not apply at all generally. This factor shows very large variations. A continuous increase has been traced in proportion to the zenithal distance of the sun, and it has been found to vary with the humidity of the atmosphere. These discrepancies and variations show that factors other than scattering operate in the illumination of the sky.

Colour of Setting Sun.—In considering selective absorption by the atmosphere, it is obvious that the phenomenon of scattering which causes reflected light to be blue leaves the transmitted light red. In consequence of this, light coming from the sky is

[1] *Physical Review* (1909), 28, 122.

subjected to a certain absorption causing a relative diminution in the intensity of the blue light or a relative increase of the red. This absorption is very much increased by the presence of condensed water vapour or mist in the atmosphere. The yellow or red colour of the sun when near the horizon, and the colouring of clouds or mountain peaks at sunset, is clearly explained by this influence. The thickness of the atmospheric layer traversed by the rays at this time is a maximum. It is obvious from a simple geometrical consideration that light proceeding from the sun when on the horizon, and more especially when below the horizon, passes through a relatively far greater thickness of lower atmosphere in comparison to upper, than when the sun is at the zenith. The absorbing influence of suspended matter present in the lower atmosphere is thus the predominant factor in determining the light from the setting sun.

If scattering of light according to Rayleigh's theory were the sole influence at work here, it would be expected that the sun viewed on the horizon would be of an invariable colour. However, observation shows that the nature of this light is very variable, showing that the elements in the atmosphere which filter out the blue rays of the transmitted light are not constant. Light transmitted from the setting sun through a clear sky is frequently not so red as would be calculated from the theory of scattering. Spectro-photometric measurements have been made of light from the sun passing through a cloudless sky when viewed below the horizon from a high mountain. The nature of the light transmitted under these conditions does not generally conform to Rayleigh's law of molecular scattering, but indicates the presence of other factors of absorption.

Cases have indeed been placed on record where, in the tropics, the air was exceptionally dry, the light transmitted from the sun on first appearing above the horizon was green. This, if authentic, would definitely establish the presence of a true absorption colour of air.

The Polarisation of Light from the Sky.—According to Rayleigh's theory, if the whole of the light proceeding from the sky is the result of scattering by molecules and particles which are small compared with the wave-length of light, then light which proceeds from that portion of the sky which is viewed in a direction at right angles to the direction of the sun's rays should be completely polarised. It might conversely be assumed that if all the light proceeding from this region were polarised, its origin would be solely due to the diffraction of molecules and small particles. The measurement of polarisation gives accordingly a method of ascertaining definitely the part played by selective scattering. The result of such measurements is to show that the light reflected at this angle of 90° is by no means completely polarised, and that the proportion of polarised to total light varies very largely from time to time.

Some recent measurements made by Boutaric[1] in Switzerland have shown that the degree of polarisation of light scattered at an angle of 90° varied between 0·4 and 0·7 of the total light. Measurements were also made on the constant of solar radiation which is discussed below. The degree of polarisation was found to vary concomitantly with these radiation values, or in other words, inversely as the absorption of the atmosphere. The variation in these values of the polarisation, and deductions which have been made from measurements on the relative luminous intensities of sunlight and skylight, have shown that it is necessary to assume that a large amount of light is reflected from the sky under conditions which do not conform to the theory of selective scattering. This is probably due to the reflection of light from particles which are large compared to the waves of light, and also to some extent from direct illumination by light reflected from the earth. The admission of these sources of light opens the possibility of the operation of such factors as the colour of the air itself due to elements which exert a selective absorption.

Experiments have been carried out on the degree of polarisation of the light of different wave-lengths proceeding from the sky. It was found that if blue rays are removed from this light by causing it to pass through a medium of complementary colour (red), so arranged that the sky appeared white when viewed through this liquid, then the light thus filtered showed exactly the same degree of polarisation as when measured after proceeding directly from the sky. It would appear from this that the blue light from the sky is not polarised, but that the polarised light is white, and that the blue colour results from the absorption by the air of reflected non-polarised light, or else possibly it is produced by fluorescent phenomena in the atmosphere.

Evidence of Solar Radiation on Composition of Upper Atmo-

[1] *Le Radium* (1914), **11**, 15.

sphere.—Direct measurements made on the surface of the earth of the solar constant of radiation—which is defined as the radiant energy falling on unit area of the earth's surface—are of course affected by any absorption which takes place in the atmosphere. The determinations made of this constant, which is of very important astronomical significance, have shown considerable variations. Much careful research has been carried out in recent years to determine the numerical value of this constant, but on account of the varying results obtained, there is still a deal of uncertainty even about the approximate value.

Careful determinations made by Abbot and Fowle[1] in America, at an altitude of 14,500 feet, give a mean value of 1·922 calories per minute per square cm. of the earth's surface. However, marked fluctuations, covering a range of 8 per cent., in the radiation received were observed. In all cases the values obtained agreed very closely with those made simultaneously at an altitude of 5,800 feet. It was concluded that the absorption of extreme ultra-violet rays by the atmosphere did not cause an error greater that 1 per cent. in the total radiation received. The observed fluctuations were attributed to changes in the actual emissivity of the sun.

In conversation, it was suggested by Mr. R. Rossi to the writer that the fluctuations are caused by ozone. While the absorption of visible light rays by oxygen and nitrogen is negligibly small, water vapour, on the other hand, has a considerable influence. Ozone, if present in only small amounts, would act similarly in causing a marked absorption of the sun's rays. As mentioned above, the presence of this gas in the upper atmosphere has been assumed as an explanation of the fact that the solar spectrum ceases abruptly in the ultra-violet at a point where ozone is known to have a deep absorption band. The question of solar radiation had been carefully studied by F. W. Very.[2]

Attention is drawn to the fact that where Nichols found an apparent emission band in the violet region of the spectrum of sky light, which attained a maximum about noon each day, other observers have found here an absorption band. This discrepancy is explained by Very on the supposition that the absorption band originates from the illumination of the lower

[1] *Astro-physical Journal* (1911), **33**, 191.
[2] *Ibid.* (1911), **34**, 371 ; (1913), **37**, 31.

atmosphere, and that the emission band asserts itself in the sky spectrum at high altitudes through the continual formation of some special material at high altitudes during the daylight hours. In accordance with the observation of Nichols that the emission band appears regularly as the day advances, Very considers that the phenomenon is connected with the formation of complex water molecules, arising from the ionisation of the upper air by the sun's ultra-violet rays. This assumption agreed with the results of the measurements made of solar radiation which gave evidence of the distribution of some invisible obstructing substance which is most potent to deplete the solar rays when the sun is highest. The results of direct measurements of the heat received by the earth from the sun gave values averaging about 2 calories per square cm. per minute. In view of the different factors of absorption, Very considers the true value to be at least 3 calories.

Part 2. The Chemical Estimation and Distinction of some Constituents of the Atmosphere

From the previous discussion it will be seen that the further elucidation of this subject of the optical properties of the atmosphere must lie in the precise determination of the presence of such bodies as ozone, hydrogen peroxide, and nitrogen peroxide. All of these gases have been thought to be produced to a larger or smaller extent by the action of ultra-violet light radiating from the sun on to the atmosphere, and also through the influence of ionisation accompanying electrical discharges in the atmosphere, and through the possible action of electrons emitted from the sun.

Though a very large amount of attention has been devoted to this subject in the past, it has not been possible to establish with any certainty the existence of these gases in the atmosphere. The results obtained by different workers in this field have been very discordant, and very few determinations have been attempted at high altitudes. The difficulties of such an investigation arise from the small magnitude of the amounts to be measured and the great difficulty under these conditions of making any distinction between the different gases in question. On account of the similarity in their chemical properties it was not possible to apply any method which would enable a satisfactory distinction or quantitative estimation to be made when the substances are present together at high dilutions.

Of these gases, the one which has always offered the greatest interest in considering atmospheric phenomena is ozone, and a very large amount of work has been devoted to carrying out comparative qualitative tests on its presence in air. The means adopted for this estimation have nearly always consisted in exposing to the air absorbent papers which have been saturated with a reagent, which reacts with ozone and thereby undergoes a marked change in colour. In this connection, the use of a mixture of potassium iodide and starch, which is coloured blue by traces of ozone, was established by Schönbein as early as 1840, and since then a number of organic reagents have been applied in a similar manner.

An investigation was undertaken by the writer[1] in order to examine comparatively some of the chemical properties of ozone, nitrogen peroxide, and hydrogen peroxide. Methods were devised of estimating ozone when in very small quantities, and distinguishing from the other gases which show very similar chemical properties. It was found in this work that the colorimetric change which is brought about by iodine in the reaction between ozone and potassium iodide cannot be used for any quantitative deductions, and that the method is unreliable even for qualitative results on account of reactions brought about by the influence of light, by impurities in the paper, and other disturbing causes. Similar objections were found to apply to all other forms of colorimetric tests. No conclusive distinction between ozone and other gases with similar properties, which have been considered as normal constituents of the atmosphere, has been possible by any of these "test papers."

Distinctive Chemical Properties of Ozone.—The reagent which finally was found to be most suitable for the estimation of ozone is a concentrated aqueous solution of neutral potassium iodide, the precaution being taken of protecting the liquid from the light during the measurement. This solution was found to react with ozone with great rapidity, even when the gas is very dilute. Reaction also takes place readily at temperatures as low as $-50°$, when the gas is passed over the surface of the solidified reagent.

A careful study was made of the chemical changes which take

[1] *Chem. News* (1914), **109**, 73.

place in this reaction, and a comparison made with those brought about by oxides of nitrogen and hydrogen peroxide. It was found that in the case of ozone, in accordance with the operation of mass action, an important influence is exerted on the nature of the products by the quantity of gas circulated and also for a given amount of ozone, by the concentration per unit volume. Thus with very dilute gas, and at temperatures above the freezing point, $-24°$, the formation of free iodine and hypoiodite results, while with a more concentrated gas, and in all cases with the solidified reagent, in addition to these products, potassium iodate is formed directly. An estimation of the products formed in the first case can readily be made by titrating the solution with standardised sodium thiosulphate solution until the yellow colour of the iodine is removed, and in the second case, the iodate can be estimated afterwards by acidifying, when decomposition into iodide and free iodine occurs, and this last is then estimated as before.

On comparing the above reactions with those given by hydrogen peroxide, it was found in this latter case that potassium hypoiodite and free iodine are formed to a limited extent, but no iodate. The reaction is not quantitative since the hypoiodite formed, when above a certain concentration, reacts with hydrogen peroxide with evolution of oxygen. Nitrogen peroxide was found to react with potassium iodide under all conditions to give mainly iodate together with some free iodine. With this last gas a very characteristic property was shown, which enabled the detection of minute traces of this gas. If in the potassium iodide solution which had been exposed to oxides of nitrogen, the free iodine, which was liberated after acidifying, was removed by titration with sodium thiosulphate, then, on standing in air, a further liberation of iodine developed with a velocity depending on the total amount of the oxides of nitrogen originally absorbed. This change is brought about through the catalytic influence of the nitrous acid formed in absorbing oxygen from the air and then undergoing reduction by the potassium iodide with liberation of iodine. This reaction enables an approximate determination to be made of these oxides when present in quantities too small to estimate by direct titration. The method thus resolves itself into a measurement of the rate at which the reagent after acidifying liberates iodine when exposed to air.

While this reaction is characteristic for oxides of nitrogen even when present with other gases, hydrogen peroxide can also be distinguished by the following separate tests. A solution of titanium sulphate in sulphuric acid is coloured yellow by traces of hydrogen peroxide, and this property forms a distinguishing test for this compound.

In the case of ozone, however, it was not found possible to characterise this gas when present together with the others. An estimation could probably be made in presence of the first two gases, if very dilute, by a method of elimination. For this purpose, a determination could be made of the sum of the three gases by absorbing in acidified potassium iodide, and then deducting the quantity of hydrogen and nitrogen peroxide as determined separately.

In the measurements made in the present work, it was found that nitrogen and hydrogen peroxide are not present at high altitudes to any detectable extent, so that the problem of the estimation of ozone was considerably simplified. The manner of applying these reagents to atmospheric tests is described below.

Part 3. The Action of Ultra-violet Light on Air

It is now a well-established fact that ozone is formed by the action of ultra-violet light on oxygen or on air, and that if initially above a certain concentration, exposure to the same light causes a decomposition of ozone into oxygen. In either of these reactions, the same stationary state is finally reached, representing an equilibrium value, when no further change in the concentration of the ozone results.

It has been found that the actual value of this equilibrium quantity varies with the nature of the light, and with the temperature and pressure of the gas. At a reduced pressure, for instance, the rate of formation of ozone is decreased, and the rate of its decomposition increased, so that the final equilibrium is represented by a smaller value.

Photochemical investigations have shown that light which is effective in causing the production of ozone from oxygen is limited to the region of the spectrum of wave-length below 200 $\mu\mu$. This corresponds to the observed fact that an absorption point of light by oxygen occurs at 193 $\mu\mu$ and below. It has similarly been found that rays which are effective in causing the decomposition of ozone lie in the range of wave-length between

185 and 300 $\mu\mu$, and this agrees with measurements which show that ozone absorbs light of wave-lengths up to 290 $\mu\mu$, and probably is related to the fact that the solar spectrum ceases at about this point.

The work of the writer has been devoted to the investigation of the action of ultra-violet light on air under different conditions of pressure, humidity, etc., so as to reproduce as far as possible the state prevailing in the upper atmosphere, and to obtain some idea of the concentration of ozone that can be reached in this way under the different conditions. An examination was also made to see if any other products such as oxides of nitrogen and hydrogen peroxide are produced by this action.

At the same time a large number of air analyses as described below were undertaken at high altitudes, and the above compounds estimated.[1] The apparatus used for these measurements on the exposure of air to ultra-violet light was specially designed so that the air could be brought into contact with light rays which had undergone a minimum amount of absorption through intervening media between the source of light and the air. It was also arranged to prevent the temperature of the air undergoing exposure from rising to any large extent.

The reaction vessel employed was constructed entirely of quartz and is shown in fig. 1; R R are two reservoirs of mercury which are respectively in electrical connection with the terminals T_1 and T_2. By connecting these terminals to a source of 100 to 200 volts potential, in series with a suitable resistance, an arc could be maintained inside the evacuated tube B. This arc, which arises through the conductivity of mercury vapour, is a very rich source of ultra-violet light. After passing through the walls of B, the light reacts with the air in the surrounding vessel, through which a circulation is provided by means of the inlet and outlet tubes E E. The metal strips attached to the outside of the tubes containing the mercury were arranged for the purpose of cooling by radiation, when the apparatus was operated in air. For the present experiments, however, the lamp was submerged in cold water. The arc was started by means of an induction coil in the following manner. A potential of 100 to 200 volts was applied to the wire at T_1 and T_2 and the secondary circuit of the coil was connected to one of the terminals at T_1 and to a wire (W) wrapped round the centre of the annular

[1] Cf. *Proc. Roy. Soc.* (1914).

jacket of the lamp. On working the induction coil a glow discharge was caused to pass through the lamp and led to the formation of an arc between the reservoirs of mercury.

The reaction vessel was connected on the one side with a reservoir of air and a series of drying tubes, and on the other with a wash bottle which contained a reagent to absorb the products of the exposure. A mercury Töpler pump was placed at the end to enable exhaustion and complete drying of the

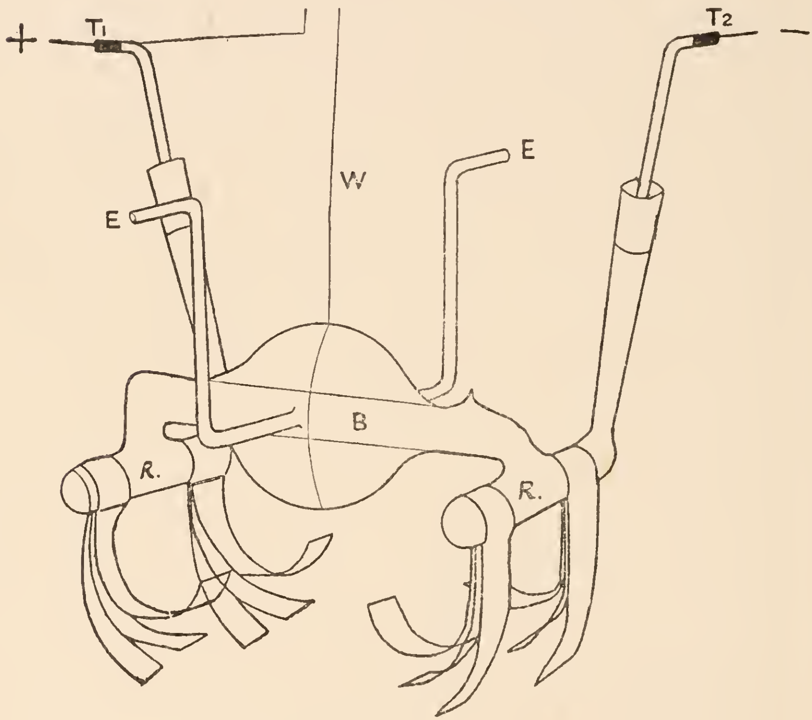


FIG. 1.

apparatus and delivery tubes and in order to conduct experiments at reduced pressures.

During the operation of the mercury arc, on account of the heating effect in the central portion of the apparatus and the water cooling of the outside vessel, a very non-uniform temperature would be obtained in the annular reaction space, and it was not attempted to keep any part of the apparatus below 15°.

Accordingly, in the respect of temperature, the conditions prevailing in the upper atmosphere could not be reproduced in these experiments. This increase of temperature would be expected to have no effect on the formation of oxides of nitrogen, which are stable at moderately high temperatures, but would lower the yield of ozone.

Equilibrium Value of Ozone.—Air which had been stored over water was passed directly through the vessel and then through the wash bottle containing a concentrated solution of potassium iodide. In measuring the equilibrium value of ozone produced by circulating the air at very low speeds, it was found that the temperature of the lamp quickly imposed a limit to the concentration of ozone. The highest yield of this gas was obtained when the arc was formed intermittently for a very short interval every half minute, thus minimising the rise of temperature. A confined volume of air in the annular space of the lamp was exposed to the radiation. The arc was formed for 1 second every 30 seconds, and the ozone estimated after different intervals. It was found that the quantity of gas formed increased rapidly at first and then slowly until after about 40 seconds' total exposure a maximum was practically reached containing 0·15 per cent. ozone. A similar result was obtained when the experiment was conducted with pure dry oxygen. In this case, the maximum amount of ozone formed was 0·2 per cent.

These values are not definite constants, since by using other types of apparatus it has been found possible to obtain a concentration of 3·4 per cent. at 20°, and 2·7 per cent. at 54°. It follows from the fact mentioned above of the formation and decomposition of ozone being effected by light of different wave-lengths, that in the present experiments the equilibrium values obtained will vary with the nature of the light, and this will be determined by the relative transparency of the quartz to the waves of different lengths. The nature of the light in the ultra-violet region of the spectrum radiated from the sun on to the upper layers of the atmosphere is not at all known, so that the conditions prevailing in this region could not be reproduced in the laboratory. The scope of the present experiments was consequently limited to the determination of the relative formation of the different products given under varying conditions by an arbitrary source of light.

Influence of Water Vapour on the Formation of Ozone.—It was found that a more valuable yield in the formation of ozone by ultra-violet light is obtained with dry air or oxygen than in the case of the moist gases. This influence of water has been noticed by earlier investigators, and is possibly due to the formation of traces of hydrogen peroxide which is known to react with ozone according to the equation:

$$H_2O_2 + O_3 = H_2O + 2O_2$$

Effect of Pressure on the Formation of Ozone.—In this series of experiments, air after drying was circulated through the reaction vessel at different pressures by means of the Töpler pump. After issuing from the radiation vessel the air was immediately led through the reagent. Measurements were conducted at pressures of 760, 70, and 30 mm. in the different cases. The same total amount of air was passed in each experiment and exposed to the radiation for the same intervals of time (10 mins.). The results showed a great decrease in the formation of ozone with decrease in pressure. Thus at 760 mm. pressure, the yield amount to 0·01 per cent., and at 30 mm. to 0·0014 per cent.

Formation of Hydrogen Peroxide by Ultra-violet Light.—Experiments which have been made on the exposure of water to ultra-violet light have indicated that a slight decomposition takes place in accordance with the reaction:

$$2H_2O = H_2O_2 + H_2$$

It has also been stated that when moist air is submitted to the action of ultra-violet light, traces of hydrogen peroxide are formed. Thus, in one case, by circulating air at the rate of 35 litres per hour through a reaction space where it was exposed to a powerful source of ultra-violet rays, and then passing the air through a solution of titanic acid, a slight yellow colour developed in the reagent, thus indicating the presence of traces of hydrogen peroxide.

Experiments were made by the writer to detect the formation of hydrogen peroxide by passing 60 litres of moist air through the apparatus during two hours, and leading through a solution of titanic acid in sulphuric acid contained in a small glass spiral washer. No change in colour was observed. A comparative test made by taking hydrogen peroxide solution showed that it is possible to detect with certainty the presence of 1×10^{-6} gram of this compound with the above reagent. In 60 litres of air, this would correspond to a volume of $1·8 \times 10^{-6}$ per cent. The amount formed under the conditions of the above experiment must therefore be below this value, which is very small compared with the amount of ozone formed. As hydrogen peroxide is decomposed by this last gas, it is doubtful whether any appreciable quantity would be permanently stable in presence of ozone.

Formation of Oxides of Nitrogen.—By employing as a reagent meta-phenylene-diamine, which undergoes a change of colour in presence of nitrogen peroxide, an indication has been obtained that very small quantities of this gas are obtained by the action of ultra-violet light on air. This method of testing is extremely sensitive and probably not free from objection. The formation of this oxide has not been demonstrated by any corroborative test.

The only method, apart from colorimetric tests with organic reagents, which appears to have been applied hitherto for distinguishing ozone from oxides of nitrogen when at high dilutions is one which consists in passing the gas into liquid air, when ozone dissolves and nitrogen peroxide separates as a solid. This method was applied in experiments made by the writer. A total volume of 66 litres of air, after passing through a concentrated solution of potassium hydroxide and then through sulphuric acid, was led through the reaction vessel, where the arc was formed continuously, and was then passed into liquid air. A period of six hours was taken for the passage of the total volume. After this time, a small quantity of white solid, which appeared to be mainly ice, had collected in the liquid air. On separating by filtration through fine cloth, and then collecting the gas evolved on evaporation in a gasometer over mercury, about a litre of gas was obtained. This did not give any coloration with tetra-methyl base paper, nor, on passing the whole through acidified potassium iodide solution, was any iodine liberated.

Though it cannot finally be stated from these experiments that *no* formation of oxides of nitrogen or hydrogen peroxide occurs through the influence of ultra-violet light, yet it is shown that the quantity obtained is negligibly small when compared with the ozone. Further researches on these points will have to take into account the refrangibility of the light applied.

The experiments show clearly that in the higher atmosphere the conditions are present for the formation of a considerable quantity of ozone, but the data are not available for calculating the magnitude of this equilibrium value.

It may be inferred that as the light of the small wave-length necessary for the formation of ozone cannot penetrate any large distance into the atmosphere, this formation of ozone must be confined to the very high layers of the atmosphere.

Part 4. The Estimation of Ozone in the Atmosphere at High Altitudes

A. *On Mountains.*—The reaction vessel which was used to enable the application of these measurements in atmospheric determinations was devised so as to be suitable for use in

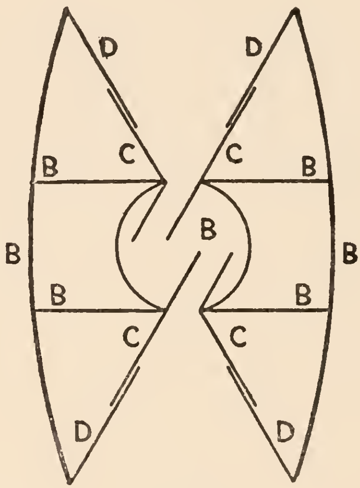


Fig. 2.

mountain districts, and also for attaching to sounding balloons. It was necessary in this last case to provide protection for the vessel on striking the ground, and in order to obtain the desired altitude, a total weight of two ounces could not be exceeded.

The absorbing vessel which was used to contain the reagent consisted of a spherical glass bulb as shown in fig. 2. This was provided with an inlet and an outlet tube terminating in conical funnels (C C) on the outside, and projecting on the inside for some distance inside the bulb. A quantity of reagent could thus be retained in the vessel when turned in any position. A free circulation of air through the vessel took place when this was placed horizontally and exposed to a wind, and similarly, when drawn in a vertical position through the air, as when attached to a balloon. The glass funnels (C C) were extended by means of paper (D D), and the whole enclosed in a cage of hard spring wire (B B) for protection. The glass vessel was blackened on the outside to protect the reagent from light. By placing a fuming liquid in the vessel and exposing to a wind, the air in passing through was seen to assume a rapid rotatory motion inside. The efficiency of the absorption of ozone during this passage was tested by taking a vessel of the above form, placing the potassium iodide reagent inside, and then joining the vessel in series with a wash bottle containing the same reagent. On passing ozonised air from a gasometer through the two vessels in succession, and varying the speed of circulation, it was found that even at higher velocities than those subsequently given by the atmosphere, more than four-fifths of the ozone was in every case absorbed by the first vessel.

An approximate calibration of the volume of air circulated was made by means of the assumption that this amount is arithmetically proportional to the velocity of the wind. A measurement was then made by placing some pure benzene in the vessel, and after exposing for definite intervals to a wind of known velocity, noticing the loss in weight. Knowing the vapour pressure of benzene at the prevailing temperature, it was possible to calculate the volume of air passed by assuming that evaporation of the benzene would take place to the saturation point. The average of a number of these determinations showed that when the apparatus was exposed to a wind for an interval, during which a horizontal flow of air of one mile occurred, the volume circulated through the vessel corresponded to 5·12 litres.

Estimations of ozone, extending over several days, were made in Switzerland, first at a point near Scheidegg (Wengern Alps), at an altitude of 6,970 feet, and then at a point near the Jungfraujoch, of 11,690 feet altitude.

During these measurements, tests were made for hydrogen peroxide by exposing titanic acid solution in an apparatus similar to that used for the ozone estimation. The colour of this reagent remained quite unchanged after exposing for two days at the different altitudes and under different conditions of weather, thus showing that there was no appreciable quantity of hydrogen peroxide in the atmosphere. It was noticed on the other hand that freshly fallen snow or hail gave a very marked coloration with the reagent. It is hoped later to conduct tests with glacier water, as this would be expected to retain the hydrogen peroxide associated with the snow.

In the estimations of ozone, made by means of potassium iodide, it was found that in no case was any potassium iodate formed. As pointed out above, this shows the absence of any appreciable quantity of oxides of nitrogen.

The results of the estimations of ozone are shown below in tabular form:

Time of measurement.	Litres of air circulated.	Total volume of ozone absorbed. c.c.	Volume of ozone per unit volume of air.
A. *Measurements at altitude of* 6,970 *ft.*			
August 22-23, 1913, 7 p.m. to 8 a.m. .	15·2	0·05	0·000003
August 23, 8 a.m. to 7 p.m. . .	113	0·31	0·0000027
August 23-24, 7 p.m. to 8 a.m. . .	438	0·87	0·00000197
		Mean	$2{\cdot}56 \times 10^{-6}$
B. *Measurements at altitude of* 11,690 *ft.*			
August 24, 6 to 7.30 p.m. . . .	165	0·62	0·0000038
August 25, 8.50 a.m. to 5.45 p.m. .	1,470	7·1	0·0000048
August 26, 6 a.m. to 6 p.m. . .	210	1·18	0·0000056
		Mean	$4{\cdot}7 \times 10^{-6}$

B. *Measurements made with Free Balloons.*—In order to obtain some idea of the amount of ozone in the higher regions of the atmosphere, use was made of the sounding balloons which are used in meteorological investigations at the Manchester University. These balloons, with the instruments attached, rise to an average height of about ten miles, and then burst. The deflated skin retards the rate of fall of the instruments to the ground. In most cases these are returned by the finder. A knowledge of the height attained and the temperature is obtained by a recording baro- and thermograph. The reaction vessel for the ozone tests was of the same form as shown in fig. 2, and was suspended vertically from the balloon together with the other instruments.

A rough calculation of the amount of air which would pass through the vessel during an ascent and descent was made from the following data. From the calibration made above, it was seen that the exposure of the vessel to a horizontal flow of air of one mile caused the passage of 5·12 litres. Expressing in centimetres, this gives for a displacement of 1 cm.

$$\frac{5{\cdot}12 \times 10^3}{1{\cdot}6 \times 10^5} \text{ c.c.} = 0{\cdot}032 \text{ c.c.}$$

since 1 mile = $1{\cdot}6 \times 10^5$ cm.

On the assumption that the volume circulated is proportional to the displacement through the air, it follows that during an ascent and descent, the mass of air passed through in grams is given by $2\,(p - p_1) \times 13{\cdot}6 \times 0{\cdot}032$, or $0{\cdot}87\,(p - p_1)$, where p is the atmospheric pressure in cm. of mercury at ground level, p_1 that at the highest level reached, and 13·6 the density of mercury. The volume circulated in litres (measured at N.T.P.) is therefore $0{\cdot}675\,(p - p_1)$.

The values for the pressures at different altitudes have been taken from meteorological tables as reproduced below:

Altitude, metres.	Pressure, millimetres.	Altitude, metres.	Pressure, millimetres.	Altitude, metres.	Pressure, millimetres.
1,000	673	4,000	458	9,000	233
2,000	586	5,000	401	11,000	168
3,000	522	7,000	308	20,000	39

At a height of about 6,000 metres the temperature is always below the freezing point of the reagent (− 24°), so that reaction must then take place with the solid. It was seen above that under these conditions the method applied did not enable a distinction between ozone and oxides of nitrogen. However, in all measurements made up to 3,600 metres, it was found that neither this gas nor hydrogen peroxide were present in any appreciable quantity. Nitrogen peroxide is of course quite stable at ordinary temperatures, and until dissolved by atmospheric water as nitric acid, any gas formed at high altitudes would remain undecomposed.

The reagent placed in the vessel in these atmospheric tests consisted of 5 to 7 cc. of the 50 per cent. potassium iodide solution as used in all the other measurements. After being returned by the finder through the post, the amount of solution remaining usually amounted to about 2 cc. The values given in the tables are calculated on the initial quantity of reagent.

Time of ascent.	Mean direction of wind.	Height attained.	Estimated volume of air circulated.	Weight of Iodine liberated in total reagent. Free I_2 hypo-iodite	Combined I_2 iodate.	Potassium hydrate.	Mean conc. of ozone.
		kilometres.	litres.	milligram equivalents.			Volume in 1 of air.
August 3, 1909 9 p.m. . .	W.S.W.	16	46·0	0·12	0·13	—	$2·6 \times 10^{-7}$
March 18, 1910 12 night .	N.	13·5	43·8	0·21	0·13	0·004	$6·8 \times 10^{-7}$
March 18, 1910 9 p.m. . .	N.	8·5	34·7	1·02	0·70	nil	$4·4 \times 10^{-6}$
March 19, 1910 2 a.m. . .	N.	19·5	48·1	0·40	1·12	—	$2·8 \times 10^{-6}$
May 18, 1910 9.40 p.m. .	S.	17·0	47·5	0·76	0·76	—	$2·8 \times 10^{-6}$
May 19, 1910 2.10 a.m. .	S.	12·0	43·8	0·13	0·34	trace	$9·5 \times 10^{-7}$
May 19, 1910 6.30 a.m. .	S.	20·0	48·3	0·21	0·40	—	$1·1 \times 10^{-6}$
March 1, 1911 5.30 p.m. .	N.W.	19·0	48·2	0·303	0·682	nil	$1·8 \times 10^{-6}$
May 4, 1911 7.15 p.m. .	N.W.	20·0	48·3	0·612	2·36	nil	$5·4 \times 10^{-6}$
August 6, 1913	N.N.E.	6·5	29·8	0·416	nil	—	$1·2 \times 10^{-6}$
						Mean	$2·1 \times 10^{-6}$

It is seen that, except in one case, a large part of the reaction had resulted in the formation of iodate. The one case in which this compound had not formed was where the ascent had only reached an altitude of 6·5 kilometres, where the temperature indicated was $-31·5°$. Since the freezing point of the reagent is $-24°$, complete solidification had probably not occurred. The fact that, as in the case of all measurements at lower altitudes, there was no iodate formed in this measurement, indicated the absence of oxides of nitrogen, and the formation of iodate in experiments at greater altitudes is due to the reaction of ozone on the solid reagent, as was established in laboratory experiments.

By considering these results together with those made on ground level at altitudes up to 3·5 kilometres, the conclusion may be drawn that there is no appreciable amount of hydrogen peroxide in the higher atmosphere, but that there is a considerable quantity of ozone.

The mean values of ozone estimated in the measurements made in the Alps were $2·5 \times 10^{-6}$ in one volume of air at 2·5 kilometres altitude, and $4·7 \times 10^{-6}$ parts at 3·5 kilometres. In the measurements made with the balloons above Manchester, the mean volume of ozone between ground level and altitudes up to 20 kilometres gave a value of $2{\cdot}1 \times 10^{-6}$. Even after allowing for the absence of this gas at lower altitudes, the measurements, though only approximate, indicate that there is no very large increase in the amount of ozone at altitudes between 4 and 20 kilometres. However, since at this last height the pressure of the atmosphere is still about 4 cm., the amount of light of wave-length below 200 $\mu\mu$, which is necessary to form ozone, would be very small. The probability thus still remains that above this elevation a largely increased content of ozone prevails.

Part 5. The Influence of Ozone on the Nature of Light from the Sky

The results in the above experiments of the approximate determinations of the quantity of ozone in the higher atmosphere supply data which enabled measurements to be made in the laboratory of the depth of colour given by this amount of ozone.

For this experiment, a glass tube of 2·8 metres length and 4 cm. diameter was taken. The walls were provided with side tubes, one near each end, to enable the passage of the ozonised gas through the tube. The two ends of the main tube were covered by thin plates of glass, which were cemented by sodium silicate solution so as to make an air-tight connection. The outside of the tube was wrapped with black paper, and a white paper disc placed over one of the end plates. On illuminating this by daylight and viewing the transmitted light through the other end, the intensity of coloration produced on admitting ozone of known concentration could be observed.

The ozone for this purpose was prepared from oxygen by passing through a number of annular glass tubes where it was exposed to the silent electric discharge produced by an induction coil. After time had been allowed for the composition of the gas in the sighting tube to become uniform, an analysis of the gas was made by passing a measured volume into an acidified potassium iodide solution.

The results given in the table below record the observations made with the tube when filled with oxygen containing different concentrations of ozone. The thickness of the layer of pure gas which is equivalent to this concentration is also given.

Percentage concentration of ozone in oxygen.	Equivalent thickness of layer of pure ozone.	Colour observed.
0·20	0·55 cm.	Colour uncertain.
0·36	1·0 ,,	Faint bluish green.
1·7	4·7 ,,	Distinct blue colour.
2·8	7·8 ,,	Indigo or steel blue.

It is difficult to compare the colour of the gas in a tube of the above nature with that of the sky on account of a large influence exerted by the nature of the illumination.

The above amounts of ozone can be compared with those found in the atmosphere. Taking the amount of this gas found in the Alps at an altitude of 3·6 kilometres as the mean concentration throughout the atmosphere, and allowing 8,350 metres as the height to which the atmosphere would extend if at N.T.P., this concentration of ozone in a vertical section of the atmosphere is equivalent to a layer of the pure gas of a thickness of 4·2 cm. at N.T.P. On comparing this with the observations made on the colour of ozone in a glass tube, it is seen that light which has been transmitted through a layer of gas of this thickness possesses a distinct blue colour. In the case of atmospheric ozone at very high altitudes it is probable that the amount of ozone increases and there is also the possibility that the blue or violet colour is intensified in this case on account of fluorescence by ultra-violet light from the sun.

With regard to the values obtained in the estimation of ozone at high altitudes, on account of incomplete absorption by the reagent, the experimental error of the measurements would be expected to give too low a value. On account of this and the probability of a large increase in the amount of ozone at altitudes above 20 kilometres, the results of these measurements indicate that ozone is an important factor in determining the optical properties of the atmosphere and the colour of the sky.

The writer wishes to express his indebtedness to the Council of the Royal Society for their courtesy in allowing the reproduction of the diagrams in this paper.

COLOUR VISION AND COLOUR-VISION THEORIES, INCLUDING THE THEORY OF VISION

By F. W. EDRIDGE-GREEN, M.D., F.R.C.S.

At the present time the whole subject of colour vision is in a state of chaos. Misstatement and erroneous deduction are found instead of actual fact, and in many books the reader cannot obtain even the most elementary idea of the true facts. This state of affairs is due to the very defective state of scientific method of the present time. The official scientific methods favour incompetence and ignorance, and are opposed to the progress of science. It is necessary for me to support such a sweeping statement, and I do so in the hope that it may lead to an alteration in the present state of affairs. The official methods of dealing with scientific papers and facts are the referee system, and the appointment of scientific committees. In the referee system, a scientific paper intended for publication is sent to a referee, who decides on its merit. If rejected, neither the communicator nor the author of the paper is told the reason of its rejection. A late secretary of the Royal Society told me that he could ensure the acceptance or rejection of any paper simply by the selection of the man to whom he sent it. It is extremely difficult to select an efficient referee. It is not of much use to send the paper to a man who knows nothing about the subject, whereas if sent to a worker on the same subject, the paper may entirely upset that worker's views, and he may for that reason alone reject it. The present state of things may be entirely remedied if a report giving the reason of the rejection were sent at least to the communicator of the paper. It would not be necessary that the referee's name should be mentioned, but the author of the paper could then have an opportunity of replying, and if the objection were one of fact, the question of the correctness of the observation could be submitted for demonstration and discussion to the special scientific society under whose province it came, as, for instance, a physiological fact for the Physiological Society, a physical fact for the Physical Society, etc.

The second scientific method is that of the appointment of scientific committees. If these committees really did their work in a scientific manner, the results would be very valuable; but when the committee will only take the evidence of witnesses, and will not examine essential facts, the only result is to set back the subject for a number of years, and to ruin the career of the man who has discovered the new facts. Twenty-five years ago I pointed out in minute detail (1) how defective the wool test for colour-blindness was. A special committee of the Royal Society was appointed to decide on the truth of my statements, but though it took my evidence it refused to examine my cases, and decided in favour of the efficiency of the wool test. A departmental committee appointed a few years ago to decide the same question, took my evidence at length, but, as before, would not let me demonstrate facts and cases to it, even though I made a strong protest against this course. The Board of Trade did not adopt my lantern (the official test of the Navy), but constructed a lantern similar to one of my discarded models; and it will be interesting to note the results of the examinations with this lantern compared with those of a specially improved wool test, in which five test colours are employed, two of which are similar to those recommended by me twenty-five years ago if a wool test were to be employed. It will be seen (2) that 52 per cent. of those finally rejected passed this improved wool test, whilst not a single person other than normal sighted were rejected by the wool test alone. It should be noted that a present examiner, and member of the committee, stated, in a book issued after the report, that the wool test was sufficiently good! This attitude, which is unfortunately so common, especially at Cambridge, of expressing a strong opinion whilst refusing to look at the facts, is not only unscientific, but absolutely dishonest, and brings discredit not only on the academic individual who evolves his science from his inner consciousness, but upon the real scientific worker who does his work with the most punctilious and conscientious accuracy.

It is necessary to make these few preliminary statements, because authority appears to paralyse the reasoning powers of nearly every one, and it will be therefore necessary for the reader to think for himself in perusing this article.

The theory of vision and colour vision which I adopted as a working hypothesis is as follows:

A ray of light impinging on the retina liberates the visual purple from the rods, and a photograph is formed. The rods are concerned only with the formation and distribution of the visual purple, not with the conveyance of light impulses to the brain. The ends of the cones are stimulated through the photo-chemical decomposition of the visual purple by light, and a visual impulse is set up which is conveyed through the optic-nerve fibres to the brain. The character of the stimulus and impulse differs according to the wave-length of the light causing it. In the impulse itself, we have the physiological basis of the sensation of light, and in the quality of the impulse the physiological basis of the sensation of colour. The impulse being conveyed along the optic-nerve to the brain, stimulates the visual centre, causing a sensation of light, and then, passing on to the colour-perceiving centre, causes a sensation of colour. But though the impulses vary in character according to the wave-length of the light causing them, the retino-cerebral apparatus is not able to distinguish between the character of adjacent stimuli, not being sufficiently developed for the purpose. At most, seven distinct colours are seen, whilst others see, in proportion to the development of their colour-perceiving centres, only six, five, four, three, two, or none. This causes colour-blindness, the person seeing only two or three colours instead of the normal six, putting colours together as alike which are seen by the normal-sighted to be different. In the degree of colour-blindness just preceding total, only the colours at the extremes of the spectrum are recognised as different, the remainder of the spectrum appearing grey.

It is obvious that this theory could not be true if the facts of colour vision were as stated in the books of twenty-five years ago. Apart from the relative functions of the rods and cones if colour vision were a secondarily developed power of discrimination, then the following should be facts:

1. There should be innumerable varieties of colour discrimination, which could be arranged in a series from total colour-blindness to super-normal colour vision.

2. The number of colours seen in the spectrum should depend upon the development of colour discrimination; those colours presenting the greatest physiological difference being the first to be discriminated.

3. The physiological difference would probably correspond roughly to the physical difference; that is to say, the largest and smallest waves would be the first to be differentiated.

4. Yellow should be a simple but secondary sensation.

5. A pure spectrum should be divisible into a series of monochromatic areas, the size of these areas depending upon the development of colour discrimination.

6. There should be defects of light perception distinct from defects of colour perception; shortening of the red or violet end of the spectrum should be distinct defects, and not necessarily associated with defective colour discrimination.

7. There should be innumerable varieties of dichromic vision.

8. There should be trichromic cases of defective colour perception, three colours being seen in the bright spectrum, yellow being seen as red-green, and blue as green-violet.

9. All colours when reduced sufficiently in luminosity and area should appear white, the colour disappearing first in the least developed portions of the retina.

10. Simultaneous and successive contrast should be increased in those with defective colour discrimination.

11. Those with defective colour discrimination should see like those with better discrimination in conditions of more difficulty.

Now all these predictions have been fulfilled, and are found to be actual facts, so that whilst the facts support the theory, they present difficulties to be solved by any other theory.

We will now review a number of facts of colour vision in order to show the requirements of any colour-vision theory. Further details will be found in the papers to which references are given.

I. The Facts of Colour Mixing

The fact that any colour may be matched by a combination of three selected spectral colours is the foundation of the trichromatic theory which was propounded in order to explain it. The trichromatic theory, which may be represented by $\alpha x + \beta y + \gamma z =$ any colour, is only one possible explanation of the facts. Let us take, for instance, the fact that when spectral red and spectral green are mixed in appropriate proportions they match spectral yellow. The other explanations of this fact are that red and green each contain a yellow element, when the two are mixed the red and green cancel each other, and only the green is left (Hering): that the yellow was the original substance, and has become split up in the course of development into a red and a green substance, and when green and red are mixed they combine into the original substance yellow (Ladd Franklin): that in previous stages of development all men saw the yellow region as red-green, but when a new colour yellow had replaced the red-green of a previous stage of development a mixture of red and green gave rise to the colour yellow which had replaced the red-green of the previous stage (Edridge-Green). Now it will be noticed that these three last hypotheses all explain the facts better than the trichromatic theory, which does not, like the others, explain why red and green should make yellow and not red-green, especially in view of the fact that it does make red-green to some persons. Many physicists confuse the mixing of objective lights with the mixing of physiological sensations. This particularly applies to those who state that the trichromatic theory is not a theory, but a fact; not only is it not a fact, but only a possible explanation of certain facts, and, as will be shown in the remainder of this article, there is not a single fact which directly supports it, but very many which show that it cannot be true. Those writers who state that three-colour photography or three-colour printing are based on the trichromatic theory ought to state that they are based on the facts of colour mixing. In the case of mixing pigments the primary colours are different, certain yellow and blue pigments when mixed make green instead of white, which is the case when pure spectral light is used. The reason of this can be made plain by examining a yellow and a blue glass with a spectroscope: it will be noticed that the yellow lets through orange and green as well as yellow rays, and the blue lets through green and violet as well as blue rays. If the two glasses be super-imposed, and then placed before the spectroscope, it will be noticed that only the green rays get through, the others being stopped by the combination of the two glasses.

II. The Simple Character of the Yellow Sensation (3, 4)

If the trichromatic theory were a fact, it should be possible to show that the sensation excited by pure spectral yellow is a composite sensation, and there should be evidence of its alleged components. Every fact, however, shows that yellow is a simple and non-composite sensation. If the eye be fatigued with pure yellow spectral light the spectrum will appear to have lost its yellow, and though yellowish red or yellowish green will appear less yellow, the terminal red of the spectrum will not be affected (5). If the terminal portion of the red end of the spectrum be isolated in my spectrometer, it will appear as a faint red upon a black background. If the eye be fatigued with red light, even by looking through a red glass held against a light for one second, the red will not be visible for some considerable time, but the eye may be fatigued for twenty minutes with yellow light without interfering with the visibility of the red light.

It is known that if the intensity of a number of coloured lights be reduced in the same proportion all the colours do not disappear at the same moment. If, therefore, spectral yellow were a compound sensation, it should change colour on being reduced in intensity. If, however, spectral yellow be isolated in my spectrometer, and the intensity be gradually reduced by moving the source of light away, the yellow becomes whiter and whiter until it becomes colourless, but does not change in hue.

The eye may be fatigued with red or green without altering the hue of spectral yellow. Spectacles glazed with red or green glass of a kind which is permeable to the yellow rays may be worn for a considerable time without altering the appearance of spectral yellow. If yellow were a compound sensation a wearer of red spectacles should see the yellow through them as green, because the yellow would fall on a portion of the retina which had been fatigued for red.

III. The Facts of Colour-Blindness

Cases of colour-blindness may be divided into two classes, which are quite separate and distinct from each other, though both may be present in the same person. In the first class there is light as well as colour loss. In the second class the perception of light is the same as the normal-sighted, but there is a defect in the perception of colour. In the first class certain rays are either not perceived at all or very imperfectly. Colour-blind individuals belonging to the second class can be arranged in a series. At one end of the series are the normal-sighted, and at the other the totally colour-blind. I have classified the colour-blind in accordance with the number of primary colours which they see in the spectrum. If the normal-sighted be designated hexachromic, those who see five colours may be called pentachromic; those who see four, tetrachromic; those who see three, trichromic; those who see two, dichromic; and the totally colour-blind. There are many degrees included in the dichromic class. There may or may not be a neutral band, and this is widest in those cases approaching most nearly to total colour-blindness.

The fact of this gradation of colour perception has now been definitely recognised. The old classification of red-blindness, green-blindness, etc., has no meaning—experts examining the same case may diagnose it differently. The late Dr. Pole, who was colour-blind (a simple dichromic), was examined by Maxwell, who stated that he was completely red-blind, and subsequently examined by Holmgren, who pronounced him to be completely green-blind! (6). Shortening of the red end or violet end of the spectrum is a distinct defect from defective colour discrimination (7). A normal-sighted person when examined with my spectrometer with a bright spectrum marks out about eighteen monochromatic divisions, those with defective colour discrimination mark out a fewer number in proportion to their defect (8, 9, 10, 11, 12). The dichromic (13, 14) see two colours in the spectrum, red and violet, with a neutral division of varying size between the two colours. The trichromic (15, 16, 17) see three colours in the bright spectrum, red, green, and violet. The orange and yellow regions are seen as red-green and the blue region as green-violet. Here we have persons who have three sensations who are to a certain extent colour-blind. Sir William Ramsay and Sir J. J. Thomson belong to this class. A trichromic in conditions of difficulty becomes dichromic. As the colours are farther apart in the colour-blind, simultaneous contrast is increased.

IV. The Evolution of the Colour Sense (18)

It is obvious that the sense of light must have been developed first, and then the sense of colour. Let us consider the evolution of the colour sense in accordance with the difference of wave-length. First there will be a colourless spectrum, then a spectrum with a tinge of red at one end and a tinge of violet at the other, then the red and violet will encroach on the white region until they meet in the centre, and a fresh colour green is developed. In further development the red-green region is replaced by yellow, the blue replaces the violet-green region, then orange becomes distinguishable, and finally indigo. Every fact points to this being how the evolution of the colour sense has taken place, and there are various degrees of colour perception corresponding to every stage in the process.

V. Normal Colour Vision (19, 20, 21, 22)

The theory of colour discrimination given accounts for the facts of normal colour perception. When a spectral light is diminished in intensity colours disappear in the order of their development. Complementary colours are a necessity of the theory. In the dichromic red and violet are complementary to each other, and a mixture of red and violet is confused with white and green. When the stage is reached that green is distinguished as a separate colour, vision assumes the trichromic character which henceforth remains, and green now becomes complementary to the other two colours.

VI. Simultaneous Colour Contrast (23, 24)

1. The colours seen by simultaneous contrast are due to the exaggerated perception of a real objective relative difference which exists in the light reflected from the two adjacent surfaces.

2. A certain difference of wave-length is necessary before simultaneous contrast produces any effect. This varies with different colours.

3. A change of intensity of the light of one colour may make evident a difference which is not perceptible when both colours are of the same luminosity.

4. Simultaneous contrast may cause the appearance of a colour which is not perceptible without comparision.

5. Both colours may be affected by simultaneous contrast, each colour appearing as if moved farther from the other in the spectral range.

6. Only one colour may be affected by simultaneous contrast, as when a colour of low saturation is compared with white.

7. When a false estimation of the saturation or hue of a colour has been made, the contrast colour is considered in

relation to this false estimation. That is to say, the missing (or added) colour is deducted from (or added to) both.

8. A complementary contrast colour does not appear in the absence of objective light of that colour.

9. The negative after-images of contrasted colours are complementary to the colours seen.

VII. Colour Adaptation (25)

Colour adaptation is the term applied to the changes that take place when the eye is subjected to light in which certain wave-lengths predominate. Colour adaptation is the means by which colours appear to remain the same when the physical conditions are quite different. Daylight differs chiefly from the Osram electric light in that it contains many more blue rays and less red. A piece of bright blue paper appears a darker blue by an Osram light than by daylight. The fact that it appears blue at all is due to colour adaptation, for if we place the blue paper in a photometer and illuminate it by Osram light it will be matched exactly by a piece of chocolate-brown paper illuminated by daylight. It will be noticed that the theory of perception of relative difference accounts for all the facts. The following are the facts of colour adaptation:

1. In colour adaptation, the retino-cerebral apparatus appears to become less and less sensitive to the colour corresponding to the dominant wave-length, and to set up a new system of differentiation.

2. When light of a composition differing from that of daylight is employed to illuminate objects, an immediate and unconscious estimation of the colours of these objects is made in relation to this light, the light employed being considered as white light.

3. No colour is seen of which the physical basis is not present in the light employed.

4. When spectral regions are examined with a colour-adapted eye, that of the dominant wave-length appears colourless, whilst those immediately on either side of it appear to be shifted higher and lower in the scale respectively.

5. There is immediate colour adaptation, as well as colour adaptation after a longer stimulation with the adapting light.

6. Colours which correspond to the dominant wave-length of

an artificial light are with difficulty discriminated from white by this light.

7. Colour adaptation may bring two colours below the threshold of discrimination so that the two appear exactly alike, although by another kind of light a difference is plainly visible.

8. Colour adaptation increases the perception of relative difference for colours other than the dominant.

9. The conscious judgment has very little effect in colour adaptation.

10. Colour adaptation greatly helps in the correct discrimination of colours, and masks the effects of the very great physical differences which are found in different kinds of illumination.

11. Spectral yellow, after colour adaptation to green, still appears yellow and not red.

12. Colour adaptation appears to produce its effects by subtraction of the dominant colour sensation, and not by directly increasing the complementary. Spectral blue does not appear brighter after colour adaptation to yellow.

VIII. After-images (26, 27, 38, 42)

As in all experiments with colour pure spectral light must be employed. If a monochromatic region be isolated in my spectrometer a negative after-image can be produced by looking at this fixedly with one eye for twenty seconds. If the eyes be kept in a vertical position, that is, one over the other, then, on the eyes resuming their normal position, the after-image can be projected upon the middle of a horizontal spectrum thrown upon a screen. As the eye is kept rigidly fixed during the fatiguing process a very clear-cut negative after-image is produced which, when thrown on the screen spectrum, enables close comparison to be made with adjacent parts. The stability of the after-image is remarkable; it does not change colour, and is not influenced by subsequent light falling on the retina when this is not of too great intensity. The after-image is in every case darker than any dark object on which it is projected.

If the portion of brain having the function of the perception of colour be continually receiving impulses which, affecting it and the visual centre, cause the sensation of light which is seen in the absence of all light stimulation, and the whole retino-cerebral apparatus be fatigued by light of a certain wave-length, a negative after-image will appear through simultaneous contrast. If one portion of the visual area be less sensitive for impulses caused by light of a certain wave-length (for instance, red), and the adjoining areas be stimulated by impulses corresponding to light of all wave-lengths, the image corresponding to the fatigued area will be relatively blue-green to the images corresponding to surrounding areas. This explanation on the theory of colour vision given is in accordance with the other facts of simultaneous contrast.

It is impossible to explain the facts of negative after-images and successive contrast on the Hering and Young-Helmholtz theories of colour vision.

The complementary to the exciting light is never strengthened in the spectrum on the screen by the after-image, as it should be according to the Hering theory. When a negative after-image has been formed in an absolutely dark room it becomes increasingly difficult to produce this after-image on the second, third, fourth, and subsequent attempts. The opposite should be the result on the Hering theory. The stability of the after-image is remarkable, it does not change colour or oscillate, and is not surrounded by the primary colour as it should be according to this theory.

The effect of fatiguing the eye with a monochromatic region produces a uniform grey band across this region. On the Young-Helmholtz theory this should vary in colour and luminosity across its breadth. On this theory the after-image should change colour on fading, because of the varying amount of fatigue of the hypothetical colour sensations. This is not the case. Regions like violet, after fatigue to red, should be very little affected, but they are the most affected. The fatiguing light should chiefly affect the region used for the fatigue. This is not the case. An after-image should not be seen in the absence of all external light.

IX. Peripheral Colour Vision (28)

The erroneous statement is continually made that the periphery of the retina is colour-blind. If red light of sufficient intensity be employed it can be recognised as the same red to the extreme periphery of the field of vision. This is exactly what we should expect on the theory given, the less developed portions of the retina requiring a stronger stimulus than the more developed portions.

Much of the experimental work on colour vision has been done with bits of coloured paper and impure colours, which is similar to conducting a chemical analysis with impure chemicals; many of the results are due to stray light and entirely different results are obtained when pure spectral light is employed.

X. Facts supporting the Theory of the Relative Functions of the Rods and Cones of the Retina (29, 30, 31)

There is not a single fact pointing to the view that the rods are percipient elements. The attribution of this function to them is the purest assumption, and recent writers are now recognising that this is the case. On the other hand on a photo-chemical theory of vision an elaborate nervous mechanism is required to repair and regulate the photograph in various physical conditions (32). This function is performed by the rods, which contain a photo-chemical substance, the visual purple, which is not present in the cones; they liberate the visual purple into the fluid surrounding the cones, and the decomposition of this photo-chemical fluid by light stimulates the ends of the cones, and the visual impulses are started.

From an anatomical point of view it seems impossible that the rods could be percipient elements. The reader should examine some recent reproductions of microscopic specimens of the retina. It will be noticed that the rods terminate in rounded knobs, many of which are in connection with one neuron. It will also be noticed that transverse neurons connect many groups of rods, and this transverse neuron is only indirectly connected with the ganglion cell leading to the fibre of the optic nerve. Now, in order that any percipient element may be able to act as such, the anatomical paths must be different, but how can the rods act as percipient elements when a large group terminate in the same path? It will be noticed that this anatomical arrangement is perfect from the point of view that the rods regulate the distribution of the visual purple into the liquid surrounding the cones.

The visual purple is found in the rods and not in the cones, but if the external surface of the retina, of a monkey which has been kept for forty-eight hours in a dark room, be examined, the visual purple will be found between and not in the cones (33).

The visual acuity corresponds roughly to the distribution of the cones. Though the rods are much more numerous in the periphery of the retina, visual acuity is very much less with this part.

The Relation between the Foveal and the Para-foveal Regions.—As there are no rods in the fovea, if the rods and cones were percipient elements of a different character, there ought to be a qualitative difference between these regions. It has, however, been conclusively proved that there are only gradual quantitative differences in the sight between the foveal and the para-foveal areas. The Purkinje phenomenon, the alteration of optical white equations by the state of dark adaptation, the colourless interval for spectral lights of increasing intensity, the different phases of the after-image, all exist, not only in the para-foveal, but also, only gradually diminished, in the foveal region.

Chemical Analogy.—The visual purple gives a curve which is very similar to that of many other photo-chemical substances. We know that with photo-chemical substances the chemical effect is not proportional to the intensity of the light; a different curve is obtained with weak light from that which is formed with light of greater intensity. It is reasonable, therefore, to suppose that the visual purple which is formed by the pigment cells under the influence of a bright light would be somewhat different in character from that which is formed in darkness. Again, from the chemical analogy which I have just given, even if the visual purple were of the same character we should not expect similar curves with different intensities of light. It is probable that both factors are in operation. This deduction gives an explanation of the Purkinje phenomenon. Not only is the visual purple decomposed and regenerated in daylight, but light is plainly a stimulus for its regeneration.

The Varying Sensibility of the Fovea (34, 35, 36).—At one moment the fovea appears the least sensitive portion, and at the next moment may be the most sensitive portion of the retina. Helmholtz, whilst recording the fact, confessed that he was quite unable to suggest an explanation. The following simple experiment shows this fact:

On opening an eye on awaking in the morning and looking at the ceiling, the central portion is seen as an irregular, circular, rhomboidal, or star-shaped black spot. On closing the eye again a bluish violet circle appears at the periphery or middle of the field of vision, contracts, and then, after breaking up into a star-shaped figure and becoming brighter, disappears to be followed by another contracting circle. If the eye be opened when the star figure has formed in the centre, it will appear as a bright rose-coloured star, much brighter than any other part of the field of vision. If, however, we wait till the star has broken up and disappeared before opening the eye, it will be found that only a black spot is seen in the centre.

This is explained on the theory that when there is visual purple in the fovea this is the most sensitive portion of the retina; when there is none there, it is blind. It also shows conclusively that the fovea is sensitised from the periphery.

Disappearance of Lights falling upon the Fovea.—If we look at two small isolated stars of equal magnitude, either may be made to disappear by looking fixedly at it, whilst the other remains conspicuously visible. The phenomenon is most marked on a dark night, and when the star looked at is in a portion of the sky comparatively free from other stars, and when one eye is used. On a very dark night a considerable number of small stars, occupying the centre of the field of vision, may be made to disappear, whilst stars occupying other areas of the field of vision are plainly visible. This fact shows that when the visual purple in the fovea is used up and not renewed, the latter is blind.

Currents seen in the Field of Vision not due to the Circulation (37).—There are numerous methods by which currents in the field of vision which are not due to the circulation can be seen. The following is one example:

If one eye be partially covered with an opaque disk whilst both eyes are directed forwards in a not too brightly illuminated room, and special attention be paid to the covered eye, an appearance of whirling currents will be seen with this eye. These currents appear to be directed towards the centre and have a very similar appearance to a whirlpool. On closing both eyes all the portion in which the whirling currents are seen appears as dull purple. These currents cannot be due to vessels, because we know that the centre of the retina corresponding to the point where the greatest movement is seen, is free from vessels. The appearance is also very different from that of the movement of blood in vessels. The experiment succeeds best if the eyes have been previously exposed to a fairly bright light. An opaque disk in a spectacle frame suffices admirably, a certain amount of light being allowed to enter the eye from the periphery.

The currents carry the visual quality, colour, and brightness of the region from whence they come into an after-image. They also tend to move an after-image towards the centre, thus if we have two similar after-images, one situated in the centre and the other a short distance from the centre, the one external to the centre may be carried into the centre and combine with the one already there.

These currents are formed by the flow of sensitised liquid.

Movement of Positive After-image (39, 40, 41).—I have shown how the positive after-image may by a jerk with the head be separated from the negative after-image and that multiple after-images can be caused by one light stimulus. This shows that the photo-chemical stimulus is external to the cones and can be moved.

Dark and Light Adaptation.—We have an easy explanation of dark adaptation by assuming that the liquid round the cones becomes more sensitive through a greater percentage of visual purple being poured into it. In light adaptation the anatomical arrangement is such as to prevent as far as possible the decomposition of the visual purple.

The above is only a very small portion of the evidence which might have been given in support of the theory advanced. The reader will find further facts in my papers in the *Journal of Physiology* and elsewhere, and I have stated in the *Journal of Physiology* that this theory should be accepted as the working hypothesis of vision until some fact is found which is inconsistent with it. I have discussed the theory with the chief workers of the world on vision and they have not been able to point out any fact which is opposed to it. Should any reader be able to do so, I should be glad to hear from him.

Conclusion

The reader might like to know my answer to any objections raised to the given theories of vision and colour vision. There have never been any, and it is therefore difficult to know how to deal with opponents who only stab in the back and will not come out in the open. It is, however, a great satisfaction to find opponents who have previously stated diametrically the opposite, now giving my facts and conclusions, even though there be no acknowledgment or even mention of my name. This is the more curious as a steadily increasing number of the ablest scientific men in this and other countries have favoured these views, and text-book after text-book has adopted them. It is obvious that any theory must explain the facts as they really are.

References

1. Edridge-Green (F. W.), Colour Blindness and Colour Perception. International Scientific Series, 1891, 1909. Kegan Paul & Co., and previous papers.
2. —— An Analysis of the Results of the Sight Tests of the Board of Trade, *Brit. Med. Journ.* 1914.
3. —— Demonstration of the Simple Character of the Yellow Sensation, *Journal of Physiology*, 1912.
4. —— The Simple Character of the Yellow Sensation, *Journ. of Physiol.* 1913.
5. Edridge-Green (F. W.) and Marshall (C. D.), Some Observations on so-called artificially produced Temporary Colour-blindness, *Trans. of the Ophthalmological Society*, 1909.
6. Pole (W.), *Trans. Roy. Soc. of Edin.* 1893, p. 459.
7. Edridge-Green (F. W.), The Relation of Light Perception to Colour Perception, *Proc. Royal Society*, 1910.
8. —— Demonstration of a Method of Testing Hue Perception, *Journ. of Physiol.* 1908.
9. —— Colour Perception Spectrometer, *Journ. of Physiol.* 1909.
10. —— The Hunterian Lectures on Colour Vision and Colour Blindness, 1911. Kegan Paul & Co.
11. —— The Discrimination of Colour, *Journ. of Physiol.* 1911.
12. —— The Discrimination of Colour, *Proc. Roy. Soc.* 1911.
13. —— Dichromatisches Sehen, *Archiv für die ges. Physiologie*, 1912.
14. —— Dichromic Vision, *Ophthalmoscope*, 1914.
15. —— Two Cases of Trichromic Vision, *Proc. Roy. Soc.* 1905.
16. —— Trichromic Vision and Anomalous Trichromatism, *Proc. Roy. Soc.* 1913.
17. —— Trichromic or Three-unit Case of Colour-blindness, *Trans. Ophth. Soc.* 1901.
18. —— Evolution of the Colour Sense, *Trans. Ophth. Soc.* 1901.
19. —— Colour Systems, *Trans. Ophth. Soc.* 1906.
20. —— Observations on Hue-perception, *Trans. Ophth. Soc.* 1907.
21. —— Observations with Lord Rayleigh's Colour-mixing Apparatus, *Trans. Ophth. Soc.* 1907.
22. —— Die Wahrnehmung des Lichtes und der Farben, *Berliner Klin. Wochenschr.* 1909.
23. —— Simultaneous Colour Contrast, *Journ. of Physiol.* 1911.
24. —— Simultaneous Colour Contrast, *Proc. Roy. Soc.* 1912.
25. —— Colour Adaptation, *Proc. Roy. Soc.* 1913.

26. PORTER (A. W.) and EDRIDGE-GREEN (F. W.), Negative After-images and Successive Contrast with pure Spectral Colours, *Proc. Roy. Soc.* 1912.
27. —— Negative After-images and Successive Contrast with pure Spectral Colours, *Proc. Roy. Soc.* 1913.
28. EDRIDGE-GREEN (F. W.), Peripheral Colour Vision, *Journ. of Physiol.* 1912.
29 —— Colour-blindness. The Royal Society of Arts, 1910.
30. —— The Theory of Vision. Int. Med. Congress, *Lancet*, 1909.
31. —— The Theory of Vision, *Ophthalmic Review*, 1914.
32. —— The Relation of Photography to Vision, *Photographic Journal*, 1910.
33. —— Some Observations on the Visual Purple of the Retina, *Trans. Ophth. Soc.* 1902.
34. —— Subjective and other Phenomena connected with the Retina, *Journ. of Physiol.* 1911.
35. —— Visual Phenomena connected with the Yellow Spot, *Journ. of Physiol.* 1910.
36. —— New Visual Phenomena, *Journ. of Physiol.* 1912.
37. —— Entoptic Currents seen in the Region corresponding to the Fovea and Yellow Spots, *Journ. of Physiol.* 1910.
38. EDRIDGE-GREEN (F. W.) and PORTER (A. W.), Demonstration of the Negative After-images of Spectral and Compound Colours of known Composition, *Journ. of Physiol.* 1914.
39. EDRIDGE-GREEN (F. W.), The After-images of Black and White on Coloured Surfaces, *Journ. of Physiol.* 1913.
40. —— Certain Phases of the Positive After-image, *Journ. of Physiol.* 1913.
41. —— The Homonymous Induction of Colour, *Journ. of Physiol.* 1914.
42. —— The After-image of White on Coloured Surfaces, *Journ. of Physiol.* 1913.

THE INTERNATIONAL STRUGGLE FOR MANUFACTURES AS ILLUSTRATED BY THE HISTORY OF THE ALUM TRADE[1]

By RHYS JENKINS, M.I. Mech. Engineers

Modern industry has grown into an organisation of a most complex structure. The development has been facilitated by the low cost of sea transport; nations have become interdependent industrially to an increased degree, the accessories required for carrying on the staple industry of one country may be manufactured elsewhere, and similarly the utilisation of the bye-products of an industry in one country may be the subject of an important manufacture in another. This condition of affairs, although it may facilitate production on the large scale and tend to cheapness in the manufactured article, does not necessarily conduce to the development of a particular nationality. We are told that exports must be paid for by imports, but if the exports be raw materials or partly finished goods, and the imports are manufactured articles in a finished condition, it is clear that the nationals of a foreign state have been doing work which should be done at home. We ourselves are mere hewers of wood and drawers of water, while the skilled and highly-paid artisan carries on his occupation in, and helps to build up, the foreign state, and the well-being of our own people stands on a lower plane.

Moreover, in so far as a nation depends upon foreign sources of supply for its manufactures, so far is its economic life at the mercy of circumstances over which it has no, or but an incomplete, control.

It behoves us then to utilise ourselves to the fullest extent the natural productions of our own country and in particular to

[1] Although this paper has a distinct bearing upon some of the problems which have become prominent in consequence of the war, it may be explained that it was undertaken in response to a suggestion made by the Editor in June, and has been written quite irrespectively of the conditions of the moment.

seek to perform the final steps of manufacture, those demanding labour of the highest skill.

Long ago England was the great wool-producing country of the world, and it was the custom to export wool in the unmanufactured state. The merchants of Italy, Flanders, and Brabant competed for it eagerly, much to the satisfaction of the growers. In process of time we began to realise that foreign states were being enriched and were growing in power by the working-up of the raw material thus so freely exported, and efforts were made to set up a clothing industry in our own country. The weaving of woollen fabrics, no doubt, has been carried on here from prehistoric times; what is meant by the term "clothing industry," is the manufacture of merchantable material which would rank with the productions of other nations in the markets of the world. We were making coarse cloths, but the finer materials used by the nobles and rich merchants were imported, although made from our own wool.

After efforts extending over centuries, the art of weaving fine cloth became fixed in England; but even then we had won but half the battle—we had not attained the necessary perfection in dyeing and finishing, and the cloths were exported white to be dyed and dressed in the Low Countries.

This was the state of affairs when in 1553 William Cholmeley, "Londyner," penned *The request and suite of a true harted Englysheman.* Cholmeley laments that either for lack of things thereunto belonging, for lack of studious desire to do things perfectly and well, or else for lack of wits apt to receive the knowledge of such things, "we were not able to adde that perfection to our commodities which nature hath lefte to be finyshed by arte." He proceeds to discover what is the source of the difficulty, and comes to the conclusion that it is "oure beastly blyndnesse which wyll not suffer us to searche for that knowledge which our wyttes are able enough to attayne . . . we beynge beastly mynded, and sekyng to gayne much by doynge lyttle, every man sekeyng his owne pryvate commoditie, without regard to the weale publike." Cholmeley states that every year at least 150,000 broadcloths are exported to the Low Countries, undyed and undressed, that there is there a gain of at least twenty shillings on each piece which might be earned by English people if the work were done within the realm. Our author recognised that this trade was not to be acquired without a struggle—

"I graunt we shoulde for a tyme have a sharpe conflycte with those stoute enymes whome we have with oure commodities and treasure enryched"—but he has no doubt that if we have courage we are bound to win in the end, as indeed we did. But the end was far off.

Industries are necessarily interrelated, and each of our staple manufactures has one or more ancillary industries. We have seen the important place which the process of dyeing takes in the clothing industry; now in dyeing, a mordant, such as alum, is required to fix the colour in the cloth. The manufacture of alum is ancillary to the great clothing industry. In former times all the alum consumed in the country was imported, and if, on account of continental wars, or from other causes, the supply of this comparatively unimportant material failed, the larger industry languished or came to a standstill. The history of the alum trade affords a good illustration of the struggle for an industry in which finally we succeeded in rendering ourselves independent of foreign sources of supply.

Up to about the middle of the fifteenth century the bulk of the alum supply of Europe came from Asia Minor. At about this date, however, attempts were made to produce alum at various places in Italy, and notably at Tolfa in the Papal States. The alum mine of Tolfa was discovered in 1462 by Giovanni de Castro, who had been the manager of dyeworks in Constantinople, and had become acquainted with the Levantine alum and knew the places where it was found. Giovanni, impressed with the value of his discovery, hastened to inform the Pope, Pius II., and to induce him to take up the matter. The Pope was at first sceptical, but, the reality of the discovery having been confirmed, he "determined to employ the gift of God to His glory in the Turkish War and exhorted all Christians henceforth to buy alum only from him, and not from the Unbelievers." The enterprise was at once embarked upon, and it is said that, in 1463, eight thousand persons were employed, and that it brought a yearly income of 100,000 ducats to the Papal Treasury.

Paul II., who succeeded Pius II. in 1464, pushed forward the exploitation of the industry and attempted to set up a Papal monopoly of alum in Christendom; he launched a Bull excommunicating all those merchants who procured alum from the Infidels, the people who bought it from the merchants, and the civil and religious authorities who tolerated within their jurisdictions the traffic of alum from the Levant. The Tolfa alum was found to be superior to that brought from the East, and it was bought readily by the dyers and other users, so much so that the demand increased rapidly. Accordingly the farmers of the undertaking made large profits; the Papal Treasury considered that a share of the enhanced returns was due to them, and extorted a higher rent. The farmers thereupon raised the price, until it became cheaper to use the inferior Levantine product, and then difficulties arose, particularly in Flanders, at that time the most important seat of the woollen industry. According to M. Jules Finot,[1] Charles the Bold in 1467 gave the people of his dominions permission to import alum from any part they chose, but before a menace of direct excommunication he capitulated to the demands of the Holy See, while the king of England on the other hand remained deaf and insensible thereto. By a treaty concluded with the Pope in 1468, Charles engaged not to permit the importation into his dominions of alum other than that from the Pope's mines. This treaty appears to have held good for about twenty years, until the price of the Papal alum was raised to such an extent as to cause a general protest. The Archduke Phillipp the Fair, who was now at the head of the Government in the Low Countries, appointed a committee of the principal citizens, merchants, and drapers of Bruges to investigate the matter. The committee recommended him to disregard the Papal pretensions and to authorise the importation of alum from the Levant. It was known that England had taken no notice of the menaces of the Holy See, and that Italian ships continued to supply her ports with alum from the country of the Infidels. The Flemish merchants took advantage of this to bring over Levant alum from England, and by the year 1505 this importation amounted to a considerable quantity. This meant a corresponding reduction in the income from the Papal mines; the Pope, Julius II., at once took steps to arrest this, and in 1506 a Bull was issued pronouncing excommunication against those merchants who went to procure alum from the dominions of the Grand Turk, and against those who bought it from the merchants, resold it, or were otherwise concerned in the traffic. Public opinion in Flanders was greatly

[1] *Bulletin Historique et Philologique*, 1902: *Le Commerce de l'alun dans les Pays-Bas et la bulle encyclique du Pape Jules II. en* 1506.

excited by this action, and it became a question up to what point the Pope had the right to make use of censures and excommunications in order to ensure his personal temporal interests. The Archduchess Margaret requested her Council to consider this question, and whether, according to precedent, it was necessary to recognise the legality of these censures, or, on the contrary, to refuse to act upon them as infringing the liberty and the security of commerce in the provinces under her government. The Council refrained from expressing their own views; they gave the opinion that an affair of this importance, during the minority of the Archduke Charles, King of Castile, should be referred to his grandfather, the Emperor Maximilian. Finally, as a result of arbitration, it was agreed that the Papal monopoly in Flanders should continue for the space of two years, but that the price was not to exceed a specified maximum.

The present writer has found no indication that the Bulls referred to were promulgated in England, so that it would appear doubtful whether the independent spirit credited to the English kings by M. Finot is really merited. It is to be borne in mind that the consumption here in the fifteenth century was quite small in comparison to that of Flanders, and that it was relatively of little importance what we did. There is evidence however that Henry VII., in a dispute brought before him in 1486, took no cognisance of the Papal claim to monopoly. Apparently the manufacture of alum was being carried on at Piombino in opposition to the Papal injunctions. A ship laden with alum of this manufacture was captured and brought into an English port by English mariners in the employ of the Pope's agent in London. A Florentine merchant possessing some interest in the cargo petitioned the king for redress. The Pope's agent urged that the Florentine was excommunicated and ought not to be heard, and that the alum was forfeited to the Apostolic Treasury; but the king, remarking that there was no prohibition against bringing alum into England, held that the capture of the ship without his consent was an act which could not be tolerated and ought not to remain unpunished.

But whether there was a free importation of alum from all parts or not, the price in England rose considerably. In 1504 it was 53*s.* 4*d.* the hundred, instead of 6*s.* which had been the standard price for many years.

Between 1514 and 1519 there was an interchange of letters between the Pope and Henry VIII. in reference to a cargo of alum consigned by the Apostolic Chamber to a merchant in London, but during the same period we find a statement of money owing to the king for Turkish alum.

Later on, in 1545, Henry took over a large quantity of alum from the farmers of the Pope's works, paying for it in lead, of which he had a large quantity on his hands, possibly a result of the spoliation of the monasteries. The transaction was thought to be a good one, as the king's subjects must have alum to dye with, and the king might set his own price, whereas it was difficult to get ready money for the lead.

The reign of Elizabeth is remarkable in many respects, perhaps not least so in regard to the definite line of policy pursued to encourage the planting of new industries. Among the industries sought to be introduced was the manufacture of alum.

In 1562 a patent was granted to William Kendall, of Launceston, Cornwall, gentleman, conferring upon him for twenty years the sole right of making alum within the counties of Cornwall, Devon, Somerset, Dorset, Hampshire, Sussex, and Surrey, he having by "his great travaile and charges founde out in sundrye partes of this our Realme certaine alume ewer in greate habundance and plentye, and by longe studye and practise devised the waye and feate to make thereof good and perfect alume to the greate commodite of us and this our Realme of England, as it is conceaved and hoped."

Another patent covering the same industry was issued in 1564 to Cornelius de Vos, "marchaunte and our liege made subject," he having found "sondrye mynes and owres of allome coperas and other mineralles within certayne partes of this Realme of England and dominions of the same and specially within our Isle of Wighte in the County of Southampton, which he entendeth at his own proper costes and charges to work and trye out to the benefitt of us our Realme and subjectes." The grant extends to the whole Kingdom, and covers copperas as well as alum.

De Vos himself did very little to develop the industry, but he very soon assigned his patent to James Blount, Lord Mountjoy, who entered into the matter with energy.

Lord Mountjoy was the owner of Canford Manor in Dorsetshire, and in the year 1566 he obtained an Act of Parliament confirming to him the patent granted to Cornelius de Vos. The Act recites that the Queen "of her moost gracious disposcion to the benefitt and profitt of thys hyr Realme of Englande, amonge sundrye other the singuler frutes of hir Majesties goodnes towardes the same, hath bin desirous that the hidden riches of the earthe should by serche and woorke of men skilfull, be founde and browght to the use and comoditye of hir sayd Realme; and to that end being informed of some hope of allome and coperas to be founde and made within her highnes dominyons, although the same hath bin ofte attempted in tymes of her moost noble progenytors, and yet never heretofore browght to effecte"—had granted a patent to Cornelius de Vos. Vos, however, not being a man of sufficient wealth and ability to carry on the undertaking, had transferred his patent to Lord Mountjoy, within whose grounds great quantities of suitable ore had been discovered, and Lord Mountjoy had caused to be made good and perfect copperas at more reasonable rates than the imported copperas, and he was "in good hope to have lyke successe in woorking of allome, which is a verie necessarye commoditie for the use of draperie."

It will be noticed that it is not alleged that alum had been made at the date of the Act, but in a letter written in the same year Mountjoy undertakes to deliver 150 tons of alum at the end of two years. He did not succeed in doing this, and there is evidence that the alum industry had not emerged from the experimental stage in 1572. From an inquisition made in 1577 it appears that there were works at Ockeman's House, at Boscombe, at Alum Chine, and on Brownsea Island. Possibly two of these were for the production of alum and the others for that of copperas.

At the death of Lord Mountjoy in 1581 the Canford Manor estate and the alum and copperas works were acquired by the Earl of Huntingdon. There was a considerable amount of litigation over the transfer, and it becomes clear in the course of the proceedings that the works were considered to be of great value. It would seem that the copperas monopoly was the main consideration.

The manufacture of copperas was a comparatively simple process, and there is no doubt that this was placed on a profit-making footing. In the matter of alum, however, although it seems clear that Lord Mountjoy did succeed in producing some quantity, we must come to the conclusion that the business was not a financial success, and that the amount produced was too small to affect the market in any way. Against this view is to be set the complaint of the Bristol merchants in 1571 of the decay of their trade, owing in part, they allege, to the circumstance that alum, which had hitherto come from abroad, was now made better and cheaper in this country; but the true cause of the decay, so far as it concerned the alum trade, was in all probability that the importers found London and Southampton more convenient ports for their purpose. There is no doubt that throughout the reign of Elizabeth alum was regularly imported from abroad, and principally from the Papal States. In the Armada times the Spaniards seized, in the Straits of Gibraltar, a ship laden with alum on its way from Civita Vecchia to England. The Pope made most energetic protests against the seizure, but apparently without result. A few years later, in 1596, a similar capture was followed by protests from the Pope. In this case the King of Spain finally agreed to restore the value of the alum, but he could not be persuaded to give up the English ships in which it was laden.

A paper of the year 1595, setting forth an estimate of the yearly consumption, indicates no other source of supply than the Papal States. The quantity mentioned in this estimate is 8,000 to 10,000 quintals; the consumption fifty years earlier was but 4,000 quintals.

The alum and copperas works were carried on during the lifetime of the Earl of Huntingdon by his lessees. Phillip Smyth, the lessee in 1591, soon after the expiry of the monopoly granted to Lord Mountjoy in 1566, petitioned for a renewal. He had leased the works for twenty-one years at a great rent, and he alleged that "he has now through his exceeding great travell and industry brought the works to such perfection, as they are not only able to furnish this realme but other countries with those commodities." Other people, however, are now about to set up works, and he will be ruined unless he is protected by a patent. This attempt to renew the monopoly, equally with another made in 1593 by the Earl on his own behalf, met with no success. At the death of the Earl in 1595 the works were discontinued. This marks the end of an abortive attempt extending over thirty years to plant the alum industry in this country.

It appears that in 1610 the grand-nephew of the Earl of Huntingdon contemplated reopening the works on the Canford estate, but by this time the centre of interest of the alum trade had passed to Yorkshire.

The discovery of the alum shales in the Upper Lias in the North Riding of Yorkshire was due to Sir Thomas Chaloner, who, in association with Lord Sheffield, Sir David Foulis, and John Bourchier, in 1607 obtained the grant of a patent for the monopoly of the manufacture of alum in England for thirty-one years. They had made out a case showing "how necessary alum was for cloth and leather, how much the Pope would suffer, that £40,000 would be saved in money and commodities annually, that many hundreds would be set on work, clothed, fed, and receive religious and other instruction, and that ships and mariners would be maintained to carry coal, urine, alum, etc."

The patentees proceeded to set up works and open the mines; their projects were on an ambitious scale, and formed the subject of much discussion. James I. became interested, and arrived at the conclusion that large profits were to be made, and that the enterprise was a fit subject for a royal monopoly. Accordingly, in 1609 an arrangement was made whereby it was transferred to the Crown, and the importation of alum from abroad was forbidden by proclamation. James, from first to last, expended large sums upon the works—according to one account as much as £120,000—and the alum monopoly was one of the few explicitly reserved from the operation of the Statute of Monopolies.

In spite of all this, many years passed before the industry was established on a satisfactory basis. Difficulty was experienced on the technical side, which was surmounted only by the introduction of trained workmen from abroad.

Then again James was, to say the least, singularly unfortunate in his choice of agents and farmers for the undertaking. He seems to have failed entirely to secure a combination of ability and honesty in the same man.

Perhaps, however, the most serious obstacle in the path of the enterprise at the outset was the exaggerated ideas which obtained as to the scale upon which it was to be carried out and the profits to be derived from it. In 1595 the estimated annual consumption of alum in England was about 500 tons; in 1609 a production of 2,000 tons per annum was contemplated, and the Crown proceeded to load the undertaking with fixed charges based upon this output. It was farmed at an annual charge of £22,000, made up of annuities to the patentees and rent to the Crown. The selling price of alum was fixed at £23 a ton, and the cost of manufacture was £10 a ton. On a sale of the full quantity—2,000 tons—this left the farmers with £4,000 a year profit, which must have appeared a very nice return. As a matter of fact, they succeeded in producing about 500 tons only per year, and some of this was exported and sold at as low a price as £15. Clearly they could not meet their engagements. They failed in 1612, and the works reverted to the hands of the king.

Notwithstanding this want of success on the part of the first farmers, there was a feeling of confidence in the ultimate results of the enterprise. A report on the state of the works in 1615 concludes thus: "So it appeareth that the works are worthy to be privileged by a King: that set many thousand poor men on work; spend nothing but base materials, coal, urine, and earth out of rocks and barren grounds; employ many ships; impoverish the Pope's coffers, and will advance his Majesty's revenue many thousands a year."

The king had already spent £50,000; he now agreed to spend £10,000 more for the repair and provisioning of the existing works and for extensions, and the works were leased for twenty-one years to another company, who contracted to deliver to the king 1,800 tons per annum at £10 a ton. Again the actual production fell very far short of the estimate, amounting only to between 500 and 600 tons.

Passing over the vicissitudes of the next few years, in 1627 we find Sir Paul Pindar in charge of the undertaking, and with marked results. The production in 1632 was 900 tons per annum; it advanced by rapid steps, and by 1637 had attained the figure of the estimate of 1609, 2,000 tons. Pindar paid the king a rental of £12,500 a year; in addition he paid rentals amounting to £2,240 to the Earl of Mulgrave and Sir William Penniman. There is no doubt that the enterprise had become a success. Another indication of this is to be found in the fact that in 1638 attempts were being made to induce some of the workmen to go over to Denmark to set up the industry there.

During the interregnum the monopoly came to an end; the patent under which Pindar worked was declared by both Houses of Parliament to be illegal and void, and the works reverted to the landowners.

At the Restoration an attempt was made to revive the royal monopoly by parliamentary enactment, but the Bill introduced into the House of Commons for this end was dropped, and Charles II. proceeded to effect the same object by acquiring the various works from their owners for a term of years, paying them by annuities. In 1665 he was in position to demise the farm of alum to a company at a rent of £4,260 per annum for the first four years, and £5,260 for the remainder of the term, the company also undertaking to pay the annuities, which amounted to £3,500.

This arrangement remained in force for fourteen years, when the surviving members of the company surrendered their lease, and the king restored the works to the various owners. This appears to mark the termination of the alum monopoly. The freeing of the trade was followed by the erection of a number of new works along the Yorkshire coast. The trade reached its culminating point about a hundred years later, when the annual production was between 5,000 and 6,000 tons, produced at sixteen works. After this, in consequence of the industry having been taken up in other localities more favourably placed in regard to fuel and to the introduction of new methods of manufacture, the Yorkshire industry gradually declined and became extinct.

The industries of England have been established only after struggle and sustained effort. The cases which have been considered are to be regarded merely as illustrations. Similar conditions attended the introduction of such diverse industries as the manufacture of fine writing-paper and of tin plates. In the one case, time after time skilled workmen were brought over from the Continent to initiate the manufacture, which in the course of a few years degenerated into the production of inferior qualities, such as packing paper, or failed altogether; in the other, after repeated attempts, success was finally obtained as the result of the invention of the rolling mill, which enabled the plates to be produced of uniform surface mechanically, thereby dispensing with the special manipulative skill required for production by hammering.

In so far as the difficulties arose from the absence of scientific and technical knowledge, or of manipulative skill, the conditions to-day are all in our favour, but the introduction of a new industry is still a matter involving sustained effort, of difficulty and risk to the person embarking upon the enterprise.

Britain is essentially a manufacturing nation, and it is as such that it has taken its high position in the world. This position is to be maintained only by constant watchfulness, on the one hand, to retain existing manufactures, and on the other, as in the evolution of industry, new processes and new products come forward to displace the existing manufactures, to embark upon these new processes and the manufacture of the new products. If, owing to our "beastly blyndnesse," as William Cholmeley put it in 1553, lack of initiative, or dread of risk, we allow the new industry to become firmly planted in foreign soil, when at last we find it ousting the productions of our own older manufacture we shall find the difficulty of acquiring it vastly increased.

The English system of patents for invention dates from the reign of Elizabeth, but originally it did not contemplate rewarding the mere ingenuity of an inventor. It was devised to encourage and protect the man who actually did something for the benefit of the commonwealth by setting on foot new manufactures which found employment for our people—if by utilising our own natural products, so much the better; something which conduced directly to the independence and power of the nation. This something might result from a scheme or device originating in the brain of the patentee, but that was not material. In process of time the position has changed entirely, and, as is well known, a patent is now granted in respect of the novelty of an invention.

In some quarters it is thought that it would be well to revert, to some extent, to the Elizabethan policy. It does seem a matter worthy of careful consideration whether, apart from the question of the protection of new inventions, some measures could not be devised to encourage, and protect in the initial stages of his enterprise, the man who expends his brain, risks his capital, struggles with, and overcomes the many practical difficulties necessarily encountered and brings a new industry into being.

ANCIENT AND MODERN DENTISTRY

By C. EDWARD WALLIS, M.R.C.S., L.R.C.P., L.D.S.

Though there is evidence in abundance that prehistoric man suffered from dental disease, the complaint from which he suffered most appears to have been the now fashionable "pyorrhœa" and not dental decay, or "caries," as it is technically known.

This disease of "pyorrhœa" is characterised by a chronic inflammation of the gums and bone (alveolus) around one or more teeth, which sooner or later leads to their loosening and ultimate loss.

When on account of this disease a tooth became so loose as to be a nuisance, its removal or extraction must have appeared to primæval man an obvious means of cure, pyorrhœa, in all probability, thus leading the way to one of the earliest surgical operations—namely, the extraction of a loose tooth.

From that early beginning when the finger and thumb were made to serve as convenient extracting appliances, an infinite variety of instruments has been devised for the purpose of grasping or digging out such teeth as ache or are otherwise an annoyance to their possessors.

On an ancient vase (fig. 1) we see the portrayal of such an operation very clearly shown, and in fig. 2 we see a drawing of ancient Greek dental appliances now preserved in the Archæological Museum at Athens.

The high state of culture of the ancient Greeks, together with their knowledge of the healing art, must soon have directed attention to diseases of the teeth, and Cicero tells us that "extraction" was first recommended about the year 1300 B.C. by a famous physician named Asklepios, a descendant of the still more famous "Æsculapius," familiar to many by the picture in the Tate Gallery described as "A Visit to Æsculapius."

About 600 B.C. Solon, who was one of the seven wise men of ancient Greece, noticed that the first set of teeth commenced to be replaced by a second set at about the age of seven years.

FIG. 1.—Ancient Vase showing a Primitive Tooth Extraction.

From *Dental Art in Ancient Times*. By permission of Burroughs Wellcome & Co.

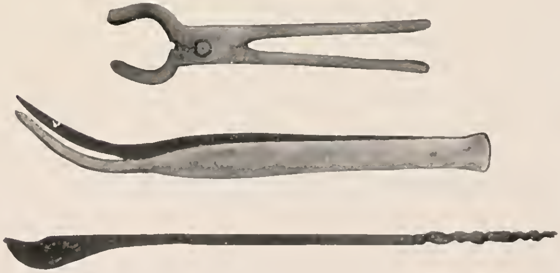


FIG. 2

From *Dental Art in Ancient Times*. By permission of Burroughs Wellcome & Co.

As the days of Hippocrates, whom we now regard as the father of medicine, drew near, we find much writing on the subject of the teeth. Thus about 400 B.C. Hippocrates recommended that black and unhealthy gums should be treated with a mixture of dill, aniseed, and myrrh dissolved in pure white wine; we have in this mouthwash the progenitor of the familiar tincture of myrrh and borax to be seen in every chemist's shop. Hippocrates advised that the mouth should be rinsed with this lotion after each meal and also before food.

The use of chalk, which is even to-day the main ingredient of most tooth-powders, was known to Hippocrates, who recommended that it should be used mixed with the "head of a hare" and "the intestines of a mouse"! What influence the last two ingredients could have been supposed to produce is hard to imagine!

Hippocrates evidently also understood the principle of what is now called "counter-irritation," for in certain cases of toothache he recommended the use of a mouthwash containing pepper and castoreum, an acrid substance prepared from some glands of the beaver. Modern dentists frequently apply to the gums near tender teeth small plasters containing capsicum, a sort of pepper which is very efficacious in allaying certain forms of inflammation.

In the case of a man named Melisandrus, who was suffering from pain and swelling of the gums, Hippocrates recommended the use of alum as a mouthwash and also that the patient should be bled in the arm.

Many people have thought that the making of dental crowns and "bridges" was introduced to mankind by our American cousins, but in various museums of ancient Greece and Rome are to be seen excellent examples of gold bridges and artificial teeth such as were probably used by the plutocracy, if not by the aristocracy, of those early days.

Aristotle, who lived about 350 B.C., wrote much on the subject of the teeth, and even discussed the relative advantages of extracting the teeth with forceps as compared with the unaided finger and thumb. Galen, another famous Greek physician, who lived about 100 B.C., was the first man to give the teeth their present names of "incisors," "canines," and "grinders," or "molars." Galen was also the first man to describe what is popularly called the "nerve" of a tooth and to recognise its sensitiveness. To relieve dental pain he recommended that a hole should be bored in the affected tooth, and that by means of this orifice appropriate remedies should be introduced.

The ancient Etruscans appear to have been highly skilled in dentistry, and many specimens have been found in tombs in Etruria of gold dental bridges and similar appliances employed to replace lost teeth. In the villa of Pope Julius III. in Rome, now used as an Etruscan Museum, is to be seen an excellent specimen of a gold dental "bridge," in actual position, in a skull which was found in an Etruscan tomb at Civita Castellana, a town in Etruria formerly known as Falerii. (The Etruscans are believed to have been partly of ancient Greek origin, at any rate their civilisation was antecedent to that of Rome.)

Coming now to the days of ancient Rome, we know from writings dating from the first century B.C. that toothache was very rife, and we read of many strange remedies. For example, Cornelius Celsus, a medical writer in the reign of Tiberius, recommends the following treatment as a means to obtain relief from toothache: "The patient should abstain entirely from wine and at first even from food, afterwards he may partake of soft food, but very sparingly lest he irritate his gums by chewing." Meanwhile he must, by means of a sponge, steam the affected part and apply, externally, a medicated pad of wool.

Celsus describes dental operations in detail, and gives a list of the instruments used by dentists.

(1) Ordinary tweezers, from which, in the course of centuries, our modern dental forceps have been evolved.

(2) The rhizagra, an instrument for the extraction of roots.

(3) The vulsellum, for removing debris of bone or tooth.

(4) The specillum, which was a sort of probe.

(5) The cautery, which was simply a metal rod made hot and applied where necessary.

Pliny the elder, the famous Roman writer on natural history, mentions many remedies for toothache. One of these, consisting of a mixture of the burnt excreta of mice and the dried liver of a lizard, was to be inserted in a hollow tooth. Pliny was evidently of a somewhat heartless nature, for he mentions casually the case of a man who, after having had

FIG. 3.

From *Dental Art in Ancient Times.* By permission of Burroughs Wellcome & Co.

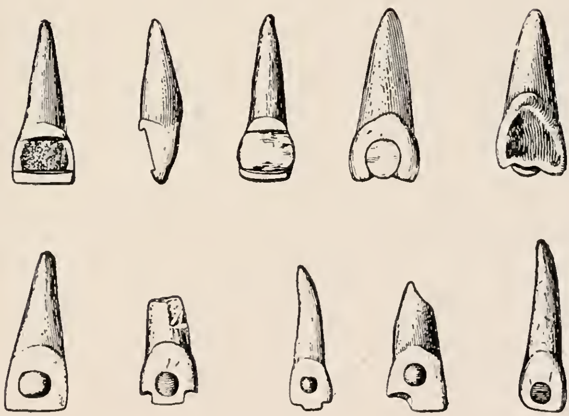


FIG. 4.

From *Dental Art in Ancient Times.* By permission of Burroughs Wellcome & Co.

a tooth stopped with wax and asafœtida, promptly committed suicide!

Many references to artificial teeth are made by the poet Martial in his epigrams written about A.D. 80. In referring to an old lady, he says, "She lays down her teeth at night just as she lay down her silken robes," and again referring to another he says, "She affects reality by wearing false teeth made of bone and Indian ivory." Another Roman writer, Scribonius Longus, tells us that the tooth-paste of Messalina was composed of calcined stag's horn, mastic, and sal-ammoniac. It will be remembered that Messalina was the notorious wife of the Emperor Claudius.

Toothpicks were in use in these days and were known as "dentiscalpia." They were usually made of lentisk wood, though toothpicks of quill were used, and some made of gold and silver have been found at various times. Pliny states that toothpicks made of vulture quills turned the breath sour, while porcupine quills made the teeth firm.

As to when the stopping of teeth was first introduced there is very conflicting evidence. One widespread statement is now known to be untrue—namely, that the ancient Egyptians used gold for stopping teeth.

Recently some most remarkable discoveries have been made in Mexico, Ecuador, and thereabouts of teeth from pre-Columbian skulls inlaid with gold and precious stones (fig. 4). A considerable collection of these is to be seen in the Historical Medical Museum in Wigmore Street, London, founded by Mr. H. S. Wellcome.

Modern Dentistry

The father of modern dentistry was a Frenchman named Pierre Fauchard, who practised about the year 1700. He was the author of the first great text-book on the subject, and very largely responsible for the introduction of artificial teeth as we have them to-day.

Fauchard conceived the plan of making artificial teeth out of thin metal plate enamelled to represent front teeth. These were kept in position by gold wires tied round the side teeth or by steel springs such as are shown in the drawings on the next page. The back teeth were merely represented by blocks of ivory. Later

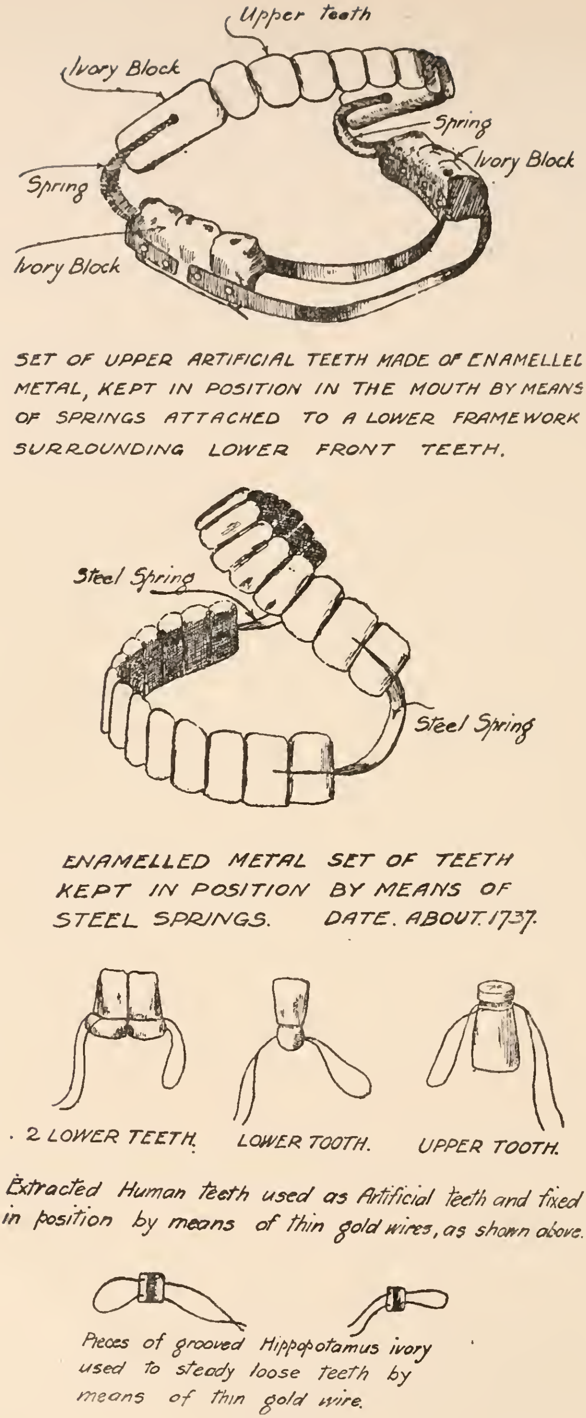

Upper teeth
Ivory Block
Spring
Ivory Block
Spring
Ivory Block
SET OF UPPER ARTIFICIAL TEETH MADE OF ENAMELLED METAL, KEPT IN POSITION IN THE MOUTH BY MEANS OF SPRINGS ATTACHED TO A LOWER FRAMEWORK SURROUNDING LOWER FRONT TEETH.
Steel Spring
Steel Spring
ENAMELLED METAL SET OF TEETH KEPT IN POSITION BY MEANS OF STEEL SPRINGS. DATE. ABOUT 1737.
2 LOWER TEETH.
LOWER TOOTH.
UPPER TOOTH.
Extracted Human teeth used as Artificial teeth and fixed in position by means of thin gold wires, as shown above.
Pieces of grooved Hippopotamus ivory used to steady loose teeth by means of thin gold wire.

Fig. 5.

on sets of artificial teeth were made of ivory or bone, the plate as well as the teeth being carved out of one block of ivory. The fossil teeth of the mastodon found in the north of Europe towards the end of the eighteenth century were very plentiful, and were regularly sold in blocks of suitable size for the making of sets of artificial teeth. Walrus teeth were also used for the same purpose.

Apart from their unattractive appearance, these ivory sets were most unhygienic, and very soon became foul and unwearable.

George Washington wore a set of teeth of this kind, which can now be seen in the Dental Museum of the London Hospital, together with a letter in his own hand written to his dentist, a Mr. Greenwood, of Philadelphia, in 1795, which reads as follows:

PHILADELPHIA,
20th Feb., 1795.

SIR,

Your last letter with accompaniment, came safe to my hands on tuesday last.

Enclosed you will receive sixty dollars in Bank notes of the United States. In addition to which I pray you to accept my thanks for the ready attention which you have at all times paid to my requests: and that you will believe me to be, with esteem-Sir

Your very H^ble^ Servant,
G° WASHINGTON.

Mr. Jn° Greenwood.

Later on, although the tooth plates continued to be made of ivory, bone, or metal, the artificial teeth themselves were extracted human teeth carefully preserved by dentists for the purpose; every dentist kept a jar of old teeth from which to make a selection, and when a dental practice was for sale, an estimate of its value depended upon the stock of old teeth in the possession of the vendor!

An enormous number of apparently healthy teeth were extracted in those days for a disease called at that time "scurvy of the gums," which was probably simply the condition we now describe as "pyorrhœa"; these teeth were fixed on the ivory and metal tooth plates and used as artificial teeth.

The invention of porcelain artificial teeth by Dubois de Chemant, a French dentist, marked an epoch in modern dentistry; he received a patent from Louis XVI. for his so-called "mineral paste" for making porcelain teeth in 1790, and in a book which he wrote declared that he had provided his artificial teeth to no less than 12,000 persons.

The introduction of porcelain teeth did not become general until 1837, when their production was undertaken by a London manufacturer named Claudius Ash.

The Introduction of Nitrous Oxide ("Laughing Gas")

The year 1844 marks another stage in the history of dentistry, for it was then that "laughing gas" was first used as a general anæsthetic for a dental operation, though its anæsthetic properties had been surmised by Sir Humphry Davy many years before.

The story of its introduction for this purpose is of considerable interest. At a popular science lecture given at Hartford, Conn., on the subject of nitrous oxide gas, a man named Cooley was among those invited to inhale the gas in order to show its effects; taking some deep inspirations, he is said to have become unconscious, falling to the ground and severely injuring himself.

A certain Dr. Horace Wells in the audience was much struck by the peculiarity of the incident, and after the lecture arranged with Dr. Colton, the lecturer, to come to his surgery and administer the gas to him while he had a molar tooth extracted by a Dr. Riggs. The following is a contemporary verbatim report by the operator, who describes the operation in the following words:

"A few minutes after I went in Dr. Wells took a seat in the operating-chair. I examined the tooth to be extracted, with a glass, as I usually do. Wells took the bag of gas from Mr. Colton and sat with it in his lap, and I stood by his side. He then breathed the gas until he was much affected by it; his head dropped back, I put my hand on his chin, he opened his mouth, and I extracted the tooth. His mouth still remained open some time. I held up the tooth with the instrument that the others might see it; they, standing partially behind the screen, were looking on. Dr. Wells soon recovered from the influence of the gas so as to know what he was about, discharged the blood from his mouth, and said: 'A new era in tooth pulling!' He said it did not hurt him at all. We were all elevated and conversed about it for an hour later."

After this Wells extracted several teeth from patients, using the gas as an anæsthetic, with uniform success. He then went to Boston and tried to demonstrate his discovery to the medical profession. After several days he was finally invited by Dr. Warren to give a talk on anæsthesia to a class at the Massachusetts General Hospital. This he did. After this he was asked to extract a tooth under the gas.

Wells says the experiment was not a success for the reason that the gas bag was taken from the patient too soon. Pain was felt by the patient, Wells was denounced as a humbug, and hissed from the room.

He finally wandered to New York, and, after roaming the streets, he was arrested on January 4th, 1848, charged with throwing vitriol. While in jail he opened his radial artery after inhaling ether to make death painless.

And so Horace Wells, to whom the world owes a profound debt of gratitude, ended his own life at the early age of thirty-two, a broken-hearted and disappointed man.

Nitrous oxide does not appear to have been introduced to England till somewhere about 1864, and it did not come into general use till many years afterwards.

A recent application of this gas is to administer it with varying percentages of oxygen in order to produce the condition of "analgesia" without "anæsthesia," that is to say, a freedom from pain when undergoing an operation as distinguished from an actual loss of consciousness.

In the early days of the administration of nitrous oxide, it was stored ready for immediate use in miniature gasometers; later, however, the celebrated Dr. Evans of Paris, who will be remembered as the saviour of the Empress Eugénie during the Commune, conceived the plan of storing the compressed gas in metal bottles, and in the Historical Medical Museum already referred to may be seen the two original copper gas bottles which he designed for the purpose.

The year 1859 marked a further advance in dentistry, for it was then that the use of "vulcanite" was introduced for the making of dental plates, thereby putting artificial teeth within the reach of people of moderate means.

Soon after its introduction various scares took place on the assumption that the vermilion used for producing red vulcanite contained mercury and would therefore lead to mercurial poisoning. There is little doubt that the early form of vulcanite did cause considerable irritation to the mucous membrane of the palate, but modern knowledge has caused these difficulties to disappear.

Later on pink celluloid was introduced as an apparently ideal substance for artificial plates owing to its mucous-membrane-like colour; the early enthusiasm of its introducers, however, soon abated when it was found that its lasting properties were only to be measured by months owing to a constant tendency to change of shape as well as certain other great disadvantages which rendered it useless as a substitute for vulcanite.

The so-called "crown and bridge-work," which we have shown existed as far back as the ancient Etruscans, was revived by our American cousins in the "sixties," and up to recently may be said to have carried all before it; marvellous mechanical ingenuity was displayed, and from the jewellery point of view, and apparently from the functional point of view, complete dental restoration was obtained.

A physician attached to one of the great London hospitals, however, about the year 1901, sounded a note of warning which seems likely to be a death knell to the famous so-called "bridge-work." The point he sought to impress upon the medical profession was the serious results that follow upon an unhealthy condition of the mouth and teeth.

Special attention was drawn to the foul condition of "gold bridges" as they existed in many mouths, the worst feature being that the greater the mechanical ingenuity displayed, the more unhealthy were the mouths concerned. The uncleanable interstices of the "bridge" were hot-beds for the growth of micro-organisms, and nowadays an increasing number of surgeons, physicians, and dentists regard these complicated pieces of apparatus as mere microbial breeding-places, and as such, the sooner removed the better. It is becoming a commonplace for physicians practising at spas and health-resorts to order their removal before commencing a course of anti-rheumatic or gout treatment, with the remarkable result that many of the chronic sufferers straightway recover their former health.

The introduction of cocaine as a local anæsthetic for operations on the eye in 1878 led to its use for a similar purpose in dentistry in the year 1886; its record for dental purposes has been by no means unchequered, with the result that a variety of chemical substances have been introduced, of which the best known is probably "novocaine," which answers the same purpose, and is less dangerous when injected into the gums and often enables teeth to be extracted quite painlessly.

Dentistry may be said, without fear of contradiction, to have made as much and possibly more progress than any other branch of surgery, though, as is not unusual, we find many of our modern improvements were by no means unthought of in the early days of Greece and Rome.

NOTES

Educational Science

Two of the best papers read during the Australian Meeting of the British Association were by Prof. John Perry and Prof. H. E. Armstrong upon the subject of science and education (*Nature*, October 1 and October 22, 1914). Both speakers deplored the low place now taken by science in education, and indeed in the State generally. Prof. John Perry says that "the classics ride us like Sinbad's old man of the sea. All over the British Empire a well-educated man cannot become a professional man of almost any kind unless he pretends to know something of the one or more dead languages, such knowledge being of no essential value to him. It is something like what the old Test Act imposed upon us; for a hundred and thirty years a British citizen perfectly competent to fill the highest posts could not take upon himself the smallest kind of public work unless he could swear to a certain formula." "The worst of it is that the average boy who has done almost nothing else than Latin and Greek at school gets absolutely no love for the classics; he never reads Greek or Latin after he leaves school." "One of the curses of intellectual England is due to schoolmasters keeping men at school and treating them as boys until the age of twenty-one. They take scholarships as stall-fed cattle take prizes at agricultural shows." "Genius is very common in both countries, but 99 per cent. of it is destroyed by the schools." "Any ordinary citizen thinks himself fit to be a member of the governing body of a school or college, and the disasters due to this belief are worse than what would occur if we gave to such men the command of ships. The ordinary man, especially the parliamentary man, who thinks that the members of a committee on some scientific business ought all to be non-scientific men will jeer at this statement, but it is, nevertheless, fatally true." Prof. Armstrong is equally vigorous. "Our schools," he says, "are for the most part in literary hands; and it would almost appear that literary and scientific interests are antagonistic, so unsympathetic has been the reception accorded to science by the schools." "By placing classical scholars in charge [of schools], we seem unconsciously to have selected men of one particular type of man for school service—men of the literary type; and this type has been preferred for nearly all school posts, mainly because no other type has been available, this being the chief product of our universities. Such men, for the most part, have been indifferent to subjects and methods other than literary—I verily believe not because they have been positively antagonistic or lacking in sympathy, but rather because of their negative antagonism." "The literary type of man apparently does not and cannot sympathise with the practical side of modern scientific inquiry, because he has neither knowledge of the methods of experimental science nor the faintest desire for such knowledge." Referring to "our chief English scientific Society," he says that "most unfortunately, the Society has no influence whatever either on political or on public opinion; it makes no attempt either to guide the public or to give dignity and importance to the cause of science in the eyes of the community. Its meetings are dull, and its belated publications by no means represent the scientific activity of its Fellows." "To improve our system we need to get rid of our blind British belief in 'men of affairs,' especially in the 'man of business,' so-called, really the man of commerce, as persons capable of ordering everybody's affairs and everybody's business." "Science must be organised, in fact, as other professions are organised, if it is to be an effective agent in our civilisation."

There are really two points at issue in these papers, first the general British disinclination for scientific work and thought, and secondly the rejection of science in education. Every one deplores the former defect, but we do not see clearly how it really depends upon the latter. There has grown up an entirely unreal system of education—as unreal as the square root of minus one, but as much loved by some people as is that mysterious entity or nonentity by others. Just as the mathematicians make books out of their imaginary quantities, so do schoolmasters try to make men out of theirs—and in both cases the results are apt to be more curious than useful. We would be the last to object to a true classical education; but then that education must always be combined with a scientific one. Unfortunately what the boy really receives is not a classical education at all, but a grammatical one—quite another thing. Nothing is more educative than a knowledge of the masterpieces of literature in all languages; but our youths do not receive any such instruction. It is doubtful whether many of them have ever even read through the Iliad so as to understand the wonderful construction and the wisdom of the great fable which it develops for the purpose of adding wisdom to mankind. This is not taught, but the boy is kept writhing on the gridiron of grammatical difficulties. Even with regard to English literature, the masterpieces are not read to the boys in an intelligent manner, but are merely used for philological texts. The result is that few of our young people are even acquainted with the masterpieces of literature, and are certainly ignorant of their beauty and incapable of appreciating it. Thus when they grow up their minds are content with the most trifling fiction and the most puerile drama. Similarly, even in the teaching of mathematics, our boys are kept trifling in the porch—worrying over permutations and combinations, or burrowing into the depths of conic sections, when they should be taken to the top of a mountain and be given a wide survey of the whole field. Thus they too are instructed only in the groundwork, and remain for all their lives ignorant of the main meaning and scope of what was intended to have been taught to them. When we add to such negative teaching in classics and mathematices equally negative teaching in the facts of nature discovered laboriously by many great workers, the square root of the sum results in the modern Briton—at least so far as his knowledge goes. We do not wish to see any branch of knowledge removed from the curriculum. All knowledge is valuable; but we do not wish to see the receptive years of youth wasted upon unimportant knowledge when they might be used for the acquisition of important ones. The real fallacy of the schoolmaster is his supposition that education is valuable chiefly as an exercise and not as an opportunity for laying in stores of information. If this were the case, nothing should be more carefully taught in schools than the game of chess, which is perhaps just as good an exercise in many respects as are mathematics or classics. But the time of youth is short, and the opportunities soon vanish; and the boy kept trifling in the porch is apt, when he becomes a man, to leave it abruptly with some anger in his heart and without ever having entered the beautiful temple within.

Bristol University

We learn with regret, which will be shared by all well-wishers of our modern civic universities, that at the annual meeting of the Court of Bristol University held on November 13 last no steps were taken to institute the reforms in the government of the University required by public opinion, or to render impossible the recurrence of those administrative irregularities so severely censured in a report published some months ago over names well known in the academic world, in the columns of the *Athenæum*. A meeting of Convocation of the University had been called for November 5, but unfortunately the thirty graduates required to form a quorum did not attend; and for the third time successively, and from the same cause, Convocation failed to fulfil the statutory duties.

The continued neglect of the Court to deal with the question of reform so urgently demanded in academic quarters, and the seeming indifference (for which, no doubt, an adequate explanation would not be far to seek) of graduates to the affairs and fortunes of the University are of grave augury for the future of it; and go far to establish the case of the reformers within the University who have so long contended that the Court, a body in which the members of Council form a numerous and powerful section, is unable to protect the interests of the University, and to exercise a salutary supervision over the Council, which is in name, but only in name, subordinate to the Court. Convocation, it has been said, has at times raised its voice but has not been in a position to intervene in affairs effectually. In short, it would appear that the safeguards designed by Charter and Statutes to prevent or remedy abuses in the government of the University are inoperative; or, in other words, that the Constitution provides no adequate machinery to ensure a sound administration of the University or to remedy abuses when they arise.

The Senate, consisting of the professors, has little right or authority in the management of the University, though constituted by the Statutes as an advisory body to Council in academic matters; and indeed it is impossible that a body whose members hold office by the precarious tenure of the

Bristol professors should exercise even a restraining influence upon the executive body, which has gone so far as to lay down the astonishing principle that criticism of the Council or its proceedings is incompatible with the duty of a teacher of the University—a principle, we may remark, incompatible with academic and personal freedom and fatal to administrative efficiency.

On what lines the reform of Bristol University should proceed it is not for us to speak dogmatically. It may, however, here be premised that it is essential that the Statutes of the University, which define the conditions under which the large powers granted to the University are to be exercised, should be strictly observed in spirit and in the letter; and that the professors, who form collectively the Senate, should hold their offices by a tenure as good as that enjoyed by the professors in similar institutions, or, at least, by the tenure required by the Advisory Committee of the Board of Education. These are mere details, but it is certain that reform is a condition *sine qua non* if Bristol University is to emerge from under its cloud and to gain the respect of other academies and the confidence of the public.

Evolution and War

The London School of Economics and Political Science is packed away in a small building near Kingsway, but it is one of the most flourishing and fruitful schools of the University of London; and the meetings of its Students' Union sometimes attract quite a considerable audience of students and their friends. Of this kind was the meeting held on November 4 under the chairmanship of Mr. Edward Twentyman, when Sir Ronald Ross addressed the Union on the subject of Evolution and War.

Sir Ronald made the present war the text of his speech. He referred to a sanguinary combat which he had witnessed once in Burma. His audience listening to the detailed horrors of it were relieved at length to learn that the battles had been of ants and not of men; but starting from this point, the speaker indicated the number of directions in which the combative instinct appeared to operate throughout Nature, with apparently disastrous results to the individual. Why, he asked, did Nature allow such an appalling phenomenon? According to Darwin the result was beneficial because it led to the elimination of the less effective. War differed from other selective agencies in that it might wipe out an entire race; and the intellectual and moral gap between man and the next highest creatures was possibly as great as it was by reason of man's warlike nature. Tribal evolution under the moulding of war would suffice to explain the development of such virtues as self-sacrifice, courage, constancy, obedience, and the honourable keeping of compacts in spite of self-interest. Such qualities would be mere foolishness to a Martian evolved in a warless environment. But we, when we blamed a man for being selfish, or a coward, or dishonourable, really accused him of being dysgenic—of not possessing the qualities which millions of years of tribal evolution should have given him. He is imperfect, like a lunatic or a deformed person. It happens that the warlike virtues are the social virtues, and so they affect social evolution enormously. In their entirety they are covered by the word "duty," and of duty sprang religion. From the warlike virtues too sprang much of poetry and music. The purely intellectual qualities of cunning, observation, accurate reasoning, the faculty of inventing tools, and of seizing opportunities are too obviously associated with the warlike spirit to need much emphasis.

Sir Ronald proceeded to quote some arguments which had been directed against the view outlined. Darwin himself had pointed out that the best and finest young men were exposed to early death during war, while shorter and feebler men were left at home to propagate their kind. The Chancellor of Stamford University had stated that war was utterly dysgenic. But the speaker pointed out that the facts were all against the theory. The warlike nations were the nations of splendid manhood. He instanced the Zulus and Masai, the Sikhs and Pathans—and referred to the miserable physique of the unwarlike tribes. He thought that modern warfare was perhaps more dysgenic than the ancient, because warfare by missiles was less discriminating than hand-to-hand combat. At the same time success in modern war depended not so much on single military virtues, as on the total complex, resulting in national efficiency.

He did not think that a true logical conclusion of this argument was that war was a thing to be eternally encouraged. It appeared to him that the function of war as an evolutionary agent was by this time largely complete. War had therefore become obsolete. Evolution has taken to itself a number of new and finer tools, wherewith to continue its great work, and the hatchet with which it formerly rough-hewed was no longer required. Our knowledge of Nature is, however, incomplete, and we must guard against meddling, for fear of disastrous results. What kinds of degeneration might be set up we do not know.

But Sir Ronald felt that it was our business as reasonable beings to control war, in the sense in which we tried to control disease. We should seek to ascertain the ultimate causes of war. He thought wars were more due to the personal ambitions of individuals than was generally admitted. The Napoleonic wars were an instance. So he considered was the present struggle. Want of preparedness too was greatly provocative of war, and this country would have bitterly to pay for its sins of omission in this respect. The speaker severely criticised the conduct of those who accepted the burden of guaranteeing Belgium's neutrality and made no preparation for enforcing it. War was mainly due to misgovernment of some kind or other. Men are still stupid enough to allow themselves to be governed by the unfit.

Sir Ronald Ross concluded by proposing two resolutions—viz. that war had been a very important factor in the evolution of mankind, and that we should now endeavour to do without it.

A vote of thanks was moved in a telling but hostile speech by Mr. Drummond Smith. Mr. Rhymer seconded. Both speakers seemed rather impressed by the skill with which Sir Ronald had placed his guns for defence in either direction. Their criticisms, however, were rather of the conclusions than of the argument. The ensuing discussion was continued with vigour, speaker after speaker rising to support or denounce (and generally denounce) Sir Ronald's thesis. A short but brilliant speech in support was made by Miss Nettlefold, and later Mr. Holloway spoke with clear, shrewd insight on the same side. Mr. Crossley, Mr. Lambert, Mr. Burke, Mr. Grimshaw, and others followed. The level of the debate was not perhaps so high as the Union's reputation, but the interest was thoroughly maintained until the vote of thanks had been carried by acclamation, Sir Ronald Ross had briefly replied, and the meeting had dispersed.

G. Taylor-Loban.

The Society of Members of the Royal College of Surgeons of England

This Society was founded in 1894 as the successor of an Association which, started in 1884, was financially wrecked by a legal trial of strength with the Council of the College in the action, "Steel *v.* Savory," when in 1892 judgment was given in favour of the Council by Mr. Justice Romer. The want of funds unfortunately prevented an appeal being made to a higher court of justice.

The objects of the Society are:

(*a*) To arouse and maintain amongst Members a permanent interest in their College by obtaining for them a share in its management.

(*b*) To promote an amendment of the charters of the College, which will permit the Members to take part in the election of the governing body.

(*c*) Whilst seeking primarily to further the interests of the Members, to co-operate with the Fellows in obtaining such reforms as are possible under the present charters.

(*d*) Generally, to promote the true welfare of the Corporation by bringing the Council, the Fellows, and Members into harmonious action.

As regards (*c*) it should be noted that the cause of the Members has been advocated by many eminent Fellows, such as Paul Swain, Timothy Holmes, W. Rivington, Oliver Pemberton, Jonathan Hutchinson, Howard Marsh, Sir John Tweedy, and Sir Victor Horsley. Fellows have always figured amongst the subscribers and life-members of this Society.

The exclusion of Members from any share in the administration of the College affairs is a grievance of old standing, but it reached its fulminating point when the Charter of 1843 was secured by the Council, which enabled them, suddenly and without consultation with the general body of Members, to elect 542 Members (*including themselves and their friends*) to what they described as "a new class of Members," or Fellows of the College, and this was done without any qualifying examination whatever. More grievous still—in future the eligibility to a seat on the Council and power to vote were restricted to the said Fellows, and the Members were thus deprived of an important and ancient right.

When the terms of the Charter of 1843 became known, great indignation arose amongst the Members, both in London and the provinces, which was voiced by the editor of *The Lancet* of that day:

"Without hesitation, we declare our belief that a more mischievous, a more iniquitous Charter was never honoured by the sign-manual of the Crown. We shall find little difficulty in establishing the fact that it was obtained from the Government —as was the Charter of 1800—by *misrepresentation and fraud*."

Sir John Tweedy, himself a Fellow and recently the President of the College, published (about 1889) an article in *The Lancet*, from which the following is extracted: "Viewed from the historical standpoint, the demand now being made by the Members is not for the acquisition of new privileges, but for the restitution of ancient rights and liberties." He proceeds to recall how by cunning and hardihood on the part of the Council the ancient liberties, privileges, and franchises of the Members, as confirmed by the Act of 1745—which has never been repealed —have been (with or without legislative sanction) filched from the body corporate, *i.e.* the general body of Members. Sir John expressed this opinion: "There is so much to be said in praise of the Fellowship as an academical distinction, that it is easy to overlook the needless injustice wrought by the manner of its institution in abolishing the constitutional rights of the Members. Sir Benjamin Brodie is credited with being the promoter of the order of Fellows, and in the account which he gave of the reasons for establishing this order not a word is said about a suffrage or a franchise restricted to the Fellows."

Sad to relate, however, when Sir John Tweedy became President of the College, no reform took place. Either Sir John failed to press his views, or he succumbed to the reactionaries of the Council.

The Society maintains that the whole property of the College belongs to and is vested in the Members, and that the Council is merely their executive committee. It is only in so far as they are Members that the Fellows have any legal interest in the property. On this point, in the pamphlet referred to, Sir John Tweedy remarked: "It would be interesting to learn by what arguments the Council justifies its administration of trust property vested in the Members" as a whole and of which the Council renders to the Members no account, or certainly not an adequate one. In November 1911 the Council was requested at the Annual Meeting of Fellows and Members to issue a balance-sheet, but they refused to do so.

At the Annual Meeting for the past thirty years the Members (supported by many of the Fellows present) have made a protest against the withholding from them of their ancient rights and privileges, and resolutions to this effect have been carried by large majorities, and often without a single vote recorded in opposition ; for the Council sits dumb and inarticulate. On at least two occasions within recent years, a requisition prepared in accordance with the regulations of the College, has been presented requesting the convening of a *statutory* meeting of the Fellows and Members, but the Council has refused the request.

We say that the Council does not in any way represent the Members, but it ventures to speak and write in their name, although the Members have not been consulted either in general meeting, or by circular letter. The Council is not elected by the Members. It takes absolutely no interest in them individually, or collectively, unless it sees a chance of inflicting a penalty on some unfortunate Member under its byelaws. The Council treats with contempt the resolutions passed at the Annual Meeting of Fellows and Members. It does not protect our interests against quacks and impostors. During the crisis produced by the National Insurance Act of 1911 the Council took no effective action and summoned no meeting to consider the position of the Members or Fellows in general practice. It spends its income, mainly derived from us, in extravagance and without reference to our wishes or opinions, *e.g.* the Council spends over £100 yearly in providing a dinner for its friends, yet every medical charity is overwhelmed with applications for assistance from Members or their widows and children, who have been (through no fault of their own) brought to penury and even forced within the walls of the workhouse. Such is the Council's extravagance, that, if it were not for the large bequest of Sir Erasmus Wilson, the College would have been long since broken and bankrupt!

If Members were represented on the Council, we contend that the affairs of the College would be placed on a better and more economical basis; that more money would be spent in philanthropy and less on mere display and dissipation, and that the Members would change their present disgust and apathy for an affectionate interest and pride in a College which took a paternal interest in their welfare. Members, when they deserve it, have a right to look to the College for advice and assistance, as well as for the censure of unfortunate errors. Censure indeed would come with better grace from a Council on which Members are represented than from one which has always been found hostile, or indifferent to the interests of the Members.

Until the Council is reformed in the direction indicated, we shall have no confidence in that irresponsible oligarchy.

On behalf of the Society,

Sidney C. Lawrence, Hon. Secretary, M.R.C.S. Eng., L.R.C.P. Lond., M.B., Ch.B., D.P.H. Lond.

22, Latymer Road,
Lower Edmonton, N.,
September 2, 1914.

CORRESPONDENCE

"EVOLUTION BY CO-OPERATION"

To the Editor of "Science Progress"

Sir,

Please allow me to correct an inaccuracy that has crept into a review of my book in your number for October 1914, which seriously misrepresents me on a vital point.

I have indeed, throughout, made a great feature of "*cross*-feeding," which is a totally different thing from "*out*-feeding," a term erroneously attributed to me by your reviewer. I have never used the term "out-feeding." The prefix "*cross*" on which I insist is to indicate that there exists normally a symbiotic relation between organism and food (*i.e.* food-supplier) analogous to the relation between the sexes. Hence my generalisation relating to nutrition—analogous to that stated by Darwin with regard to fertilisation: "Nature abhors perpetual in-feeding." The term "out-feeding" by no means conveys the symbiotic connotation on which I lay chief stress. On my view no amount of mere "out-feeding" or mere "vegetarian" feeding can be of avail in the long run in progressive evolution unless attended by adequate services and counter-services, *i.e.* a fundamental and adequate symbiosis.

(I consider "cross-feeding" and symbiotic strenuousness primitive as compared with "in-feeding" and (parasitic) indolence just as some botanists consider dicotyly a primitive character, and as some believe that trees and shrubs are primitive compared with herbs among flowering plants.)

Like other independent writers before me, I am quite content to bear my share of contumely for championing a new cause, although I could wish indeed to have expounded my thesis with more lucidity and co-ordination. I would point out that even "descent with modification" was greatly ridiculed at first. I am merely emphasising that in this modification nutrition has played a most prominent part, and further that it owes its prominence as a factor to its vital connection with the (past and present) bio-social and bio-economic history of the particular organism, *i.e.* with its general use-relatedness to the web of life.

I have thus been led to the "arch-heresy" of "evolutionary ethics" quite *logically* and not, as your critic seems to think, from a zealot's wish to apply his particular system to nature. Here again I may say that I am quite prepared to take this onus upon me notwithstanding the extreme disparagement the subject has received, and I feel confident in the ultimate establishment of the principles I am here advocating. So far as parasites are concerned, after all it is only due to a corollary of the Natural Selection theory that we persist in classing them amongst the "fittest." If the corollary of any theory of mutual service on the other hand demands their classification in the opposite category, there is very good reason I think on (modern) physiological grounds for the belief that parasitism *does* result in inferiority and extreme morbidity (*vide* its general connection with the realms of pathology, its extreme liability to infection, hyper-parasitism, etc., etc.), and that thus the economically and bio-socially inferior are also seen as the physiologically inferior, *i.e.* as the least fit in genuine survival. I have been taken to task before for holding this view, but I have not yet come across one single solid argument that would invalidate my view. Neither can it be maintained, I believe, that the distinction between physiology and pathology has as yet been so clearly established as not to permit at least of two (divergent) opinions on the subject.

The real issue is between the relative importance attached to work and co-operation on the one hand or perpetual idleness and depredation (rank "militarism") on the other. I presume it is best at least to preserve an open mind, and likewise to remember Huxley's pregnant warning that "what we call rational grounds for our beliefs are often extremely irrational attempts to justify our instincts." In my opinion the claims of competition and of warfare as factors in evolution have been sufficiently urged, whilst those of co-operation (mutuality, symbiosis, etc.) have not had anything like deserving attention paid to them. Once this is seen, it will not be a far cry to the rehabilitation of "evolutionary ethics."

I am glad your reviewer sees a chance of my thesis securing "a patient and attentive hearing," once it is worked out more amply and more systematically. I hope shortly to publish a fairly large volume in expansion of my theory, and I trust it will prove satisfactory on that score. Perhaps you will kindly allow me to add that a number of leading men of all sections of thought have already expressed approval of my main thesis, and that a terse and accurate statement of my views is to be found in a review in the *British Medical Journal*, April 24, 1914.

Believe me, Sir,

Yours faithfully,

HERMANN REINHEIMER.

REVIEWS

GENERAL

Immanuel Kant. A Study and a Comparison with Goethe, Leonardo da Vinci, Bruno, Plato and Descartes. By HOUSTON STEWART CHAMBERLAIN. Authorised translation from the German by LORD REDESDALE, G.C.V.O., K.C.B. With an introduction by the Translator. 2 vols. (London: John Lane. Price 25*s.* net.)

WHATEVER virtues the German nationality may possess, that of conciseness does not appear to be one. In a land where laborious industry is the rule, and reverence for authority universal, it is perhaps not unnatural that an author should be estimated, not according to the novelty of his ideas, but according to the magnitude of the tome in which he embodies them. The high-class reader of this country will probably consider that a work of very nearly a thousand pages of German metaphysics is a book that he may safely leave alone. Previous experience will perhaps have taught him the futility of the general run of such works: and (in many cases, but not in this) the strange marvel that any one can be found to write so lengthy a disquisition on so slender a basis, is only exceeded by the further marvel that others can be found to read it when it has been written.

Lord Redesdale opens his introduction with a defence of German metaphysics against its depreciation by "a man of acknowledged ability and great public worth." It is very true that German metaphysics requires defence, and a more strenuous one than Lord Redesdale has been able to give it. At present German metaphysics calls up a jumble of more or less unintelligible verbiage: which, when it momentarily becomes comprehensible, is found to contain attacks upon science; strange, contradictory and absurd statements [1]; no general results whatever. The incomparable confusion of ideas is rivalled only by the confusion and prolixity of the style of expressing them.

The work of Mr. Houston Stewart Chamberlain is well above the ordinary level of German metaphysical treatises. I do not mean that it has any greater scientific value than the others: for, having no scientific value whatsoever, it is in this respect precisely on a level with the rest of contemporary metaphysical literature. But it does have a literary value, not commonly found among the lights of German philosophy: and these literary qualities have been admirably preserved and reproduced in the excellent translation of Lord Redesdale. The book is palpably the product of the studio rather than the laboratory. It is written in a gorgeous highly coloured style, and deals in the usual intellectual subtleties, which may certainly charm and interest, though they cannot enlighten. The only people who need be disappointed in this book are those who imagine that metaphysics is a science; and therefore that its purpose is the pursuit of truth. Metaphysics is not a science: it is a cross between a religion and a fine art:

[1] Such as that everything is the contrary of that which it is: or that the whole need not be greater than the part.

it gently titillates the elegant emotions of a cultivated mind, while affording pleasant and refreshing exercise to the intellect.

And so the heavily-coloured writing of this book will appeal to many whose emotions and intellect are in a suitable condition for mild stimulation of this character. But its substance regarded in the cold light of fact need engage very little of our attention. Birds with brightly-coloured plumage do not give forth the best song. The cockatoos and birds of paradise do not enchant us with their voices: but we listen with wonder to the songs of the little grey nightingales and garden-warblers. And so it often happens in human existence. If therefore we have before us a work of metaphysics, let us not look in it for any advancement of truth or knowledge: for we shall find none.

Those who think this condemnation too severe should study the language in which metaphysicians attack the doctrines of science. All systems of ethics derived from science or evolution, for instance, are referred to by Kant as a "disgusting jumble of higgledy-piggledy observations and half-sophistical principles." Kant was of course an extreme dualist: monism seemed to him absurd. Yet it is quite certain that the philosophic conclusions of modern physiology and biology are bringing us nearer to genuine monism than could have been imagined possible in Kant's time. "All monism," says Chamberlain, "be it what it may, leads in the end to a Buddhistic contemplation of the navel." I may reply on the same plane of discussion that all dualism leads in the end to the contemplation of a pair of donkey's ears. Herr Chamberlain (for the author has adopted the German nationality and language) simplifies the discussion by the announcement that "All monism is a lie."

Whereas a man of science must decline to take seriously any conclusion or theory reached by the methods of metaphysics, yet he cannot deny that, as literature, metaphysical works have an interest of their own. Chamberlain's method of dealing with Kant is to discuss his personality and philosophy in relation to those of Goethe, Leonardo da Vinci, Giordano Bruno, Plato, and Descartes. On all these subjects, he has much to say that is interesting and suggestive. It is not apparent why he should have selected just these six, except that he himself happened to be specially interested in them; and indeed I may remark of the whole book that the author travels from one subject to another in complete obedience to his own predilections, and without attempting to concert any individual scheme or doctrine. The work is not integrated, but is an expression of the author's personality; so that as far as the book is concerned, its qualitative value would be little affected if it were either much shorter or much longer than it actually is. A profound conviction is expressed of the value of amateur work: "Of the thinkers who have made epoch in the world, hardly one has been a philosopher by profession." The high place in the history of science and of all thought, occupied by amateurs, has often been noticed: and may be explained by reference to several reasons. In the first place, the successful amateur is an amateur usually only in name. He has nearly always begun his studies while still young, and he has pursued them with a fervour which rivals that of the professional. If he has had no university training, he has been led by powerful instincts to subject himself to that hard intellectual discipline without which no great results can be achieved. He differs from the professional not so much in the concentration or discipline of his powers, but in the particular line of study pursued. The professional, in early years, is guided and controlled almost entirely by his elders and by the necessities of his profession, in the course of study adopted. The amateur, on the other hand, passes from one thing to another in accordance with his own interests; and the knowledge thus accumulated will be in many ways different from that of the professional. He will be ignorant of much that is included in the ordinary curriculum: but on the other hand he will have accumulated a wide knowledge on many unusual and neglected subjects. His outlook is thus somewhat different: he approaches problems from a new angle, and in conjunction with the work of professionals, a parallax is obtained, often leading to remarkable discoveries.

Yet every one knows that the average amateur is vastly inferior to the average professional. There are few whose desire for light is so keen as to supply an incentive to labour as great as that which the professional is forced to undergo. Moreover, it happens that those whose desire is thus powerful, voluntarily enter the professional career at an early age. In cases where they are prevented from doing so, they commonly are condemned to some business, which draws upon their time and energy and destroys their value. The number of people who, while avoiding a professional career and training, have yet been able to devote their whole time and energy to scientific pursuits, is very small: but where they have succeeded, the success has often been immense. Chamberlain's book is valuable even from this aspect alone.

Lord Redesdale, himself likewise an amateur in philosophy, has carried out what must have been the very difficult work of translation with much success. A certain number of criticisms, however, are called for.

Kant's main creed is translated by Lord Redesdale: "The greatest business of man is to know what a man must be in order to be a man"—a phrase of singular vacancy and fatuity, which is quite unfair to the German original. Lord Redesdale constantly betrays a partiality for sentences which wriggle about like corkscrews—or like spirochætes! He translates "Ich zitierte . . . das biblische Wort" as "I alluded to the allusion in the Bible": a very unhappy instance of verbal reduplication. He does not always keep his language on a level with the thoughts expressed: as for instance in the translation of "zusammengestoppelten Beobachtungen" as "higgledy-piggledy observations." The adjective higgledy-piggledy is scarcely legitimate at all in any serious literature: and it is singularly ill-adapted for conveying the writings of such a philosopher as Kant. A still worse fault of the same kind is the translation of "gerade im rechten Augenblick" as "at the psychological moment." If the language of the nursery is unsuitable for a translation of Kant, still more so is that other language known as journalese. The expression "psychological moment" first came into use during the siege of Paris in 1870, when the *Kreuz Zeitung* published an article touching on "Das psychologische Moment," meaning of course the "psychological momentum" or factor of the situation. The French mistook "Das Moment" for "Der Moment," and assuming it to be a moment of time, ridiculed the supposed absurdity of the German expression. It was doubtless this absurdity which caused the sentence to be so promptly taken up by English journalists—of course in its wrong sense, and without any appreciation of the meaninglessness which so much tickled the French humour. At the present time the great majority of scribblers in England, when they wish to express the idea "at the critical moment," use the phrase "at the psychological moment"; and it is unfortunate that Lord Redesdale should countenance the expression. The advantage obtained seems to consist in the use of a longer word for a shorter one, a more technical word for a less technical one, and a word less generally understood and less precise in meaning for one that is more precise. The desire to use long words, the meaning of which is only partially understood, is characteristic of most mediocre people. In the present case the word psychological has no meaning whatsoever, and we might with just as much reason speak of "the ecclesiological moment," or "the mineralogical moment," or "the organo-therapeutic moment." Any one of these would be quite as fitting a substitute for "the critical moment."

Herr Chamberlain and Lord Redesdale pay special attention to the personality of Kant, and this constitutes an interesting feature of the book. The son of a Scotsman, he was born in Königsberg, and during his entire lifetime never once left that town. He was "a small wizen man, hardly above a dwarf in stature, with sharp inquisitive features, and an eye that penetrates your very soul." He was immaculately dressed, "as well groomed as any Beau Brummell," a fact which may represent, as Lord Redesdale affirms, "his one sacrifice to the Arts," but was more probably a mere manifestation of vanity. He had read enormously, and appeared to have an intimate knowledge of the world derived from books alone. It is related (though not in the present work) that Kant's afternoon walks in Königsberg were so regular that the good citizens of the town used to set their clocks as he passed their windows. On one occasion, when his house was broken into by a burglar, "Kant rushed upon the thief with the concentrated rage of a wounded tiger," and the burly intruder had sufficient sense of propriety to flee panic-stricken before the wizened dwarf. But why should the philosopher have been so indignant, if it is true that "his only gems were his thoughts, his wealth the rich mine of wisdom and reason"? We should have been glad to learn whether Kant had any bad points to set against the truly startling virtues with which he is credited by his panegyrists.

That Kant was a man of gigantic intellectual powers cannot for a moment be called in question: that his writings have enormous powers of proselytism is equally obvious. It still remains true—partly indeed for this very reason—that in the whole history of modern philosophy there has been no influence of so malign a character as that of Kant. I do not mean to imply that there are not many other philosophers—such for instance as Hegel—whose philosophies are far less attractive to the man of science, even than that of Kant. The writings of Hegel are too esoteric to influence any one outside the small circle of metaphysicians: those of Kant, on the other hand, have exercised a profound control over the fundamental philosophical concepts of the nineteenth century. To shake off those concepts is incredibly difficult: it needs a generation like that of Voltaire, who derided authority in its most sanctified thrones, and gave mortal offence to the established convictions of his century. It may well be that the present war will decide the future trend of philosophical thought—a matter which to some seems more important even than redistributions of territory. Certain it is that the irreverence, the overthrow of authority, the materialism and radicalism, can never arise from the German type of mind, with its strongly mystical and reverential tendencies. In the past, it is the French who have attacked authority with their light derisive scepticism: it is the Scottish and English philosophers who have introduced the most solid and overwhelming revolutions of thought. And so when, in Lord Redesdale's words, Chamberlain "lovingly and eloquently" beckons us to the treasure-house of Kant, some among us may still mockingly and irreverently decline. We distrust even the personality of that little man, who never passed beyond his native town, and whose dress would in our time probably cause him to be described as a "nut." But above all we repudiate any suggestion that the great work of modern science can be in the smallest particular affected by anything that may have been said or thought by the German metaphysician of a hundred years ago.

Hugh Elliot.

Robert Boyle: A Biography. By FLORA MASSON. [Pp. ix + 323.] (London: Constable & Co., 1914. Price 7*s*. 6*d*. net.)

THIS book is rather a history of Richard Boyle and his children, including Robert Boyle, than of the latter only—and it is a most interesting one. Mr. Richard Boyle, the second son of the second son of a country squire—one of the Boyles of Herefordshire—was born in 1566. He was apprenticed to the law, but as he thought he would not make a fortune in that, emigrated to the then foreign country of Ireland, where he soon advanced in the favour of men of influence, married a fairly wealthy wife, and after her death acquired great landed possessions by the purchase of the estates of Sir Walter Raleigh. After successfully rebutting a serious charge against him, he acquired the favour of Queen Elizabeth and was ultimately made Earl of Cork. He seems to have been a man of wonderful character and capacity, and his wealth appears to have been due to the admirable manner in which he developed his estates, he being one of the first of that type of Englishman who do such things. By his second marriage he had a large number of children, and Robert Boyle, the great man of science and discoverer of Boyle's Law, was the youngest of his sons.

Robert was born on the 25th January, 1626, when his father was sixty years old, was partly educated at Eton, and was then sent for five years to Geneva and Italy. Before he returned, the Earl of Cork was nearly ruined by one of the Irish rebellions; but a small estate still remained in the possession of Robert—enabling him to devote himself to science and literature.

The book is chiefly derived from the information contained in the Lismore Papers, especially the private diary of the great Earl, and is full of what might almost be called family gossip. We say this in no depreciatory vein, because this gossip throws much fascinating light on the history and the manners of the time. Amongst other details, the life of Robert Boyle is very prettily delineated. He was never married, but always lived in the closest relations with his sister, Lady Ranelagh, and the other members of his family, most of whom acquired high positions and titles. His life covers the periods of the great rebellion, the plague, and the fire of London; and he was one of those who originated the Royal Society; and Newton came after him. He died at the age of sixty-five, full of works and honour.

The book should be read by all those who are interested in the history of science. It makes no pretence to analyse Boyle's contributions to "philosophy" with any completeness; but it describes in a charming style the life and the times of one of the first and also of one of the greatest of men of science. Boyle did not possess the great genius either of Descartes or of Newton. His was a gentle, studious, and eminently truthful and thoughtful character; but he broke ground which has ultimately yielded an immense crop of benefits to humanity in the lines of physics and chemistry. Miss Flora Masson is to be warmly congratulated on her book. The portrait of Boyle is an excellent reproduction from that in the possession of the Royal Society.

Memorabilia Mathematica; or, The Philomath's Quotation Book. By ROBERT EDOUARD MORITZ, Ph.D., Ph.N.D., Professor of Mathematics, Washington. [Pp. vii + 410.] (New York: The Macmillan Co., 1914. Price 12*s*. 6*d*. net.)

NO books are more interesting than analecta; and no analecta are more interesting than scientific ones; and no scientific ones more interesting than those on mathematics—but the latter are so rare that we must warmly welcome Professor Moritz's interesting work. In his preface the author explains that "ten years have been devoted to its preparation, years which, if they could have been more profitably, could scarcely have been more pleasantly, employed"; and the book actually contains more than two thousand analects from mathematicians of distinction and others who have written on mathematics. Foreign quotations have always been given in English, and the name of the author and the work from which the extract is taken are appended after each. The analects are classified under twenty-one subjects, as for instance, The Nature of Mathematics, The Value of Mathematics, Study and Research in Mathematics, Modern Mathematics, The Mathematician, Mathematics and Logic, The Fundamental Concepts of Time and Space, and Paradoxes and Curiosities. An excellent index easily enables us to locate our favourite passages. Perhaps the most interesting chapters are the two devoted to Persons and Anecdotes; and the reader would gladly have seen these much extended. But the author's hope has evidently been that "the present volume will prove indispensable to every teacher of mathematics, and to every writer on mathematics"; and this plan requires rather a quotation of opinions than an exhibition of personalities. There are many stories about Newton, but the one which narrates how his dinner was eaten by some one else while he was in one of his moods of abstraction is not among them. On the other hand we have the famous story of the professor who wrote on his blackboard that he would not "meet his classes to-day." One of the students scratched out the first letter of the word "classes." Presently, however, the professor returned and struck out the next letter of the same word. But Professor Moritz appears to be doubtful whether the story is to be told of Lord Kelvin or of Professor J. S. Blackie—and we must confess that we have heard it generally quoted of the latter. Another story of J. J. Sylvester was told by W. P. Durfee, who instanced, as a case of the manner in which Sylvester forgot propositions, that "I remember once submitting to Sylvester some investigations that I had been engaged on, and he immediately denied my first statement, saying that such a proposition had never been heard of, let alone proved. To his astonishment, I showed him a paper of his own in which he had proved the proposition." The chapter on mathematics as a fine art is one of the most interesting, and well exhibits the æsthetic pleasure to be derived from these beautiful studies. Evidently most mathematicians have been as much carried away by this æsthetic pleasure as are poets and musicians by the joy of composition; and it is true that mathematics and the highest arts are sisters who live hand in hand upon the summits of Parnassus. What is there more beautiful than a geometric theorem or an analytic series? Thus J. W. A. Young remarks: "It was a felicitous expression of Goethe's to call a noble cathedral 'frozen music,' but it might even better be called 'petrified mathematics.'" For mathematics and for high art there is one requirement—a sense of order. De Morgan said that "the moving power of mathematical invention is not reasoning, but imagination." Both are the moving powers, not only of mathematics, but of art. When Plato said that "God geometrises" he merely expressed the truth that this sense of order belongs to the highest part of the mind. The lower type of mind does not possess it. And some one else has said, "To the small mind there is nothing great, to the great mind nothing small." The author has fully achieved his purpose, and, as he himself remarks, "the absence of similar English works has made the author's work largely that of the pioneer." Not only mathematicians, but all men of science, will thank him for his labours.

Plague and Pestilence in Literature and Art. By RAYMOND CRAWFURD, M.A., M.D. Oxon., F.R.C.P., Fellow of King's College, London. [Pp. viii + 222.] (Oxford : At the Clarendon Press. Price 12*s*. 6*d*. net.)

AT first sight it might seem extraordinary that any one should find a connection between literature and art, and plague and pestilence ; but Dr. Raymond Crawfurd's fine book amply justifies the venture. As a matter of fact there is a close connection, because, indeed, art should find its greatest themes in those matters which most concern humanity, and among these there has been nothing so germane to us as the frightful pestilences which have destroyed us probably for untold ages and which are still capable of sudden disastrous revivals. Both literature and art in their most genuine periods did not hesitate to describe and figure these terrible experiences of mankind. Dr. Crawfurd's book is full of beautiful reproductions of beautiful pictures. Of course, in the highest period of art, painters treated everything in a manner which scientific men would call diagrammatic rather than realistic—that is to say, all the events of an outbreak of plague might be crowded into the different parts of the same picture. But this does not in any way detract from the artistic beauty of those classical productions. The seriousness, the clarity, the realism, the finish, and the technical capacity shown in them should be compared with much of our present-day rubbish which passes under the names of impressionism and so on.

The history begins practically with an account of the great plague of Athens derived from Thucydides. Dr. Crawfurd gives a detailed discussion of the nature of this outbreak and concludes that it was plague ; but we must confess still to feeling some doubts as to whether it was not smallpox. He also discusses the views of the ancients as to the cause of these terrible inflictions, and does so in a very able manner. Next he comes to the great pestilences of the Middle Ages ; but the book is not a scientific treatise of epidemics, and the author therefore does not cover the whole ground, but confines himself to such epidemics as have been best described in literature and art. His narrative concludes with the outbreak of plague in Napoleon's army in Egypt and with the fine picture of Baron Gros.

It is very curious how little interest men have taken in the scientific study even of such important matters as the diseases which destroy them by hundreds of thousands and millions ; and how slowly the true theory grows in the human mind. At a very early day Lucretius really formed something like a true conception when he attributed plagues to atoms ; but men persisted in accusing Heaven of causing them. A no less wonderful history is the scientific history that records the manner in which we have gradually discovered the truth ; but (and we rather regret it) the author does not deal at all with this theme, though we think the book might easily have terminated with some account of the important discoveries of Kitasato and Yersin and of the epoch-making discovery that this terrible infliction, so often attributed to wrathful gods, is simply due to such a contemptible thing as a rat flea. The book contains no list of contents and no index ; but is nevertheless one which should be possessed by all medical men and which will be of interest to all lovers of art and science.

R. ROSS.

SOCIOLOGY

Interpretations and Forecasts. A Study of Survivals and Tendencies in Contemporary Society. By VICTOR BRANFORD, M.A. [Pp. x + 83.] (London : H. K. Lewis, 1914. Price 5*s*. net.)

MR. VICTOR BRANFORD'S book will please and soothe many a fireside, especially the well-screened and cushioned firesides of the gentler sex. It consists of a number of addresses—to a woman's club, to a Chelsea association, to working-men students, and so on; and deals with some Illustrations of Sociology, the Sociologist, the Citizen as Sociologist, the Citizen as Psychologist, the Sociologist at the Theatre, the Mediæval Citizen, the Present as a Transition, Town and Gown in America, and Conclusion. Some of the sub-headings are: The Science of Looking Around and the Art of Creating Eutopias, Matriarchs, Old and New, the Eugenic Theatre, the People and their Rulers, the University Militant and the City Resurgent, Eugenics and Civics. The reader will be able to gather the general trend of the book from this catalogue; and it contains many good things. The list of "Guildsmen Performers" of an old medical play and the remarks thereon, and the observations made by the shade of Dante when he visits a modern American city, are good. But the addresses are really lay-sermons on various sociological themes, and have the frequent quality of sermons in that the talking is not confined to a single small section of the subject but rather deviates through all the speaker's views on things in general. Thus, whatever the heading may be, we are sure to find much the same themes dealt with, such as civics, eugenics, citizenship, and so on. The style is excellent modern English style, and each sentence is clear and well balanced; but it has the defects of that style, in that it is wanting in antithesis. Now without antithesis, which is the salt of style, language becomes insipid. In another sense, antithesis is the chiaroscuro of literature, serving to define outline and concentrate light. A book like this ought to be compared with the older English style of Locke and Hume or the modern French style of, let us say, Taine. At the end of our readings of Mr. Branford's book (and it is impossible to take the whole of it at a gulp) we find that our memory of it has already become nebulous. He does not possess the art of putting his matter into compartments; and, in fact, it is necessary to use his excellent index in order to ascertain his views on any given point or person.

Sociology is not popular, because the man in the street connects it with the unpractical. We all hope for utopias, and believe in civics and eugenics and Cities Beautiful. There is little use in preaching to us on these matters, just as it is possibly unnecessary to advise those who have been overtaken by a flood to save themselves from drowning. What is of real service is to show us exactly how the various desiderata can be obtained; and here we cannot help pointing to English cities, which are really fortuitous collections of slums, though all of them are governed by free municipalities, and many of them contain universities as well. If then we British, with our scheme of government supposed to be so perfect, fail to reach the City Beautiful, who can succeed? It is true that, as Ruskin so convincingly showed and the author has so well illustrated, the mediæval cities may have approached this ideal; but then we forget that assassination lurked round those carved corners and under those magnificent groins; while the death-rate due to disease was probably sixty per mille or more. The interest of social art lies not in the principles (which Mr. Branford deals with) but in the method.

At this moment, the reader will remark that the author has failed to make at least one "forecast." He gives many hits at militarism, and, if we take that word in the evil sense of warlike aggression, he is quite right. But since his book was published the present war has broken out—in spite of his warning. This shows the salient defect of our sociologists. In his own words applied to others, "they are wanting in that relation to the common stock of human experience that can give them an abiding reality for the race." He talks of Cities Beautiful; but in a few months numbers of Cities Beautiful have melted away to nothing in that terrible solvent of prosperity which nature imposes upon man—war. We all admire lofty aspirations ; but we cannot forget that there must always be some one to get up early in the morning, to make the fires, to clean the boots, and to cook the food ; and, when war comes, to destroy our fellow creatures. The real question is, not what we wish, but what we can actually do in the conditions which the present state of things imposes on us. Mr. Branford leads us quite in the right direction—towards his City Beautiful ; but at the end of our long journey with him we find ourselves only upon the brink of that great chasm which still divides us from the phantasm on the far horizon.

R. Ross.

Some Main Issues. A Collection of Essays by G. Walter Steeves, M.D. [Pp. iii + 109.] (London : Chapman & Hall, 1913. Price 3*s.* 6*d.* net.)

A book of essays by a medical man is always expected to be charming, and therefore medical men who attempt such excursions from their ordinary duties must face the somewhat jealous criticism of expectancy. Dr. Walter Steeves's unpretentious little book will, however, not meet with censure, but will certainly appeal to many reflective readers. The author is already well known both as a physician and as the writer of a most delightful little monograph on Francis Bacon from the standpoint of a bibliophile ; and his essays exhibit the same characters. His subjects are Toleration, The Child, Appearances, Courage, Is it Worth While ?, Letter Writing, In Sickness and in Health, Choice in Literature, Gratitude, Egotism, Gifts, and The Book Collector. The style is simple and of the best ; but the author gives us many acute observations and weighty judgments, and does not hesitate to speak very plainly where he thinks it necessary. The whole is illuminated by the calm and gentle light of quiet wisdom. It is, of course, a book to be kept and studied on many occasions rather than to be read through at a sitting. That is to say, it is the kind of book which comes into our life, and stays there.

A Way of Life. An address to Yale Students, Sunday Evening, April 20th, 1913, by William Osler. [Pp. 61.] (London : Constable & Co., 1913.)

Another booklet by another physician, already famous among his brother medical men for his excellent addresses both on scientific and literary lines, will be welcome to all. It is merely an address to students ; but we all belong to this category, and Sir William Osler's teaching affects every one. The style is delightful and full of apothegms which we can put under the pillow and sleep upon. For example, "The load of to-morrow," he says, "added to that of yesterday, carried to-day, makes the strongest falter. Shut off the future as tightly as the past. No dreams, no visions, no delicious fantasies, no castles in the air, with which, as the old song so truly says, 'hearts are broken, heads are turned.' To youth, we are told, belongs the future, but the wretched to-morrow that so plagues some of us has no certainty, except through to-day. Who can tell what a day may bring forth ? Though its uncertainty is a proverb, a man may carry its secret in the hollow of his hand. Make a pilgrimage to Hades with Ulysses, draw the magic circle, perform the rites, and then ask Tiresias the question. I have had the answer from his own lips. The future is to-day—there is no to-morrow ! The day of a man's salvation is now—the life of the present, of to-day, lived earnestly, intently, without a forward-looking thought, is the only insurance for the future. Let the limit of your horizon be a twenty-four hour circle." This is a fine philosophical attitude—not perhaps the finest. It will not lead to very high and long-continued efforts, requiring the closest and most constant retrospection and forethought, the retrospection of doubt, and the precognition of the future; but it is the philosophical vesture which may be most safely worn by all of us ordinary men and women. With it, at least, we do what we have to do. Some must do more than they have to do. For them, the suffering of that great ambition. But to the world at large our own is enough.

The Application of Logic. By ALFRED SIDGWICK. [Pp. ix + 321.] (London: Macmillan & Co., 1910. Price 5*s*. net.)

Elementary Logic. By ALFRED SIDGWICK. [Pp. x + 250.] (Cambridge: University Press, 1914. Price 3*s*. 6*d*. net.)

MR. ALFRED SIDGWICK is a philosophical writer whose position in relation to the thought of his time is somewhat difficult to evaluate. On matters connected with logic he expresses general agreement with Dr. Schiller. Such a statement, at first sight, seems sufficiently definite. And yet, as we examine more closely the trend and substance of his work, the impression we receive is different. It is not merely different in manner. Mr. Sidgwick's work is a suave, calm, dispassioned appeal to the reason. Dr. Schiller is an impassioned controversialist. The substance seems to be different. In the light of Dr. Schiller's bitter attack on formal logic, nothing would appear more absurd than to write one more elementary text-book elucidating the well-worn theme. Yet the first part of Mr. Sidgwick's *Elementary Logic* is a text-book pure and simple. It is a very good one, perhaps the best extant.

Mr. Sidgwick approaches the subject in a manner reminiscent of an unusually good teacher. "Gentlemen," he says to his readers in effect, "I am afraid you are going to be bored with this subject. I can assure you I am much more bored than you are. Moreover there is a considerable amount of sham and pretence about it. It is a game and very little else. It is true in a way, but not in the way you may be inclined to think. Though it professes to be the science of accurate thinking, it really has very little bearing on the reasoning of everyday life. But we must pass this examination, so here goes." And a very good compendium of elementary logic follows.

There does seem to be an inconsistency. Such an attitude a tutor can well adopt not only in logic but in most of the subjects of a university course of education. But why should Mr. Sidgwick write another text-book? If the subject is both useless and harmful, the natural inference is that it should be abandoned as far as is possible and practicable. And, moreover, the number of those who are obliged to study logic for examination purposes is very small. There must be some explanation of its fascination for those who think with Mr. Sidgwick. To philosophers and logicians, logic is dull and trite. It is the opponents of logic who find it interesting.

We will now try to express as briefly as possible Mr. Sidgwick's attitude towards formal logic. He is in agreement with the most formal of the formalists in stating that every deduction naturally resolves itself into a varying number of syllogisms. But then, we are informed, no syllogism, and inferentially no deduction, is applicable to real life unless we subject it to further examination. Every syllogism is necessarily liable to the fallacy of ambiguous middle. The reason is that the very constitution of a syllogism, the statement that A is B, implies the placing of individuals in a class. No individuals are exactly alike, and, in any unexamined case, the difference between the particular member of a class and those previously examined may vitiate the conclusion. The above is a very brief and crude summary of Mr. Sidgwick's argument, but it expresses the gist of it. A very important part of ordinary thinking is, therefore, the examination of the premises in the light of the particular conclusion which it is sought to deduce.

The natural inference from this line of argument seems to be that logic should be regarded as a strictly formal science, like mathematics, and that no logical process can guarantee the material truth of the conclusions. Many logicians have adopted this attitude more or less consistently. We should naturally expect that it would appeal to Mr. Sidgwick and that he would be classed as a consistently formal logician. It is, however, not accurate so to describe him, natural as is the mistake of attributing to him such an attitude.[1] Why it is not his attitude is not quite clear. He does not, like Dr. Mercier, object to the syllogism as a logical form. The reason appears to be that he regards the process of the formal deduction of conclusions from premises as so negligible a portion of the process of actual thinking that no science dealing with it is required. But then he has written a text-book on elementary logic. The truth is that Mr. Sidgwick has not clearly and explicitly stated his position on this matter. Were Mr. Sidgwick merely writing an elementary text-book, indeterminateness on this or any other philosophical question would not be of great moment. But to one who, like Mr. Sidgwick, is attacking old-fashioned logic, the lack of a clear and definite attitude on fundamentals is a serious omission.

Having said so much by way of criticism, it only remains to express the highest appreciation of the general tone and substance of Mr. Sidgwick's work. The works reviewed, especially the *Application of Logic*, are an admirable résumé of the puzzles and difficulties that naturally arise in everyday disputes. Purely formal fallacies are rarely of importance. The material problems—definition, ambiguity, the degree of applicability of general rules—are of greater moment. These Mr. Sidgwick examines thoroughly and well. Such an examination we can read and appreciate entirely apart from philosophical differences. Any one interested in the tricks and devices of the skilled disputant, in the kinds of fallacies which are commonly accepted as truth and in the best methods of combating them, can be referred with confidence to Mr. Sidgwick's work.

H. S. SHELTON.

MATHEMATICS

Theory of Functions of a Complex Variable. By DR. BURKHARDT. Translated by PROFESSOR RASOR. Published by Messrs. D. C. Heath & Co. [Pp. xiii + 432.] (Price 12*s*. 6*d*. net.)

THIS important translation of one portion of Dr. Burkhardt's *Funktionen theoretischer Vorlesungen* will be cordially received by all who are interested in the teaching of mathematics. The German book, since its publication in 1897, has passed through four editions, the translation by Professor Rasor being based upon the 1912 edition. The book is a masterly introduction to the Theory of Functions; the subject is developed with exceptional power, and the style has the ease and subtle charm of simplicity. In the course of successive editions Dr. Burkhardt has carefully revised his matter, and a comparison between the present translation and the first edition has proved how unremittingly the author

[1] I myself so described him in a recent number of the *Quarterly Review*, but I am given to understand that the description, though not inaccurate in the sense in which I use it, is misleading.

has worked at improving this book. English mathematicians are deeply indebted to their American colleagues for many excellent translations of German books, and this last contribution is by no means the least under which we are placed. Unfortunately, in England an ignorant dogmatism about the uselessness of translations has suppressed many attempts to render into English some of the best German books; this policy has placed difficulties in the way of young students, who are deterred often from reading German books by the technical terms for which they cannot find the English equivalent terms, even if such exist. I hope that Professor Rasor will add to the debt which we all owe him by continuing the series of translations and giving us at least another volume of Dr. Burkhardt's *Vorlesungen*. It is needless to describe the merits of the original work, which are well known. Professor Rasor has added to the book collections of examples which will be welcome to the teacher; they are well chosen and apposite; many of them are original and skilfully devised.

C.

The Elements of Non-Euclidean Geometry. By D. M. J. SOMMERVILLE, M A. D.Sc. [Pp. xvi + 274.] (London: G. Bell & Sons, Ltd., 1914. Price 5*s*.)

THE book which Dr. Sommerville has written upon non-Euclidian geometry is an extension of lectures delivered in 1913 at the summer colloquium arranged by the Edinburgh Mathematical Society. These courses, if estimated by their first-fruits here presented, must be of great value. The book itself is one of a series of mathematical books for schools and colleges published by Messrs. George Bell & Sons. The author in a preface seems to justify its presence in such a series by expressing a hope that the book may prove serviceable to scholarship candidates. This expectation belies either the book or its hypothetical beneficiaries. It should be regarded rather as addressed to the class of readers to whom the lectures were originally delivered: it is to mathematicians and to teachers of mathematics that the subject of metageometry appeals. The school-boy has other tasks, and it is an error to place before an immature judgment subjects which demand philosophical insight. The book really concerns the school-boy in quite another way; for the study of this subject by the masters who lay down his course of ordinary geometry cannot fail to have direct beneficial effect. It is unfortunate that we have had to wait so long for such a book as Dr. Sommerville here offers; the book might have been written fifteen years ago, and in that case would have exerted a powerful influence in the movement which has ejected the elements of Euclid from its firm position as the foundation of our geometrical instruction, and has yet done so little to rebuild where it has pulled down. Geometrical reformers have too often lacked the respect for the genius of Euclid which they might have acquired from a study of non-Euclidian geometry. It is hard for those of us who were taught "Euclid" in our school-days, with the clear, logical sequence of its propositions, to realise the lack which is now felt by those who have been trained upon modern substitutes. Many of this later school will, on reading Dr. Sommerville's book, be sensible of the loss which they have sustained in studying geometry in books whose claims are based solely upon either utility or elegance.

The purist may with some justice find fault with Dr. Sommerville's presentment of the earlier portions of the subject; this weakness is almost inherent in a book which is an elaboration of lectures. The author himself is conscious of this joint in his armour, and refers to another book those students who wish to study the development of the subject from a set of axioms. However, the discussion of fundamental assumptions has not been neglected, and the subject is developed systematically, the one omission being the absence of any mention of the existence-theorem for parallel lines, with the fruitful discussion of the axioms upon which Euclid's definition is based. Some gain would have resulted from following the traditional order in Chapter II., thus bringing the discussion of trigonometry into closer relation with the simpler geometrical theorems and postponing the discussion of the absolute. Though the arrangement of this part of the book may be challenged, it could be defended. But no justification can be imagined for the section which includes pp. 94-100. In these few pages the author summarises *pure projective geometry*. The summary is introduced, it is true, with an apology that "unfortunately most English text-books start by assuming metrical geometry," but there are fortunately in the English language excellent text-books which give a thorough treatment of the subject, and to two of these reference is made by the author. It is a sad thing to find in such an excellent book a belief expressed that a summary of six or seven pages can provide any reader with a working knowledge of projective geometry. Surely if a writer expounds the subject of which he treats coherently and consistently, he may expect his readers to provide the necessary preliminary information.

The book begins with a historical section which, coming from the pen of the historiographer of the subject, is, as we should expect, admirable. Mathematics contains in its history many romances, but the historians of few subjects have a more fascinating chapter to write than that which tells of the discovery of metageometry. Dr. Sommerville tells here the story of the two generations of the Bolyai who worked in this field, he recalls the solemn adjuration of the father who, foiled in his attempts to prove the parallel-postulate, took refuge in poetry and bade his son avoid the loathsome subject: then he gives the triumphant letter of the son in which he announces to his father the great discovery which he has made. "I cannot say more now, except that out of nothing I have created a new and another world." This new world is described in Chapter II. In Chapter III., the world of elliptic geometry, even more bewildering in its structure than Bolyai's, is explained—the geometry in which the straight lines are of finite length. In the following chapter elliptic geometry is developed and explained by analytical methods. This chapter is of great interest, and the reader will probably leave it only with regret that it was not a little longer, and that the author had not included some account of hyperbolic geometry. Perhaps this subject was passed over because it receives in Prof. Liebmann's *Nicht-euklidische Geometrie* such full treatment. In Chapter V. the representation of non-Euclidian geometry in Euclidian space is discussed, and leads up to a chapter on space-curvature and the general philosophy of the subject. Here the author has received expert advice from high philosophical authorities, but at the end he shows true mathematical instinct in giving a quotation from *La Science et l'Hypothèse*, for the mathematician even when deaf to the charms of the most cunning philosopher always listens to the greatest master of his subject in this generation. After this the reader will be conscious of a certain bathos when his attention is called to radical axes, homotethic centres, and the like. Interesting as such elaborations of the theory are (and every reader should study the sections on "marginal images" and "the conic"), it may be suggested that some of these discussions would have been better placed in small-print appendices. The arrangement of the subject-matter is the weak side of the book. Students who read Prof. Liebmann's book will feel the value of a carefully devised sequence in the subject. There is no object in instituting a comparison between the two works. Both books are indispensable to the student, and he is fortunate in having two such books to read.

But there is one superiority of the German authority to which reference may be made without any invidiousness. *Nicht-euklidische Geometrie* is one of a series in which the various branches of mathematics are treated by well-known authorities; *Non-Euclidean Geometry* is an isolated treasure found surrounded by *Problem papers in arithmetic for preparatory schools*, *Arithmetic*, and *Statics*. What a difference! The lack of such a series in English as the *Sammlung Schubert* is a subject for national mathematical sorrow. As far as can be judged the *Sammlung* is a private venture, unsupported by Government subsidy or university patronage, and yet its enterprising publisher, G. J. Göschen, has already issued sixty or seventy books and has twenty more on the stocks. It is to the honour of Messrs. George Bell that they have published a mathematical book from which they cannot expect to derive a large profit. But if they do not gain by it financially, they have the credit of issuing from their house a sound, useful treatise upon an important subject. Considering the enormous number of high-priced mathematical text-books used in schools and the profits which certainly accrue to some publishers from this source, it is regrettable that apart from the great University Presses of Oxford and Cambridge so little is done for the advance of mathematical knowledge by the publication of treatises from which profit cannot be extracted.

C.

Plane and Solid Geometry. By W. B. FORD and C. AMMERMAN. [Pp. ix + 321 + xxxiii.] (New York: The Macmillan Company, 1913. Price 5*s*. 6*d*. net.)

OF the making of elementary books on geometry there is no end, and the study of them is a weariness of the flesh. So the reviewer frames his lament, but it would be unfair to make such a sweeping verdict without justifying the attitude of mind which forces the unwilling utterance. The review of this book will therefore take a somewhat general trend, and the remarks in it will apply not only to the book before us, but to others of the same brood, whose paternity, though unmentioned, is no secret.

Some fifteen years ago an agitation was raised in this country, primarily by engineers, against the method in which geometry was then taught. The writer of this review willingly acknowledges the wisdom and the justice of a great deal that was said by those who led the attack. The methods then employed were open to charges which could not be refuted. The text-book was of very great antiquity, and designed to meet the needs of pupils of mature intellect and of a different civilisation from our own; the boys and girls of tender years who tried to learn geometry from it often did not realise what was being taught them, and in many, perhaps most, cases studied the text only. Sufficient to say that the reformers, as usual, triumphed, and the system of teaching the subject was reorganised. The panacea which found favour, to judge by the literature issued, was a utilitarian treatment of the subject. Though the ancient Greek philosopher declared that there was no royal road to geometry, the modern civil engineer decided to make one. Planes were called tables, lines were christened strings, while for points even such curious objects as human heads were substituted. All was done under the spell of the word practical. Every stage in the development of the subject at which difficulties were encountered was zealously attacked by a host of minor Euclids, until in the modern geometry no landmarks remained such as I. 5 and I. 47 (to use a forgotten nomenclature), which earlier generations, after surmounting *non sine sudore*, honoured and remembered for the effort which the ascent had cost. If the reformers had insisted upon preparatory work, in which tables and strings and even men's heads took their proper stations, all might have been well with us to-day ; but revolution was preferred to evolution.

The first requisite for the pupil learning geometry is either a good teacher or a good text-book. Fortunately we have many good teachers, but unfortunately no Legendre came forward at this juncture to write an English *Elements de Géométrie*. The writers of the modern text-books on geometry wrote, not to satisfy the mathematician, but to please the engineer. Even when attempts are made in their books to explain fundamental terms, the explanations often are such that they cannot satisfy either the logician or the mathematician, and every weakness in an explanation is covered by a cloud of illustrations drawn from a knowledge of the outside world of phenomenon which the scholar does not generally possess. The old-fashioned plates of the out-of-date cyclopædia are pressed into the service of an appeal to the visual organs, while too often the mental vision is dulled rather than sharpened by this process of illumination. The works attest to the ingenuity and the versatility of their writers, but they do not satisfy the geometricians, and they do not even placate the engineers. That the scheme inaugurated by the new text-book is failing is the verdict of competent observers, and especially of those who examine the scholars trained under it. Many of the teachers who have marked out their school curricula by the new charts are justly alarmed by soundings now being taken, and know that their ships are in dangerous waters.

The human factor was neglected in the old system of geometrical education ; in the new system it is outraged. Formerly only a half, though the more important half, of the subject was taught ; to-day the two parts are crammed down indiscriminately. Practical geometry, with its wholesome fare, attracts every boy and girl ; but it is easy to have too much of it—besides, it is not geometry, except in the narrower philological meaning of the word. Geometry, in spite of the writers of our text-books, remains an abstract science, the type indeed to which all sciences which are not merely descriptive or classificatory will ever strive to conform. The continual intrusion of the crude imperfections of practical illustration does not, after a certain point is reached, aid in the development of abstract ideas ; indeed, such a proceeding often hinders the delicate and subtle mental growth which must accompany the study of the subject. How the modern method of unfolding the theory confuses the student may be shown by analysing a problem which appears in one of the well-known new text-books ; perhaps it is reproduced in them all, for some forms of error are highly infectious. The problem is, To find the locus of a man's head climbing a ladder. The concocter of this precious example intends the answer to be a line parallel to the ladder, and clever persons who are "in the know" might give the required answer. But let us consider what the words of the problem will suggest to a conscientious boy of average intelligence who is ignorant of the tricks of the trade. First he will attempt to settle the shape of the man's head, and will be fortunate if he fixes at once upon a perfectly bald specimen ; his second difficulty will be in deciding where the head finishes and the body begins. On getting over these anatomical details he will be confronted by the irregular motion of the action of climbing, the ups and downs due to the bending and straightening of the knees, also by the swinging motion from right to left which is inseparable from the shifting of the balance of the body from one leg to the other. The proposer of the problem will protest against my analysis as far-fetched. I know that it is not. There is a large class of boys, not the really clever ones, but a set who think carefully, and who are usually classified as duffers by persons who are not sharp enough to understand the working of their pupils' minds, and it is these boys, the backbone of the school and the future makers of the nation, who are honestly perplexed by the slipshod nonsense which is nowadays mixed up with geometry.

Here is a second example, chosen this time from the text-book to which this long harangue is attached: it is an example found in a section in which parallel lines are treated carefully and thoroughly; at such a point in his geometrical studies surely the student needs no practical conundrums to distract him from important considerations which require his undivided attention. The problem runs thus, "Two parallel pipes for hot and cold water lie flat along the same wall; at the ends of each of them an elbow is screwed on which turns the pipe through a right angle. If the pipes connected to these angles also lie flat against the wall, will they be parallel?" As if all this was not enough, a diagram is supplied so complete in its details that one regrets that the hot and cold pipes are not distinguished. It would be useless to analyse this problem, it suffices to refer to the preface, from which one surmises that this may be "an illuminating diagram drawn from the Arts." From the same source we learn that "the function of such problems is not to train the students in the technique of the Arts" (for which at least we are grateful, remembering how dependent we are upon the Art of Plumbing), "rather it is to illuminate the geometric facts and to make clear their importance and their significance." How can an ugly drawing of two cast iron pipes with a knobby elbow make clear the subtle theory of parallels? Every infant knows the answer required; it is so obvious that most students will cunningly spend time in trying to find out where the catch is. At any rate the elements of Euclid were not open to the reproach of being too obvious. It is not my wish to suggest that the book by Messrs. Ford and Ammerman is not a good book; on the lines on which it is constructed it is excellent. The defects to which I have drawn attention are inherent in its avowed design.

Besides the objective of the practical, the modern text-book writer also aims at smoothing down the difficulties of the subject, and in this he carries the sympathy of all. But authors who sacrifice the subject to its simplification cannot earn the lasting gratitude of their readers. It must be remembered, too, that it may be a sound policy to give the longer proof when it fits more naturally into the development of the subject. Students have told me that in the older Euclid system it was I. 5 which gave them their first real insight into geometrical method. The ambition of a writer on geometry should be to give a connected and logically coherent view of the subject, and not to frame a system of propositions, each of which is expressed in the fewest possible number of words. Difficulties must occur in any geometrical system, and the test of a system comes when the student faces these difficulties: if he has been trained to think, he will master them; if he has not, he will not be helped by practical illustration. It is of importance in judging a book to examine how the writers deal with crucial points; two such tests suggest themselves, the theory of parallels and the theory of proportion. In Ford and Ammerman parallels are clearly and well explained, whatever one may think of the illustrations appended, but the same cannot be said of their discussion of proportions. The criticisms made upon this part of the book apply, however, to other authors who have had the temerity to run counter to the teaching of Euclid and de Morgan.

In discussing the teaching of proportion, it is unnecessary to deal with how far incommensurable magnitudes should be included. Every teacher who is not confined by an antiquated syllabus must include in his course of geometrical teaching the ratio of commensurables. Ratio and proportion are important notions which every educated citizen should possess; in the course of geometry and in the geometrical text-book they should be presented in the clearest form. In the book, here reviewed, it is a little disconcerting to find that the definition of ratio is first introduced in the midst of a subsection which is headed Measurement of Angles, while in spite of its position, the earliest illustrations of ratio are chosen not from angular, but from linear magnitudes. The words in which the authors carefully weigh their final decision must be given, in order that those who are not familiar with the modern geometrics may have an opportunity of judging how far Euclid has been improved upon by his modern rivals. Here is the sentence which summarises our authors' views upon a most important and difficult conception. "The numerical measure of one quantity divided by the numerical measure of a second quantity of the same kind, provided the same unit has been used in each case, is called the ratio of the first quantity to the second." The statement is so comprehensive and is so carefully worded that one regrets that it fails so completely to be a definition. It reminds us of a statement made by a prominent champion of geometrical reform who once wrote that the test of the utility of a proposition was that it could be used. Here we have a more complicated fallacy, for, omitting unnecessary verbiage, and avoiding the undefined words "division" and "measure," the only way in which the definition can be stated is in the following terms: the ratio which the ratio of one quantity to a certain quantity, called the unit, bears to the ratio of a second quantity to the same unit, is called the ratio of the one quantity to the second quantity. The word to be defined must be used no less than three times before the definition is complete. If teachers of geometry upon the modern lines will turn to the definitions given to their pupils in the text-books which they use, they may discover one of the reasons why their scholars are finding the new geometry a difficult subject. Once more it is the desire to make the subject practical which has led the authors into such serious error. They might with advantage reflect upon the danger of mixing; we all know that there is an excellent drink called water and one perhaps equally excellent, but certainly less accessible, called Château Lafitte, and it is generally acknowledged that one whose taste is unspoilt can enjoy either of these beverages; but what can be the state of the man's palate who deliberately mixes the two? The most charitable view is that he hardly appreciates the excellence of either when taken separately.

Too little has been said of the special features of the book. It has many good points: it covers both plane and solid, and it abounds in instances of apt illustration. Teachers who desire a text-book upon the so-called modern lines will find much that will please them between its covers.

C.

Analytic Geometry and Principles of Algebra. By ALEXANDER ZIWET and LOUIS ALLEN HOPKINS. [Pp. viii + 369.] (New York: The Macmillan Co., 1913. Price 7*s.* net.)

THIS book is an attempt to unite—more completely than is usually done—elementary analytical geometry of two and three dimensions with the portions of algebra which are needed for the discussion of the straight line and conic in two dimensions and of the plane and quadric surfaces in three dimensions. It really matters little whether stress is laid upon analytic geometry or upon algebra; the important fact is that the two subjects should not be divorced, and their union in this book is welcome.

Algebra is for the student often a collection of loosely jointed subjects: in this book he will learn at least the true place of some algebaric forms. The chapter on complex numbers is out of place; it forms part of another set of ideas, and its presence suggests difficulties in connection with geometry which are not discussed. It seems a pity also that the authors did not assume a knowledge in the reader of differentiation of algebraic functions. One result of modern changes for the better in teaching is that such a course is now possible; timid authors who cater for the old and the new schools of teaching retard a wise and progressive movement. The discussion of algebraic curves, short as it is, is a step in the right direction; it would have been better to have continued it further and given a discussion of asymptotes and singular points. Room for this extra could have been found by the omission of Part II. of the chapter of which Part I. is algebraic curves; the treatment of transcendental curves is in no way germane to the general lines of thought of the book; these curves are introduced apparently to satisfy that most insatiable of all readers—the practical man.

The book is well written, clearly illustrated, and will be of great service to the large class of students who find in mathematical books on analytical geometry too much and too little.

C.

An Elementary Treatment of the Theory of Spinning Tops and Gyroscopic Motion. By HAROLD CRABTREE, M.A. Second Edition. [Pp. xv + 193, with illustrations.] (London: Longmans, Green & Co., 1914. Price 7*s*. 6*d*. net.)

THIS is the second edition of a very attractive book, and is considerably larger than the first edition, which appeared in 1909.

The most notable additions are discussions on the motion of the Gyro-compass and on the rising of a Spinning Top. After elementary explanations in the first part of the book, these two subjects are treated with considerable mathematical detail in two appendices of twenty-eight and fourteen pages respectively. In the former is given an adaptation of Schuler's investigation, while in the latter the results of Jellet and Gallop are obtained. Other additions are sections on the recently invented mono-rail car of Schilowsky, on the swerving of a "sliced" golf ball, and on the drifting of projectiles, together with two plates and an index to the whole. The sections on Brennan's mono-rail car have been altered.

The matter has been arranged with great care and presented with clearness, while all parts of the book are illustrated by numerous and excellent diagrams.

A special feature is the repeated discussion of the same problem from different points of view. This should tend to promote a fuller understanding of the subject by that class of readers for whom the first edition was said to be intended—viz. the abler mathematicians at our public schools and first-year undergraduates at the universities. As the book now stands, however, it is only the first part that need, or perhaps can, be regarded as intended for such a class of readers, as many of the subjects treated are not found in the text-books on Rigid Dynamics usually used by more advanced mathematical students, while the latter part contains mathematics which is beyond the average first-year undergraduate.

ASTRONOMY

Stellar Movements and the Structure of the Universe. By Prof. A. S. EDDINGTON, M.A., F.R.S. [Pp. xii + 266.] (London: Macmillan & Co. Price 6*s*. net.)

IN this book, which forms the eighth in Macmillan's excellent series of Science Monographs, the author gives an account of the many recent advances in our knowledge of the sidereal universe. There are many causes which have contributed to the rapid advance within the past few years of our knowledge of stellar statistics, upon which any discussion of the structure of the stellar universe must necessarily be based. A most important factor has been the application of photography to astronomy; the development of and great advance in photometric methods has led to the accurate determination of the photographic magnitudes of many stars, giving valuable data upon which to base star-counts; it has provided a method by which the proper motions of stars may be accurately derived from photographs taken with the comparatively short interval of ten or twelve years; it has enabled stellar parallaxes to be determined with much more precision and with far less trouble than by visual measurements, and, by so doing, has greatly increased our knowledge of the distances of individual stars; it has enabled stars to be easily classified according to the type of their spectra; and, with the aid of the spectrograph, has been the means of bringing about a very rapid increase in our knowledge of the radial velocities of stars—not a single radial velocity being known with accuracy twenty-five years ago. Also there has been a great increase of our knowledge of the proper motions of stars, due merely to lapse of time, and these furnish the best data for the determination of the average distance of groups of stars.

After a brief account of the data upon which any theory of the structure of the stellar universe must be based—a knowledge of the position in space, motion, luminosity, and spectral type of each star—the author passes on to discuss in some detail the nearest stars, of which our knowledge is unusually full, their parallaxes and in many cases their radial motions being known; this is followed by a chapter on moving clusters, the discovery of which has given a means of calculating, within small limits of error, the parallaxes of a large number of stars. The motions of the stars in general are next considered, and it is explained how, by means of a statistical discussion of the proper motions and of the radial velocities of a large number of stars distributed over the sky, the direction and magnitude of the solar velocity may be calculated. The investigation of the distribution of the proper motions showed that the stellar motions are not distributed at random, but that there exists a systematic irregularity, capable of explanation by supposing —as announced at the British Association meeting in 1904 by Kapteyn—that the stars tend to move in two favoured directions. A clear account is given of the methods by which the directions of the two star-streams relative to the sun, their relative velocities and distances, and the distribution of the stars between them have been determined. Schwarzschild's alternative explanation of the phenomenon—that there is one direction in which the stars have greater mobility than in perpendicular directions—is compared with the two-stream theory, and the mathematical theory of each is briefly and lucidly given.

The next chapter contains an account of the phenomena associated with the spectral type. Although the average proper motions of the stars, when grouped according to type, in the order which is supposed to be that of stellar evolution, pass through a maximum with type F stars, and then decrease, it was announced in 1910 by W. W. Campbell, as a result of a study of the radial velocities of a large number of stars, that there is a progressive increase in the average linear velocities. This remarkable result has been since confirmed by Boss and others from discussions of the proper motions, and has given rise to several tentative but interesting explanations. Then follows an account of Russell's theory of "giant" and "dwarf" stars, of spectral type M, which supposes that this class can be subdivided into two, not in reality related, the one subclass consisting of very luminous, and the other of feebly luminous stars; the evidence for and against this theory is discussed. Other remarkable phenomena associated with the spectral type are also brought under consideration.

The utility of star-counts, and their value as data in the general statistical investigations which aim at determining the density, luminosity, and velocity laws in the idealised stellar system, is explained. Other data which are necessary are derived from the proper motions of stars within different limits of magnitude, and from the parallax determinations. In these investigations it is assumed that there is no absorption of light in space. Although this is almost certainly not true, the amount of absorption must at most be very small, and its magnitude is so uncertain that no useful purpose would have been served by introducing it. The various methods of deducing the three fundamental laws from the observational data are carefully explained.

In the last chapter of the book is sketched an attempt to found a dynamical theory of the stellar system, the starting-point being the knowledge derived from the study of moving clusters.

The author has gathered together in this book, and weaved into a connected whole, the results of numerous researches, both by himself and others, which hitherto were only to be found scattered about in various scientific journals; the results of the most recent researches available up to the time of going to press have been incorporated. Although fresh facts are continually being discovered and our knowledge is increasing rapidly; and although the new light which will in the course of time be thrown upon many of the points discussed may lead to a recasting of our ideas, it is well to have a logical presentation of the advances of recent years by one who has contributed so much to them, and a survey of the present state of our knowledge. The book will be of great value not only to the student, but also to the original investigator who needs to obtain information upon any particular point; whilst the valuable list of references and bibliography given at the end of each chapter indicate where fuller information may be obtained by those who desire it. The book, however, should appeal to a wider circle than the student and investigator, for the general scientific reader will find the information presented in a clear and simple manner that will appeal to him, nearly all of the mathematical matter which it was necessary to include having been collected into two chapters, which can be omitted by him with but little detriment to the general argument.

One misprint may be noted: on p. 158, table 24, "kms. per sec." should head the second column instead of the third.

H. S. J.

PHYSICS

The Theory of Relativity. By L. SILBERSTEIN, PH.D. [Pp. viii + 295.] (Macmillan & Co. Price 10*s*. net.)

THE modern theory of relativity was propounded in its essentials by Einstein in 1905, and won immediate recognition on the Continent: nevertheless, the first English book on the subject has only recently appeared. The theory was enunciated to deal with the problems connected with the propagation of electromagnetic disturbances—light—in moving media, which had formed one of the chief fields for experimental and theoretical researches at the end of the nineteenth and the beginning of this century. Neither the equations of Maxwell, as modified by him for a moving ponderable medium, nor those of Hertz and Heaviside, gave results in agreement with experiment ; for the work of Fizeau, and later Michelson and Morley, showed that a moving medium imparted in virtue of its motion to the light travelling through it—or to the contained æther, on the æther hypothesis—not the full velocity of the medium, as demanded by the Hertz-Heaviside equations, nor a velocity about half that of the medium, as required by Maxwell's modifications, but one determined by the index of refraction of the medium, according to Fussel's well-known expression, which could not be deduced on the basis of the older electromagnetic theory. H. A. Lorentz, in his famous essay, with the aid of his hypothesis of "ions," or electrons as we should now say, fixed in the ponderable body, deduced the right value of this coefficient ; but to explain the results of the other experiments of Michelson and Morley, which showed that the measured velocity of light from a terrestrial source was independent of the direction of propagation with respect to the earth's motion—that is, independent of the motion of the source—Lorentz had to introduce the *ad hoc* hypothesis of a slight contraction of a solid body in the direction of its motion through the æther, the so-called Lorentz-Fitzgerald hypothesis. This most artificial assumption was accepted for want of a better : it is a merit of the relativity theory that, even if its fundamental concepts be very difficult of acceptance, once they are accepted it gives an account of all positive and negative experimental results, without special assumptions for special cases, and it introduces symmetrical transformations of the widest application.

With a historical introduction on these lines, tracing the origin and growth of the problems of the propagation of light in moving media, and the various attempts to solve them, and showing the relativity theory to be the outcome of attempts to explain experimental results, Dr. Silberstein occupies the first chapters of this *Theory of Relativity.* This introductory treatment goes far to make the theory comprehensible, and to show the need of it; it is excellently carried out. He then proceeds to deal with Einstein's conception of simultaneity, based on the transmission of light signals for co-ordinating time systems in different places. Two events in different bodies, in general moving relatively to one another, are defined as simultaneous by the aid of the postulate that the time of passage of the light from one body to the other, and of its return, shall be equal, time of arrival and departure from the second body being measured at that body. This principle is of fundamental importance; as the author says, "To have initiated a critical analysis of the conception of simultaneity at all is certainly a great merit of Einstein's." From it, together with the second postulate that the measured velocity of light is independent of the relative motion of the source, can be deduced the so-called "Lorentz transformations," which transform the laws of physical phenomena invariantly from one system to a second moving with any relative translation. One of the first results of the theory is the reduction of the æther to nothingness. For as no system has any preference over any other with respect to the propagation of light, we are left with a choice between no æther at all or, since every body may be considered as having an equal right to its own æther, a triple infinity of æthers—one supposition being as comforting as the other.

Passing on to consider various representations of the Lorentz transformation, the author gives an account of Minkowski's celebrated work, showing that the transformation corresponds to a rotation in a four-dimensional space, the fourth dimension being a modified time. He gives an exposition of Minkowski's matrices, and adds a most interesting and original account of a quaternionic method of dealing with the "four vectors" involved, demonstrating the convenience of quaternions in this case. Some of the most remarkable results of the relativity theory are contained in the chapter on the composition of velocities. The theory does not modify by appreciable terms our classical Newtonian formulæ as long as the ratios of the velocities in question to that of light are small, but when these ratios approach unity entirely new formulæ must be used for compounding velocities, which are such that if an observer measures the velocities in opposite directions of two different bodies relative to himself, no matter how near the velocity of light these may be, the velocity of the one body as measured by an observer on the second will still be less than that of light. In fact, a relative velocity greater than that of light is impossible, in so far as it cannot be dealt with by the relativity theory, which is based on the use of light signals.

There is much more of the greatest interest in the book, to which reference cannot be made here for want of space. The vector methods exposed in the author's book on vector mechanics, recently reviewed in these columns, are used with great lucidity, and the construction and style of the book make it very attractive to read. Far more comprehensible, if less comprehensive, than Dr. Laue's book, which has only appeared in the original German, Dr. Silberstein's work is, we think, the badly needed book which is to make the theory of relativity accessible to the large number of physicists who wish to obtain a clear idea of its scope and achievements without devoting themselves exclusively to this subject.

E. N. DA C. A.

Photo-Electricity. By A. L. HUGHES, D.Sc., B.A. [Pp. viii + 144.] (Cambridge University Press. Price 6*s*. net.)

SINCE Hertz in 1887 showed that a spark passed between metallic terminals more easily when the negative terminal was illuminated, and Hallwachs supplemented this observation by the discovery that a negatively charged body rapidly loses its charge when illuminated with ultra-violet light, the study of the liberation of electricity by light—photo-electricity—has received the attention of a very large body of physicists, including some of the most distinguished names of the period, and has afforded one of the most baffling problems of modern electrical science. A short time ago Dr. Allen summarised the work done in this region in a book reviewed in these columns, and there has recently appeared a book by Dr. Hughes, now under review, devoted to the same subject.

Dr. Hughes has himself carried out important investigations in this field, and brings to his task a very complete knowledge of the literature of the subject and of the peculiar difficulties of its problems. This is fortunate, for without an intimate acquaintance with the technique of the experiments it is hopeless to attempt a clear exposition in the face of so much indeterminate and contradictory work as has been carried out in photo-electricity. He considers first the ionisation of gases and vapours by ultra-violet light, and rightly points out that the interpretation of results would be much easier in this case than in the case of solids, if only the experimental difficulties connected with the extremely short wave-lengths involved could be overcome. This is perhaps the most promising field for obtaining definite results. This chapter gives an excellent account of the photo-electric effect in gases, which was badly needed in English. Then follow chapters on all the main problems connected with solids—the velocity of emission of the liberated electrons, the effect of varying the frequency and state of polarisation of the light, the effect in thin films, in non-metals, and in fluorescent and phosphorescent substances. The final chapter deals with the sources of light used, and resumés much information extremely valuable to the practical worker. In the course of the book one meets with interesting points which have escaped the notice of English physicists, such as Pohl and Pringsheim's suggestion to account for the increased sensitiveness of the colloidal modification of metals, and the isolated experiments of Dernber on the production of *positive* rays by light. The style throughout is clear and concise.

Unfortunately the work of Freydenhagen and Küstner, which has pointed to a very great reduction of the effect by the special treatment of the metals in high vacua, appeared too late to be mentioned in the book. Whatever may prove to be the correctness of the interpretation placed upon these experiments, there is no doubt that the nature of the phenomena involved in the photo-electric effect is very obscure. Dr. Hughes gives an excellent account of all the work done up to the end of 1913: extensive and painstaking as it has been, the subject still seems to await decisive experiments which enable us to put a satisfactory interpretation on the facts already known.

E. N. DA C. A.

TELEGRAPHY

Text-book of Wireless Telegraphy. By RUPERT STANLEY, B.A., M.I.E.E. [Pp. xi + 344, with illustrations.] (London: Longmans, Green & Co., 1914. Price 7*s*. 6*d*. net.)

A BOOK on wireless telegraphy that begins with the sentence, "In its first state the earth was a mass of gaseous matter or nebulæ at a very high temperature, revolving round the sun," is a book of a new type. The author recognises that, in its latest development, the study of radio-telegraphy involves a knowledge, not merely of electrotechnics, but of the constitution of the earth's crust and of the atmosphere that surrounds it; that the study of the problems of wireless telegraphy has for its laboratory the entire globe; and that wireless signalling is a subject which, while of fascinating interest to the electrical engineer, opens up possibilities of the examination of the nature of the upper atmosphere which, hitherto, have hardly been realised. The early chapters, after the opening one, are such as might be found in any text-book on electricity and magnetism; unfortunately for the student they are seldom found there. The chief electrical and magnetic phenomena are dealt with from the standpoint of the electron theory, now accepted as the most likely explanation of all electrical action, and the result is a clear statement which should make them intelligible even to those who may not have had much scientific training. The section dealing with "How ether waves are propagated and received" is an admirable statement of the theories now recognised as the best explanation of the observed effects. In this chapter it is stated that "The whole question of ether wave propagation is one of the outstanding problems of radio-telegraphy," a statement with which every worker in wireless telegraphy will agree. The author is to be congratulated on his lucid account of the causes of the phenomena that have been observed, as far as it is possible to treat them at present. The chapters dealing with the coupling of circuits and of the systems of transmission now in use are simple and adequate, and easy to be understood by those who have no great knowledge of mathematics. The book can be safely recommended to those who wish to gain a knowledge of the salient features of a wireless telegraph transmission, but who have not been able to obtain the scientific equipment to enable them to make it a study of professional importance. It is not a book, however, merely for the amateur. It contains information about such recent developments as the Lieben and Reisz current relay, and the continuous wave system which Marconi has developed during the last year or two, which should be useful to all wireless engineers. It fills a gap between the classical works of Fleming and Zenneck and the popular handbook which sets out to explain to an amateur how he may work a wireless station at home with the smallest possible expenditure of time and money. Now that amateurs have been forcibly prevented from continuing their experiments by the war, it is to be hoped that the many enthusiastic people, who have expended their energies on wireless stations in their homes, may take advantage of the opportunity this book affords them, of gaining a scientific appreciation of the phenomena they have observed.

CHEMISTRY

Spectrum Analysis. Applied to Biology and Medicine. By the late C. A. MACMUNN, M.A., M.D. With a Preface by F. W. GAMBLE. With illustrations. [Pp. xiv + 112.] (London: Longmans, Green & Co., 1914. Price 5*s.* net.)

THERE is considerable pathos in this scientific work. The late Dr. Macmunn was one of the many enthusiasts who take up various branches of science as amateurs, but nevertheless succeed in developing them to a large extent. He was born in Ireland in 1852, was trained at Trinity College, Dublin, and was engaged in an arduous medical practice at Wolverhampton. In spite of this he had time to devote his leisure to the subject of this book—which is not exactly at the centre of clinical work. In addition, he was an enthusiastic volunteer and served with distinction in the South African war, being noticed in dispatches. Largely as a result of this, his health broke down, and Professor F. W. Gamble, who gives an excellent preface to the work, thinks that his death in 1911 was probably traceable to the effects of the campaign. Dr. Macmunn was made a county J.P. and a Life Governor of Birmingham University. The salient point of his researches was pressed upon the theme of animal and vegetable pigments. "No doubt," says Professor Gamble, "the professional physiologist will find ground for criticism in regard to Macmunn's treatment of certain problems of chromatology; but when his isolated position is considered, his scanty leisure, his want of laboratory equipment, it is not the occasional incompleteness of treatment that we wonder at, but at the fact that under conditions that ordinarily occupy the whole life of a medical man, Macmunn not only fulfilled the duties of a magistrate and a colonel of the Territorial Force, but found time and energy to publish original observations which have always to be reckoned with in the medical and biological study of pigments." As it is, the little book will be useful to all medical men. It is lucidly written and gives very complete information, not only regarding the spectroscope but about pigments—about which doctors should have certainly some knowledge. In addition, the author has certainly added details of considerable value to our knowledge of the subject.

Chemistry and its Borderland. By A. W. STEWART, D.Sc. [Pp. ix + 314. With 11 illustrations and 2 plates.] (London: Longmans, Green & Co., 1914. Price 5*s*. net.)

WE hear much in these times of the great opportunity which has come to British commerce, of taking over numerous industries in which Germany has hitherto had a prominent part or a monopoly. Although many of the industries which have been named are within the immediate powers of our manufacturers to develop, a considerable proportion of them are so much the outcome of German chemical research, that without a full understanding of the relations between research and trade it would be impossible for us to include them among our spoils of war. This is, of course, realised by our industrial authorities; but to judge from newspaper correspondence, not a few of the public seem to think that all that will be required is to subscribe capital, erect plant, and go full steam ahead forthwith. Let those who are of this way of thinking read Dr. Stewart's book, and they will get some idea of the extent of their fallacy. They will learn that if they want new industries they must get into touch with pure science; that if a nation says in effect, "What is the good of research?" it will be answered, in a most distasteful manner, by some other nation which is more enlightened.

And if any of these readers are already convinced on these points, they will find great pleasure in following the author's delightful accounts of the ramifications of chemistry in all sorts of directions. His method of exposition, and his striking similes, bring the recent main advances of chemistry within the grasp of any layman of good education, and there is not a teacher nor a researcher who will not gain something as he reads.

The book would be valuable alone for the sake of the discussion of the present state and the promotion of research; and it may give a fillip to the growing feeling of scientific workers throughout the country that it is time that their labours met with a more adequate requital from those who profit by them.

Methods of Quantitative Organic Analysis. By P. C. KINGSCOTT, D.I.C., A.R.C.Sc., A.I.C., B.Sc. (Lond.), and R. S. G. KNIGHT, D.I.C., A.R.C.Sc., A.I.C., B.Sc. (Lond.), (Carnegie Research Scholar). [Pp. x + 283, with diagrams.] (London: Longmans, Green & Co., 1914. Price 6*s*. 6*d*. net.)

THE authors do not state whether this book is intended for the use of students or as a book of reference for advanced workers, but it is certainly very difficult to imagine a student learning how to perform a combustion from the account given on pp. 25 to 27. Apart from the somewhat unusual place assigned according to the diagram on page 26 to a "spiral containing fused lead chromate or silver gauze," the account given is far too scrappy, not to say racy, and describes the completion of the operation in the following words—"At the end of this period, the absorption tubes are removed, disjointed! (the note of exclamation is ours), and the ends of each absorption vessel are protected by caps." Passing on to the estimation of nitrogen by Kjeldahl's method, the reader is instructed to heat a known weight (without any indication as to the quantity to be taken) of substance with 20 c.c. of concentrated sulphuric acid over a very small Bunsen flame nearly to boiling point for about an hour, and then to "add about 8 gms. of powdered K_2SO_4 and heat again for about $\frac{3}{4}$ hour." There is no suggestion that the heating should be continued only so long as is necessary to render the liquid a light straw colour and the addition of copper sulphate or mercury is not described as a routine operation, but would appear only to be necessary in certain specified cases. The statement that during the process of distilling over the ammonia into the standard acid the caustic alkali should be added from time to time is of course ridiculous.

The style throughout the book is extraordinary. What, for example, is to be said of the statement, "In such cases it is necessary to put on a blank," by which is presumably meant that a control experiment should be performed? A good many references to original papers are given, but although a list of abbreviations is given the abbreviations actually employed in the text rarely coincide with those given in the list, and several journals are quoted which find no mention in the list; moreover, there is a complete lack of system about the quotation of dates; sometimes they appear before the volume and page and sometimes after, but as often as not they do not appear at all.

The Raw Materials for the Enamel Industry. By JULIUS GRÜNWALD, Dr.Ing. Translated by H. H. HODGSON, M.A., B.Sc., Ph.D. With 21 Illustrations. [Pp. viii + 225.] (London: Charles Griffin & Co., 1914. Price 8*s*. 6*d*. net.)

THE production of enamelled utensils made of cast and sheet iron forms a sort of connecting link between the ceramic and iron trades. The art of enamelling is of great antiquity, but its application in the industrial, as distinct from the artistic, world is quite modern, and has become important only during the last fifteen years. There are now, or were before the war, over forty thousand persons employed in Germany and Austria in enamelling iron objects, and possibly one or two thousand in this country. The importations from Germany are very large. The industry is therefore one to which attention may well be directed at the present time, and the appearance of this book is timely in the highest degree. It completes Dr. Grünwald's trilogy on the subject, the other two translations being *Enamelling on Iron and Steel* and *The Technology of Iron Enamelling and Tinning*. Dr. Grünwald is one of the foremost experts in enamelling technology.

The significance of the recent change in the industries in Germany, from the period when old empirical methods were employed to that in which the operations are conducted by scientifically trained men, is typified in enamelling. It is already clear that a similar change is taking place in this country, and the improvement in technical literature is one of the signs of the change. The present book is an excellent example of what is required. It gives a clear and detailed account of each of the new materials used—over twenty in number—its composition, preparation for use, and source of supply. The difficulties caused by variations in the chemical composition or physical condition of the constituents of enamel are discussed and remedies given. The author, however, admits that many problems concerning enamel manufacture still await solution.

The book will be useful not to enamelling works alone. It will also be a handy book of reference in the ceramic industries, in which the same raw materials are used with few exceptions. The translation appears to be a good one, and the book is well printed and easy to read.

T. K. ROSE.

The Metallurgy of the Non-Ferrous Metals. By WILLIAM GOWLAND, F.R.S., A.R.S.M., M.I.M.M., etc. With 195 illustrations. [Pp. xxvii + 496.] (London: Charles Griffin & Co., 1914. Price 18*s*. net.)

METALLURGICAL literature is enriched by the publication of this excellent text-book, the product of the long experience and ripe judgment of a man who may justly be regarded as the *doyen* of scientific metallurgy in this country. It would have been a distinct loss to the industry if Professor Gowland had allowed the opportunity to be lost of putting the results of his life-long studies on record.

The book gives an authoritative statement of modern practice in the extraction of metals from their ores, and a discussion of the principles and conditions on which the success of the processes depends, with other information necessary for completeness. It is concise, mainly owing to the discrimination used in omitting details which are not essential, but partly from the clear and simple language chosen. The chapters dealing with gold, silver, copper, and lead form the greater part of the book, and in these everything of importance has been dealt with. It must have cost the author something of a struggle to omit all reference to obsolete processes and matters of historical interest, considering his services to archæology, but his rule in this respect has evidently been rigid. The other common metals—zinc, tin, nickel, aluminium, mercury, and the rest—are treated in a more summary fashion, and the account affords little more than an adequate framework for the acquisition of more detailed knowledge of these subjects.

If any part of the book were selected for special praise, it would be the section on copper, on which Professor Gowland can speak with almost unique authority, but the general accuracy of statement is well maintained throughout. Nothing has been observed which can fairly be called a mistake, the few remarks to which exception can be taken referring to matters in a state of the most rapid progress, in which nothing but a weekly bulletin could be strictly up-to-date. Professor Gowland apparently does not take seriously the development of the counter-current decantation method in the cyanide treatment of gold and silver slimed ore, a method which has progressed of late, owing to the litigation in connection with slime filtration. Then on p. 217 the statement is made that "at Cripple Creek practice is divided between rolls and ball mills." This requires qualification. Again, on the same page, "with dry crushed ores percolation is easy" is a statement needing a little expansion.

The book may be confidently recommended to all students of metallurgy, and nowadays every man engaged in practical metallurgical work is of necessity a student.

T. K. Rose.

Minerals and the Microscope. By H. G. Smith, A.R.C.S., B.Sc., F.G.S. [Pp. xi + 116.] (London: Thomas Murby & Co., 1914. Price 3*s.* 6*d.* net.)

It is a pleasure to record the advent of this most useful little volume. All teachers and students of the subject, which finds an extensive application not only in mineralogy and geology, but also in metallurgy and mining, will feel grateful to the author for filling so successfully what has hitherto been a most unfortunate gap in mineralogical literature. Teaching experience in this subject has consistently proved that while a course of lectures can do much to smooth the way for the beginner and inspire him to overcome his early difficulties, yet so new to most students are the principles involved, and the manifold facts to be correlated, that it becomes necessary to turn to a reliable book for amplification and guidance. A number of large treatises of vast erudition and undoubted accuracy have been published in recent years, but in the hands of the inexperienced student they are apt to confuse and discourage rather than to help. For the first time a small, clearly written book is available, planned out by an authoritative and experienced teacher to meet the needs of those commencing a difficult study.

The first part of the book deals with the optical properties of minerals, and is undoubtedly the most lucid description of that fascinating subject which has yet been written. The characters of minerals as seen in thin section under the microscope are stated and explained in turn (1) in ordinary transmitted light, (2) in reflected light, (3) with the lower nicol inserted, (4) with both nicols inserted, and (5) with both nicols inserted in convergent light. Next follow detailed descriptions of the chief rock-forming minerals, each of which is admirably illustrated by one or more photographs. There are also beautiful photographs of interference figures, and a very successful reproduction in colour of Newton's scale as exemplified by a quartz wedge between crossed nicols. Mr. Smith is to be congratulated both on his skill as a photomicrographist and on his choice of material. The book concludes with two sections dealing with the rapid determination of refractive indices, and with a short introduction to petrology. Altogether we have nothing but praise for a wholly admirable book which will lighten the task of many a student and demonstrator, and which ought to be in the hands of all who are making their first acquaintance with microscopical mineralogy and petrology.

ANTHROPOLOGY

Wookey Hole: Its Caves and Cave-dwellers. By HERBERT E. BALCH, F.S.A. [Royal 4to, pp. xiv + 268, with 36 plates and 55 text-figures.] (Oxford University Press, 1914. Price 25*s*. net.)

THE caves of the south-west of England, such as "Kent's Hole," Torquay, the Cheddar Caves, and the so-called "King Arthur's Cave" on the Wye, have been fruitful in relics of early man; and the small "Hyena Den" at Wookey Hole itself, which was explored by Prof. Boyd Dawkins, was one of the first caves in this country to be opened up scientifically. The great cave at Wookey Hole, where the river Axe flows forth from its subterranean channel, two miles from Wells, has long been famous, and has been regarded with superstitious awe by the country-folk for centuries. In the seventeenth century certain vandals destroyed much of the beauty of the cave by cutting down and carrying away many of the stalactites which decorated the larger chambers. Yet, in spite of this, it has remained for Mr. Balch to open up the inner recesses of the immense cavern, and to reveal to us the most interesting story of an ancient tribe who made their abode there. The great cave is only a few yards from Boyd Dawkins' "Hyena Den," but the cave-dwellers, whom it is the author's main purpose to describe, have no connection with the so-called "cave-men" of the Late Pleistocene. They are a much more recent people who inhabited the cave during the Prehistoric Iron Age and throughout the centuries of the Roman occupation of Somersetshire, and who are obviously closely related to the inhabitants of the neighbouring Lake Village at Glastonbury.

Mr. Balch has been systematically exploring the caves for several years, and much of the work has been of a laborious and even dangerous character, for many of these old water-channels are very difficult of access. In particular, the descent of a large and deep swallet which exists at Eastwater is evidently a feat which only expert rock-climbers could have accomplished. The chief work, however, was in excavating the floor of the great cave near its mouth, where the rich accumulation of Iron-Age relics was found. Numerous pieces of pottery, iron swords, saws, sickles, and other iron instruments, bronze implements, worked stones and bones, remains of human and other skeletons, Roman coins, and many other objects were dug up, and careful observations were made of the exact positions in which the various specimens were found. The work thus accomplished is one of the most valuable contributions that have ever been made to our knowledge of the Iron Age in the West of England, and may be compared with the elaborate researches on the site of the Glastonbury Lake Village carried out by Bulleid and St. George Gray.

The book opens with a long preface by Prof. Boyd Dawkins, who was, as already stated, a pioneer in cave-hunting in this very district. It must be said, however, that some of the statements in this preface are highly questionable, and others are quite misleading. For instance, the discredited terms "river-drift man" and "cave-man" are retained (the river-drift men being made to include the Mousterians, who derive their name from the famous cave Le Moustier), and the reader is led to suppose that all the Late Paleolithic hunters resembled the Eskimos, although it is now known that some of the Late Paleolithic types were

A vessel of the Early Iron Age settlement.

This is one of the oldest vessels found, and H. E. Balch believes it was probably brought from Armorica by the settlers.

(Reproduced by kind permission of H. E. Balch,)

utterly unlike that boreal race. The author's nine chapters deal successively with the general conformation of the group of caves, with the Iron Age and Romano-British settlement, the Neolithic and Bronze Age peoples of the neighbourhood, the Paleolithic relics, with certain ramifications of the cavern that he has recently explored, with the strange noises of the cave (which have been famous since the days of Clement of Alexandria, and are caused by the escape of imprisoned air), with the Eastwater swallet, with the historical references to Wookey Hole, and with the geological features of the locality. The whole book is profusely illustrated, many of the drawings and three "period-restorations" being by John Hassall, R.I. Most of the plates are good, but some of the representations, especially those of the bones, are on much too small a scale. Hassall's two restorations of the Iron Age are fascinating, but his third, which is of Pleistocene times, suffers from an over-dose of realism: half-a-dozen quite independent tragedies are being enacted within the space of about an acre !

The second chapter, dealing with the Iron Age and Romano-British settlement, is much the longest (100 pages) and most important in the book, The cave does not appear to have been inhabited during the Neolithic and Bronze Ages, but a tribe acquainted with the use of iron occupied the place several centuries before the beginning of the Christian era, and it was inhabited almost or quite continuously down to the time of the Roman evacuation of Britain. The author thinks this tribe probably arrived about 300 to 200 B.C., certainly not later. For the greater part of the seven centuries a considerable clan lived here, though at one time the only occupant of the refuge was a solitary keeper of goats, perchance the original of the legendary "Witch of Wookey." The cave-folk appear to have been but little affected by the Roman conquest, but certain objects, particularly glass and coins, were more abundant in the upper and more recent layers of refuse. From the time of their first appearance they were in many respects a civilised community. They grew corn on the adjoining hills, had goats and dogs, used oxen as beasts of burden (an iron shoe for an ox was found), worked bronze, lead, tin, and even silver, as well as iron, decorated their pottery beautifully, were skilled weavers, and constantly played games. Apart from the pottery, some of the most remarkable articles found are the weaving combs, made of Red Deer's antler, and the iron saws. The iron specimens are well preserved owing to the exceptional dryness of part of the cave. Yet, in spite of this skill in the arts, Mr. Balch found convincing evidence of cannibalism both in the Pre-Roman and Roman strata, which is another instance of the sociological truism that intellectual progress and moral advancement may be almost completely divorced.

The decoration of some of the earlier pottery is peculiar, but resembles that seen on some of the Glastonbury Lake Village specimens, and is identical with the contemporary pottery of Armorica. This type of pot is quite absent from the south-eastern counties, though somewhat similar specimens have been found at Hunsbury, Northampton. Mr. Balch infers from this that the people of the Lake Village and cave were not invaders from the south-east, but were voyagers from Brittany, who sailed around Cornwall and settled in Somerset, among the less cultured Bronze Age tribes. They were probably of Iberian stock, though no doubt of Keltic speech. If this be the true theory, these Iron Age settlements must of course be sharply distinguished from the Iron Age Brythons who entered the country by the time-honoured Kentish route.

The chapter on Paleolithic relics, largely derived from Boyd Dawkins' finds in the Hyena Den, is much below the standard of the rest of the book, as Mr. Balch is unreliable when he passes outside his own special subject. Much indispensable information is omitted, and on p. 176 there is an extraordinarily misleading story of the geographical changes which are supposed to have occurred at the end of the Pleistocene. There are also numerous inaccuracies in the zoology. The author confuses the uninitiated by calling the great Irish deer the "Irish elk"; on p. 170 there is a reference to "the jaw of an elk," and it is only on finding another mention of the same specimen eighteen pages later that one discovers that the animal referred to is not the elk at all, but the great Irish deer. Boyd Dawkins' list of mammals on p. 186 ought to have been modernised. It is impossible to know which hare is meant here by the name *Lepus timidus*, and we are even given the unpardonable anachronism, *Homo paleolithicus*.

Both text and plates are beautifully produced, but we notice a misprint on p. 56—"the same the same."

The long chapter on the Iron Age relics will make this book indispensable to students of Late Prehistoric times.

A. G. THACKER.

BIOLOGY

Zoological Philosophy. By J. B. LAMARCK. Translated, with an Introduction, by HUGH ELLIOT. [Pp. xcii + 410, 8vo.] (Macmillan & Co. Price 15*s*. net.)

WE would say at once that we cordially greet this translation of Lamarck's great work. There is some justification for the opinion that this modern pioneer in Zoological Philosophy has been as hardly treated by more recent students of the subject as he was by the scientific world in his own day. He lives, in fact, in our minds only as the author of a theory which is now almost universally discredited, while the great advances he made in other branches of zoological science, his single-hearted devotion to science, and his courage, at a time when the views he was led to adopt were in opposition to those held by the powerful religious authorities of his day, have been forgotten.

Lamarck's Zoological Philosophy is indeed a landmark in science, a very remarkable achievement by a very remarkable man. It deserves to be widely read in these controversial days, not only with the object of examining many valuable observations which he made, and which have for long lain hidden in his unread pages or been forgotten, but also for the sake of the example given us of his treatment of controversial matters. In these pages is set forth the broad-minded, generous spirit of the man, his fearless attack on what he deems to be superstition hampering truth, his bold effort to transform a dead into a living science, and, with all, his clear recognition of the fleeting value of those great advances which he had himself made in the science he loved. He gave his life and all his energies to this work without a thought of honour, without a hope of recompense.

It was his experience of teaching, he tells us in his Preface, which made him feel how useful a philosophical zoology would be, and this "sketch," as he modestly calls it, was designed "to help me in teaching my pupils; nor had I any other aim in view." And useful it must indeed have been, shedding a flood of light especially upon those lower animals which had for so long been neglected, correlating structure and function, welding together the sciences of comparative anatomy and physiology, and illuminating the whole with a vivid imagination which could not fail to arouse enthusiasm in all but the most stupid minds.

In the writer's opinion the divorce of physiology from morphology in more modern days has, until recently, seriously interfered with the advance of both these branches of animal biological science. The teaching of any subject which is concerned only with structure becomes rapidly stereotyped. When the principles of structure are once understood by the student, the learning of the details of that science becomes a dry task. The inevitable result surely must be that fewer and fewer students are impelled to specialise in that branch of knowledge, the interest in the subject wanes and the stimulus to research declines.

Lamarck, although primarily a systematist, clearly recognised that it is not so much the structure of an animal as its behaviour which is of importance to the student of biology; that it is the correlation of structure with function which will permanently absorb his attention, which will imbue him with living interest in the organisation and in the fate of an animal or of a species. He found that, after all, it is the broad problems of life, the origin, the growth, and the fate of living things which arrest thought and stimulate research, and he knew that only by welding together the study of all branches of biology could he succeed in achieving that object. The result was that he became imbued with many new ideas; they called loudly for exposition, and he proceeded to deal with them with a vigour and thoroughness which, in view of the facts at his disposal, must ever be remembered as a remarkable advance on the work of those who had gone before him.

Moreover, he did this with marked single-mindedness, with a clear appreciation of the changes which further knowledge and research would certainly bring about. "The thoughts, arguments, and explanations set forth in the present work should therefore be looked upon merely as opinions which I propose, with the intention of setting forth what appears to me to be true, and what may indeed actually be true." "In publishing these observations, together with the conclusions that I have drawn from them, my purpose is to invite enlightened men who love the study of nature to follow them out, verify them, and draw from them on their side whatever conclusions they think justified." Finally he concludes his Preface, from which the above quotations have been taken, as follows: "I shall have attained my end if those who love natural science find in this work any views and principles that are useful to them; if the observations which I have set forth, and which are my own, are confirmed or approved by those who have had occasion to study the same objects; and if the ideas which they succeed in giving rise to, whatever they may be, advance our knowledge or set us on the way to reach unknown truths."

One finds in these extracts the spirit of a great man, prepared for the discovery of new facts, the elaboration of theories which will take the place of his own, anxious to learn whatever they may teach, without fear of results, without anxiety for the permanence of his own views; the spirit of a man with a broad understanding and a generous heart, a lovable man whose whole life is devoted, not to the acquisition of fame, but to the search for truth. One is convinced, therefore, that no matter whether his theories eventually be proved to be true or not, they were honestly conceived and are worthy of our respect.

Mr. Elliot's translation is as nearly literal as possible; he has succeeded in overcoming considerable difficulties in order to reproduce, in language used by modern zoologists, terms which are now obsolete, and has traced a number of generic and specific names used by Lamarck which are no longer recognisable by the majority of zoologists. The judgment shown in these matters gives the reader great confidence that in this translation there can be no mistake as to what Lamarck actually meant, and, as he has been constantly misunderstood and misrepresented, this is a matter of very considerable importance.

But Mr. Elliot has done more than present us with an admirable translation of Lamarck's great book. He has added an Introduction of some eighty pages which must, we think, be considered to be a very valuable aid to knowledge of the man and his work. In order to understand the causes which induced the author to arrive at a conception of the theory by which alone he is remembered to-day, in order to grasp fairly its development and its relation to the thought and work of his contemporaries, such an introduction is of great help. So far as we know, no such serious attempt to accomplish this purpose has hitherto appeared in our own language.

At the same time, while we cordially appreciate Mr. Elliot's work, we are not entirely in accord with his judgment of the author. The immense difficulties under which Lamarck laboured, although acknowledged, are perhaps not sufficiently recognised. The faulty tools with which he had to work, the scarcity of facts at his disposal, the bigotry which surrounded him and under which he must have been brought up, must all have conduced to make the researches which he embarked upon extremely difficult to carry out. Recognition of these difficulties deserves a somewhat warmer acknowledgment, and Lamarck's claim to fame might well have been even more decisively maintained than Mr. Elliot's words convey.

Thus without disagreeing with the ultimate logical conclusions Mr. Elliot arrives at, there is still some room for regret that, while he properly concerns himself fully with the refutation of the author's theories in the light of modern scientific thought and methods, he grants but little room for expression of his great gifts and the use he made of them in dealing with the facts he had at disposal. We feel bound to conclude, therefore, that this introduction, written as it is with scrupulous care, and important as it is as a contribution to the history of the subject, in which Lamarck himself is dealt with more fully than in any other book with which we are acquainted, would, we think, have been of still greater value if the benefits conferred on Biological Science by the author had been more generously set forth.

The book will probably be read mainly for knowledge, at first hand, of the theory of the inheritance of acquired characters, as Lamarck conceived it; not, indeed, because the author himself thought so highly of its value, but because it has occupied such a prominent position in the eyes of younger generations of zoologists, and has played such an important part in the foundation of various theories of heredity which have subsequently been evolved.

In this connection section 5 of the introduction will be found of great value, though here and there, again, we find reason to differ from Mr. Elliot. As an example, the following quotation from p. xlv may be given: "Lamarck committed the error, eminently excusable in the age in which he lived, of assuming that when he has formed a theory which will fit the facts, and when he can think of no other theory which will also fit the facts, then that theory must be true."

Now it would seem very doubtful if Lamarck ever did suppose that his theory "must be true." The character of the man is entirely opposed to that view. The theory was evolved to explain, as best he could, certain facts which he had observed, but there is no evidence whatever that he declined to consider any other theory, and none that he considered his judgment final. As a matter of fact it was the best theory of his day and a remarkable advance on any other.

If an apology for Lamarck be required it is not difficult to find one which is perhaps more in accord with facts than Mr. Elliot is prepared to admit. But is an apology required? Of the many attacks which have been directed against the theory that acquired characters can be inherited, that of Weismann is the most famous, and the essential part of that theory is the base on which is founded all other attacks which now hold the field. According to Weismann, the germ-plasm must be "totally separate and cut off from the body-material or soma-plasm," Mr. Elliot tells us. But, as a matter of fact, it is not so "cut off." The germ-cells grow by virtue of the material supplied to them by the somatic cells which surround them. The protoplasm of the ovum in the ovary is in direct continuity with the protoplasm of certain of the follicular cells amongst which it is embedded; it is thus that the ovum acquires the nutriment which enables it to grow and to develop into a ripe ovum. If then it is true, as there is surely sufficient reason to believe, that changes in the environment induce changes in the somatic tissues, who is bold enough to assert that these changes do not tend to exert modifying influence on the "soma-plasm"; and, if so, on what ground is it denied that a variation so induced does not in any way affect the germ-plasm?

It must be borne in mind that the term "influence of the environment," as here used, includes not only the influence of external conditions on the superficial somatic tissues, but the influence exerted on all somatic tissues which can be affected by such changes as may be induced by variation in the food supplied to the parent body. Such influence may be transmitted either directly through the blood, or indirectly by means of the secretions of various organs (*e.g.* ductless glands), and may exert very profound influence on the growing germ-plasm. The internal secretion of the generative gland itself may be affected by this means.

Is it not eminently possible that the increased power of variation with which cultivated plants are credited may be derived from such a source? Their capacity for wide variation is associated with a great change in the nutriment provided to them by special cultivation of the soil, and may well be largely determined by that means.

To what extent changes in the quality of nutrition conveyed to the germ-cell may affect the growing plasm, it is not possible to say in the present state of our knowledge; but if the effect should be that the development of certain inherent qualities is stimulated while that of others is destroyed, the normal balance of power will thus be definitely changed, and, if so, will be inherited in that changed condition.

Mr. Elliot claims that "a physiological somatic modification can only be caused by a factor which operates for an appreciable proportion of the life of the soma." And on that ground he bases his contention that "the germ-cell which has existed from the most extreme antiquity" must remain under any modifying influence for a proportionately lengthened period in order that it shall be affected at all.

But in view of the results obtained by many experimental embryologists it is hardly possible to accept Mr. Elliot's claim; indeed, it does not necessarily appear to be a fact which can be so stated. Moreover, if the germ-cell has existed from the most extreme antiquity, that is no proof that the germ-plasm is incapable of modification; it is begging the whole question to assume this. Whether a drastic alteration in the relative value of the inherent qualities of germ-plasm requires many generations or not, does not concern us; if it takes place at all, then Lamarck's theory is not yet "entering upon the final stage of oblivion," as his translator is disposed to believe.

It is perhaps permissible to urge, here, that, in dealing with unknown quantities of such extreme delicacy it is premature to assert dogmatically what is and what is not possible in regard to them. Anatomically the germ-cell is not isolated from the somatic cells, the germ-plasm and soma-plasm are in continuity—indeed, it is impossible to say where the one ends and the other begins. Physiologically, therefore, it is impossible to doubt that changes in the constitution of the soma-plasm are communicable to the germ-plasm. To what extent, however, it is necessary for the constitution of the soma-plasm to change in order that it may affect the germ-plasm to an extent which is sufficient to modify its hereditable qualities, is another matter, which still awaits definite proof.

We incline to believe therefore that Lamarck's theory is not yet dead: that it may still "succeed in giving rise to" ideas which may "set us on the way to reach unknown truths."

WALTER HEAPE.

Zoological Philosophy. By J. B. LAMARCK. Translated with an Introduction by HUGH ELLIOT. [Pp. xcii + 410.] (London, 1914: Macmillan & Co., Ltd. Price 15*s.* net.)

FOR many years the inheritance of acquired characters was one of the most eagerly discussed questions, and then, largely through the influence of Weismann and his school, it was almost generally admitted that such characters were not inherited. Recently certain biologists in this country and on the Continent have again been urging that a number of observations tend to show that the changes wrought in a living being by the action of the environment persist even when the causes producing them have been removed. Throughout all this long controversy the name that has been quoted most frequently is undoubtedly that of Lamarck; and it is opportune that at this time, when it appears as if the question is to be reopened, a translation of his famous *Zoologie Philosophique* should appear.

The present volume is a translation, as literal as possible, of this work, preceded by a very serviceable introduction by the translator. As far as can be judged, it follows very closely the original, save for a few rearrangements that in no way affect the sense. The task is no light one, for it must be remembered that the original was published more than a century ago, when the concepts current in science differed widely from those prevalent to-day. This difficulty has been admirably surmounted, so that the reader can now have ready access to the subject-matter without the barrier of an obsolete terminology in a foreign tongue. One or two slips have crept in: *e.g.* "warm" instead of cold on p. 81, "a enquiry" on p. 371, and on p. 237 "Rhedi," although occurring in the original, might have been put Redi in the translation; but these are not of sufficient importance to detract from the value of the work. Of the three parts—zoology, physiology, and psychology—into which the book is divided, the second is the least interesting. It is naturally an explanation of animal functions in the terms of physics and chemistry, but as the outlook of these sciences has changed so fundamentally it is often difficult to appreciate the arguments made use of. The most interesting section is the zoology, for here the main theories are expounded.

To the ordinary reader of biology Lamarck is closely associated with the somewhat fantastic explanations that the giraffe has got its long neck because it wished to reach up to the trees, and that the storks, etc., have long legs as they desire to walk in the water and at the same time object to wetting their bodies. Although these theories, reminiscent of the "Just-so Stories," were undoubtedly put forward by Lamarck, it is, as the translator points out, grossly unfair to iudge him by these alone and not take into account the other parts of his work. They do not even form the main thesis of his theory. He insisted that the whole animal kingdom, "from monas to man," was to be regarded as a complete series, the gaps in which are due to our ignorance of intermediate forms and not to its incompleteness. The series was a linear one, with all the large groups on the main line and the smaller groups constituting branches. Moreover the difference between species were not due to arbitrary acts of creation, but are the result of orderly changes during a continuous development from lowest to highest. In fact he put forward a complete theory of evolution, and this fifty years before the publication of *The Origin of Species*. This in itself entitles him to a prominent place in the history of biology.

Having established the fact of evolution, he proceeds to discuss the causes of it, and of these he finds two. The more important is the innate tendency in all living things to evolve in the direction of greater complexity of organisation. The second is what we now term use-inheritance, and as this originated with him it is naturally the one to which he devotes a great deal of his attention. This was the weak point in the structure, and on it the fire of criticism was directed so strongly that people lost sight of the remainder. It was a poor explanation of the facts available, and the most that can ever be said in its favour is that it was plausible and that by implication it recognises the wonderful adaptation of structure to function.

Besides these theories the book contains notable improvements in classification among other good things; the essential differences between vertebrates and invertebrates are first definitely established; Crustacea and Arachnida are separated from Insecta; annelids set apart from the remaining "worms"; and infusorians (our modern Protozoa) removed from polyps, and recognised as the lowliest members of the animal kingdom. Throughout the book it is repeatedly urged that all problems must be attacked by the scientific method, and this is applied to numerous questions, still being discussed in biology, with a varying degree of success. The many points raised cannot be adequately dealt with here, and the reader is referred to this volume, which may be recommended as a very faithful rendering of the original. It is throughout well written and carried out in an able and conscientious manner.

C. H. O'D.

An Introduction to the Study of Plants. By F. E. Fritsch, D.Sc., Ph.D., and E. J. Salisbury, D.Sc. [Pp. vi + 397, with 8 plates and 222 figures in the text.] (London: G. Bell & Sons, Ltd. Price 4*s.* 6*d.* net.)

To plan an elementary course of instruction in Botany in which the subject-matter is arranged in logical sequence, so that the course shall not degenerate into a disconnected series of lessons, is probably more difficult than in the case of other science subjects. The most obvious cause of this difficulty is an unavoidable one —the seasonal periodicity of plant-life; the teacher must use what materials are available at different times of the year. Again, it is generally realised that if Botany is to remain an educationally valuable as well as attractive subject, form and function must be correlated throughout an elementary course; and since the physiology of plants, like that of animals, depends largely upon chemical and physical processes, a practical acquaintance with the elementary facts of Chemistry and Physics is essential for the proper understanding of the functions of plants. Yet many students begin work in Botany with little or no previous knowledge of these subjects, and the teacher must do what is possible to remedy the defect.

If the planning of a satisfactory course of work in elementary Botany is a difficult task, that of writing a book which shall reflect such a course is very much more so. Professor Fritsch and Dr. Salisbury have succeeded in producing the nearest approach to the ideal elementary botanical text-book that we have encountered. In the preface they make the modest claim that they have gone beyond the usual scope of an elementary introduction to plant-life by including a chapter on the soil and a fairly detailed account of plant-communities. The latter certainly forms a very attractive feature, for in no other general elementary work have we found such a well-written and well-illustrated account of the vegetation of woodlands, heaths, moorlands, ponds, streams, and the sea-coast. But in every part of the book one is impressed by the careful and skilful treatment and arrangement of the subject-matter—the care and skill which make a book easy reading for beginners, while keeping it strictly accurate and scientific throughout. In every respect it is a thoroughly satisfactory introduction to Botany, and if we have any fault at all to find with it, it is that rather too much is done for the student, especially as to illustrations. However, the book is not merely one for students in schools and colleges; it is one that can be warmly recommended to all who are interested in plant-life.

This book will undoubtedly make and maintain a prominent place of its own among the numerous elementary botanical works already in existence, and it certainly deserves to take high rank, for the authors have spared no pains over its production and have in consequence given us a thoroughly good "introduction to the study of plants." The book is extremely well got up, and its price is very reasonable.

F. C.

A History of Land Mammals in the Western Hemisphere. By W. B. Scott, Ph.D., D.Sc., LL.D. [Pp. xii + 693, with 32 plates and 272 other figures.] (New York: Macmillan & Co., 1913. Price 21*s*. net.)

America, and especially North America, has extensive fossil beds, which during the last three decades have yielded an astonishing variety of animal remains. They have been worked at by a number of investigators, and the past history of the continent itself and certain of the animal groups that have dwelt in it can now be outlined with some degree of certainty. In this book we have a review of the present position of mammalian palæontology in the United States by one of its leading workers and most able exponents.

The opening chapters deal with geological and palæontological methods, and a description of the most striking features of the mammalian fauna of the continent to-day. These are followed by a brief account of the various alterations that have taken place in the land area and the changes in the mammalian groups accompanying them.

The larger part of the book deals with the history of the main groups of the mammalia; a most fascinating series of stories, to which the first part of the book furnishes a complete introductory guide. Naturally the various groups are most unevenly represented by fossil remains. It is obvious that solitary forest dwellers or small arboreal forms did not stand as good a chance of preservation as the herbivores that roved the plains in vast numbers. The carnivores, too, although some forms may have hunted in packs like wolves, were mostly solitary, and could never have been as numerous as the animals upon which they preyed. These difficulties, and the obvious imperfection of the geological record, render some of the histories exasperatingly short and incomplete; but, brief though they are, they are full of interest. The accounts are given in a clear and concise manner, and, as the author starts with the forms as we know them to-day, and traces them backwards through less and less familiar ones, they are most easy to follow.

It is remarkable that horses passed through a large part of their development in North America, and yet the immediate ancestors of the horses now found there must be sought for in Europe, as the stock appears to have died out completely. Perhaps more strange is the fact that in all probability the camels also originated there, and did not leave until late Miocene or early Pliocene times. On the other hand, the elephants came from the old world to form a conspicuous feature in the mammalian fauna from late Miocene to Pleistocene times. They originated in the old world, and in tracing their story the author judiciously incorporates the facts of their early development, whose discovery in Egypt we owe to Dr. Andrews.

Much though we know of some families, far more remains to be discovered, and Prof. Scott's book should prove an inspiration to further efforts, particularly to palæontologists in his own land, who owe him a deep debt of gratitude.

The outline of classification given on p. 59 is in the main a good one, but the reasons adduced for separating infra-classes Didelphia and Monodelphia are obsolete. It is stated on p. 58, and again on p. 629, that the Monodelphia are marked off from the lower mammals by the fact that they alone possess a placenta. This is not the case. The author himself admits that in one of the Didelphia, *Perameles*, a placenta is present. Now, if this is so, that alone should be sufficient for him to remove *Perameles* from the Marsupialia, an obviously impossible proceeding. It would appear as if he has accepted on trust a statement made long ago by some one who was not acquainted with the early developmental phenomena in marsupials, and the fact is that there is no reason to regard *Perameles* as exceptional. Many, perhaps the majority, of the marsupials have placentæ. It so happens that this criticism does not alter the classification given, which rests on surer anatomical grounds, but it is quite time that this oft-quoted statement was given up.

The book, as a whole, is exceptionally good, and the illustrations worthy of special note. A copious index and a well-drawn-up glossary allow of ready reference, and enable a reader not versed in palæontological terminology to follow intelligibly the remarkable animal histories that are so well related in its pages. In it are presented a very clear idea of the most important animal migrations, both from without and within the continent itself, that have taken place, and also of the changes that have occurred in the structure of the mammals as the regions once more or less wooded became transformed into open, grassy plains.

It is one of the most interesting, as well as most useful, books on palæontology that have appeared.

C. H. O'D.

Entomology, with special reference to its Biological and Economic Aspects. By JUSTUS WATSON FOLSOM, SC.D. (Harvard). Second revised edition, with 4 plates and 304 text-figures. [Pp. vi + 402.] (Philadelphia: P. Blakiston's Son & Co., 1913.)

THIS second edition of Dr. Folsom's interesting work will doubtless receive a warm welcome from entomological students as well as from the general reader, to whom it is also intended to appeal. Much new matter has been incorporated, notably an entire chapter on the important subject of transmission of diseases by insects; a few new text-figures have also been introduced and the bibliography has been considerably extended. The opening chapter, relating to general classification, is purposely brief, owing to the existence of many excellent works on this subject. The system adopted, however, is essentially that of Brauer with certain modifications by Packard. The Anatomy and Physiology of insects are then dealt with in an effective and interesting manner, being succeeded by an account of their Development, both embryonic and post-embryonic. The three following chapters (IV, V, & VI) are concerned with the Adaptations of Aquatic Insects, Colour and Coloration, and Adaptive Coloration. Chapters VII, VIII, and IX are of a more economic nature, and are entitled respectively Insects in Relation to Plants, Insects in Relation to other Animals—in which prominence is given to their connection with birds—and Transmission of Diseases by Insects. The last-named subject, sparingly dealt with in the first edition, covers 19 pages and is divided into sections relating to the diseases under consideration. The subject matter, however, although one of the chief additions to the revised volume, is unfortunately hardly up to date; for instance, under the section Trypanosomiases, no mention is made of the transmission, by *Glossina morsitans*, of the Rhodesian form of sleeping sickness, although this was shown to be the case as early as March 1912. The genus *Glossina*, also, is stated to contain only eight species, whereas, at the end of 1912, at least fifteen valid species were recognised. Interesting chapters on Interrelations of Insects and Insect Behaviour follow, the latter considered under the head of tropisms, instinct, and intelligence, and are succeeded by one on Distribution, which is treated both from geographical and geological aspects. The concluding chapter (XIII)—Insects in Relation to Man—gives among other things an account of the progress of economic entomology in the various states of America. A useful bibliography, containing well over one thousand references, and extending to 48 pages, completes the volume ; the text-figures, many of which are original, are on the whole clear and well reproduced ; the plates are similar to those of the first edition with the exception of the frontispiece, which has been omitted.

This work is founded mainly on American types, and as far as possible only the commonest species of insects are referred to, "in order that the reader may easily use the text as a guide to personal observation." Although the book consequently will appeal more particularly to American readers, it will be found to contain much that will be serviceable to all who are interested in insect life in other countries.

H. F. C.

The Genitalia of the British Noctuidæ. By F. N. PIERCE, F.E.S. [Pp. xii + 88, with 32 plates.] (Liverpool, 1909. Price 7*s*. 6*d*. net.)

The Genitalia of the British Geometridæ. By F. N. PIERCE, F.E.S. [Pp. xxix + 88, with 48 plates.] (Liverpool, 1914. Price 10*s*. net.)

(Both published by the Author, The Elms, Dingle, Liverpool.)

THE titles of these two volumes are perhaps a little misleading as they do not treat of the whole of the genitalia of the species included in the two groups. The first deals only with the male ancillary appendages, and the second includes with these a description of the corresponding parts of the female. No account is taken of the internal genitalia, the ovaries, testes, etc.

The author was induced to examine the clasping organs in the hope that a detailed study of their parts would solve, or at any rate throw light on, the various riddles that confronted the systematists. This hope was in part realised, for certain doubtful species of *Miana* can readily be distinguished by this means, as also can several separate species formerly included together as *Hydrœcia nictitans*. It has, however, proved no panacea for all the ills the systematist is heir to. The mode of preparation is given and a general account of the clasping organs followed by a definition of the many terms necessary to describe the various parts of these complex structures. The remainders of the volumes are devoted to an account of the modifications of the parts in practically every species of these two large groups of our British moths. Some idea of the industry and patience of the author may be gathered from the fact that the forty-eight plates of the second of the two volumes contain more than 1,500 figures, and that at least two preparations of each species have been made and in a number of cases a great many more. The drawings were drawn to scale by aid of a camera lucida (the exact magnification is not given, though it would of course enhance the value of the plates).

The full extent of a new field of investigation cannot easily be judged, but this at any rate has already yielded some practical results and will probably serve as a stimulus to other workers. No such comparative work has been published previously in this, or indeed any other country, although the genitalia of certain species have been included in general anatomical descriptions. The author announces his intention of issuing a similar volume on the Tortricidæ at an early date, and we wish him the success he certainly deserves. When this is complete the whole will form a monograph that should find a place in all reference libraries and on the shelf of all entomologists.

C. H. O'D.

The Peregrine Falcon at the Eyrie. By FRANCIS HEATHERLY, F.R.C.S. [Pp. 73, a frontispiece and 29 illustrations.] (London: Country Life, Ltd. 1913. Price 5*s*. net.)

APART from the fact that in England, at any rate, the peregrine falcon is the largest bird of prey now left, the very name conjures up the age of chivalry when the knight, falcon on wrist, rode forth to the chase. Though no longer the friend of princes, a glance at the illustrations will suffice to show that the bird has shed none of his regal bearing.

The present book is the attractive record of the home life of a pair of birds and is based on the observations made by the author and his friends at the eyrie of a peregrine falcon during three successive breeding seasons. In the third year the watching was done from a tiny shelter slung on the face of the cliff within two or three yards of the nest and the watch continued in relays without intermission for thirteen days and nights from the second day of hatching. A peculiar interest is added to the narrative by the fact that the rôles of the two sexes are the reverse of that usually found in birds. It is the tiercel that remains to take charge of the young while his mate the falcon hunts for the food. In correlation with this it is to be noted that the falcon is about one-third as large again as her spouse, to which fact he owes his name.

The whole book is delightful in its reading matter and in its "get up." Both author and publisher deserve to be congratulated upon its production. The plates are, on the whole, extremely good and a number of them have underneath the light value, exposure, stop, etc., just the data that are required if one desires to emulate the success achieved in them. In this connection too the concluding chapter is useful, since it contains a frank account of the outfit and gear employed by the author and his friends.

To the naturalist it is an eloquent plea for the putting aside of the shot-gun and blow-pipe and substituting for them the camera and the note-book. We can heartily endorse the author's views on this matter as expressed in the dedication—"This book is dedicated to all egg collectors in the hope that some day they will realise that the shell is not the most important part of a bird's egg."

C. H. O'D.

MEDICINE

Twelfth Annual Report of the Imperial Cancer Research Fund (1913—1914), under the direction of the Royal College of Physicians of London and the Royal College of Surgeons of England. By E. F. BASHFORD and others.

THIS Report begins with a criticism of alleged cancer "cures," although it points out that fewer of these claims have been made in the past year. The action of thiosinamin is included, and the error of the "distinguished German investigator" is pointed out to him. It seems that the error is due to the fact that certain strains of mouse tumours disappear spontaneously, and that these strains should not be used when testing for therapeutic results.

The elaborate experiments on mice have been continued ; they have for their aim "the modification or control of the growth of cancer." The study of the constancy and variability of tumour cells has also been continued because it may throw light on the association of "chronic irritation" with some forms of cancer, and objection is taken to the haphazard hypotheses which have appeared in both lay and medical journals. The alteration of carcinoma into sarcoma has again been investigated, as well as the nature of the resistance to cancer which can be induced in animals. The fact is mentioned that cancer cells retain their biological properties when transplanted into fertilised hens' eggs. Abderhalden's test has also been reinvestigated, and a note of warning is issued as to its reliability.

The statistical portion of the Report is occupied with the discussion whether there is a real general increase of cancer and with the question of the possible existence of "cancer houses and areas." Dr. Bashford concludes that the latter are a myth, a conclusion which has been widely repeated in the lay papers.

There is no doubt that, as in former years, the Imperial Cancer Research Fund has done an immense amount of work ; but one cannot help feeling that Dr. Bashford and his colleagues are trying to run before they can walk. It is all very well to transplant masses of cells from one animal to another and then *post mortem* to cut sections of them, for this shows some phenomena interesting to pathological anatomy. But are we justified in saying from this that we are authorities on the biology of tumour cells? We might as well assert that the political economy of Siam may explain why the individual Siamese have black skins. The incessant transplantation of masses of mouse tumour is undoubtedly of importance, but what is required is to ascertain the life-history of the individual cells and what prompts that life-history. In what way is a cancer cell different from a normal cell ; what makes it a cancer cell ; what makes it divide so rapidly ; what makes it infiltrate and gives it the power of growing in the abnormal surroundings of other tissues—these are questions which do not appear to be discussed. It is difficult to believe that the mere transplantation of mouse tumour masses can possibly answer them, and there is a danger of the workers falling into a groove that can only lead them into a vicious circle. It is to be hoped that the Imperial Fund will investigate the individual cell more, especially with the object of finding the immediate cause of both its normal and malignant proliferation. What makes a cell divide seems to be a fundamental question.

The efforts made to elucidate the resistance to animal tumour growth are excellent, but they seem rather to be beginning at the end of the problem. It is true that questions of heredity have been investigated, but the predisposing causes of the disease are hardly touched upon. We are informed that "chronic irritation" predisposes certain tissues to cancer, but the expression itself is not defined. The term "irritation" is a clinical one ; what is the effect of it on a single cell? What is the difference between the effects of chemical and mechanical irritation on the single cell? The probable causes of benign tumours, which are a predisposition to cancer, are not mentioned.

The Imperial Fund has never advanced a definite working hypothesis, but it has stated its belief that cancer is not an infective disease caused by a foreign organism. No explanation is offered as to why an overgrowth of certain cells of the animal economy which were at one time normal ones should kill their host and thereby kill themselves—a form of racial suicide which does not help in the reproduction of species and which seems to have no parallels in nature. There is a vague hint that the cause of cancer is of some obscure intracellular nature, but this is not borne out by research into industrial cancer, which, by the way, is not mentioned. Infective agents undoubtedly do cause tumours, even though some of them may be of a doubtful nature, as admitted on page 7 of the Report, where reference is made evidently to the work of Rous; and it would therefore seem dangerous to assert definitely that cancer is not due to some similar agency.

In the statistical part of the Report, Dr. Bashford has rushed in where mathematicians fear to tread. While he has wisely stated that it is impossible to decide whether there is a real general increase of cancer, he has gone to the length of coming to a certain conclusion that cancer areas and houses are "a myth." To arrive at this conclusion he begins by criticising the statements of those who first suggested it. The late Dr. Law Webb, who is quite irrelevantly stigmatised as a general practitioner and not an expert pathologist, was apparently the first to suggest the cancer house. Since then the question has been taken up on several occasions and quite recently by Sir Thomas Oliver. The Report shows how these gentlemen formed their opinions by inquiry in a few towns, villages, and houses, and points out that the numbers of houses were not sufficient on which to base a definite conclusion: "The mere enumeration of 1,000 or even 10,000 houses with 1, 2, 3, and more cases of cancer may merely be a result of the great frequency of the disease." In other words, Dr. Bashford shows how Law Webb and the others are wrong in their theory because they were random sampling.

Having exposed this pitfall, Dr. Bashford then proceeds to fall into it himself. He investigated some of the same towns and villages, certainly perhaps with greater precision, but he does not investigate many thousands of houses, and therefore he is as much guilty of random sampling as the people he criticises. Admittedly he adds his and others' experiences with mice in "cancer cages," but is it even reasonable to compare rows of cages in a laboratory with human dwellings distributed over towns and countries? Indeed Dr. Bashford's random sampling is even worse than Dr. Law Webb's, for the former arrives through it quite unjustifiably at a certain conclusion summed up in the word "myth," whereas the latter, only a general practitioner, merely suggested the cancer house as a field for research. Surely, with the enormous incidence of a disease like cancer, it would only be by accurate notification through several decades throughout a country or countries, that definite error-reduced proof could be obtained as to whether cancer houses did or did not exist.

Dr. Bashford's criticisms are not always wise, as has been proved in the past; they seem to lack an open-minded purpose. He labours under a disadvantage in that he is the Director of the Imperial Fund. Among laymen in this country and among science workers in others a different interpretation is put to the prefix 'Imperial" to that accredited to it by research workers here. In America especially the term seems to mean an official connection or subsidy from the Government, and utterances from it are treated in the same way as we now believe in the statements of the Official Press Bureau. In reality there is little difference between the constitution of the Imperial Fund and that of several others established in England and other parts of the world. All are striving to elucidate the problem from one or other standpoint. Yet Dr. Bashford is handicapped from the fact that, although most other workers read his publications from the same point of view as they do those of other research funds, the lay public takes them all as statements of fact without any possible error due to experimental fallacies. Dr. Bashford does not appear to realise this position; his statements are published without reserve in the lay press, which ought to make him most careful that he is on the surest ground before he issues them. When he asserts that cancer is not an infective disease or that there are no cancer houses, although he may be right, yet suppose he is wrong! Then he will lose even more prestige than he does by criticising vaunted cancer-cures. The public always finds out the real value of the latter; why not leave it to them? Dr. Bashford may find out that he has done a serious thing when he set his pseudo-official *cachet* against a Commission on cancer or against general notification of it. On the other hand, it must be remembered that, under the deplorable way in which medical research is forced to be carried out in this country, publications of an interesting if not a startling character have to be made periodically, or the public will get tired of subscribing to the funds. Dr. Bashford is in a difficult position, it must be admitted: he is on a pedestal which is none of his making; being saddled with an Imperial title requires the greatest diplomacy—a word abhorred in science; and therefore it would appear that a guarded constructive policy for him would be preferable to a destructive one. Proved basic facts alone count in the cancer problem; it matters not who discovers them, whether they be eminent German professors or quacks; and the Imperial Cancer Research Fund, by virtue of its position, should be open equally to both.

H. C. ROSS.

The Brain in Health and Disease. By JOSEPH SHAW BOLTON, M.D., D.Sc., F.R.C.P., Professor of Mental Diseases, University of Leeds; Medical Director West Riding Asylum, Wakefield. [Pp. xiv + 479, with illustrations.] (London: Edward Arnold, 1914. Price 18*s*. net.)

THE author of this work, for the last twenty years, has devoted a great amount of time to researches upon the anatomy and histology of the human cerebral cortex in health and disease and correlated the same with his clinical experience as an asylum medical officer and superintendent.

The present volume is in great part a collection of previously published papers and monographs; and although many of the deductions and generalisations arrived at by the author may not be acceptable to most psychiatrists, nor the psychology to most psychologists, yet no one who is acquainted with the subject can fail to appreciate and highly appraise the value of the vast amount of careful histological researches, admirably illustrated by photo-micrographs, that this work contains.

Dr. Shaw Bolton, while assistant pathologist to the London County Council Asylums, Claybury, published a valuable paper in the *Philosophical Transactions of the Royal Society*; in this monograph he carefully mapped out the visuo-sensory cortex (primary visual) and the visuo-psychic (lower associational) areas of the cortex cerebri. Bevan Lewis had long previously mapped out the motor area of the cortex by the distribution of the Betz cells and had shown that it was limited to the ascending frontal convolution; but it was not until twenty-five years later when the experiments of Sherrington and Grünbaum demonstrated the fact that the ascending parietal convolution in anthropoid apes did not respond to stimulation, that the correctness of the localisation of the motor cortex by the histological method employed by Bevan Lewis began to be accepted. This view was firmly and finally settled by Campbell, who by careful histological investigation of the cell and fibre architecture of the cortex in man, anthropoid apes, and other animals mapped out definite homologous histological areas. This method was simultaneously carried out by Brodmann, who, more than any other investigator, has successfully studied histological localisation in the brains of man and the various orders of Mammalia.

Dr. Bolton deserves immense credit for his pioneer researches on the visual cortex which formed the prelude to three years' further work at the Claybury Laboratory upon "The Histological Basis of Amentia and Dementia," a valuable monograph which appeared in vol. ii. of *The Archives of Neurology*. This elaborate research, involving an enormous number of micrometric measurements of the different layers of the cortex at different periods of life (viz. in fœtal life, in early infancy, and in the adult both in health and disease, which Dr. Bolton has since amplified and extended), formed the foundations of the premises upon which he now supports his theories regarding the functions of the different cell laminæ of the cortex and the application of the same to the explanation of mental disease.

The author's researches certainly tend to prove that the outer or pyramidal layer of cells "subserve the associative, psychic or educative functions in contradistinction to the organic or instinctive functions of the cerebrum which are subserved by the inner polymorphic layer"; the middle or granule layer is, according to the author, receptor in function. There is much to be said in favour of this very important generalisation. Dr. Bolton's researches show that it is the pyramidal layer which suffers most in dementia and there is a failure of its development in amentia; but it is doubtful whether the true insanities or disorders of mind can be explained by histological changes in the cortex, so far as these changes are recorded by the methods at present known.

In the present work, a complete account of Dr. Bolton's researches will be found, and this may be regarded as the most valuable part; and if the author is somewhat unmindful of the work of others and omits to mention important investigations, being himself satisfied that he has solved all the difficult problems of disorders and diseases of the mind by mechanistic explanations—at any rate it cannot be said that he has not honestly laboured for twenty years at a most difficult subject and produced a large amount of valuable work in support of his views.

It is a pity that Dr. Bolton has not given a bibliography, or even references, to the authors he does quote; such a misconception as occurs, for example, on p. 67 might thus be avoided: "It has, however, been definitely stated by Campbell in his recent monograph, which has attracted great attention, the prefrontal region is of slight structural and functional significance." *The Localisation of Cerebral Function* is presumably the work referred to, which was published in 1905, with the aid of a grant from the Royal Society; and considering the short period of time complete localisation by histological methods has been attempted, and having regard to the fact that this work of Campbell was the first of its kind published, it can hardly be spoken of as *recent*. The author gives two somewhat sketchy although not uninteresting chapters, one on Language and Thought, the other on Feeling, Emotion, and Sentiment. But justice can hardly be done to these important subjects in twenty pages, and it would have been better if the author had omitted some of the chatty paragraphs respecting "The mechanical employment of the language machine," and have found space for mention of the Diaschisis theory of Monakow and a consideration of Apraxia in connection with Aphasia. Again, to take an instance of many in which the author is unconvincing in his arguments: on p. 415 he states "that the living spirochæte in the brain is a necessary accident to dementia paralytica, owing to it being so difficult to get rid of it, rather than the cause of this particular form of dementia paralytica." It is unnecessary to pursue the author further in his questions throwing doubt upon the causal connection of the spirochætes in the brain with the histological changes, since he stands at variance with the vast majority of neurologists and psychiatrists who have investigated this matter. Dr. Bolton might with advantage have referred to the researches of Förster and Tomasczewski; these observers have removed small cylinders of brain during life by the Neisser-Pollak puncture method, and even within such a limited amount of material have found the living spirochæte in 40 per cent. of sixty cases.

The author takes no account of the fact that a positive Wasserman reaction of the cerebro-spinal fluid is found by reliable observers in 90 to 98 per cent. His opinion that "dementia paralytica is a branch of mental disease, and that the subjects of this form of mental disease would, if they had not been syphilised, have suffered from one or the other of the types of primarily neuronic dementia," is not likely to be accepted until he brings forward much more weighty and conclusive evidence than is contained in this work. We are surprised to find no mention made of the researches of Alzheimer, nor of the Stäbchenzellen, which this very reliable observer regards as characteristic of the histology of general paralysis. The reader may be ignorant of the pathology of this disease, but it is questionable whether he will be enlightened by the simile in the following statement: that he had prepared silver chrome preparations which prove that the larger neurones lying in the interspaces "resemble halfpennies lying in a two-inch wire mesh more than anything else"; neither can the deduction that follows be readily comprehended: "*It must therefore be accepted that the most characteristic and the obviously syphilitic member of the trinity of cortical changes can exist in gross form in the absence of dementia paralytica.*" He refers, however, in this passage to the existence of the plasma cells. Dr. Bolton should study the literature before stating that "This is important and positive histological evidence in favour of my arguments that dementia paralytica is mental disease—syphilis of the encephalon—and not the latter alone." Plasma cells in the perivascular sheaths are evidence of chronic irritation of toxins invading the lymphatic system of the brain, and that they are found so constantly in cases of general paralysis is because the toxins of spirochætal colonisation exist just the same as in sleeping sickness, which is dependent upon the trypanosome invasion of the central nervous system.

The chief and considerable merit of this book is the original work and deductions regarding the histology of the cortex cerebri and its evolution in structure and function in their application to Amentia and Dementia. It hardly fulfils the functions of a text-book on the Anatomy, Physiology, and Pathology of the brain, nor will it serve as a text-book on Psychiatry; nevertheless Dr. Bolton's position as an original investigator and alienist physician should make his work widely read and appreciated by all who are interested in the subject of the Brain in Health and Disease.

F. W. Mott.

Nature and Nurture in Mental Development. By F. W. Mott, M.D., F.R.S., F.R.C.P., LL.D. Edin. [Pp. xii+151 with diagrams.] (London: John Murray, 1914. Price 3*s.* 6*d.* net.)

The excellent series of Chadwick Lectures by Dr. F. W. Mott, F.R.S., which originally appeared in the October and January numbers of this Journal (1913 and 1914 respectively), have now been republished in an expanded form in this small book, which will be welcome to all teachers at schools and to educational departments throughout the world. The work is quite intelligible to any educated person, and gives a fairly complete review of the subject, while it also contains many original observations and interesting quotations. Dr. Mott is such an authority on the subject that this work is sure to have a large sale; and the physiological side of psychology is perhaps the most important, as it is the most interesting, branch of biology.

I. K. Therapy. With Special Reference to Tuberculosis. By W. E. M. ARMSTRONG, M.A., M.D. Dublin, Bacteriologist to the Central London Ophthalmic Hospital, late Assistant in the Inoculation Department, St. Mary's Hospital, Paddington, W. [Pp. x + 83.] (London: H. K. Lewis, 1914. Price 5*s*. net.)

MUCH of what is called biological science is really science of a very low order, consisting of little more than the cataloguing of phenomena and of minute differences; and much pathological writing is, in general, little more than the expression of opinion, not based upon exact measurements or supported by strict reasoning. Since the creation of bacteriology by Pasteur and, still more, by the exact methods of Koch, Behring, and many others, pathology has begun to work upon a higher plane, and investigations on immunity and treatment are perhaps approaching the level of many physical researches. Dr. Armstrong's book deals with Carl Spengler's Immunkoerper treatment, begins with a review of certain immunity questions, explains the Immunkoerper treatment, offers some figures on the results, and gives a short chapter on technique. It is very well written, and the general discussion of the manner in which the body opposes bacterial invasions is so well done that it will interest all scientific readers, apart from physicians. The discussion is necessarily very brief but admirably clear, and Spengler's system is well described. The tables on pages 42 and 43, showing the clinical results, are difficult to understand, as the headings of the columns are not easily understood; and this is a very important defect, especially because the table is not sufficiently discussed. Up to the present the treatment has been employed almost entirely against tuberculosis, but the same principles will apply to many bacterial diseases and probably to those due to many animal parasites. The non-expert reader will be especially interested in the theorem that the forces which contend against bacterial invasions (antibodies) reside not so much in the serum of the blood as in the red corpuscles themselves, and require for their full development a separation and ionisation of these corpuscles. It thus follows that blood diluted millions of times has often stronger immunising properties than concentrated blood. The research is altogether very interesting.

Practical Tropical Sanitation. A Manual for Sanitary Inspectors and others interested in the Prevention of Disease in Tropical and Sub-Tropical Countries. By W. ALEX. MUIRHEAD, Staff Serjeant, Royal Army Medical Corps, and Assistant Instructor at the School of Army Sanitation, Aldershot. [Pp. xv + 288 with illustrations.] (London: John Murray, 1914. Price 10*s*. 6*d*. net.)

LIKELY to meet the needs of a large number of Sanitary Inspectors throughout the British Empire. There has been an abundance of works on tropical hygiene, and several on the subject of practical sanitation from the point of view both of the health officer and of the sanitary engineer; but works written expressly for Sanitary Inspectors are far from common and, we may add, far from good. This book by Serjeant Muirhead is an exception. It is brief but very thorough in details, well illustrated, grammatically and intelligibly written, and covers most of the subject. It should have a large sale because all municipalities and town councils should supply copies to their sanitary departments. The book begins with a brief account of the causes of disease, especially of tropical diseases, and a short description of mosquitoes. It then deals with Disinfection, Air and Ventilation, Water Supply, Food, Collection, Removal and Disposal of Refuse, Habitations, and Sanitary Law and Practice; and concludes with a useful appendix containing many technical details. It is dedicated to Sir Ronald Ross.

LOCAL

Federal Handbook. Prepared in connection with the Eighty-fourth Meeting of the British Association for the Advancement of Science held in Australia, August, 1914. Compiled under the authority of the Federal Council of the Association. Edited by G. H. Knibbs, C.M.G., F.R.A.S., F.S.S., and published by the Commonwealth Government. With maps and illustrations. [Pp. xvi + 598.] (Albert J. Mullett, Government Printer, Melbourne.)

This interesting handbook has been compiled under the authority of the Federal Council of the British Association, and is edited by Mr. G. H. Knibbs, C.M.G., F.R.A.S., F.S.S., and is published by the Commonwealth Government. The various chapters are written by different persons of distinction, deal with the history of Australia, the aborigines of Australia, the geography, climate, vegetation, animal life, geology, astronomy and geodesy in Australia, agricultural development, mining fields, manufactures, education, political systems, and miscellaneous notes. There are many maps, diagrams, and photographs of natives, etc., and the book will be very useful for reference, apart from the meeting of the Association.

BOOKS RECEIVED

(*Publishers are requested to notify prices*)

A Text-book of Inorganic Chemistry. Vol. i. Part I. An Introduction to Modern Inorganic Chemistry. By J. Newton Friend, D.Sc. (B'ham), Ph.D. (Würz), Carnegie Gold Medallist; H. F. V. Little, B.Sc. (Lond.), A.R.C.S., D.I.C., and W. E. S. Turner, D.Sc. (Lond.), M.Sc. (B'ham). Part II. The Inert Gases. By H. Vincent A. Briscoe, B.Sc. (Lond.), A.R.C.S., D.I.C. With Frontispiece, Plate and 88 other Illustrations. London: Charles Griffin & Co., Ld., Exeter Street, Strand, 1914. (Pp. xv + 385.) Price 10*s*. 6*d*. net.

A Text-book of Insanity and Other Mental Diseases. By Charles Arthur Mercier, M.D., F.R.C.P., F.R.C.S., late Lecturer on Insanity at the Medical Schools of the Westminster Hospital, Charing Cross Hospital and the Royal Free Hospital; Author of "Sanity and Insanity," "Psychology, Normal and Morbid," etc.; Swiney Prizeman for 1909. Second Edition. Entirely Re-written. London: George Allen & Unwin, Ld., 1914. (Pp. xx + 358.) Price 7*s*. 6*d*. net.

Transpiration and the Ascent of Sap in Plants. By Henry H. Dixon, Sc.D., F.R.S., University Professor of Botany in Trinity College, Dublin, Director of Trinity College Botanic Gardens. London: Macmillan & Co, Ltd., St. Martin's Street, 1914. (Pp. viii + 216.) Price 5*s*. net.

The Master-Key. A New Philosophy. Addressed to Psychologists, Scientists, Theologians, Christians, Jews, Agnostics, Spiritists, Ascetics, Orientalists, and Educated Persons generally. By David Blair, Wimbledon Ashrama Society, 96, High Street, 1914. (Pp. vi + 118.)

A Theory of Civilisation. By Sholto O. G. Douglas. London: T. Fisher Unwin, Adelphi Terrace. Leipsic: Inselstrasse 20. (Pp. 246.) Price 5*s.* net.

Projective Geometry. By G. B. Mathews, M.A., F.R.S., Lecturer in Pure Mathematics in the University College of North Wales, Bangor. London: Longmans, Green & Co., 39, Paternoster Row. New York: Fourth Avenue and 30th Street; Bombay, Calcutta and Madras, 1914. (Pp. xiv + 319.) Price 5*s.*

Seventy-fourth and Seventy-fifth Annual Reports of the Registrar-General of Births, Deaths, and Marriages in England and Wales. (1911 and 1912.) Presented to both Houses of Parliament by Command of His Majesty. London: Printed under the Authority of His Majesty's Stationery Office by Darling & Son, Ltd., Bacon Street, E. T. Fisher Unwin, W.C., 1913 and 1914. (Pp. cvi + 577 and cxii + 609.) Prices 5*s.* 8*d.* and 5*s.* 9*d.* (Cd. 6578 and 7028.)

Leçons de Mathématiques Générales. Par L. Zoretti, Professeur à la Faculté des Sciences de Caen. Avec une Préface de P. Appell, Paris. Gauthier-Villars, Imprimeur-Libraire du Bureau des Longitudes de L'Ecole Polytechnique, Quaie des Grands Augustins, 55, 1914. (Pp. xvi + 753.) 205 Figures. Price 20 francs.

Flies in Relation to Disease. Non-Bloodsucking Flies. By G. S. Graham-Smith, M.D., University Lecturer in Hygiene, Cambridge. Cambridge: at the University Press, 1914. With Illustrations. (Pp. xvi + 389.) Price 12*s.* 6*d.*

Vital Statistics Explained. Some Practical Suggestions by Joseph Burn, F.I.A., F.S.I., Member of the Council of the Institute of Actuaries, and Actuary to the Prudential Assurance Co., Ltd. With a Preface by Sir William Collins, M.D., etc., Chairman of the Chadwick Trust. London: Constable & Co., Ltd., 10, Orange Street, Leicester Square, W.C., 1914. With Plates and Diagrams. (Pp. x + 140.) Price 4*s.* net.

The Roman Cemetery in the Infirmary Field, Chester. By R. Newstead. From the Annals of Archæology and Anthropology, Vol. vi. No. 4. With Plates and Illustrations. (Pp. 46.)

The Chemical Examination of Water, Sewage, Foods and other Substances. By J. E. Purvis, M.A., University Lecturer in Chemistry and Physics as applied to Hygiene and Public Health, St. John's and Corpus Christi Colleges, Cambridge; and T. R. Hodgson, M.A., Public Analyst for the County Boroughs of Blackpool and Wallasey, formerly of Christ's College, Cambridge. Cambridge: at the University Press, 1914. (Pp. 228.) Price 9*s.* net.

Elementary Theory of Equations. By Leonard Eugene Dickson, Ph.D., Professor of Mathematics in the University of Chicago. First Edition: One Thousand. New York: John Wiley & Sons. London: Chapman & Hall, Ltd., 1914. (Pp. iv + 183.) Price 7*s.* 6*d.* net.

The Vaccination Question in the Light of Modern Experience. An Appeal for Reconsideration. By C. Killick Millard, M.D., D.Sc., Medical Officer of Health for Leicester, Medical Superintendent of the Isolation and Smallpox Hospitals, Leicester, formerly Medical Officer of Health for Burton-on-Trent, Medical Superintendent of the Birmingham City Hospitals. London: H. K. Lewis, 136, Gower Street, W.C., 1914. With Illustrations. (Pp. xvii + 243.) Price 6*s.* net.

The Coco-nut. By Edwin Bingham Copeland, Professor of Plant Physiology and Dean of the College of Agriculture, University of the Philippines. London: Macmillan & Co., Ltd., St. Martin's Street, 1914. (Pp. xiv + 212.) Price 10*s*. net.

Life and Human Nature. By Sir Bampfylde Fuller, K.C.S.I., C.I.E., Author of "Studies of Indian Life and Sentiment." London: John Murray, Albemarle Street, 1914. (Pp. xi + 339.) Price 9*s*. net.

Industrial Chemistry. For Engineering Students. By Henry K. Benson, Ph.D., Professor of Industrial Chemistry in the University of Washington. With Illustrations. New York: The Macmillan Company, 1913. (Pp. xiv + 431.) Price 8*s*. net.

The Spectroscopy of the Extreme Ultra-violet. By Theodore Lyman, Ph.D., Assistant Professor of Physics in Harvard University. With Diagrams. London: Longmans, Green & Co., 39, Paternoster Row. New York: Fourth Avenue and 30th Street; Bombay, Calcutta and Madras, 1914. (Pp. v + 135.)

Cocoa. By Dr. C. J. J. van Hall, Director of the Institute for Plant Diseases and Cultures, Buitenzorg, Java. With Illustrations and Map. London: Macmillan & Co., Ltd., St. Martin's Street, 1914. (Pp. xvi + 515.) Price 14*s*. net.

Fabre, Poet of Science. By D. C. V. Legros. With a Preface by J. H. Fabre. Translated by Bernard Miall. London: T. Fisher Unwin, Adelphi Terrace. Leipsic: Inselstrasse. (Pp. 352.) Price 10*s*. 6*d*. net.

Models to Illustrate the Foundations of Mathematics. By C. Elliott. Edinburgh: Lindsay & Co., 17, Blackfriars Street, 1914. (Pp. viii + 116.) Price 2*s*. 6*d*. net.

Lead Poisoning. From the Industrial, Medical, and Social Points of View. Lectures delivered at the Royal Institute of Public Health by Sir Thomas Oliver, M.A., M.D., F.R.C.P., Consulting Physician, Royal Victoria Infirmary, and Professor of the Principles and Practice of Medicine, University of Durham College of Medicine, Newcastle-upon-Tyne; late Medical Expert, Dangerous Trades Committees, Home Office. London: H. K. Lewis, 136, Gower Street, 1914. (Pp. x + 294.) Price 5*s*. net.

A Text-Book of Physics. Parts I and II. Static Electricity and Magnetism. By J. H. Poynting, Sc.D., F.R.S., Hon. Sc.D. Victoria University, Late Fellow of Trinity College, Cambridge, Mason Professor of Physics in the University of Birmingham; and Sir J. J. Thomson, O.M., M.A., F.R.S., Fellow of Trinity College, Cambridge; Cavendish Professor of Experimental Physics in the University of Cambridge; Professor of Natural Philosophy at the Royal Institution. With Illustrations. London: Charles Griffin & Co., Ltd., Exeter Street, Strand, 1914. (Pp. xiv + 345.) Price 10*s*. 6*d*. net.

Acoustics of Auditoriums. By F. R. Watson. Bulletin No. 73, Engineering Experiment Station. Published in the University of Illinois, Urbana. European Agent: Chapman & Hall, London. (Pp. 32.) Price 20 cents.

The Biology of the Blood-Cells. With a Glossary of Hæmatological Terms: For the Use of Practitioners of Medicine. By O. C. Gruner, M.D. (Lond.), Author of "Studies in Puncture-Fluids"; "A Code-system for the Hospital Pathologist": Pathologist to the Royal Victoria Hospital and to the Maternity Hospital, Montreal; Assistant Professor of Pathology, M'Gill University, Montreal, etc.; late Clinical Pathologist, General Infirmary at Leeds. Bristol: John Wright & Sons, Ltd. London: Simpkin, Marshall, Hamilton, Kent & Co., Ltd. Toronto: The Macmillan Co. of Canada, Ltd. (Pp. xii + 392.) Price 21*s*. net.

Handbook of Photomicrography. By H. Lloyd Hind, B.Sc., F.I.C., and W. Brough Randles, B.Sc., Lecturer in Biology, Derby Technical College. With 441 Plates, comprising 8 three-colour reproductions of direct colour-photomicrographs and 85 half-tone reproductions of photomicrographs, and 71 text illustrations. London: George Routledge & Sons, Ltd., Broadway House, 68-74, Carter Lane, E.C. (Pp. x + 292.) Price 7*s.* 6*d.* net.

The Deposits of the Useful Minerals and Rocks. Their Origin, Form, and Content. By Prof. Dr. F. Beyschlag, Geh. Bergrat, Direktor der Kgl. Geolog., Landesanstalt, Berlin; Prof. J. H. L. Vogt an der Universität, Kristiania; Prof. Dr. P. Krusch, Abteilungsdirigent, A. D. Kgl. Geolog., Landesanstalt U. Dozent. A. D. Kgl. Bergakademie, Berlin. Translated by S. J. Truscott, Associate Royal School of Mines, London. In three volumes. Vol I.: Ore-Deposits in General — Magmatic Segregations — Contact-Deposits—Tin Lodes—Quicksilver Lodes. With 291 Illustrations. London: Macmillan & Co., Ltd., St. Martin's Street, 1914. (Pp. xxvii + 514.) Price 18*s.* net.

Algebraic Invariants. By Leonard Eugene Dickson, Professor of Mathematics in the University of Chicago. Mathematical Monographs. No. 14. Edited by Mansfield Merriman and Robert S. Woodward. First Edition, First Thousand. New York: John Wiley & Sons, Inc. London: Chapman & Hall, Ltd., 1914. (Pp. x + 100.) Price 5*s.* 6*d.* net.

Constructive Text-Book of Practical Mathematics. By Horace Wilmer Marsh, Head of Department of Mathematics, School of Science and Technology, Pratt Institute. Vol. iii. Technical Geometry. First Edition, First Thousand. New York: John Wiley & Sons, Inc. London: Chapman & Hall, Ltd., 1914. (Pp. xiv + 244.) Price 5*s.* 6*d.* net.

ANNOUNCEMENTS

MEETINGS OF SOCIETIES

ROYAL SOCIETY. Ordinary Meetings, 4.30 p.m., January 21, 28, February 4, 11, 18, 25, March 4, 11, 18, 25.

ROYAL ASTRONOMICAL SOCIETY. Meetings, 5 p.m., January 8, February 12.

ROYAL METEOROLOGICAL SOCIETY. Annual General Meeting, January 20.

THE INSTITUTION OF MECHANICAL ENGINEERS. General Meetings, 8 p.m., January 22, March 19. Annual General Meeting, February 19.

CHEMICAL SOCIETY. Informal Meeting, 8 p.m., January 14. Meetings, 8.30 p.m., January 21, February 4, 18, March 4, 18.

PHYSICAL SOCIETY. Meetings, 5 p.m., January 22, February 26, March 26; 8 p.m., March 12. Annual General Meeting, 8 p.m., February 12.

OPTICAL SOCIETY. Meetings, 8 p.m., January 20, March 11. Annual General Meeting, 7.30 p.m., February 11.

The medals of the Royal Society for this year have been awarded as follows:—

Copley Medal . .	Sir Joseph John Thomson.
Two Royal Medals .	Prof. E. W. Brown and Prof. W. J. Sollas.
Davy Medal . .	Prof. W. J. Pope.
Rumford Medal . .	Lord Rayleigh.
Hughes Medal . .	Prof. J. S. Townsend.
Darwin Medal . .	Prof. Poulton.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury.

SOME ASPECTS OF THE ATOMIC THEORY

By Prof. FREDERICK SODDY, M.A., F.R.S., *University, Aberdeen.*

Either matter must occupy space continuously or it must exist in the form of discrete particles. The historical origin of the Atomic Theory of matter is to be found in the choice between the two possible answers to these mutually exclusive alternatives. But this is little more its real origin than is Prout's celebrated guess that all elements are built up of hydrogen the real origin of our present far-reaching conclusions as to the structure of atoms. The true origin of the Atomic Theory is recognised universally to have been during the first decade of last century in Dalton's discovery of the simple laws of chemical combination, though, even to the discoverer himself, the laws of gaseous behaviour, upon which later the totally distinct but inextricably interwoven Molecular Theory was to be based, undoubtedly played a part in directing the interpretation he put upon these laws.

Henceforth science was to deal no longer with atoms as the end results of a purely mental process of the subdivision of matter, a process which must of necessity have an end if matter does not occupy space continuously, but with atoms of definite mass, determinable simply and exactly relatively, that is, the mass of any one kind of atom in terms of that of any other. Absolutely, of course, these masses have only been precisely determined in the present century. In the words of Sir J. J. Thomson in his recent Romanes Lecture, "Dalton traced the atoms of the different elements in all their migrations from one compound to another by means of their weight; this was a quality they could neither change nor disguise; until quite recently, however, this was about the only quality of the atom of which this could be said." And, it may be added, it was the difference of the weights of atoms of different elements which made the test of value. The long and now admittedly mistaken efforts on the part of what may be termed the thermodynamical school of physical chemists, especially in Germany, to dispense entirely with atomistic conceptions of matter as unnecessary and unproved speculations, always failed to render intelligible the simple laws of chemical combination for which that theory had primarily been created to account. Of chemical origin, its development proper proceeded almost entirely along chemical lines, from the recognition and differentiation of the various kinds of atoms by their weights, to the architecture of complex substances, the number and relative arrangements of the atoms therein, the manner in which they were held together by units of combining power or valency, and to the beautiful space chemistry of the particular varieties of complexes, in which the same atoms may be grouped in two ways absolutely identical on any two-dimensional representation, but in space of three dimensions having the relations to one another of an object to its mirror image. But although this is the real and only Atomic Theory, it is not the Atomic Theory about which much, perhaps most, has been said and written.

Like the Atomic Theory, the Molecular Theory, which in the past has so often usurped its name, originated in the laws of chemical combination, for the one special case of gases. Gay Lussac's law of combining volumes led Amedeo Avogadro to his famous generalisation that equal volumes of all gases, under the same condition of temperature and pressure, contain the same number of *molecules*. It is not a general theory, and indeed we know now cannot apply to some states of matter, though the labours of van't Hoff and succeeding physical chemists of the present era have extended it to the *interior* of liquids with suitable important and extensive modifications. But, quite apart from this, the conception of the molecule is a distinct and independent development of mathematical and experimental physics.

The molecule is not the atom, though in certain special and rather exceptional cases, that of the monatomic gases, such as helium and mercury vapour, it happens that the same particle is both the atom and the molecule of the element in question. This one case, which also includes that of the vapours of most metallic elements, when these are gasified at sufficiently high temperature and often when they are dissolved in mercury to form liquid amalgams, furnishes a common point of contact between conceptions which are by their nature and origin very largely distinct.

The Molecular Theory of the physicist, as it now appears, involves nothing of the conception of ultimateness implied in the Atomic Theory. It is based on underlying principles which apply to discrete freely moving particles in general, be they molecules in the ordinary chemical sense or microscopically visible masses, so long as they are free to move about as individuals, as in a gaseous or fluid medium, or even, possibly, in free space, as in a nebula. It was something of a lucky, if confusing, accident that in the simple gases on which Gay Lussac experimented and Avogadro theorised, the molecules are composed of so few, often only two, individual atoms, and that since then even certain monatomic molecules should have been found to be capable of existence.

Of course it may be urged that the conception of ultimateness no longer, with recent acquisitions of knowledge, does apply to the chemist's atom. But it must be understood that the atomic theory of Dalton has always been concerned with the ultimate particles of the elementary substances, rather than with the ultimate particle of matter in general, so that the new knowledge in fact involves only confirmation of old ideas. The atom of uranium is still the ultimate particle of the element uranium. Its disintegration during radioactive change into an atom of radium, three atoms of helium, and two electrons, is in no way inconsistent with the statement that the atom of uranium is the ultimate particle of the element uranium. Indeed, such evidence proves positively that no smaller particle of the element uranium than the single Daltonian atom can be or ever will be known. For could the term atom have any other meaning than this? If the chemist's atom ever had meant the ultimate particle of matter in general without reference to one and one only particular element, how could it ever have been applied except to the one kind of atom, or one kind of matter, hydrogen, the lightest particle of matter then and for that matter still known to science?

It is only comparatively recently with the study of radioactive changes, and the recognition of the profound difference between, for example, the disintegration of a radium atom and the decomposition of a water molecule, that the true conception of the atom as distinct from that of the molecule has become at all general. One would search in vain for any such distinction in, for example, Sir Arthur Rucker's Presidential address on the "Atomic Theory" to the British Association in Glasgow in 1901, or in Clerk Maxwell's earlier articles on the same subject.

Two recent developments have gone to make clearer than ever before the essential difference between the chemist's atom and the physicist's molecule, and also the artificialities that have crept alike into chemical and physical science by the unconscious effort to give to the molecule the generality of application which in fact applies only to the atom. In the first place, Perrin's successful application of Avogadro's law to microscopic particles of solids, such as gamboge, suspended in liquids, particles which contain probably billions of molecules of gamboge, in the ordinary chemical sense of the word, is a striking proof that no conception whatever of ultimateness enters into the molecular theory. The size of the particle is of no significance. The sole exact definition of a molecule in the physical sense is that it is a separate particle capable of free and unhampered, or better un-anchored, movement, in which movement it is subject to free and unceasing interchange of kinetic energy by collision with other similar freely moving particles. Under such conditions of free motion and mutual collision with interchange of kinetic energy the average amount of kinetic energy of translation which each molecule secures is the same for all molecules independently of their mass, and is simply proportional to the absolute temperature. The ideal gas laws, $pv =$ constant, or $pv = RT$, on which the kinetic theory of gases has been founded, are expressions of the foregoing generalisation, the product pv being a measure of energy, which is proportional to the kinetic energy of translation of the individual particles or molecules of the gas, and quite irrespective of the nature of the gas or whether its molecules are light or heavy.

The molecule, unlike the atom, is thus not distinguished by its mass, but by the fact that, independent of its mass, it possesses a definite kinetic energy of translation or sensible heat energy at any definite temperature. This test, as Avogadro first accomplished, enables us to determine the mass and therefore the number of atoms in the molecule, to distinguish the lighter from the heavier and to select those which are ultimate particles in the chemical sense of being the smallest particle of the compound which can exist and which if further subdivided will give rise to new substances different in properties from the original. This process of selection, and the fact that of all the molecules containing any one element, some one or more are always found which contain but one atom of that element, represent the invaluable use chemists have made of the molecular theory in their determination of relative atomic weights.

But the molecular theory applies to any size of particle of any complexity. It is limited only by the condition of free interchange of kinetic energy of translation. The quantum of kinetic energy of translation which each molecule possesses at a definite temperature being fixed, the velocity of translation is inversely proportional to the square root of its mass. This velocity—in the case of hydrogen at ordinary temperature rather more than a mile a second—is in the case of fine microscopic particles still great enough to endow them with the most animated Brownian movement, whilst in the case of particles large enough to be perceived by a simple lens, it is still appreciable. Beyond this it becomes inappreciable to measurement. It is this and only this cause which limits the further application of the gas laws on which the molecular theory is based to suspensions of particles large enough individually to be seen by the naked eye. But in imagination it may be extended to a collection of billiard balls floating in a liquid of equal specific gravity, each one of which would be found to be in perpetual Brownian motion on the average with the same amount of kinetic energy as that possessed by a hydrogen molecule at the same temperature, were it not for the fact that the velocity of so great a mass corresponding to this minute quantum of kinetic energy is altogether inappreciable by experiment.

When we contrast with such a conception of a molecule that usually entertained by the chemist, we bring in at once the conception of ultimateness derived from the Atomic Theory. The chemist's molecule is a conception independent of the dynamical law of the equipartition of energy on which the physical molecular theory is based. It stands in the same relation to compound substances as the atom does to elementary substances, and is the ultimate particle of such a compound substance, or the particle than which nothing smaller can exist, exhibiting unchanged the properties of the substance. True, the conception of compounds must be extended to include not only compounds of atoms of different elements, but also compounds of atoms of the same element. To the chemist oxygen and ozone are such compounds, elementary oxygen being oxygen in the special so-called "nascent" condition.

In general the chemist usually has in mind the molecule with the fewest number of atoms to represent the composition and chemical properties of the compound with which he deals. Such a molecule may or may not in fact exist, or it may exist only over a very limited range of physical conditions far removed from those for which his other knowledge of the compound has been derived. But however artificial or special the conditions under which the molecular weight has been determined, the tendency is irresistible to look upon such molecules as having a real general existence. In similar way the physicist extends his molecular conceptions derived from the study of liquids and gases to matter in general.

It is interesting therefore to note that the molecular conception, either as a physical or chemical unit, fails completely in the case of crystalline solids. This, the second advance referred to previously, follows from the recent application of the X-rays to crystalline structure by Prof. Bragg and his son, who have shown that any atom of chlorine in the interior of a rock-salt crystal, for example, is fixed in space symmetrically with reference to several sodium atoms around it and with equal justice may claim any one of them as its partner in the hypothetical molecule $NaCl$. It seems therefore that such molecules cannot really exist in crystalline solids. Between the crystal as a whole and the single constituent *atoms* of which it is composed there are no intermediate or penultimate particles that can be considered to have a separate existence.

With so much of historical comment on past developments let us refer to a few aspects of more modern advances, some of which are set forth by Sir J. J. Thomson in his recent Romanes Lecture.[1]

The first striking point is the enormous advance in sensitiveness attained when electrified atoms, or gaseous ions, commenced to be studied. "An unelectrified atom is so elusive that unless more than a million million are present we have no means

[1] *The Atomic Theory*, Romanes Lecture, 1914, by Sir J. J. Thomson, O.M. (Oxford: Clarendon Press, 1914. Price 1*s*. 6*d*. net.)

sufficiently sensitive to detect them, or, to put it another way, unless we had a better test for a man than for an unelectrified molecule, we should be unable to find out that the earth was inhabited. . . . A billion unelectrified atoms may escape our observation, whereas a dozen or so electrified ones are detected without difficulty." Owing to the charge the ions may be sorted out by suitable application of electric and magnetic forces (positive ray analysis), and we get from a mixed gas a limited number of sharply defined streams. "This shows that all the atoms of an element are alike; this has sometimes been questioned." The possibility that the weights of the several atoms of the same elements may differ by varying small amounts on either side of a mean value, much as the individual velocities of gas molecules differ from the mean velocity according to the kinetic theory, is, of course, old. As we read in another connection, "The statistician is content to know that the average height of male adults is, say 5 ft. 6 in. and their waist measurement 3 ft., but it is evident such knowledge would be a very unsatisfactory equipment for one's tailor." It is interesting to know that the results of the positive ray method of gas analysis give no support to such a view, especially as the older line of argument, derived from the extreme sharpness of the individual lines in the spectrum of an element, must now be reconsidered and probably rejected altogether. Spectroscopic quantities seem to depend on atomic charge and not on atomic mass. But that a totally opposite conclusion in the single case of the element neon has been drawn from the work of Ashton by this method in the Cavendish Laboratory is not referred to, nor is the newer theory of isotopic elements founded on the study of radioactive change. Such elements, so called because they occupy the "same place" in the periodic table, are identical in chemical properties and in such physical properties as do not depend directly on the mass, but they may differ by whole units in atomic weight. The work on radioactive change, which has resulted in much that is really new and important in the Atomic Theory and in knowledge of the structure of the atom, it is scarcely necessary to refer to here in detail, as much of it is contained in the recent discussion at the Royal Society on Atomic Structure (see SCIENCE PROGRESS, July 1914, p. 169). As, however, the knowledge of the existence of isotopes brings a new possibility to be taken into account in the discussion of the cause of the variation of a few atomic weights from approximate whole numbers, it may be briefly referred to. In radioactive change two kinds of change are observed. In the one there is expelled from the disintegrating atom an α-particle, which is an atom of helium that has lost two electrons, in the other a β-particle, which is an electron or unit charge of negative electricity. After the first kind of change, the radio-element changes in chemical nature in a manner corresponding with a shift of two places in the direction of diminishing mass in the periodic table, whilst after the second kind of change the shift is one place in the opposite direction. Hence two β-ray changes and one α-ray change, in any order of sequence, result in the radio-element reverting to the same place as at first in the periodic table, and though its atomic mass has been reduced by four units, that of the helium atom expelled, its chemical properties are identical with that of its original parent. It is reasonable to suppose therefore that not only the radio-elements but possibly some of the stable elements are mixtures of such "isotopic" elements possessing the same nett atomic charge and occupying "the same place" in the periodic table, but differing by whole numbers in atomic weight. From such evidence as this it was first proved for the sequence of the radio-elements that the successive places in the periodic table correspond with unit changes in the atomic charge (now called the atomic number), as suggested by van der Broeck. Subsequently this was confirmed by Moseley's work on the wave-lengths of Barkla's characteristic X-radiations for all the elements systematically. On Rutherford's theory, the atomic number, or number the place occupies in the sequence of elements—starting with hydrogen as unity, helium, two, and so on—is the positive charge on the nucleus.

The positive ray method and gaseous diffusion methods would be almost the only ones capable of distinguishing a heterogeneity of the kind supposed in the theory of isotopes, and many conclusions can scarcely be profitably discussed until the homogeneity of the elements has been systematically re-examined by one or other of these methods. The researches on the variation of the atomic weight of lead from different radioactive minerals, so far as they have yet been pursued, shows that in the case of this one element, at least, the conclusion drawn from the theory is borne out.

Along these lines we arrive at the view that the periodic law represents a periodic variation of the properties of elements, not with the atomic mass as first concluded, but with the atomic number or nett nuclear charge of the atom. At once there arises the question, what is the relation between atomic weight and atomic charge? The fact that the vast majority of the atomic weights are approximately whole numbers, in terms of oxygen as 16, suggests that unit change of atomic charge is accompanied by a change in mass, approximately some multiple of the hydrogen atom. In α-ray changes at the extreme end of the periodic table, the change of charge is two and the change of mass four, and this holds fairly well in the early, but not in the subsequent, part of the periodic table where the atomic weight is greater than twice the atomic number. Sir J. J. Thomson advances grounds for believing that as far as the first 18 elements, there are in reality two series, one with even and one with odd atomic weights, the members of each series increasing with the common difference four. There are, however, two exceptions—beryllium, which on this scheme should have an atomic weight of 8 instead of 9, and nitrogen, which should be 15 instead of 14. Hydrogen, moreover, does not seem to fit in at all.

With regard to the exceptions from the rule of the atomic weights being approximate integers, the suggestion is made that the excess or deficiency of mass corresponds with the absorption of liberation of energy in the formation of the element, equivalent to that possessed by the mass gained or lost, moving with the velocity of light. Thus per 35·5 grams of chlorine, the half-gram in excess or deficit of the nearest whole numbers would correspond with $2{\cdot}25 \times 10^{20}$ ergs, the amount of energy required to keep the *Mauretania* going at full speed for a week. The enormous amounts of energy liberated from heavy atoms in radioactive changes certainly suggest such a possibility. It is interesting also to recall that the very recent results of Hönigschmid on the atomic weights of uranium and radium, 238·15 and 225·95 respectively, make that of the former about 0·2 unit in excess of that of the latter *plus* that of the three helium atoms expelled in the transformation. But it has still to be proved how far the unsuspected existence of isotopic elements may not be accountable for the departure of atomic weights from approximate whole numbers in certain cases.

Dealing with the question of the present possibility of artificial transmutation, one conclusion should be widely noted. The question of artificially disintegrating elements by physical means, such as by the electric discharge in vacuum tubes, freshly raised a short time ago by experiments made by Prof. Collie, has been very widely discussed. The frequent appearance of helium and neon in such experiments has been taken to be evidence of transmutational changes. But Sir J. J. Thomson concludes, "I have never, however, been able to get any evidence that I regard as at all conclusive that the atom of one element could by such means be changed into an atom of a different kind; in other words, that by such means we could produce a transmutation of the elements." The Hon. R. J. Strutt's recent experiments also point in the same direction. On the other side, however, Prof. Collie and his co-workers, who have maintained that the apparently miraculous appearance of helium and neon in vacuum tubes submitted to the electric discharge is not a consequence of faulty experimentation and contamination by air, have recently renewed the discussion in the current number of the *Proceedings of the Royal Society* and reaffirm their view that the phenomenon is inexplicable on ordinary lines.

The ratio of mass to weight being constant, for all elements from the lightest to the heaviest and even for radioactive substances, and the view that, in the formation of elements generally, changes may occur in mass, lead to the conclusion that even in the region of subatomic changes mass must be proportional to weight. This, if sound, would be of fundamental importance in the theory of gravitation. But, as regards the electrons, it is unknown whether these possess any weight at all, or whether, as might be expected on one of the electrical theories of gravitation, the weight may be abnormally large in comparison with their mass. It will be realised how far we still are from the profitable discussion of the significance of atomic weights. The difference of 0·2 unit noted between the atomic weight of uranium and that of its products, it seems, may arise in at least three ways: (1) by the loss of energy, as Sir J. J. Thomson suggests; (2) by the two electrons, which the atom loses, being abnormally heavy in regard to their mass; (3) by the existence of stable isotopes of uranium differing in atomic weight. Two such are in fact known, differing in atomic weight by four units, but one is probably present in too small proportion to account for the discrepancy in the atomic weight.

Around the views taken of the electron, the atom of negative electricity divorced from matter, the greatest divergence of opinion will probably be found. It possesses inertia or mass, but it is yet unknown whether it possesses weight and, if so, whether the ratio of mass to weight is the same for electricity as for matter. Can it in any sense be reasonably regarded as one of the primordial atoms of matter out of which Prout, and others who have followed him, regarded all atoms as built up? We read, "Since the electron can be got from all the chemical elements, we may conclude that electrons are a constituent of all the atoms. We have thus made the first step towards a knowledge of the structure of the atom and towards the goal towards which since the time of Prout many chemists have been striving, the proof that the atoms of the chemical elements are all built up of simpler atoms—primordial atoms, as they have been called." There has been, of course, abundant evidence ever since the work of Faraday on electrolysis in liquids that electric charges are associated with the atoms of matter, and play a vital part in chemical changes.

The above reasoning might be, and has indeed been mistakenly used to show that hydrogen is a common constituent of all matter, since, whatever the matter you may put into a vacuum tube, hydrogen is always obtained from it by the electric discharge. Yet a billion atoms of hydrogen would be difficult to detect, whereas a dozen or so electrons are ample for detection. The use of these refined experimental methods carries with it the necessity of not seeming to draw such simple conclusions. Not that the conclusion is not probably correct, but the evidence here alleged in its support is worthless.

The withdrawal of an electron from the hydrogen atom results in the well-known hydrogen ion, the material particle which confers upon a whole class of substances the common quality of being acids. The hydrogen ion is at once the simplest atom of matter known, and yet according to present views it does not contain a single electron! The α-particle expelled in radioactive changes is the helium ion, the second simplest atom of matter known, and again it does not contain a single electron. If it were necessary to resuscitate Prout's rather obvious hypothesis as to the constitution of matter it would be less fanciful to start from the evidence of radioactive changes, which, many years ago, showed that the α-particle or helium ion is certainly one such primordial material constituent common to the atoms of all the radio-elements. But to prefer to regard the atom of negative *electricity* as the primordial atom of *matter* seems to be a relic of an earlier confusion between electricity and matter that the facts, if not the language of science, have long outgrown.

One gets into closer touch with reality in the recent determinations of the number of electrons present in the atom and in the conclusion that in each atom this number is approximately one-half of the atomic weight. The scattering of X-rays by the electrons in the atom we find likened, in a manner that will be helpful to those unable to follow the reasoning, to the scattering of ordinary light by the molecules of air, which produces the blue of the sky, and like the latter method has been found useful to determine the number of scattering points through which the beam passes. Sir Ernest Rutherford's determination of the value of the positive charge of the central nucleus of the atom by the scattering or deflection of the α-rays confirms the aforementioned result. However, these methods and also the later and more refined one of Moseley on the precise Atomic Number are at fault in one not unimportant respect. They can only reveal the outer electrons of the atom. They cannot reveal the electrons in a central concentrated nucleus where their charge is merged in that of a superior positive charge. Of these nuclear electrons, as of so much that is fundamentally new about atomic structure, we have learned only by radioactive change. Their precise number is vague, and though not necessarily large, they are of interest in that they alone are entitled to be considered primordial constituents of the atom in the Proutian sense. A glass rod is rubbed. Do the atoms of matter therein suffer transmutation? If they do not, how can the electrons which have been removed by the rubbing be regarded as primordial constituents? Again, the hydrogen atom loses its electron and becomes a hydrogen ion. Is this a transmutational change? If not, why speak of the electron in question as a primordial constituent? But in radioactive changes, not only the changes effected by the expulsion of the material α-particles, but also those effected by the expulsion of β-particles, or negative electrons, are equally entitled to be considered transmutational in character. By a β-ray change radium-D, which is the isotope of lead, passes into radium-E, which is the isotope of bismuth, and so on. The example serves to bring out into clearer relief the normal rôle of the electron, which so far from being a primordial constituent is a partner of the material atom, the loss or gain of which is accompanied by no fundamental change of the atom. But even so it seems a forced mode of expression to represent even the β-ray electron as a primordial material constituent, especially as the a-particle is such a primodial material constituent which does not contain a single electron.

THE ELECTRICAL PROPERTIES OF CONDUCTORS AT VERY LOW TEMPERATURES

By FRANCIS HYNDMAN, B.Sc.

Some of the properties of matter which are of special interest, in that they help to form an idea of its constitution, are only capable of exact measurement under very difficult experimental conditions. Great interest attaches just now to the electrical properties, owing to the new developments in the theory of the electric current which have been advanced in quite recent years, and which are indicating relations between widely different properties of matter. The most interesting and instructive field for investigations on these properties of matter is at very low temperatures, because there phenomena are found which are either non-existent or unmeasurable at higher temperatures, and which throw important light on electric theory.

In the number of July 1913, vol. viii. p. 26, I gave an account of some of the new investigations in the low temperature region, which had been rendered possible by the discovery of the means of liquefying helium, a gas which boils normally at the extremely low temperature of 4·25° K. (– 268–84° C.). Since July 10, 1908, when Kamerlingh Onnes first liquefied helium at Leiden, the technics of the subject have been advanced in the most active way. Measurements of all kinds are now undertaken in a systematic manner in liquid helium, which has been transferred to the special vessel required for them. Here it can then be held under reduced pressures, which allow of a reduction of temperature to 1·6° K,[1] and the maintenance of a constant temperature from this up to 4·25° K.

Before passing to the particular subject of this paper it will be useful to consider briefly the exact present position with regard to the thermodynamic properties of helium as related to those of an ideal monatomic gas, or to the perfect gas where

[1] See *Communications from the Physical Laboratory of the University of Leiden*, by Prof. H. Kamerlingh Onnes, Director of the Laboratory.

p = RT/v, as these latter have been assumed for the electrons in the theories of Rieke, Lorentz, Plank, etc. At high temperatures they all approximate, so that at very high temperatures the difference between them may be put equal to zero. At low temperatures, however, the pressure in an ideal gas approaches a value which depends on the molecular weight, and the density, but is independent of temperature. This value is called the *zero point pressure.* Its value for helium at normal density is found to be 0·25 mm., but it increases more rapidly than the density. The theory shows that in the ideal monatomic gas there will be a *zero point energy*, which has been assumed by Plank for his newest ideas in the quantum theory of electric conduction, in which the electric oscillators of the atoms absorb radiation energy continuously, but only emit it by whole units or quanta. This theory has been of considerable service in explaining what seemed to be difficulties in the accepted electron theory of conduction. According to this theory, conduction of electricity is due to the motion of the free electrons between the atoms of the conductor. The electrons are continually colliding with the atoms, and in doing so give up current energy. It was formerly suggested that the electrons could be treated as molecules of a condensible monatomic gas, which would all freeze down to the atoms of the conductor at very low temperatures. This would naturally result in an enormous increase in the resistance of the conductor, which, as Kelvin indeed supposed, would have an infinitely large resistance at the absolute zero.

The very careful measurements which have been made with platinum at various temperatures for the purpose of thermometry have shown that the resistance at continually decreasing temperatures was taking a course which, if continued, would tend to become zero at some point above the absolute zero of temperature, and hence that it was necessary to suppose that it would attain a minimum, and then increase to infinity at the absolute zero of temperature. In considering the motion of the electrons it follows that the resistance to their free migration increases with their velocity and their number per unit volume, and is inversely proportional to their mean free path. Assuming the two latter factors constant, the velocity will decrease with decreasing temperature, and will become zero when this is zero. However, if with decreasing temperature the number of electrons per unit volume increases, the resistance will increase from this cause. Hence it might reach a minimum at a low temperature and thence increase to large values. The only way to decide which of these possibilities is the more correct was by direct experiment at extremely low temperatures.

With very, but not absolutely, pure platinum wire it was found that the resistance decreased to a very small constant minimum value at about 5° K. The same kind of phenomena was observed with gold, and in each case the purer wires gave smaller values for the constant, which was clearly due to small residual impurities.

This result was very interesting and important but not decisive, and hence it was necessary to employ the purest obtainable metal, *i.e.* mercury. This was most carefully distilled and drawn into fine capillaries, which were connected together so as to give sufficient resistance to be measurable. The result obtained was extremely definite and remarkable. A sudden fall in resistance occurs at 4·25° K., and at 4·2 K. the resistance is less than 10^{-6} times its value at 0·0° C. Further, at the lowest attainable temperatures this extremely low value was maintained. Subsequently it was found that lead exhibited the same effect at about 6° K., a temperature which is not easily maintained constant as it is above the boiling-point of liquid helium. With tin the corresponding value was 3·8° K., which could be determined exactly. These are the values obtained with small currents as the highest temperatures of the point of change from ordinary to super-conduction.

TABLE I

Relative resistances of metals, R_T/R_o.

T°K	Platinum.		Gold.	Mercury.	Tin.	Lead.
	Wire.	Very pure.				
169·29	—	$58·1 \times 10^{-2}$	$59·3 \times 10^{-2}$	58×10^{-2}	65×10^{-2}	$59·4 \times 10^{-2}$
77·93	—	19·9	21·9	27·9	30	25·3
20·2	$1·71 \times 10^{-2}$	1·4	0·8	5·6	—	3·0
14·2	1·35	1·0	0·3	3·3	—	1·2
4·24	1·19	0·9	0·2	$4·5 \times 10^{-4}$	—	—
4·19	—	—	—	$< 10^{-9}$	—	—
3·82	—	—	—	—	$7·5 \times 10^{-4}$	—
3·75	—	—	—	—	$7·0 \times 10^{-7}$	—
2·3	1·19	—	—	—	—	—
1·7	—	—	—	—	—	$1·3 \times 10^{-9}$
1·5	1·19	—	—	—	—	—

Hence an entirely new condition of matter has been discovered, one, namely, in which an electric current will flow along a wire of the material without any appreciable potential difference between the ends. The condition is, however, not at all a simple one, and although it is by no means fully investigated, enough has been proved to show that much further research is necessary before this super-conductive state will be properly understood. It will be sufficient here to give some of the details of two of the lines on which investigation has proceeded, and some of the results and conclusions reached in others. It is stated above that the critical value of the temperature which marks the boundary between the normal and the super-conductive states is only a constant for small currents. Investigation on this point shows that at this temperature, and at every one below it, there is a limiting value of the current which must be observed if the super-conductive state is to be retained. Above this value the current requires an appreciable difference of potential to force it through, but one which seems to be proportional to the excess of the current above the limiting or "threshold value," as it is named by its discoverers. Hence just above the threshold value, as, of course, below it, Ohm's law does not apply, the deviation becoming proportionately less and less for greater currents, but the exact relation is not yet known. It is also not known yet whether the change at the threshold value is due to a uniform variation of the state of the whole conductor or whether it is due to some local heating caused by some less conductive portion which raises the local temperature above the critical at that point, and hence produces a point for the production of Joule-heat, which changes the whole condition of the conductor. To determine this will require many and careful measurements with conductors arranged so that the rate of withdrawal of heat can be regulated. Also a significant fact was observed that with a lead wire in a vacuum the current could rise far above the value which had been found for the threshold value using a naked wire immersed in liquid helium, which has been shown to be an excellent insulator.

The other factor which causes a variation in the state is the magnetic effect of the current itself. This also has only been investigated to a limited extent, but owing to its extreme importance and its apparent relation to the threshold value, these first measurements have a particular interest. When the super-conductive state was first discovered it seemed as if many hitherto unattainable physical possibilities would be achieved at last. One was the production of a large magnetic field, say 10^5 gauss, without the use of iron. Clearly, if the field itself had no contrary effect, and fairly large currents could be employed, a large field could be obtained in a limited space with the expenditure of a comparatively small amount of energy. Unfortunately for this vision it was found that the currents large enough to give the desired fields created such disturbances in the super-conductor itself as to bring it out of this condition into one where Joule-heat was developed to an extent which appears to render the attainment of the desired field impossible.

The exact course of the threshold value with various conductors and temperatures is shown in Table II. It will be noted that the current densities vary very much for the various conductors, and as a comparison the relative specific conductivities are given. The results for lead and tin are not nearly as complete as for mercury, but they are sufficient to show the similarity of the phenomena. Table III. shows the increase of potential difference with current density at a temperature where the mercury is super-conductive. It is clear that there is a very sharp increase at current densities just above 1024, so that this is properly termed a threshold value.

TABLE II

Current densities in amperes per mm.² at which potential difference appears at the extremities of the conductor.

σ/σ_{Hg}	1	5·0	7·5
Temperature.	Mercury.	Lead.	Tin.
about 6·0° K.	—	very small	—
4·25	very small	33	—
4·18	15·6	(<270 in vacuum)	—
4·10	68·8	—	—
3·75	—	—	0·28
3·68	—	—	2·8
3·60	42·7	—	—
3·28	65·7	—	—
2·69	—	—	28·0
2·45	10·23	—	—
1·70	—	600	—
1·60	—	—	large

TABLE III

Potential difference and current density in mercury.

Temperature.	Current density Ampères per mm.^{2}	Potential difference. Microvolts.
2·45° K.	944	<0·03
—	1024	0·56
—	1064	1·5
—	1096	6·3
—	1120	very large

The investigations on the effect of powerful magnetic fields were more easily carried out with lead or tin wires than with mercury, and indeed if any practical application of the phenomena is possible it will have to be with wires which can be wound into close coils. The results with lead and tin are given in the accompanying table for two temperatures, in such case. With lead, however, both occur in the superconductive region, but with tin the critical value 3·8° K. occurs between.

TABLE IV

Effect of magnetic field on resistance.

Field.	Lead $\frac{w}{wo} \times 10^4$.		Tin $\frac{w}{wo} \times 10^5$.	
	at T = 4·25° K.	T = 2·0° K.	T = 4·25° K.	T = 2·0° K.
gauss.				
0	—	—	70	—
100	0	0	—	0
200	0	0	—	50
300	0	0	—	60
500	0	0	—	—
600	1	0	—	—
700	2	0	—	—
800	24·9	1·0	—	—
900	24·95	2·0	—	—
1,000	25·0	24·0	71	61
2,000	25·55	24·55	80	62
2,500	25·83	24·83	90	75
5,000	27·23	26·23	140	130
10,000	30·00	29·00	242	240

Hence it has the same kind of effect as that of heating the conductor or of too large a current. In the case of lead the change occurs at considerable fields, and does not preclude the possibility of some arrangement which would enable large fields to be obtained with only a small loss of energy. How difficult, if not almost impossible, this is with conductors in the ordinary state at low temperatures has been shown by Fabry, who has calculated the conditions to obtain a field of 10^5 gauss with copper conductors at the temperature of liquid air. He found that for a coil with an internal space of 1 cm. radius 100 kilowatts of electrical energy would be necessary to maintain the necessary current. However, the development of heat would be very large, and would require the use of and the evaporation of 1,500 litres of liquid air per hour. Even if it were possible to bring the cooling liquid into close and rapid enough contact with the layers of the coil it would take about 700 kilowatts to produce this amount of liquid air, so that the total energy expenditure would amount to about 800 kilowatts. However, it is almost certain that this amount of heat could not be conveyed away sufficiently rapidly from a coil of this size. If the coil is made larger the ampère turns must be increased to give the same field so that the amount of liquid air used would have to be still larger. Probably it would be so difficult and costly as to be impracticable.

Before considering the deductions which can be drawn from these experimental results it will be useful to refer to investigations on other phenomena.

The Hall coefficient appears to increase in most substances to about hydrogen temperature and then to decrease, but the measurements are not very decisive and they have not been carried below 14·5° K. yet.

The Thermo-electric power would appear to decrease to zero at $T = 0$, and to be at low temperatures proportional to T^3. The Peltier effect to approach zero at $T = 0$ and to be proportional to T^4. The Thomson effect also to reach zero and to be proportional to T^3.

The magnetic change in the resistance in the ordinary conductive state does not in general call for any particular comment. In the case of bismuth, gold, silver, copper, palladium, and mercury and some alloys of these the resistance increases with increasing fields, and the change is greater in some cases at low temperatures, in others higher, but it follows a regular course for each conductor. However, with iron and nickel the behaviour is irregular, that of nickel being only slight in comparison with the behaviour of pure iron, the character of which will be gathered from the following table:

TABLE V

Magnetic change in the resistance of iron

Values of $\frac{\Delta w}{w} \times 10^4$.

H.	T = 288° K.	T = 20·3° K.	T = 14·5° K.
990	+ 2·8	—	− 1·7
1,500	+ 3·8	− 2·0	· −
2,520	+ 5·7	− 2·9	− 2·6
3,750	+ 6·0	− 2·7	− 3·1
4,940	+ 5·4	− 2·2	− 2·4
6,110	+ 3·2	− 0·9	− 1·4
17,260	+ 0·3	+ 0·7	+ 0·3
8,260	− 2·1	+ 2·6	+ 2·6
9,065	− 4·7	+ 3·6	+ 3·6
9,750	—	+ 4·6	+ 4·7
10,270	− 9·1	+ 5·2	+ 5·4

From this it appears that the effect reverses between the ordinary temperature and that of liquid hydrogen, and that there is a neutral zone at about H = 7,000, where change of temperature has no effect upon the resistance when under this field. It should be noted that at this temperature, and even more markedly below, a very small admixture causes an enormous change in the electrical resistance, so that quite small traces of impurity can be determined with great accuracy. The effect is very marked in some other metals, for instance, an addition of $2/_{0}$ of gold to pure silver reduces the conductivity at liquid hydrogen temperatures to about $\frac{1}{50}$ of its value. For the consideration of the vibrators in the atoms and their relation to the atoms themselves a study of the paramagnetism is instructive. In a very large number of substances Curie found that the susceptibility is inversely proportional to the absolute temperature. However, the majority of paramagnetic substances deviate from this and approxmiate to one where the susceptibility $\chi = \mathrm{C}\mathrm{T}^{\frac{1}{2}}$. Also the deviations from Curie's law appear to be governed by the law of corresponding states. With platinum there is very little change of χ with temperature as also with many other elements. There is an interesting difference between the behaviour of crystallised and anhydrous manganese sulphate which may serve as an indication of the general results found.

TABLE VI

Susceptibility of Magnesium Sulphate.

T.	Crystallised $Mn\,SO_4 \cdot 4H_2O$. $\chi\,T \cdot 10^6$.	Anhydrous $Mn\,SO_4$. $\chi\,(T + \Delta')\,10^6$.
air	19,140	27,910
169·6	18,910	27,920
77·4	19,120	27,870
70·5	19,030	27,960
64·9	18,950	26,590
20·1	18,370	26,210
17·8	18,170	24,420
14·4	17,760	

If the special deviation in the anhydrous salt at very low temperatures is not considered, it appears that with $\Delta^1 = 24$ the anhydrous salt follows the relation expressed with fair approximation. If the number of magnetors is calculated for each molecule they are each found to be 29. The same result is found for crystallised and anhydrous ferrous sulphate, where the number of magnetors is found to be again equal, but 26, putting $\Delta^1 = 31$. The deviations may be considered as due to the much greater effect which the energy of the magnetors has in relation to the heat energy at very low temperatures.

By using the conceptions of zero point energy and of Plank's radiation theory it is found that the zero point energy $= \frac{1}{2}$ hv (where hv = constant of Plank's radiation formula, and v = frequency of the rotational movement of the vibrator). The other Plank's constant k appears in the value which is found to a first approximation for Δ^1 of Table VI. *i.e.* $\Delta^1 = \frac{hv_0}{k}$. If this has a value of 6·6 for crystallised salt, and 83 for the anhydrous, it is found that the observed and the calculated values agree within the errors of experiment until very low temperatures are reached as mentioned above. The quantity v_0 is inversely proportional to the moment of inertia of the molecules. Hence where this is very large with large molecules as with gadolinum sulphate $Gd_2(SO_4)_3\,8H_2O$ the deviations from Curie's law might be expected to be small, which indeed is the case even to hydrogen temperature.

Platinum on the other hand must have molecules with very small moment of inertia to account for its large deviations. If the moments are calculated for $MnSO_4$ and $MnSO_4 . 4H_2O$ they are found to be equal to $8{\cdot}7 \times 10^{-41}$ and $109{\cdot}7 \times 10^{-41}$ respectively. If then the water molecules are arranged symmetrically the distance between the centres of the water molecules and of the sulphate molecule is found to be $4{\cdot}4 \times 10^{-9}$ cm. This is smaller than the usually assumed radius of the hydrogen molecule (1×10^{-8}), and seems to indicate that there is some interpenetration of the molecules of salt and water.

Reference has been made to the quantum theory above for an explanation of electric conductivity, and that the free electrons in their path are continually encountering the atoms of the conductor. The oscillators in the atoms will have motions which will depend on the physical conditions, and if their motion is large and rapid they will oppose more resistance to the passage of the free electrons than if it is small and slow.

The mean free path of the electrons may be put as inversely proportional to the mean amplitude of the vibrations which disturb them. At the critical value for the super-conductive state the excess of energy of these vibrators above the zero point energy must have fallen to a small value, so that the resistance to the motion of the electrons suddenly becomes nearly zero.

As indicated the conductivity of mercury, for instance, becomes at this critical condition about 10^{10} as great as ordinary temperatures. If, then, the assumption is made that the number of electrons per unit volume remains the same, and then the mean free path is calculated in the usual way from the conductivity, values are found which are very large. Thus if 10^{-7} cm. is taken for the value at ordinary temperatures that at 2°45 K. becomes 10^2 cm., which is clearly of the order of the length of the conductor. It is not essential to suppose that any particular electron actually travels this distance, if it is possible to suppose that the atoms are touching in the sense that the addition of one free electron at one end of the series would cause a free electron to be projected with the same velocity from the other end of the series, the bound electrons moving up so as to readjust the distribution. This movement has no effect as it is one of the fundamental conceptions of the electron theory that the movements of the bound electrons do not give rise to any current energy. An analogy can be found in the movements of a long row of equally heavy billiard balls. Here, as is well known, if all the balls are touching in a long row, on striking another ball smartly against one end the ball at the far end is thrown off without any movement of the intervening balls. Hence the super-conductive state may be conceived as one in which the atoms are so closely bound that the movement of an electron from one to the other is of the same nature as the movements of the bound electrons in the atoms. Hence the effect of any disturbing influence, such as high current density, magnetic field, etc. This causes the vibrators between the atoms to separate them so that the electrons cannot pass from one to another without doing work, with the result that an Ohmic resistance is developed.

THE ANTHOCYAN PIGMENTS

By ARTHUR E. EVEREST, M.Sc., Ph.D., *Univ. Coll., Reading.*

The name Anthocyan appears to have been introduced by Marquart (*Die Farben der Blüten*, Bonn, 1835) to designate the blue pigments present in flowers. Later there arose the belief that the red and purple flower pigments were all merely another form of the same blue anthocyan—or, as Fremy and Cloëz styled it, cyanin—and that the variation of colour was merely due to the nature of the cell sap; this resulted in the name anthocyan being indiscriminately applied to all of them. The present use of the term anthocyan as designating a large class of naturally occurring plant pigments gradually became general, as, from time to time evidence accumulated to show that the red, purple, and blue pigments differed considerably among themselves.

The rapid and very important advances that have taken place in this field of research during the last decade make it advisable to deal more fully with this latter phase of the work than is necessary with the work that preceded it, but at the same time the qualitative work of earlier investigators needs due recognition.

As early as 1836, Hope, in a paper read before the Royal Society of Edinburgh (March 21; cf. *Journ. f. pr. Chem.* (10), 269, (1837), concluded, as the result of experiments on a large number of different kinds of flowers, that the pigments, or chromules, present were formed from faintly coloured chromogens, by a variety of changes. Of these chromogens, according to Hope, there were two types, one called by him Erythrogen, which by the action of acids yielded red pigments, and a second, named by him Xanthogen, which with alkalies gave rise to yellow pigments. He concluded that, in orange, red, purple, and blue flowers both were present, whereas in yellow and white flowers only xanthogen was found. From his examination of leaves, he concluded that chlorophyll was accompanied by xanthogen, but that, excepting in cases where reddening was obvious—*e.g.* autumn leaves—no erythrogen was present.

In the following year, Berzelius (*Annalen*, 1837, **21**, 262) published the results of experiments on the pigments present in some berries, *e.g.* cherry, black currant, and in autumn leaves, *e.g.* red currant, and also of his attempts to purify and isolate them. For the red leaf pigments he suggested the name Erythrophyll (leaf red), but pointed out that it would not be wise to use this term, as the pigments of flowers and berries appeared to belong to the same class. Here we see an indication of the expansion of the term anthocyan to cover other than flower pigments.

In his attempts to prepare the pure pigments, Berzelius made use of the precipitation, by means of lead acetate, of the insoluble lead salts of these pigments, and of their regeneration, on decomposing these compounds by means of sulphuretted hydrogen. Berzelius did not obtain any pure pigments, but the above-mentioned method, either as used by him, or with such small modifications as the decomposition of the lead salt by means of hydrochloric acid instead of with sulphuretted hydrogen, has been used by a large number of later workers, though of these, only Grafe (see below) obtained crystalline pigments by this means. Berzelius was not of the opinion that all these pigments could be looked upon as being the same blue substance changed by variation in the cell sap.

The next work of interest was that of Morot (*Annales des Sc. nat.* (3), **13**, 160 (1849-50), who attempted to prepare the blue pigment of the cornflower by repeated precipitation of aqueous solutions by means of alcohol. He did not obtain any pure product, but the method is of interest in that, improved by the use of modern apparatus, it constituted the first step of the process whereby Willstätter and Everest (see below) isolated the pure cornflower pigment. Morot still had nitrogen containing impurities present in his products, and in the presence of nitrogen saw a possible connection between this pigment and chlorophyll, but he was doubtful whether the nitrogen was really a constituent of the pigment. He described the decolourisation on standing in solution, which is characteristic not only of this pigment, but also of nearly all anthocyans. That the decolourisation observed by Morot occurred in other cases was proved by the work of Fremy and Cloëz (*Journ. de Ph. et de Chim.* (3) **25**, 249), who, moreover, by allowing such a decolourised solution to evaporate in the air, whereby the colour returned as the solution became concentrated, showed that the pigment was not destroyed by this change. They, however, looked upon the decolourisation as the result of a reduction of the pigment. These workers used, for their experiments on blue pigments, the cornflower, violet, and iris, and on red ones, the dahlia, rose, and peony. In each case they attempted to purify the pigment by use of the lead salt, as described by Berzelius. In no case, however, did they obtain a pure product.

Fremy and Cloëz discussed the general ideas then current regarding the plant pigments, and pointed out the uselessness of assuming, as so many workers about that time did, that a relationship existed between chlorophyll and the blue and yellow pigments, for, as at that time pure chlorophyll had not been obtained, and the flower pigments were almost uninvestigated, no reliable conclusions could be drawn. They concluded that all anthocyans were one and the same substance—they called it cyanin—and the colour variations were due to the properties of the particular sap. They distinguished three flower pigments, (1) Cyanin (red or blue), (2) Xanthins (yellow, insoluble in water), (3) Xantheïns (yellow, soluble in water). We see here a clear distinction made between the carotin derivatives, corresponding to 2, and the flavone and flavonol derivatives, to 3, both of which occur as yellow flower pigments. These authors considered that (1) and (3) were in no way related to each other, for although they almost invariably found (3) occurring in flowers containing (1), they never observed a blue flower turn yellow, nor a yellow flower turn blue.

Filhol (*Compt. rend.* **39**, 194; *Journ. f. pr. Chem.* 1854, **63**, 78) investigated qualitatively a large number of flowers, and confirmed previous workers' observations that yellow pigments—for which he retained the name Xanthogen (cf. Hope)—were present, not only in yellow and white, but also in red, purple, and blue flowers. He concluded that, with some few exceptions, all the red, purple, and blue pigments were derived from the same anthocyan. He examined the decolourisation of anthocyans in solution, and, finding that the addition of any acid caused the reappearance of colour, concluded that the decolourisation could not be the result of a reduction as suggested by Fremy and Cloëz; in his opinion it was due to the mixing of the pigments with other contents of plant cells, from which they were kept apart in the living plant.

Martens in 1855 (cf. *Jahresber.* 1855, 657) attacked the problem from another point of view, attempting to elucidate the mode of formation of the anthocyan pigments in plants. He further confirmed the presence of the yellow pigments, for which he used the name Xantheïn (cf. Fremy and Cloëz) in anthocyan-containing flowers, and as the result of his work was led to put forward the hypothesis that both yellow and red pigments have their origin in a faintly yellow substance produced in the sap of all plants, and which by oxidation, particularly under the influence of alkalies and light, produces the different yellow pigments, from which, by further action of light and oxygen, the red pigments are produced. It is interesting to note that the relationship thus suggested by Martens as existing between the yellow pigments and the anthocyans is that which has been revived in more recent years by Wheldale; Keeble, Armstrong, and Jones, and others (see below).

In 1859, Morren put forward the suggestion that the blue flower pigments (anthocyans) were the alkali salts of acids which in the free state are red, and for which he uses the name Erythrophyll (cf. Berzelius). His conception of the blue pigments has been confirmed by recent work, but not so that of the red colouring matters.

A number of workers have, at different times, attempted to prepare pure anthocyan pigments by making use of the lead salts (cf. Berzelius), thus Glénard (*Compt. rend.* **47**, 268; *Jahrber.* 1858, 476), working with red wine, and using ethereal hydrochloric acid for the decomposition of his lead salt, obtained a pigment which he called œnolin, and for which he put forward the nitrogen-free formula $C_{20}H_{16}O_{10}$. His pigment was, however, by no means pure. Senier (cf. *Jahresber.* 1878, 970), using rosa gallica, and decomposing his lead compound, suspended in alcohol, either by sulphuric acid or sulphuretted hydrogen, prepared a pigment, for the lead salt of which he gave the formula $C_{21}H_{29}O_{30}Pb_2$. Heise (cf. *Chem. Centralblatt*, 1889, **2**, 953), by similar means, prepared two pigments (A and B) from red wine, using sulphuretted hydrogen for decomposing the lead salt, and suggested that Glénard's compound was a mixture of these. His examination of these products was not complete. Glan (*Dissertation*, Erlangen, 1892), examining the pigment of the deep-red hollyhock, also obtained two products, and in 1894, Heise (cf. *Chem. Centrblt.* 1894, **2**, 846) further prepared two pigments (A and B) from the bilberry (in this case using ethereal hydrochloric acid to decompose the lead salt), and obtaining them in a fairly pure, but amorphous condition, showed that the one (B) was a glucoside of the other (A). He gave analyses and formulæ, but these have proved to be incorrect, though the relative amount of glucose present in his glucoside (B) given by him, has proved approximately correct.

The result of this work of Heise and Glan was to produce a general tendency to consider that the anthocyan pigments were present in plants both as glucosides and non-glucosides, the former predominating somewhat. Molisch, in 1905, decided in favour of their being glucosides; but Grafe, who carried out a continuation of Molisch's work on a preparative scale, reverted to the earlier ideas. The results of recent work have definitely proved that in all investigated cases, these pigments occur only as glucosides, the non-glucosides obtained by the above workers being merely the result of hydrolysis during preparation.

In 1895, Weigert published (*Jahrber. der k. k. önol. and pomol.*, Lehranstalt in Klosterneuburg, 1894-95.) a classification of the anthocyan pigments, thereby completely dispelling the one-pigment idea that had so often been brought forward. In this connection a short summary of the views of earlier workers upon this point will not be out of place. Berzelius was not of the opinion that only one anthocyan pigment existed, whereas Fremy and Cloëz considered that all red, violet, and blue flowers contained the same blue pigment (cyanin), its colour having been changed by the conditions prevailing in the various cell saps. Filhol, as also Wigand (*Bot. Ztg.* 1862, 123), likewise asserted that all red and blue flower colours were produced by different forms of one and the same anthocyan, and Hausen (*Die Farbstoffe der Blüten and Früchten*, Würzburg, 1884, p. 8) went still further, being of the opinion that not only all red colours in flowers, but also those in fruits, were due to one and the same pigment. Wiesner (*Bot. Ztg.* 1862, 392,), however, cast considerable doubt upon the identity of all these pigments.

Weigert, as a result of the comparison of the behaviour of the anthocyan pigments with various reagents, in particular with regard to the colour of their lead salts, and the colour changes that took place on addition of acid or alkali, distinguished two great classes of these compounds; the first—wine-red group—such as gave blue or blue-green lead salts, and whose acidified solution, on addition of alkali, became blue or blue-green, and a second—the beetroot-red group—those which gave red lead salts, and whose acid solution showed no change of colour, or slight change to violet-red, on making alkaline. The anthocyan pigments are, however, of so varying a character that this simple classification of Weigert by no means covers all cases, for even by comparison only of the colour changes on acidification or making alkaline, and by means of the lead compounds, quite a number of sub-groups can be observed.

Overton (*Pring. Jahrber. f. wiss. Bot.* 1899, **33**, 222) also came to the conclusion that a considerable number of different anthocyan pigments existed.

In all the above-mentioned work, either qualitative results only were aimed at, or the preparations were amorphous and lacking the essential characteristics of chemically pure products. From these observations, however, it had become clear that in the anthocyans a large class of new pigments were awaiting chemical investigation, and, moreover, in the light of the work of Heise and Glan, it was evident that at least certain of these pigments must be looked upon as belonging to a class of nitrogen-free glucosides. An expression of this view was made by Molisch in 1905.

In 1903, two papers, both describing the same set of experiments, were published by Griffiths (*Berichte.* **36**, 3959; and *Chem. News*, **88**, 249), which, had they been followed up, might have had very considerably greater influence on this field of work than they have had. He describes the preparation for the first time of an anthocyan in a crystalline condition. His work was carried out with geranium and verbena flowers, but the pigment obtained from the latter contained nitrogen and sulphur and was doubtless impure; that from the geranium contained neither of these elements, and was the only one analysed. His method of preparation consisted in extracting the pigment from the petals with 90% alcohol, and, after filtration, evaporating in vacuo. The residue thus obtained was extracted with absolute alcohol, filtered, and the filtrate again evaporated in vacuo, when the pigment separated in crystalline form. He did not attempt to decide whether the pigment was a glucoside or no. The description of his experiments was very superficial and imperfect; thus, for example, the fact that a fresh 90% alcoholic extract of geranium petals has a fine scarlet colour, but passes in a few minutes to a practically colourless solution, which, however, regains its original colour as evaporation takes place, is not even mentioned.

Very different in character from these papers of Griffiths is the beautifully clear and descriptive publication of the botanist Molisch (*Bot. Ztg.* 1905, 145), and a greater incentive to research upon these pigments than lies in this paper one cannot easily imagine. Molisch, after giving a summary of the literature dealing with the then very doubtful appearance of solid anthocyan pigments in plants, described how, on examination of a number of anthocyan-containing flowers and leaves, he found that these pigments existed not only in solution in the cell sap, but were without doubt, in many cases, present also in the solid state, sometimes as small spheres, sometimes as definitely crystalline formations. Of some of the more well-defined cases he gave illustrations. Having described the appearance of anthocyan crystals in the living plant, he followed up these observations with a description of his attempts to obtain crystals of these pigments outside the plant. In this, also, he was, in several cases (pelargonium, rose, anemone fulgens), successful, and of the resultant microscopic crystals he gave illustrations. Having thus established the fact that some of these pigments readily crystallise, he pointed out that it should not be difficult to prepare them in sufficiently large quantities for chemical examination. In a very slightly modified form, the method whereby Molisch obtained his crystals is so simple and certain, that it is worth describing. One or two petals of the flower are laid upon a piece of glass slightly larger than a microscope slide and upon which are one or two drops of 75% acetic acid, the cell structure broken down by rolling a glass rod over them, leaving the petals flattened on the glass with the cell sap beside or upon them; two or three more drops of 75% acetic acid are then placed on the petals, and a microscope slide pressed down upon them; the whole is then placed under a clock-glass (to ensure slow evaporation), when, after some 12 to 24 hours crystals begin to appear, either round the edge of the slide, or round or on the petals. Scarlet pelargonium gives the best results and very rarely fails; in some cases, using these flowers the author has obtained clusters of crystals so large as to be readily discernible by means of the naked eye.

As mentioned above, Grafe was moved, by the work of Molisch, to attempt the chemical investigation of some of these pigments. He published three papers (*Sitzber. k. Akad. d. Wiss*, Vienna, 1906, 975; 1909, 1033; and 1911, 765), in the first of which he described experiments with red cabbage leaves, and rose petals, from neither of which could he obtain any crystalline pigment, the blue-black berries of ligustrum vulgare, from which he obtained a crystalline product, but was unable to obtain any agreement in his analyses of it, and finally, with the flowers of the hollyhock (althæa rosea), from which he obtained two pigments, one crystalline, one amorphous, and of which he gave analyses. In each case he used the lead compound for the preparation of his pigment, and decomposed it by means of sulphuretted hydrogen. After separation of the hollyhock pigment in this manner, he further purified it by solution in alcohol and precipitation of the pigment by addition of ether; by further solution of the pigment in alcohol and evaporation of the solvent, the pigment separated in deep red crystalline leaflets. A portion of his product from decomposition of the lead salt by sulphuretted hydrogen was insoluble in alcohol, but soluble in water, and from this he obtained the amorphous pigment which he described. He gave the crystalline compound the formula $C_{14}H_{16}O_6$, the amorphous product $C_{20}H_{30}O_{13}$, and considered the latter to be a glucoside of a dibasic acid which contained hydroxyl groups, and probably also an aldehyde group. From the amorphous product he obtained glucose by hydrolysis, but apparently did not examine the non-glucoside pigment produced by this reaction. In his second paper he continued his investigation of the hollyhock pigment, and discussed the formation of the anthocyans in the plant. The third paper of the series contains an account of his further attempts to prepare the pigment of the red cabbage in a crystalline form. Despite the fact that Molisch had failed to obtain crystals by his method, Grafe attempted to produce them by using that process on a larger scale. Failing in his attempts, he tried dialysis—previously used in other cases by Portheim and Scholl (*Ber. deut. Bot. Ges.* 1908, **26a**, 480)—as a means of purification, but again failed to obtain any crystalline product. Grafe then turned his attention to the pigment of the scarlet pelargonium, which had so readily yielded Molisch crystals. He was successful in obtaining this pigment in a fine crystalline condition (microphotographs of the crystals were given) by carrying out Molisch's experiment on a large scale, by use of the lead salt method, or by dialysis. Beyond the crystalline pigment, Grafe described, as in the case of the hollyhock, an amorphous compound, and here also considered the latter to be a glucoside, the former sugar-free. The crystalline compound he described as very unstable and deliquescent; as this is not true of the pure pigment, his crystals must have contained impurities —for it he gave the formula $C_{18}H_{26}O_{13}$, and considered that it was a tribasic acid, having two hydroxyl and two carbonyl groups within the molecule. To the amorphous substance he gave the formula $C_{24}H_{44}O_{20}$, and from it by hydrolysis, obtained glucose; here again, he does not appear to have examined the resulting non-glucoside pigment. Of the crystalline substance he obtained 10 grams, together with 15 grams of the amorphous compound, from some 28 kilograms of fresh petals.

Having thus overcome the experimental difficulties involved, and having for the first time obtained considerable quantities of an anthocyan in a crystalline condition, it is to be regretted that Grafe drew such incorrect conclusions from his results. Doubtless Grafe had in mind the conclusions of Heise and Glan, that both glucoside and non-glucoside pigments were present in the plants they examined, when, on finding that his amorphous product reduced Fehling solution, but that his crystalline substance did not, he concluded that the former was a glucoside, the latter not, and convinced himself of this by hydrolysis of the amorphous product whereby he obtained glucose. Recent work has proved that Grafe's amorphous product must have been an impure specimen of the glucoside, containing reducing sugars, whilst the crystalline substance was the glucoside in practically pure condition; the possibility that such glucosides when pure do not reduce Fehling solution never appears to have occurred to Grafe, and he never seems to have attempted to hydrolyse his crystalline pigments. He concluded that his work, together with that of Heise and Glan, proved the coexistence, in the cases examined, of glucoside and non-glucoside in the plants. This conclusion has, however, been proved erroneous by the work of Willstätter and Everest, who have shown that only the glucoside exists in these plants. Grafe's conclusions led him to suggest that the two pigments he obtained were related to one another, and might be formed one from the other according to the following scheme:

$$C_{24}H_{44}O_{20} + H_2O \rightarrow C_6H_{12}O_6 + C_{18}H_{34}O_{15} \text{ (hypothetical)};$$

$$\text{then: } C_{18}H_{34}O_{15} - 4H_2O + O_2 \rightarrow C_{18}H_{26}O_{13}.$$

A somewhat similar scheme was put forward by him for the hollyhock pigments.

During the years covered by this series of papers by Grafe, a considerable amount of work had been published by botanists, dealing with the formation of the anthocyans in plants. Miss Wheldale, as the result of much botanical work, came to the conclusion that the anthocyans are derived from colourless or faintly coloured chromogens (probably flavone or xanthone derivatives) by oxidation, most probably as the result of the action of peroxidases. She considered that the chromogens were produced by hydrolysis of glucosides that existed in the plant, this reaction being reversible. An essential feature of her theory is that the oxidation of the chromogen, with production of anthocyan, can only take place after the hydrolysis of the glucoside. To represent these changes she proposed the following scheme:

$$\text{Glucoside} + H_2O \rightleftarrows \text{Chromogen} + \text{sugar},$$
$$\text{then: Oxidation of chromogen}^{1} \rightarrow \text{Anthocyan pigment.}$$

As chemical evidence of the latter part of these changes, Nierenstein and Wheldale (*Ber.* 1911, **44**, 3487) and Nierenstein (*Ber.* 1912, **45**, 499) brought forward products obtained by the oxidation of quercetin and chrysin respectively with chromic acid, and which they described as "anthocyan-like" products. The reactions from which they drew this conclusion are, however, by no means sufficient to show that any relationship exists between these compounds and the anthocyans. In this connection, it is necessary to mention the observation of A. G. Perkin (*Journ. Chem. Soc.* 1913, 650), that gossypetin by oxidation in alkaline solution yielded a substance (gossypeton) which was deep blue in alkaline solution, but became red on acidification, accompanied, if in concentrated solution, by the precipitation of the red pigment. He pointed out the bearing of this observation upon the theory of Miss Wheldale. The fact, however, that this pigment is stable to alkalies is not in agreement with the properties of such anthocyans as have as yet been investigated.

Keeble, Armstrong, and Jones (*Proc. Roy. Soc. B.* 1912, **85**, 215, B. 1913, **86**, 308 and 318, B. 1913, **87**, 113, and Keeble and Armstrong, *Journ. Genetics*, 1913, **2**, 277) have published a number of

[1] This oxidation may, or may not, be accompanied by polymerisation.

interesting papers upon the formation of anthocyans, and the hypothesis which they support is very similar to that of Miss Wheldale, but they part company with that author in regard to the process necessary subsequent to hydrolysis of the glucosides, for they maintain that the oxidation must be preceded by reduction of the non-glucoside flavone or flavonol derivative.

In 1913, Willstätter and Everest (*Annalen*, **401**, 189) published an account of investigations upon the anthocyan pigments, and in particular of the pigment of the cornflower. In this communication some important conclusions were arrived at. It was proved that the blue form of cornflower pigment was a potassium salt, the free pigment being violet in colour, whereas the red form was not, as had always been assumed by previous workers, the free pigment, but an oxonium salt in which the pigment was combined with an equivalent of some mineral or plant acid. The anthocyans were found to be most stable when in the form of these oxonium salts. It was definitely proved that the decolourisation in solution, so often mentioned by other workers, was not due to reduction.

Having obtained the cornflower pigment pure and crystalline in the form of its chloride, they proved that it was a disaccharide from which, on hydrolysis, two molecules of glucose were split off from each molecule of pigment; the sugar-free pigment was obtained in a finely crystalline condition. Microphotographs of the crystals were given.

The first step in their preparation of the pure glucoside consisted in repeated rapid fractional precipitation of the blue pigment from aqueous solution by means of alcohol. By this means a pigment about 30% pure was obtained, at which stage it was converted into the chloride by solution in hydrochloric acid, nearly all the impurities (chiefly pentosans) were precipitated by the addition of alcohol, but the pigment remained in solution; on addition of much ether to the clear solution, the pigment was precipitated. After further fractional precipitation from alcoholic solution by means of ether, the pigment was taken up in alcohol, some dilute hydrochloric acid added, and slow evaporation allowed to take place, when the glucoside separated as chloride in well-formed crystals. The pure glucoside does not reduce Fehling solution. By boiling the crystalline glucoside with 20% hydrochloric acid for three minutes, it was quantitatively hydrolised, yielding glucose, and the non-glucoside pigment which separated from the aqueous acid, also as chloride, in the form of long needles.

They introduced a reaction whereby it is easy to decide whether the glucoside, the non-glucoside, or a mixture of the two is present in any specimen of anthocyan pigment. This was based upon the fact that the glucosides remain entirely in dilute acid solution (preferably sulphuric acid) when shaken with amyl alcohol, whereas the non-glucoside anthocyans, when similarly treated, pass quantitatively into the alcoholic layer, producing a red solution. Moreover, when shaken with sodium acetate solution, the red amyl alcoholic solution so obtained becomes violet—or red-violet—but the pigment remains in the alcohol; when shaken with sodium carbonate solution, it turns blue, or blue-green, and the colour descends quantitatively to the aqueous layer.

By means of this reaction they were able to show that in every anthocyan containing flower, leaf, or fruit examined, the anthocyan was present entirely as glucoside. This generalisation, which so completely reversed the views of previous authors, has been further confirmed by Willstätter's later work. To prevent confusion these authors proposed the terms Anthocyanins and Anthocyanidins for the glucoside and non-glucoside pigments respectively, and in agreement with this assigned to the glucoside present in the cornflower the name introduced by Fremy and Cloëz, Cyanin, whereas to the sugar-free pigment obtained by hydrolysis of cyanin the name Cyanidin was given.

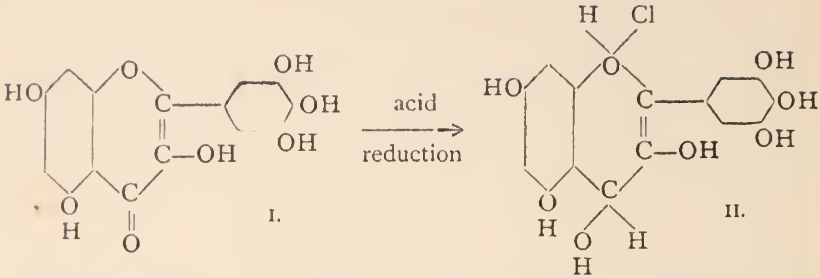
By careful oxidation of cyanidin with hydrogen-peroxide, a yellow crystalline product was obtained which, in its reactions, closely resembled the pigments of the flavonol series.

It is surprising, in view of the fact that the crystalline nature of several of the anthocyan pigments had been established, and that the papers in which they were prepared are even cited by her, that Miss Wheldale (*Biochem. Journ.* 1914, **8**, 204) further published work in which she concluded that the fact that she failed to obtain a crystalline pigment, and that her product had no melting-point, was evidence of the high molecular weights of the anthocyan pigments. By comparison with the case of cyanidin chloride, the melting-point evidence collapses at once, and it appears as though the non-crystalline condition of her pigment can only be taken as evidence of the presence of a small quantity of impurity.

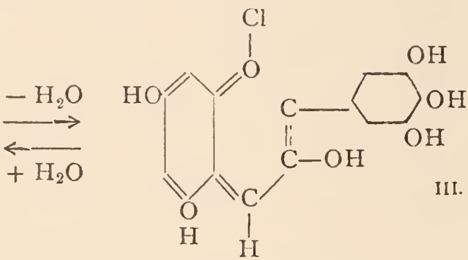
That luteolin and morin give red pigments on reduction in acid alcoholic solution by means of sodium amalgam has been known for many years (cf. Rupe, *Die Chemie der natürlichen Farbstoffe*, vol. i. pp. 77 and 85). Quite recently, Watson and Sen (*Journ. Chem. Soc.* 1914, 389) obtained a red pigment, from quercetin in like manner, and this was followed by production by R. Combes (*Compt. rend.* 1913, 1002) of a red pigment, identical with that which he had obtained from the red leaves of ampelopsis hederacea, by reducing in the same way, the yellow pigment he obtained from the green leaves of the same plant. A further paper of Combes (*C. rend.* 1913, 1454) described the reverse change, viz. from red to yellow, by means of oxidation with hydrogen peroxide. In each case he obtained crystalline compounds and compared their melting-points; he did not, however, give analyses, nor state whether he worked with glucosides or no.

That a series of red pigments, whose properties coincide in every way with those of the anthocyanidins, may be produced by reduction of the flavonol derivatives by various methods, the best of which appears to be treatment of the pigment dissolved in a mixture of five volumes absolute alcohol and one volume concentrated hydrochloric acid, with magnesium, has been confirmed by Everest (*Proc. Roy. Soc.* 1914, B, **87**, 444), and, moreover, in the same paper he described the production by the same means of anthocyanins from the glucoside flavonol derivatives present in various flowers, showing the direct formation of red glucoside pigments from the yellow flavonol glucosides without intermediate hydrolysis. This important observation makes the hypothesis of Miss Wheldale and others, in which hydrolysis of the flavonol glucoside is an essential factor, unnecessary, and, moreover, the above-mentioned observations show that reduction, not oxidation, appears to be necessary for the passage from flavonol to anthocyan.

The results obtained, coupled with those of Willstätter and Everest, in their investigation of the cornflower pigment, led Everest to suggest the following scheme as a typical representation of the passage from flavonol to anthocyan, and to draw attention to the fact that all available evidence pointed to the formula III being that of a typical anthocyan pigment.



I. Flavonol derivative. II. Colourless, or faintly coloured intermediate product.



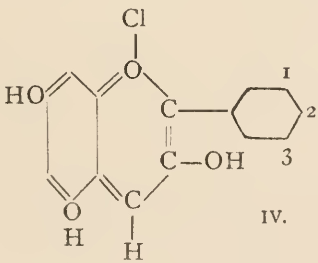
III. Anthocyan pigment.

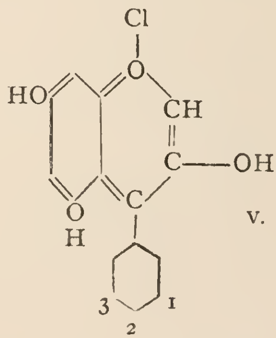
The most recent paper upon this subject is an account of the most valuable and beautiful work yet published upon the anthocyan pigments. It is a brief account of the continuation of the investigations of Willstätter and his collaborators (Willstätter, Mieg, Bolton, Zollinger, Mallison, and Nolan, *Sitzber. der k. Akad. Wiss. Berlin*, 1914, **12**, 402). They have examined the pigments of the rose (gallica), cranberry, grape (deep coloured wine grape), bilberry, delphinium (purple), pelargonium (scarlet), and hollyhock (deep red), and in each case isolated the pure crystalline glucoside, as chloride, and from it, by hydrolysis, obtained the pure crystalline non-glucoside. They have established beyond doubt the formulæ and structure of these compounds—indeed, this paper represents so far-reaching and important an advance in the chemistry of the anthocyan pigments, that its value cannot be over-estimated.

The methods of isolation of the glucosides have been so improved, that they now consist of but a few simple operations; thus, for example, in the case of the grape pigment, it is only necessary to extract with cold glacial acetic acid, and after filtration, precipitate the pigment by means of ether, dissolve the washed precipitate in excess of warm aqueous picric acid solution, and allow to cool, when fine crystals of the picrate separate. The picrate may be decomposed by methyl-alcoholic hydrochloric acid, and the chloride thus obtained recrystallised from aqueous alcoholic hydrochloric acid. This method is also applicable to the bilberry, cranberry, and hollyhock pigments. In other cases, the isolation of the glucosides was obtained by use only of the knowledge of the solubilities of their chlorides.

Both the results of analyses and those of decomposition of the pigments by heating with alkali, agree in pointing to a formula that is closely related to that of flavonol.

All these pigments, on heating with alkali, yielded phloroglucinol and a phenolic derivative of benzoic acid (which varied with the pigment taken). In each case all but one of the hydroxyl groups present in the molecule can be accounted for in the decomposition products, hence the remaining one, as in flavonol, must be attached to a carbon atom in the pyrone ring. The evidence produced proves that all the anthocyan pigments examined were derived from IV or V, but there is at present insufficient evidence for decision between these two formulæ.


IV.


V.

As on heating with alkali, cyanidin chloride ($C_{15}H_{11}O_6Cl$) yields phloroglucinol and 3·4 dioxy-benzoic acid; pelargonidin chloride ($C_{15}H_{11}O_5Cl$) yields phloroglucinol and p-oxy-benzoic acid; and delphinidin chloride ($C_{15}H_{11}O_7Cl$) yields phloroglucinol and gallic acid, it is evident that cyanidin has two hydroxyl groups substituted in the positions marked 1 and 2, pelargonidin one only, in the position 2, whilst delphinidin has three, in the positions 1, 2 and 3.

From analyses of the colourless iso-forms of the anthocyan pigments, Willstätter has shown that they are produced by the addition of one molecule of water per molecule of pigment, which observation is in complete agreement with the scheme (see p. 610) put forward by Everest in the paper mentioned above, and indeed, Willstätter concludes that such of the anthocyans as are γ pyrone derivatives should be produced by the reduction of flavonols.[1]

It is interesting to note that in the cornflower, and in the rose (gallica), the same pigment, cyanidin, and combined with the same amount of glucose (2 molecules) is present, and, moreover, in the cranberry the pigment is cyanidin combined with but one molecule of glucose. The delphinium pigment is the most complex, yielding on hydrolysis one molecule delphinidin, two molecules glucose, and two molecules p-oxy-benzoic acid.

There doubtless remains much work to be done in this field of research, but the time has at last arrived when one may speak with certainty of the structure of many of the members of this beautiful series of pigments, and, moreover, as the flavonol derivatives have been synthesised by Kostanecki, the production of anthocyans from them, as mentioned above, completes the synthesis of at least one type, and, in addition, it may well be expected that these great advances in our knowledge of the chemistry of these pigments will materially assist in the elucidation of the processes whereby they are formed in the tissues of the living plant.

[1] Although his evidence brought him to support the structural formula previously put forward by Everest for the Anthocyan pigments, Willstätter, in the abovementioned paper, denied that Everest's pigments were true Anthocyans. In a paper published since the present article was written (*Sitzber. der k. Akad. Wiss.* Berlin, 1914, 769–77) Willstätter, having repeated Everest's work, withdraws his criticism, and admits that by the reduction of quercetin, a pigment identical in every respect with natural Cyanidin (the pigment of the cornflower, rose, and cranberry) is obtained. Thus the crowning feature in this field of research—the complete synthesis of these pigments—receives confirmation from an independent source.

VERTEBRATE PALÆONTOLOGY IN 1914

By R. LYDEKKER, B.A., F.R.S.

The war has left its fell mark on the palæontological work of 1914, as on everything else; and the output (or, at all events, the published work which has reached this country) has consequently been very much below the normal. Up to the end of July matters in this respect went on much in the ordinary way; but immediately following the outbreak of the war there was, of course, a complete cessation of all scientific (as well as other) literature from Germany and Austria, while, for a time at any rate, but few scientific publications were received from France, while the subsequent arrivals have been irregular and much fewer than ordinary. Belgium, of course, has entirely ceased even to think about scientific work. Italy and America, on the other hand, have maintained scientific work without interruption, as has likewise, to a great extent, the United Kingdom, where the leading biological and palæontological journals have continued to appear with their normal regularity.

In spite of all these hindrances, and bearing in mind the possibility that a certain amount of published work may never have reached this country, there is still a fair tale to record, although there is nothing of what may be called an epoch-making character or even of surpassing interest.

The most important part of the year's work is undoubtedly that on the mammal-like reptiles and their structural resemblances and relationships to the stegocephalian amphibians, as well as those of the latter to fringe-finned fishes.

To the editor of *Nature* the writer is indebted for permission, as in previous years, to reproduce the purport of a number of paragraphs written by himself for that journal. His thanks are also due to the American Museum of Natural History and to the *Museums Journal* for permission to reproduce photographs.

Commencing with faunistic mammal papers, attention may first be directed to an account by Mr. L. Glauert, in vol. iii., part 3, of *Records of the West Australian Museum*, of further discoveries of mammalian remains in the so-called mammoth-cave of that colony. The most interesting of the new remains represent a species of spiny ant-eater or echidna, believed to have been double the size of the living Australian *Echidna aculeata*, and also exceeding in size any of the previously described extinct forms, one of which has been referred to the genus *Zaglossus* (*Proëchidna*), now restricted to New Guinea. The new specimens are, however, considered to represent a still larger species, for which the name *Zaglossus hacketti* is proposed. Remains of the Tasmanian wolf and of the Tasmanian devil are also recorded from the same cavern.

Here it may be mentioned that in No. 5 of *Memoirs of the Melbourne Museum* Mr. F. Chapman discusses the fossil Tertiary cetaceans and fishes of Australia in connection with the relative ages of the Tertiary formations of that continent, as compared with those of Europe.

In vol. xliii., part 4, of the *Records of the Geological Survey of India* Dr. G. E. Pilgrim discusses the correlation of the Indian Siwaliks with European upper Tertiary mammaliferous horizons. It is concluded that while the topmost conglomerates of the Siwaliks of the Punjab (with remains of camels, and of buffaloes specifically inseparable from the living Indian representative of the group) are the equivalent of the Upper Pliocene, the Bugti beds of Baluchistan correspond to the Lower Burdigalian or Upper Aquitanian of Europe. Several forms, including two genera akin to the sabre-toothed tigers (*Paramachœrodus* and *Sivœlurus*) and a genus of bears (*Indarctos*), are described as new. The middle Siwaliks of Dhok Pathan display a marked faunistic affinity with the Lower Pliocene Pontian horizon of Eastern Europe, especially as regards the occurrence of giraffoids in the latter. It should be stated that Dr. Pilgrim's paper was published in 1913. In addition to giving a list of the genera and species hitherto recognised from this formation, the author names and describes a new species of the catlike genus *Pseudœlurus*. In the *Records* of the Indian Survey (vol. xliv. pp. 225–33) Dr. Pilgrim shows that *Indarctos* is closely related to *Hyænarctus punjabiensis* of the same formation.

An important contribution to the mammalian palæontology of Eastern Europe is embodied in the first part of an illustrated memoir, by Mr. A. Borissiak, on the fossil mammals of the Sebastopol district, published, with a French translation, in part 87 of *Mém. Com. Géol. Russ.* ser. 2. It comprises 154 pages of text and ten quarto plates. This fauna, it appears, was discovered in 1908 during the progress of some drainage-works, and probably extends over a considerable area, as isolated bones have been found in other parts of the city and its environs. The deposit, which is very rich in bones, skulls, and teeth, forms an ossiferous breccia, in the shape of lenticular masses intercalated in a stratum of greenish white limestone. No complete skeletons occur, the bones being mixed up *pell-mell* in the bed. The fauna evidently corresponds to the Sarmatian horizon of Russian Poland, and contains, among other forms, representatives of the three-toed *Hipparion*, of hornless rhinoceroses (*Aceratherium*), and of the extinct antilopine genus *Tragoceros*, together with a new generic type of the giraffe family, for which the name *Achtiaria* is proposed.

A summary of the fossil faunas and floras of Austria, by Dr. F. König, was published during the year in Vienna, in *Österreich. Zeitschrift für Berg- und Huttenwesen*, 1914, Nos. 1, 2, and 4. Land vertebrates, including mammals, reptiles, and amphibians, occupy a considerable portion of this contribution, which is illustrated with a double plate of restorations of some of the larger and better-known species. Although the author notes the poverty of Austrian fossil vertebrate faunas, as contrasted with those of many other countries, he has still been able to make out a respectable list of fossiliferous horizons and of genera by which they are represented.

Here, too, may be appropriately mentioned an article by Dr. W. D. Matthew, published in the *Bull. Geol. Soc. America*, vol. xxv. pp. 38 *et seq.*, relating to the bearing of the Palæocene vertebrate fauna on the Cretaceous-Tertiary problem. But, in spite of the fact that all Dr. Matthew's contributions to palæontology teem with interest, this brief mention must suffice in this instance, as the paper is rather off the line of the present review.

In South America Dr. H. von Ihering has contributed to *Notas Preliminares da Revista do Museu Paulista* (vol. i. No. 3, pp. 126–48) an article on the Cretaceous and Tertiary strata of Argentina and the migrations of mammal-faunas on the American continent. The alleged occurrence of placental mammals with dinosaurs in the Cretaceous *Notostylops*-beds is confirmed, and the author's theory of a former "Archelenis" connecting Brazil with West Africa is reviewed. The remarkable suggestion is made that ground-sloths (which would almost of necessity likewise imply true sloths and ant-eaters) and glyptodonts, in place of being autocthonous South American groups, really came from Asia by way of "Archigalenis," a land-bridge connecting Eastern Asia with Central America.

Intimately connected with the above is a memoir occupying the whole of vol. xxv. of the *An. Mus. Nac. Hist. Nat. Buenos Aires*, and dealing with the mammalian fossils of the Tertiary Araucanian formations of Argentina, as specially represented by those of Monte Hermoso and the Rio Negro. The author, Señor Cayetano Rovereto, records a large number of species, many of which are described as new, and likewise names several new genera. Most, at any rate, of the species belong to types already familiar through the works of the late Dr. F. Ameghino, Dr. W. B. Scott, and others; and it may be a question whether some, at least, of the generic types described as new are not based on trivial characters or on those due to immaturity. Several new generic names are also proposed for large extinct birds, two of these, *Hermosiornis* and *Procariama*, being regarded as the representatives of a family, *Hermosiornidæ* (or, as it should have been termed, *Hermosiornithidæ*), allied to the existing Brazilian seriema, while a third, *Prophororhacus*, is referred to the extinct *Phororhacidæ*, as represented by *Phororhacus*, one of the most gigantic and at the same time the biggest-headed bird that ever stalked over this earth. A few land-tortoises and other reptiles are also referred to, and in some cases named.

An important contribution is made to our knowledge of the recently discovered Miocene mammal-fauna of British East Africa by Dr. C. W. Andrews in the *Quart. Journ. Geol. Soc.* vol. lxx. pp. 163–86.

Reverting to South America, it may be mentioned that for many years past the palæontologists of the Buenos Aires Museum, following the lead of the late Dr. Florentino Ameghino, have assigned to the Tertiary mammalian faunas of the country a much greater age than appears to be indicated by their relative degree of evolution and various other factors. As there appears to be evidence of the presence of man among the more recent of these faunas, one consequence of this putting-back of the palæontological clock is to give to the human race in Argentina a much greater age than it seems entitled to. The latest development of the subject occurs in an article published in the Buenos Aires journal *La Nacion*, where positive claim is advanced to the discovery of decisive evidence of the existence of man in Argentina during the Miocene epoch. The basis of this claim is the discovery by Señor Carlos Ameghino, in a deposit in the Chapalmalal gulley, on the Atlantic coast of the province of Buenos Aires, of a femur of an ancestral member of the group of ungulates typified by *Toxodon platensis* of the Pampean beds, in the shaft of which is embedded part of what has been identified as a flint arrow-head. A figure in the article shows that this presumed arrow-head is broken off at the level of the surface of the bone; but no explanation is offered how such a feeble weapon could have penetrated a solid bone like a toxodont femur. Various other traces of man are stated to have been obtained from the Chapalmalal beds, which are regarded as older than the Pampean formation, in which the so-called "*Homo pampaeus*" occurs; and if the views of Señor Ameghino with regard to the arrow-head be accepted, it must apparently be admitted that a human being acquainted with the use of fire, and capable of manufacturing bows and arrows, lived with the extinct Chapalmalal fauna. This, however, is far from affording proof that man, in common with the rest of the fauna, was of Miocene age, and in existence prior to the union of South with North America; this contention being so absolutely improbable, not to say impossible, that it is unworthy of serious consideration.

Among papers on particular groups or species of fossil mammals, bare mention of the title must suffice in the case of one by Dr. O. Schlaginhaufer, in the *Neujahrsbl. naturfor. Ges. Zürich*, vol. cxvi. pp. 1–19, on the most noteworthy remains of fossil men and manlike creatures. The same course may also be taken with regard to the full description by Monsieur E. G. Dehaut (*Hist. Zool. et Pal. de Corse et Sardaigne*, fasc. 5, Paris, 1914) of the remains of a monkey from the Sardinian Pleistocene, for which the author has previously proposed the new generic name *Ophthalmomegas*, in allusion to the large size of the eye-sockets.

In the article on the present subject published in SCIENCE PROGRESS for April 1913, reference was made to the lower jaw of an anthropoid ape from the Tertiary of the Pyrenees. This interesting specimen has been redescribed and figured by Dr. A. Smith Woodward in vol. lxx. (pp. 316–20, plate xliv.) of the *Quart. Journ. Geol. Soc.*, where it is identified with *Dryopithecus fontani.* It was obtained from the Upper Miocene of Seo de Urgel, Lérida, Spain. In several respects, notably in the form of the jaw at and near the point of insertion of the digastric muscle and in the small size of the first molar, *Dryopithecus* approximates to the contemporary macaque-like genus *Mesopithecus*, and may therefore be regarded as a primitive type. The relatively small size of the first molar in these genera is, however, paralleled among modern anthropoids, although to a somewhat less degree, in the gibbons and gorilla.

Considerable interest attaches to a short article by Fräulein Albertina Carlsson in the *Proceedings of the Zoological Society* for 1914 (pp. 227–30, pl. i.) on the skulls of two small carnivores from the well-known Oligocene Phosphorites of Quercy, Central France. Both belong to species named by the late Dr. H. Filhol, the historian of the Quercy fossils. One of the specimens represents *Cynodictis intermedius*, a member of a genus in some degree intermediate between the civets and mongooses (*Viverridæ*) on the one hand and the dogs (*Canidæ*) on the other; the base of the skull being dog-like, while the brain-case approximates to the civet-type. The dentition of the genus is also to a considerable degree intermediate between the two. It will be remembered that the late Sir William Flower grouped cats, civets, and hyænas in one subordinal group (Æluroidea), dogs in a second (Cynoidea), and bears, weasels, and raccoons in a third (Arctoidea). We now know that civets pass imperceptibly into dogs, and dogs into bears, and thus into raccoons. The affinities of *Cynodon gracilis*, the second species in Fräulein Carlsson's communication, are not discussed by the author.

Those interested in the past history of the great family of mice and rats (*Muridæ*) will find a store of information with regard to Hungarian fossil forms in a memoir by Prof. L. von Méhely, published in the *Ann. Mus. Nat. Hungar.* vol. xii. pp. 155–243, with eight plates. Rodents from the Red Crag and Forest-bed of the east coast form the subject of a paper by Mr. M. A. C. Hinton (*Ann. Mag. Nat. Hist.* ser. 8, vol. xiii. pp. 186-95), in which a new squirrel is named.

Reference has been made above to new artiodactyle ungulates, including a new genus of the giraffe tribe; and in this connection mention may be made of a note by Mr. H. Stokes in the *Irish Naturalist* for the year under consideration (vol. xxiii. pp. 113–18) on the mode of occurrence of skulls and skeletons of the so-called Irish elk in the bogs of Howth and Ballybetagh, county Dublin.

Another contribution to the past history of the deer tribe is made by Mr. E. von Niezabitowski, in the *Bulletin* of the Cracow Academy for January 1914 (p. 56), on the occurrence of fossil remains of the reindeer in Galicia, and the local forms by which it is there represented.

From a distributional point of view some interest attaches to the discovery of remains of the musk-ox in the diluvium of the Emschertal, Westphalia, recorded by Mr. P. Kukuk in *Zeits. deutsch. Geol. Ges.* vol. lxv. pp. 596–600, pls. xix. xx., 1913.

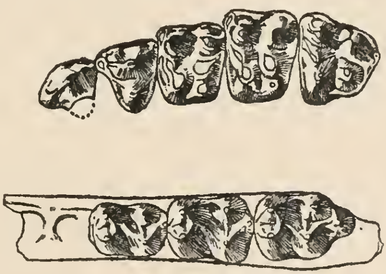


FIG. 1.—The hinder left upper and lower cheek-teeth of *Homacodon vagans*, an Eocene Bunodont Artiodactyle.
(From Sinclair, *Bull. Amer. Mus. Nat. Hist.*)

In the same serial for 1914 (vol. lxvi. pp. 1–33, pls. i.–iii.) Dr. E. von Stromer continues his account of the Tertiary fauna of the Wadi Natrun, in the Fayum district of Egypt, dealing in this instance with remains of hippopotamus from the Middle Pliocene beds. These are identified with *Hippopotamus* (*Hexaprotodon*) *hipponensis*, a species with six lower incisors, originally described by the late Prof. A. Gaudry from the Algerian Tertiary.

Few groups of mammals offer greater difficulties to the systematist than the early Tertiary bunodont (*i.e.* those in which the cheek-teeth are surmounted by simple, cone-like cusps) forerunners of the artiodactyle, or even-toed, ungulates. This is partly due to the close resemblance, *inter se*, of many of these animals, and in part to the resemblance of their cheek-teeth (fig. 1) in some cases to those of apes and in others to those of early perissodactyle, or odd-toed, ungulates. An attempt to put matters on a better footing in the case of the North American Middle and Lower Eocene representatives of the group has been made by Mr. W. J. Sinclair in an article published in the *Bulletin of the American Museum of Natural History* (vol. xxiii. pp. 267–95). Several new genera, such as *Wahsatchia* and *Bunophorus*, are named, and the whole group is included in the family *Dichobunidæ*, as typified by the European Oligocene genus *Dichobune*. *Sarcolemur*, originally regarded as a lemuroid, and *Microsus* of the Middle Eocene may have been the ancestors of the selenodont, or crescent-toothed, artiodactyles of the Upper Eocene Uinta beds.

Three publications dealing with the horse family (*Equidæ*) and its extinct forerunners were published during the year in America. *The Evolution of the Horse*, as the first is entitled, is an illustrated guide to the members of the group exhibited in the American Museum of Natural History, in which Dr. W. D. Matthew discusses the evolution of the horse-group in nature, while Mr. S. H. Chubb deals with the origin of the domesticated breeds and the structure, growth, and succession of the teeth. In the second publication, issued as a guide-book to the remains of extinct perissodactyles allied to the existing horse group preserved in Yale University, Dr. R. S. Lull records the various exploring and collecting expeditions from 1870 onwards which have contributed to the collection, and concludes with a brief summary of the equine pedigree. Finally, in a memoir published by the Irving Press, New York, under the title of *The Horse, Past and Present*, Prof. H. F. Osborn has done much the same good work in respect to the collection illustrating the horse-group and its ancestors in the American Museum of Natural History.

Here may be mentioned a curious incident relating to extinct Perissodactyla which occurred during the year. In 1872 Sir R. Owen described the skull and part of the skeleton of a small perissodactyle obtained by the Rev. R. Bull, then vicar of Harwich, from the London Clay of that parish, under the new generic and specific name of *Pliolophus vulpiceps*. A cast of the skull, a small fragment cut from one side of the original specimen, and two or three limb-bones have been in the collection of the British Museum ever since the date of Owen's paper, but what had become of the remainder of the skull was unknown till this interesting and valuable specimen was presented in the autumn of 1914 by Mrs. Bull, the widow of the original owner, to the Natural History Branch of the British Museum. It may be added that *Pliolophus* is now known to be identical with *Hyracotherium*, a genus described by Owen himself at an earlier period, and one of the forerunners of the horse-line.

The evolution of the great primitive American Tertiary perissodactyles of the family *Titanotheriidæ* has continued to engage the attention of Prof. H. F. Osborn, who has published a short paper on the subject in the *Bull. Geol. Soc. America*, vol. xxv. pp. 403–5, in which certain modifications are made in his earlier phylogenetic scheme. On page 406 of the same

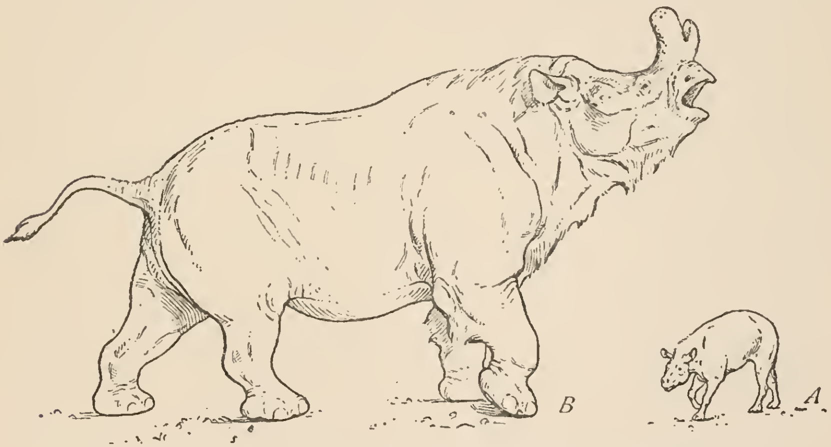


FIG. 2.—Restoration of the earliest and latest Titanotheres—*Eotitanops* (A) and *Brontotherium* (B).
(From Osborn, *Bull. Geol. Soc. America.*)

issue he gives restorations, to scale, of the earliest and latest representatives of the group, namely *Eotitanops* of the Lower Eocene Wind River beds, and *Brontotherium* of the Oligocene White River horizon. Compared with the former, the latter shows an advance in specialisation and bodily size (fig. 2) analogous to that between the modern horse and the Eocene *Hyracotherium*. Alike from the stratigraphical and the systematic point of view, a finely preserved palate of a rhinoceros from the Upper Miocene Mæotic strata of the Odessa district, described and figured by Mr. E. Kiernik in the *Bull. Ac. Sci. Cracovie* for 1913, pp. 808–64, pl. lxviii. (1914), is of more than ordinary interest. It is allied to *Aceratherium blanfordi* of the Bugti beds of Baluchistan and *A. schlosseri* of the Miocene of Samos, with both of which it agrees in its extremely short-crowned molars. Indeed, its resemblance to the latter is so close, that it probably belongs to the same species, as also does another skull from the Odessa neighbourhood previously described by Prof. Niezabitowski as *Teleoceras ponticus*.

In another communication (*Bull. Amer. Mus. Nat. Hist.* vol. xxxii. pp. 261–74, 1913) Prof. Osborn shows that a skull from the Eocene of Wyoming, described by the late Prof. E. D. Cope in 1884 under the name of *Triplopus amarorum*, does not belong to that rhinoceros-like genus at all, but is a member of the *Chalicotheriidæ*, or perissodactyles with edentate-like claws, of which it is the earliest known representative. It is now made the type of the new genus *Eomoropus*, and regarded as a specialised offshoot from the stock which gave rise to the titanotheres, on one hand, and to the fore-runners of the horse-group on the other. For many years American palæontologists regarded the *Chalicotheriidæ* as the representative of a distinct subordinal group, the Ancylopoda, of equal rank with the Perissodactyla, but they are now classed as a family of that suborder.

The next paper for mention is one by Mr. Ivar Sefve, who recently made an expedition to Peru and Bolivia for the purpose of collecting fossil bones, on a new species of *Macrauchenia* from Ulloma, in the country last named. The paper is published in vol. xii. (pp. 205–56, pls. xiv.–xviii.) of the *Bulletin* of the Geological Society of Upsala. *Macrauchenia*, it may be recalled, was a half camel-like, half horse-like, three-toed ungulate, the remains of which were originally discovered by Darwin in Patagonia, where they had been weathered out from the superficial deposits. Long included in the Perissodactyla, it is now regarded as the typical representative of a distinct South American subordinal group, the Litopterna. The skeleton of the Bolivian species—*M. ullomensis*—exhibits several structural peculiarities, notably in the pelvis, which is very strongly welded in an unusually complex manner with the vertebral column; the sacrum being also remarkably stout and massive.

Although detached skulls, teeth, and bones of the American mastodon (*Mastodon americanus*, or *Mammut americanum*, as it is now designated in its native country) are abundant enough in the superficial deposits of the United States, entire skeletons are sufficiently rare to merit special notice. The latest discovery of this nature, as recorded by Mr. C. Schuchert in the *American Journal of Science*, April 1914 (vol. xxxvii. pp. 321–30), occurred in Connecticut in August 1913, when some Italian workmen engaged in digging a trench to drain a swamp, came on what proved to be an entire skeleton, which has now found a home in the Peabody Museum, Yale. Some specimens of the American mastodon are furnished with a small single tusk on one side of the lower jaw, while others carry a pair. Such individuals, according to a supplemental note to Mr. Schuchert's paper by Prof. R. S. Lull, are males.

Several papers published during 1913 had not come under my notice when writing the article for that year. Among these is one by Dr. R. N. Wegner on fossils from Miocene beds near Oppeln, Upper Silesia, published in the sixtieth volume of the *Palæontographica* (pp. 175–274, 7 pls.). It contains the description of a new race of the widely spread *Mastodon* (*Tetrabelodon*) *angustidens*, for which the name of *M. a. austro-germanica* [*us*] is proposed.

Among the numerous series of remains obtained from the asphalt-beds of Rancho La Brea, California, which, as noticed in an earlier article of the present series, formed a death-trap during the Pleistocene period for the animals of the surrounding country, those of ground-sloths are some of the most common. The identification of the species to which these remains belong forms the subject of a note in *Science*, ser. 2, vol. xxxix. pp. 761–3, and of a longer article in the *Bulletin* of the Geological Publications of California University, vol. viii. pp. 319–34, by Mr. Chester Stock, who concludes that all are referable to the typical *Mylodon harlani* of North America.

The affinities of the Multituberculata, that remarkable group of primitive, and yet in some respects specialised, mammals, which ranges from the Trias to the Lower Eocene, form the subject of an article contributed by Dr. R. Broom to the *Bull. Amer. Mus. Nat. Hist.* vol. xxxiii. pp. 115–34. The group has been pushed from pillar to post, some naturalists, like Dr. Gidley in 1909,[1] including them in the marsupials, while others consider that their relationships are with the monotremes (duckbill and echidna). Dr. Broom adopts the latter view, and considers it

[1] See the article on Vertebrate Palæontology in SCIENCE PROGRESS for that year.

probable that mammals originated during the Triassic period from the cynodont reptiles of South Africa, and that from this original stock there diverged at an early period a branch which gave rise to the Multituberculata, and later, after considerable specialisation and degeneration, to the Monotremata. In the general form of their skulls, and to some degree in their dentition, the multituberculates show an approximation to the rodents, but this is probably a case of parallelism in development. Very noteworthy is the fact that the Tertiary American genus *Polymastodon* makes a nearer approximation to monotremes than does the Triassic African *Tritylodon*. The former genus was described by the late Prof. E. D. Cope so long ago as

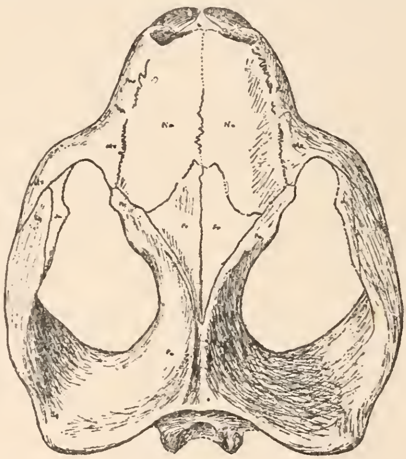


FIG. 3.—Upper view of skull of *Polymastodon taöensis*.
Fr, frontal; *Ju*, jugal; *Mx*, maxilla; *Pa*, parietal; *Pmx*, premaxilla; *Sq*, squamosal.
(From Broom, *Bull. Amer. Mus. Nat. Hist.*)

1870, on the evidence of a fragmentary skull and teeth from the Puerco, or Lowest, Eocene of New Mexico, and has hitherto been very imperfectly known. An expedition despatched by the American Museum to the Puerco beds of New Mexico was, however, fortunate enough to discover a number of remains of the genus, among them being a skull which, although much crushed and broken, was found to be capable of restoration. In the general characters of its dentition and its relative shortness and breadth, the skull (figs. 3, 4), which measures about 6 in. in length, distantly recalls that of a rodent, the dental formula being—

$$i. \frac{2}{1}, \ c. \frac{0}{0}, \ p. + m. \frac{3}{3}.$$

Among its peculiarities are the cutting-off, by processes of the parietals and nasals, of the frontals from the orbits, and the apparent absence of lachrymals.

In his account of the Fishes of the *Terra Nova* Expedition, published by the British Museum, Mr. C. T. Regan dissents from the opinion that the carnivorous mammals of the Patagonian Santa Cruz beds are related to the Tasmanian thylacyne.

Turning to birds, reference may be made to an important article in the *Transactions* of the New Zealand Institute for 1903 (vol. xlvi.), in which Mr. H. Hill gives a full account of the history of the early discoveries of moa-remains in New Zealand, together with a discussion as to their geological age, and the probable date of extermination of these, for the most part, giant birds. Contrary to the view generally

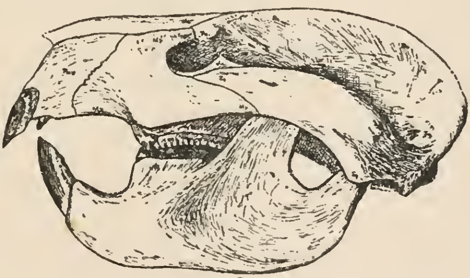


FIG. 4.—Side view of skull of *Polymastodon*.
(From Broom, *op. cit.*)

accepted, that moas were killed off by the Maoris within the last few centuries, the author asserts that these birds lived only during the late Pleistocene—the epoch of intense volcanic action in New Zealand, and that they all perished suddenly as the result of such seismic disturbances and the emission of poisonous vapours, long previous to the advent of the Maoris or any other human race in the islands. Basing his views solely on the result of observations on the east coast, Mr. Hill observes that none of the numerous moa remains found in caverns shows any evidence of having been touched by men or dogs; and he further considers that at the zenith of the great seismic cataclysm the birds rushed to the upland caves for refuge —where they were in many cases imprisoned by the fall of pumice in front of the entrance—while others perished in the open, choked by clouds of ashes or poisoned by noisome vapours; the remains of these latter being subsequently carried down to the lowlands by floods. No reference, it may be added, is made to the comparatively fresh condition in which moa-remains have been found in many districts, or to their alleged association with Maori camping-places or camp-fires.

Bird-remains from the Argentine Tertiaries have been referred to in an earlier portion of the present article.

To Prof. S. W. Williston, of Chicago University, may be accorded the credit of having given to the world by far the most readable and interesting book on recent and fossil reptiles that has ever appeared. Well illustrated, and containing 251 pages of letterpress, it bears the title of *Water-Reptiles of the Past and Present*, and is published by the Chicago University Press. In the preface the author states that he offers the work, to the best of his ability, "as an authoritative and accurate account of some of the creatures of earlier days which sought new opportunities by going down from the land into the water." No fewer than fifteen ordinal groups of reptiles are recognised, of which the great majority (excluding, of course, pterodactyles, and likewise dinosaurs) have purely aquatic representatives. Of some, indeed, like ichthyosaurs, the ancestral terrestrial forerunners are still unknown, or at all events not known with certainty; but in the case of the others the probable line of descent of the aquatic from terrestrial types is traced with a masterly hand. Like Prof. Osborn, the author is convinced that the earliest reptiles were terrestrial; and it is to be hoped that the appearance of his work will put an end to the nonsense that has been written about the aquatic ancestry of ichthyosaurs and plesiosaurs.

Perhaps the most interesting papers on fossil reptiles published during 1914 are two which appeared in the *Aëronautical Journal* for October, No. 72 (pp. 1–20), in the first of which Messrs. E. H. Hankin and D. M. S. Watson discuss the nature of the flight of pterodactyles, as deduced from their anatomical structure, while in the second Mr. G. H. Short takes into consideration their wing-adjustment. From the structure of their skeleton, it is considered that these reptiles were more specially adapted for flight than any other vertebrate animals; the largest of them, with bodies not much bigger than that of a cat, having a wing-span of about 21 feet. Moreover, they were unable to fold their wings against the sides of the body, only the outer half of each being capable of being bent backwards in the direction of the body. They could not walk like quadrupeds, and if they waddled like ducks, it must have been with extended wings: this being so, it seems doubtful if they could swim, or even touch and rise from the surface of the water, let alone dive. On land, they may perhaps have pushed themselves along on their bellies like penguins and divers, and they may have started their flights by throwing themselves from cliffs. It is further shown that the upper wing-bone, or humerus, is articulated to the shoulder-girdle by a hinge-joint, working only in an up-and-down direction, instead of, as in birds, by a ball-and-socket, permitting movement in any direction. In consequence of this, combined with the fact that the flexure of the complex elbow-joint permitted of a somewhat wider range of movement, it is inferred that the flight of pterodactyles was chiefly or solely of the soaring type of that of the albatross, this being probably connected with the weakness of the flying-muscles, as indicated by the low elevation of the crest of the breast-bone. This weakness of the flapping muscles is not, however, to be taken as an indication of poor flying power; but rather that every ounce of such power was used.

One difficulty, to which the authors make no allusion, on the theory that these reptiles could not dip into the water when in flight, arises in connection with their feeding habits. Unless they subsisted on insects, they must have obtained their food in the water. That they did not live on insect-food is rendered practically certain by the loss of teeth in the later forms, which implies a change of diet, as, for example, from hard-scaled fishes to soft belemnites or other cephalopods. It may be added that the authors are disinclined to accept the hypothesis, referred to in a previous article of this series, of increased atmospheric pressure in earlier epochs as an aid to the flight of pterodactyles.

Among other publications relating to fossil reptiles which have appeared during the year, a foremost place may be assigned to a series of three articles by Dr. H. von Huene, published as pt. 1 of ser. 2, vol. xiii. of the *Geol. und Pal. Abhandlungen.* They respectively relate to (1) the history of the Archosauria, (2) certain pterodactyle skulls, and (3) the structure and affinities of the Saurischia. In the first it is held that the so-called pseudosuchians, or proterosuchians, as represented by *Aëtosaurus* and its relatives, form the clue to the genealogy of the Archosauria (dinosaurs, crocodiles, and pterodactyles). In the opinion of the author, the saurischian, or sauropod, dinosaurs are the direct descendants of the Pseudosuchia, while one lateral branch from the latter has given rise to the bird-like dinosaurs (Ornithopoda or Ornithischia) and birds, and a second to pterodactyles (Ornithosauria). A shorter paper on this subject, by the same author, is published in the *Geological Magazine*, decade 6, vol. i. pp. 444–5.

As the article on pterodactyle skulls is of a very technical nature, it may be passed over without further mention, and the same must be the case with the one on the saurischian, or, as they are more commonly termed, sauropod dinosaurs. Two other papers on the origin and morphology of dinosaurs, by the same author, have appeared respectively in the *Neues Jahrbuch für Mineralogie* and the *Zentralblatt für Mineralogie* for 1914.

In an article in *Naturwissenschaftliche Wochenschrift* for July 5, Dr. E. Hennig directs attention to the extraordinary number of dinosaurian remains which have been collected in Germany and her East African colonies during the last five years or so. In Germany the most important of these discoveries have been made in the Keuper of Halberstadt and the corresponding formation of Trossingen and Pfaffenhofen, Württemberg, while those from German East Africa occur in the Jurassic and Cretaceous strata of Tendaguru and other districts. The dinosaurian finds from the Swabian Trias formed the subject of a communication made by Dr. E. Fraas at the eighty-fifth *Versammlung deutscher Naturforscher und Arzte* in September 1913; and Dr. O. Jaekel has described the discoveries at Halberstadt in vol. i. of the *Paläontologische Zeitschrift*. According to the first part of the last-named communication, which was published in 1913, the removal of some 100,000 cubic metres of rock brought to light no fewer than one hundred dinosaurian skeletons. In the second part a new species of turtle of the genus *Stegochelys* is described as *S. dux*, and a figure given of the restored skeleton of *Plateosaurus*.

In connection with the Tendaguru dinosaurs it may be mentioned that Mr. C. Schuchert, in the *Amer. Journ. Sci.* for 1913 (vol. xxxv. pp. 35–8), points out that the largest member of the genus originally described as *Gigantosaurus*, but now known, on account of the preoccupation of the former name, as *Tornieria*, is believed to have been fully twice the length of *Diplodocus*, or at least 150 feet; the neck apparently exceeding that of the American species by a length of about 15 feet. Before the outbreak of the war hopes were entertained that at least one skeleton of these "super-dreadnought" dinosaurs would be set up in the Berlin Museum.

During the year under review and at the close of 1913 much information has been published with regard to the dinosaurs of the Cretaceous formation, Alberta, Canada. Among these, the skull of a new generic type of the horned group *Styracosurus albertensis*, from the Red Deer River, is described and figured by Mr. L. M. Lambe in the *Ottawa Naturalist* for December 1913 (vol. xxvii. pp. 109–16, plates x.–xii.). It was found by the well-known collector Mr. C. H. Sternberg in the summer of the same year. The skull is long, depressed, and wedge-shaped, with a single nasal horn of somewhat unusual shape; but its chief peculiarities are the large size of the supratemporal fossæ, and the production of the hind border of the great occipital flange into four pairs of spines, of which the three innermost on each side are greatly elongated. Although this dinosaur may be generically identical with an imperfectly known species from the Cretaceous of Montana, referred by Cope to the genus *Monoclonius*, under the name of *M. sphenocerus*, it is considered that the two are certainly specifically distinct.

In a second article (*op. cit.* pp. 129–35, January 1914), after describing the fore-limb of an unnamed carnivorous species from the Belly River, the same writer proposes the name *Protorosaurus* for a horned (ceratopsian) dinosaur from the aforesaid valley first described as *Monoclonius belli.* The name *Protorosaurus* is, however, the original form of the well-known Permian genus nowadays frequently referred to as *Proterosaurus*, and the name *Chasmosaurus* was accordingly suggested by Mr. Lambe, in the paper next cited, to replace the preoccupied term. It is distinguished from its larger ally *Torosaurus* of the Montana Laramie by certain details in the structure of the skull, into the consideration of which it will be unnecessary to enter on this occasion. The type specimen of *Chasmosaurus* has, however, a special interest of its own on account of being associated with impressions of the skin. The sculpture takes the form of large irregular plate-like scutes, quite unlike the smaller non-imbricating scales in the impression of part of the skin of a trachodont dinosaur from the same formation, of which illustrations are given in Mr. Lambe's paper.

Further particulars of the structure of *Chasmosaurus* are given by the same writer in a third communication (*op. cit.* pp. 146–55), where a well-preserved skull of a trachodont dinosaur from the Belly River beds is described as a new genus and species, under the name of *Gryposaurus mirabilis.*

In a fourth communication (*op. cit.* vol. xxvii. pp. 13–20) Mr. Lambe describes two new generic types of Belly River dinosaurs—one, a member of the carnivorous group, under the name of *Gorgosaurus libratus*, and the other, a trachodont, as *Stephanosaurus marginatus.* The latter, which is identified by the author with a species previously described by himself as *Trachodon marginatus*, is characterised by the great elevation of the vertex of the imperfect skull, as well as by the shortness of its beak.

An apparently identical dinosaur is described by Mr. Barnum Brown in the *Bull. Amer. Mus. Nat. Hist.* vol. xxxiii., under the name of *Corythosaurus casuarinus.* The skull shows the enormously elevated vertex, formed by the nasals, frontals, and prefrontals. As minor features of the skull (the figure of which is herewith reproduced) may be mentioned its relative shortness, the narrow beak, and the small size of the narial aperture. As Mr. Brown doubts the identification of Mr. Lambe's specimen with *M. marginatus*, he may be justified in giving a new name to his own specimen ; but as to the generic identity of the two, there can be no reasonable doubt.

At the close of this paper Mr. Brown proposes a revised classification of the *Trachodontidæ*, which he divides into the sub-families *Trachodontinæ* and *Saurolophinæ*, the latter characterised by the presence of a cranial crest which is lacking in the former. The first group is represented by the genera *Trachodon*, *Kritosaurus*, *Hadrosaurus*, and *Claosaurus*, and the second by *Saurolophus*, *Hypacrosaurus*, and *Stephanosaurus.*

In another paper (*op. cit.* pp. 539–48) Mr. Brown discusses *Anchiceratops*, a member of the horned group from the Edmonton beds characterised by the great size of the knobs bordering the nuchal flange, and the pair of large oval vacuities by which the latter is pierced. Special interest attaches to this type from the fact that it serves to explain the mode of origin

FIG. 5.—Skull of *Stephanosaurus casuarinus.* About one-tenth natural size.

(From Barnum Brown, *Bull. Amer. Mus. Nat. Hist.*)

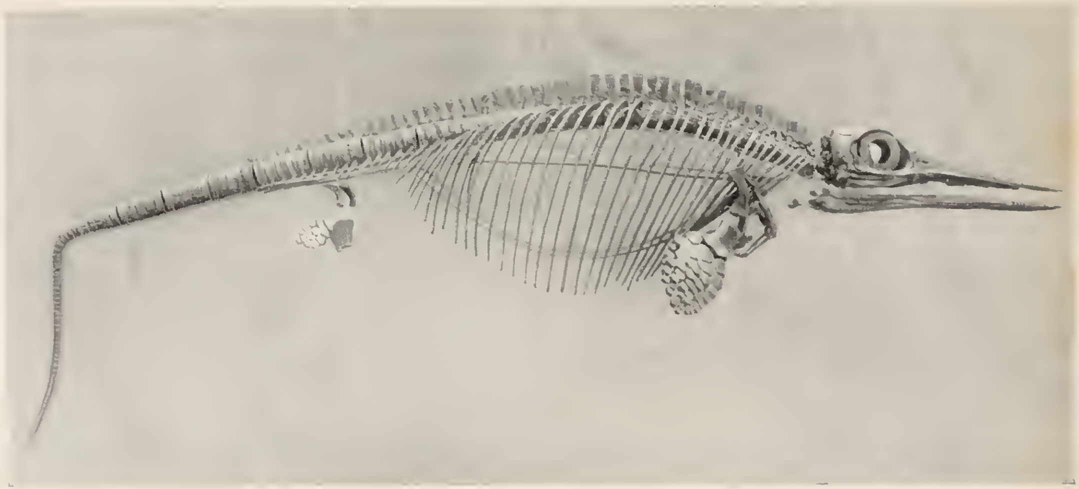


FIG. 6.—Skeleton of *Ophthalmosaurus icenicus*, as mounted in the British Museum (Nat. Hist.).

(Modified from a figure in *The Museums Journal.*)

of the ceratopsian flange, which, in its fullest development, recalls that of a fireman's helmet. In the smaller and less specialised type represented by *Monoclonius* the supra-occipitals form a pair of hook-like opposing processes on the hind border of the upper surface of the skull, leaving a mushroom-shaped interval between them, and a pair of very large vacuities in the skull-roof. In *Anchiceratops* the supra-occipital processes have united in the middle line, where only a remnant of a central fontanelle is left, while the vacuities in the lateral portion of the cranial roof are much smaller. Finally, in *Triceratops*, the largest and latest member of the group, all vacuities have disappeared from the cranial roof and the nuchal flange attains its maximum development.

In a third paper (*op. cit.* pp. 549–58) Mr. Brown describes a nearly complete skull of *Monoclonius* from the Belly River beds, which exhibits very clearly the features just referred to; while in a fourth (*op. cit.* pp. 567–80) he proposes the name *Leptoceratops* for a small member of the horned group, in which the nasal horn has disappeared.

According to a statement issued by the Smithsonian Institution, the nearly complete skeleton of a dwarf horned dinosaur (Ceratopsia) has been discovered recently in the Montana Cretaceous. The skull measures only 22 in. in length, against from 6 to 8, or even 9 ft., in the larger members of the group, the whole size of the new form being only about one-fourth that of the latter.

In this place may be mentioned *Youngina capensis*, a new type of thecodont reptile from the Permian of the Cape, described by Dr. R. Broom in the *Proc. Zool. Soc.* (pp. 1072–7) for the year under review. Although its affinities are very doubtful, some of its characteristics are such as might be looked for in the ancestors of the sauropod, and possibly certain other dinosaurs.

Brief reference will suffice for an illustrated notice in the December number of the *Museums Journal* of the skeleton of an ichthyosaur (*Ophthalmosaurus icenicus*) from the Oxford Clay of Peterborough, recently placed on exhibition in the geological department of the Natural History Branch of the British Museum. As mounted, this specimen (fig. 6), the first entire articulated skeleton of an ichthyosaur ever placed on exhibition, measures just over 13 feet in length. The number of pairs of ribs is about

50, of which the first half-dozen or so are crowded together in order to lie beneath the scapulæ. Another feature is the sudden downward bending of about fifty of the terminal vertebræ of the tail to form the lower support of a vertical caudal fin, quite different in structure from that of a fish.

These Oxford Clay ichthyosaurs were practically toothless, and thus different from their Liassic predecessors, which carried a powerful dentition. This implies a difference in food, and as the Oxfordian species were probably more pelagic in habit than those of the Lias, they may have preyed on the contemporary giant belemnites, whereas the earlier forms were largely fish-eaters. As already noted, a similar difference distinguishes the giant Cretaceous pterodactyles from their smaller Liassic forerunners, and an analogous change of diet probably also occurred in their case.

The Lower Liassic plesiosaurs of Halverstadt form the subject of an illustrated article by Dr. T. Brander in vol. lxi. of the *Palæontographica*, but as the species belong to well-known types, detailed notice is unnecessary. The name *Leurospondylus ultimus* is proposed by Mr. Barnum Brown (*Bull. Amer. Mus. Nat. Hist.* vol. xxxii. pp. 605–15) for a new generic type of plesiosaurian from the Upper Cretaceous Edmonton beds of Alberta, which is of special interest on account of being the latest member of its order at present known. It was a relatively small species—the vertebral column measuring about 7 feet—and related to *Elasmosaurus*, among its distinctive features being the medium length of the neck, the shortness and width of the centra of the vertebræ, and the single-headed ribs.

In addition to the gift of the type skull of the perissodactyle mammal *Pliolophus vulpiceps*, to which allusion has been made above, Mrs. Bull, widow of a late vicar of Harwich, has presented to the British Museum (Natural History) a skull and three shells of one or more of the three large species of marine turtles of the genus *Lytoloma* which occur in the so-called septaria of the London Clay of the Essex coast. In his above-mentioned report on the *Terra Nova* fishes, Mr. Regan expresses the opinion that the giant Tertiary horned tortoises of Queensland and Patagonia represent, respectively, distinct generic types, and therefore lend no support to the theory of a former land-bridge between those areas.

Under the name of *Testudo gymnesicus*, Miss D. M. A. Bate has described (*Geol. Mag.* decade 6, vol. i. pp. 100–6) certain remains of a presumed new species of giant land-tortoise from the Pleistocene formation of Menorca. The nature of the remains is, unfortunately, insufficient to afford a clue as to the affinities of the species. The description of a new species of the chelonian genus *Stegochelys* has been already noted. Of far greater interest is a paper by Mr. Watson (*Proc. Zool. Soc.* 1914, pp. 1011–20) on *Eunotosaurus africanus*, of the South African Permian. This remarkable reptile presents many features suggestive of an ancestral type of Chelonian—among them the abnormally expanded and flattened ribs. As the temporal region of the skull is roofed over in the cotylosaurian fashion, it is manifest that if *Eunotosaurus* really be ancestral to the Chelonia, the roofed skull of the marine turtles will be a primitive and not (as generally supposed) a specialised feature, and consequently that the open skulls of ordinary tortoises have been carved out of a roofed type. The jaws of the extinct genus are toothed.

Much work has been published during the year on the generalised reptiles of the Permian and Trias of North America and South Africa. Among articles on this subject, reference may be made in the first place to one in the April number of the *American Museum Journal* for 1914, in which Prof. H. F. Osborn records the acquisition by the American Museum of the fine collection of Permian South African reptiles made by Dr. R. Broom. The author remarks that these reptiles represent the climax of development of the amphibian stock, and the first attempts of vertebrates to progress on land. Reptiles of this early type are common to South Africa, Texas and New Mexico, and part of Russia; those from the first and third localities being much more nearly related to one another than are those from the second to either. The Texan reptiles, observes the author, never advanced beyond the old style of crawling, with their bodies close to the ground, but in South Africa many of the groups, through a powerful development of the limbs, succeeded in elevating their bodies well above the ground—a distinct advantage which gave the start that culminated in the development of mammals.

In connection with the stratigraphical relationship of the Permian reptiles of South Africa to those of Russia, published in the *Journal of Geology* for November and December 1913 (vol. xxi. pp. 728–30), Dr. Broom expresses the opinion that the dicynodonts of the Durna valley, Russia, represent the Cistecephalus zone in South Africa, which likewise contains dicynodont remains of very similar type. If this be so, the Cistecephalus zone will represent the topmost Permian, and the underlying Pariasaurus zone the middle Permian.

Interesting suggestions with regard to the early evolution of the reptilian skull appear in an article by Prof. Williston in vol. i. No. 8 of *Contributions from the Walker Museum.* As the result of a study of the skeleton of the lizard-like genus *Aræoscelis*, from the Permian of Texas, the author comes to the conclusion that in the earliest reptiles it is much more probable that the bony skull-roof inherited from stegocephalian amphibian ancestors should have been perforated only once, rather than twice, on each side, and consequently that the two bony temporal arcades of the modern New Zealand tuatera (*Sphenodon punctatus*) really represent a more specialised type than does the single arch of lizards. When the matter is put before us in this manner, it is difficult to refrain from wondering why it was never thought of before. *Aræoscelis*, in which there is certainly but a single arcade, is regarded as the typical representative of a group—Aræoscelidia—which should include the European Permian genera *Protorosaurus* and *Kadaliosaurus*, and the position of which should be next the Squamata (lizards and snakes). Ichthyosaurs, which never possessed a lower temporal vacuity, and are a primitive group, are not improbably related to the Aræoscelidia. The Permian genus *Palæohatteria*, on the other hand, which has been associated with the European representatives of the last-named group, is in essential characters akin to the Pelycosauria, in which it should typify a special family. If the foregoing view with regard to the conformation of the primitive reptilian skull be well founded, it follows that the tuatera must surrender its hitherto unchallenged position as one of the most primitive reptiles with which we are acquainted.

In this connection may be noticed a paper by Mr. Watson in the *Ann. Mag. Nat. Hist.* ser. 8, vol. xiv. pp. 84–95, on the bearing of the skull of the extinct *Pleurosaurus* on the question of the homologies of two of the bones in the temporal region of the skull of lizards. In the tuatera there is but a single element in this position, which manifestly represents the squamosal, and the author is led to conclude that in true lizards the anterior element corresponds with this bone and the posterior one with the quadrato-jugal. It is added that *Pleurosaurus* may be regarded as a member of an ancestral suborder of lizards, for which the name Acrosauria may perhaps be used.

In vol. iv. pl. 4 of the *Annals* of the Transvaal Museum, Dr. E. C. N. van Hoepen has furnished a further contribution on the fossil reptiles of the Karroo beds, dealing in this instance with the lower jaw of the dicynodont *Lystrosaurus* (*Ptychognathus*). After describing the various elements which go to form this compound jaw-bone, the author refers to a paper by Mr. Watson published in the *Ann. Mag. Nat. Hist.* for December, 1912, in which he found that some of the features described as distinctive of the lower jaw of *Dicynodon* do not accord with his own interpretation of the structure of that of *Lystrosaurus*. A re-examination of the lower jaws of both genera served, however, to confirm the author's original diagnosis.

Bare mention will suffice for the description by Mr. Watson in vol. xiv. of the *Ann. Mag. Nat. Hist.* (pp. 95–7) of a new South African species of *Dicynodon*; and, likewise, in the *Proc. Zool. Soc.* 1914 (pp. 1021–38), of various South African carnivorous anomodonts, or theraspids. Among these, special interest attaches to a skull of *Lycosuchus* on account of its exhibiting both paired prevomers and an azygous vomer, thereby serving to confirm the opinion that the lacertian vomers do not represent the mammalian vomer.

In the pelycosaurian group Prof. E. C. Case has shown in the February number of the *American Naturalist* for 1914 that the curious "sail-backed" reptile, *Edaphosaurus crucifer*, of which a restoration is given, is perfectly distinct from the genus *Dimetrodon*, with which it has been incorrectly identified. So far, indeed, from the two being identical, *Dimetrodon* appears to have been carnivorous, whereas *Edaphosaurus* probably subsisted on molluscs or insects, with perhaps an occasional vegetable meal. Unlike most of its contemporaries, *Edaphosaurus* had a head small in proportion to the body; while the dentition consisted of a marginal series of sharp conical teeth, and of crushing teeth on the palate, the latter opposed by a corresponding series on the inner side of the lower jaw.

In this place may be noticed Mr. Watson's description (*Proc.

Zool. Soc. 1914, pp. 995–1010) of *Broomia perplexa*, a small primitive reptile from the South African Mesozoics which may be related to the lizard-like *Aræoscelis*. In the same communication a new genus is proposed for the so-called *Protorosaurus huxleyi*.

The discovery in the Permian of Beaufort West of a well-preserved skull of the gigantic amphibian-like reptiles of the group typified by *Pariasaurus* has enabled Mr. Watson (*Proc. Zool. Soc.* 1914, pp. 155–80) to amplify and correct our knowledge of the structure of the palate and cranial roof. The palate, which is covered with an armature of small, sharp teeth, differs from the primitive reptilian type merely by the foreward extension of the pterygoids over the prevomers to articulate with the premaxillæ—a peculiarity which may be purely adaptive. In the skull-roof the main peculiarity is the presence of only a single element between the quadrato-jugal and tabulare; this element probably representing the mammalian squamosal.

As regards the systematic position and affinities of the genus, the author confirms the opinion that it has much in common with amphibians, reptiles, and mammals. It is, however, a true reptile, referable to the group Cotylosauria, in which are also included *Seymouria*, *Diadectes*, *Procolophon*, and *Pariotichus*. But there is only a step from such a generalised reptilian type to stegocephalian amphibians (and so to crossopterygian fishes), the sole points of difference between cotylosaurians and temnospondylous amphibians being the reduction of the vertebral intracentra, the expanded and thickened neural arches, the horizontal plane of the articulating facets of the vertebral zygapophyses, and the reduction of the bones in the upper row of the tarsus to two.

In a rather later communication (*Ann. Mag. Nat. Hist. op. cit.* pp. 98–102) the same author proposes a reclassification of the pariasaurian group: *Pariasaurus* itself being restricted to the relatively small *P. serridens*, to which *Propappus* is nearly related. *Anthodon* and the new genera *Bradysaurus* (typified by the skeleton in the British Museum originally described as *Pariasaurus baini*) and *Embrithosaurus* are larger forms.

In yet another paper (*Proc. Zool. Soc.* 1914, pp. 749–86) Mr. Watson reviews the entire group of mammal-like reptiles, for which he suggests the name Therapsida, although acknowledging that the earlier terms Anomodontia and Theromora have claims to recognition. In this great group or stem he includes at least some of the American pelycosaurians. The sub-group Dinocephalia, as typified by the African *Tapinocephalus*, figures largely in this paper, which also contains descriptions of three new genera and species.

Very interesting is the suggestion that the stapes of the mammalian ear represents the hyomandibular of fishes, which articulates with the quadrate; and it is probable that in the embolomerous Stegocephalia, which have just arisen from [crossopterygian] fishes, the primitive connection between the distal end of the hyomandibular, or stapes, and the quadrate still persists.

Procolophon trigoniceps, a cotylosaurian reptile from South Africa, is the title of another paper by Mr. Watson in the journal last quoted (pp. 735–47), in which it is shown that the genus represents a group of the cotylosaurian section of the mammal-like reptiles very different from the one including *Pariasaurus* and its allies. *Procolophon* is, in fact, the latest of the Cotylosauria, and therefore exhibits several specialised features unknown in the earlier forms, such as the large size of the quadrato-jugal and orbits, the peculiar type of humerus and dentition, and the inclusion of three vertebræ in the sacrum.

In some respects the most interesting of all Mr. Watson's contributions during the year to our knowledge of primitive tetrapods is one on a limb-bone from the Lower Carboniferous of Scotland, which apparently represents a true reptile. This article—illustrated with a plate—is published in the *Geological Magazine* (decade 6, vol. i. pp. 347–8), where the new generic and specific designation *Papposaurus traquairi* is proposed for this "grandfather of all the reptiles." The result of recent investigation has been to show that the embolomerous stegocephalian amphibians of the Carboniferous Burdiehouse Limestone of Scotland are more or less intimately related to the cotylosaurian reptiles; and when more is known of *Papposaurus*, it may prove to be the missing link between amphibians and reptiles.

In yet another communication (*Geol. Mag.* decade 6, vol. i. pp. 395–8) Mr. Watson reopens the question as to the nature of the animal which made the well-known footprints in the Trias of Cheshire and elsewhere, described as *Chirotherium*. The apparent fact that these tracks were made by a digitigrade animal, coupled with other circumstances, leads the author to reject the idea that they were made by stegocephalian (labyrinthodont) amphibians in favour of the view that they are probably the footprints of a primitive reptile more or less nearly akin to the dinosaurian genus *Plateosaurus*.

From the Upper Carboniferous Gaskohle of Bohemia Mr. K. Hummel has described in the *Zeits. deutsch. Geol. Ges.* for the latter part of 1913 (vol. lxv. pp. 591–5, pl. xviii.) one of the small newt-like stegocephalians of the group Microsauria. It is identified with the genus *Ricnodon* of Fritsch, and is nearly allied to, if not identical with, the species described as *R. dispersus*. Both the known species are very rare in the Gaskohle.

In this place mention may be made of a paper by Dr. J. E. V. Boas, published in the *Morphol. Jahrbuch* (vol. xlix. pp. 229–301), as being one which deals in part with reptiles and in part with fishes, and also relates as much to existing as to extinct forms. Of an extremely technical nature, it deals with the hind part of the cranial roof and the palato-quadrate bar in the lung-fishes (Dipnoi), and their presumed representatives in the skulls of terrestrial vertebrates.

As regards fossil fishes, reference may be made to the republication in vol. iv. of the *Bulletins* of the Connecticut Geological and Natural History Survey of an article by Prof. Eastman (originally issued in 1911) on those of the local Trias. It is prefaced by a note on the study of fossil fishes in general, and contains descriptions, for the most part illustrated, of local species.

A new subgeneric type of Australian lung-fish (*Ceratodus*) is indicated by a tooth from the Cretaceous of New South Wales described by Mr. F. Chapman in the *Proc. R. Soc. Victoria*, vol. xxvii. pp. 25–7. The tooth, which has been converted into precious opal in the process of fossilisation, belongs to the lower jaw, and resembles the Jurassic members of the genus in carrying only four ridges, although in the nature of the grinding surface it is very similar to the teeth of the living *C.* (*Neoceratodus*) *forsteri*. The name of *C.* (*Mesoceratodus*) *wollastoni* is proposed for the Cretaceous species. It may be added that in the *Revue scientifique* for December 5–12, 1914, a writer suggests that a tooth of *Ceratodus* in the museum at Cairo indicates the occurrence of that genus in Egyptian strata.

THE PREVISION OF EARTHQUAKES

By CHARLES DAVISON, Sc.D., F.G.S.

BETWEEN foreseeing and foretelling an unexpected event, there would seem to be little if any difference, beyond the fact that the one may be conducted in private while the other implies publication of some kind. But, to the corresponding words "prevision" and "prediction," somewhat different meanings seem to be attributed, prevision being apparently considered as an approximate, and prediction as an accurate, form of forecast. This, at any rate, is a distinction that will be assumed in the present paper, for the prediction of an earthquake, the accurate forecast of its occurrence at a particular time and in a particular place, is a problem far beyond our powers, and likely to remain so for many a year to come.

It must be admitted, moreover, that this is the only kind of forecast that is of any practical value. It is futile to urge that a great earthquake will occur somewhere upon the globe on a certain specified day, if the particular region to be affected is unknown, as such an earthquake actually does occur on an average more than once a week. And, on the other hand, it is of little advantage to proclaim that a certain area will be devastated at some unknown time within the next few years, or even within the next year, for no population will consent to spend their nights out of doors for so long and so indefinite a period. Until, therefore, we can fix the area in one case, or the time in the other, within fairly close limits, the forecasting of earthquakes can have little practical importance. But the problem is so complex, and at the same time its solution would possess such untold value for the dwellers in seismic countries, that it does seem worth while to examine the progress that has been made in the hope that further knowledge and greater experience may in time to come lead us to the desired goal.

It is clear that if, of the two elements, it be possible to determine only one with accuracy, it is more useful to know the site with precision, and the epoch with somewhat less certainty. At any rate, it is in this direction that hope lies. If we could but ascertain certain phenomena, one or more of which invariably precede a great earthquake, we should be on the high road to success. The phenomena may belong to different categories. They need not be the same in every case. Nor need they be manifest without instrumental or other aid. In all probability, the phenomena that herald an earthquake will only be revealed by careful and detailed study; otherwise they would have been discovered long ages ago. Thus, we may at once rule out the favourite portents of past times, such as the appearance of a comet or of strange lights in the sky, the arrival of unusual birds, a dull, heavy condition of the atmosphere formerly known as "earthquake weather," or the depressed feelings of neurotic persons.

If we reflect on the mechanism of a great earthquake, on the enormous masses that are displaced in some cases, on the stupendous forces that are involved, and on their gradual increase until they are sufficient, and more than sufficient, to sweep away the resistance opposed to them, it would seem that there must be some indication of their growth, some sign of incipient motion, that might be revealed by painstaking investigation.

Most earthquakes probably originate at a depth of several, though not many, miles below the surface. The sliding movement along a fault or fracture, to which they are due, usually dies out before the surface is reached, so that no visible effect of the motion is manifest, and it is only by the study of the evidence available that we can determine the position of the fault responsible for the earthquake. But, in a few great earthquakes, the displacement underground is so considerable that it is continued right up to the surface, and there it remains and displays to us something of the magnitude of the region in which the earthquake originates, something also of the nature and extent of the initial movement, until, by the gentle but continual action of the weather, the fault-scarps are worn down and all traces of the disturbance are obliterated.

In the earthquake which visited San Francisco and many another coast-town of California in 1906, the surface displacements exceeded in one respect those of every other earthquake with which we are acquainted. The destruction of San Francisco was due, not so much to the strength of the shock, as to the ravages of the fires which followed it. And the fires spread almost unchecked owing to the dislocation of the water-mains by an extraordinary movement along a fault which has been traced in a roughly north-west and south-east line for a total distance of about six hundred miles from near Cape Mendocino on the north to the Colorado Desert on the south. It was only along the northern half of the fault that movement occurred in 1906. But, for a total length of 290 miles (including some submarine portions), the surface-crust on both sides of the fissure slipped in opposite directions, that on the south-west side to the north-west, and that on the north-east side to the south-east, tearing apart with resistless force every work of human hands that crossed its line. Water-mains were cut through, and the severed ends separated. Roads, fences, bridges and piers were split across and their ends shifted by amounts which at the surface ranged from eight to more than twenty feet.

These displacements of course represent the sum of those on both sides of the fault. And this is all that could be gathered from the evidence visible to the unaided senses. The precise nature and amount of the several movements could be determined only by a comparison of the trigonometrical surveys carried out in the district some years before and shortly after the earthquake. By this it was found that both sides had moved in the directions mentioned above, that the maximum movement occurred in the immediate neighbourhood of the fault, and that it diminished rapidly with increasing distance from the fault, so that, at a few miles from it on either side of the displacement, if it did not actually die out, it was less than could be detected by the accurate instruments at the disposal of the surveyors. Thus, if a straight line twenty miles long had been traced before the earthquake in a direction at right angles to the fault, the line after the earthquake would have been severed at the fault, the ends separated by about twenty feet, and the portions near the fault curved, so that on the south-west side the concavity would face to the north-west and on the north-east side to the south-east.

Taking into account the magnitude of this extraordinary movement and the concentration of the greatest damage wrought by the shock along the line of the fault, there can be no doubt that the earthquake was due partly to the sudden shift at the last moment, partly to the intense friction that must have arisen with the scraping of the rocks on the two sides of the fault.

Earthquakes are by no means rare in California, but many years have elapsed since there was any considerable movement along the fault in action in 1906. During this long interval we may imagine the forces along the fault as gradually increasing until in that year they were strong enough to overcome the resistances opposed to them. Then with great rapidity, but certainly not instantaneously, the sliding movement of each side took place. Months passed before equilibrium was once more approximately attained. Small shocks, each the result of a minor slip, were at first comparatively frequent. A few still occur from time to time, but we seem almost to have reached another period of quiescence, during which the forces are slowly gathering which in years to come will terminate in yet another violent shock.

So far as the actual earthquakes are concerned, these periods of quiescence and gradually increasing forces are apt to terminate somewhat suddenly. But, before the critical moment arrived when the crust gave way, its deformation must have already begun. The imaginary line, referred to above as drawn during the early days of quiescence at right angles to the fault, must have shown signs of curvature before its severance along the fault took place. It is not, of course, necessary to draw the whole of this line. A few points upon it at definite intervals apart would be ample. Even the two groups of four stone pillars, erected two on each side of the fault after the earthquake of 1906, would suffice. These pillars were actually placed so as to afford a simple means of measuring fresh displacements along the fault. But they may also be found to furnish evidence of a coming earthquake by a slight and continual increase in the distance between those on opposite sides of the fault.

It will be obvious that this method of foreseeing earthquakes, for which we are indebted to Prof. H. Fielding Reid,[1] is at present in an early stage of development. Until another earthquake occurs along the same fault we have no conception of the time occupied by the process of preliminary curvature, or whether the displacement occurs as a climax to a rapid increase of curvature. The time involved may be too short to be of practical service. But the method of forecast is well worthy of examination and development. It is quite possible that it may, in course of time, lead to valuable results.

[1] The California Earthquake of April 18, 1906, *Report of the State Earthquake Investigation Commission*, vol. ii. 1910, pp. 31–2.

The second method, now to be described, rests on a more definite foundation. It depends on observations actually made on the slight shocks which preceded the great earthquake that devastated the provinces of Mino and Owari in Japan on October 28, 1891. As in the Californian earthquake of fifteen years later, this earthquake was accompanied, or rather caused, by unusual fault-displacements which to a great extent left visible traces on the surface of the ground. The actual length of the displacement was less than in the Californian earthquake. The part of the surface-fault affected was traced for forty miles, though it is probable that its total length was not less than seventy miles; but in this case the vertical displacement was considerable. In one place it attained a height of about twenty feet. The horizontal movement was less notable, and was variable in amount from one up to about thirteen feet. In addition to the displacement which resulted in this fault-scarp, there must have been other movements along a more deeply seated fault, which is roughly parallel to, though possibly branching from, the more conspicuous fault. Both faults, as well as other minor fissures which may have been in action, will be referred to here as the fault-system.

A very marked feature of this earthquake was the great number of shocks that followed it. All of them were much slighter than the original earthquake, but many, if they had occurred alone, would have attracted attention as strong or violent earthquakes. At first they occurred with great frequency, more than a thousand being recorded at Gifu during the first week. They were felt in all parts of the fault-system, though more frequently in some than in others. But, after the lapse of a few months, they occurred more rarely and became almost limited to definite portions of the fault-system, such as the central and terminal regions, and finally to the central region alone. It is chiefly in these two respects—great frequency and concentration of activity—that the after-shocks of this earthquake were distinguished from those that preceded it.

We are indebted mainly to the labours of the late Prof. Milne for our knowledge of the earthquakes of this district, his great catalogue of 8,331 Japanese earthquakes during the years 1885–92 providing all the materials necessary for our present purpose. The area mainly affected by the earthquake of 1891 occupies about 20,700 square miles, but the great majority of the shocks originated within a more limited region of about 1,345 square miles, or 13 per cent. of the above. This may, for convenience, be termed the "earthquake zone."

During the whole of the eight years the earthquakes of the zone were, area for area, more frequent than in the region outside. But the relative frequency was far from constant. In 1885 earthquakes were $5\frac{1}{2}$ times as frequent in the zone as in equal areas outside; in 1886, 4 times; in 1887, $2\frac{1}{4}$ times. Possibly this decline in relative frequency during these three years represents merely the fading activity of the after-shocks of the last great earthquake in the same district, which occurred in 1859. At any rate, in 1887, it reached its lowest figure. In the following year the relative frequency rose to $5\frac{1}{2}$, in 1889 to 7, and in 1890 and in 1891 up to October 27 to $10\frac{1}{2}$. On the next day the great earthquake occurred, after which, during the remainder of the year, the relative frequency was 139, and during 1892, 156, the latter higher figure being probably due to the suppression of sympathetic earthquakes in the surrounding district. Thus, the first and most obvious symptom of the coming earthquake was a rapid increase in the frequency of shocks in the earthquake zone with respect to that of the shocks in the area immediately outside it.

Another significant feature of the fore-shocks of this earthquake is their distribution along the fault-system. During the five years 1885–89 they shunned as far as possible those areas which, towards the end of 1891, became most prolific in after-shocks; their distribution in space was apparently without law. But with the beginning of 1890 a remarkable change took place. There were still one or two districts in which they were more numerous than elsewhere; but, on the whole, the centres of the fore-shocks cling to and mark out the fault-system that came into action in 1891. Except for one portion of the whole area, and that is occupied by mountains, the distribution of the centres follows with remarkable uniformity the outline of the fault-system—not only the actual course of the fault-scarp, but its continuation to the south-east as well as the course of the deep-seated fault of which there appeared no actual trace at the surface. Then came the great earthquake, and immediately the whole aspect of the distribution was changed. Thus the second and no less significant feature of the fore-shocks is that, within two years before the earthquake, they not only became relatively more frequent, but were distributed with some approach to uniformity over the entire fault-system.

These two properties of the fore-shocks of the Mino-Owari earthquake seem to be prophetic of the coming earthquake. True it is that, so far, they are only known to foreshadow the occurrence of one great earthquake. But there is reason to suppose that they are not so confined. Think, for a moment, of the probable cause of the fore-shocks. For at least four or five years the forces which at last culminated in the great crust-movement of 1891 had been gradually increasing. The contest became one between these growing forces and the resistance to motion along the fault-system. It is improbable that the resistance to motion would be uniform throughout. At different points there would be regions all along the faults within which the resistance to motion was greater than elsewhere. Until these areas of local resistance were cleared away, there would be no displacement on a great scale. Here and there, then, small slips along the fault would cause a fore-shock, and the effect of the slips in all parts of the fault would be to equalise over the entire fault-system the effective resistance to motion. The further growth of the forces would then precipitate the great displacement all over the fault-system and give rise to the great earthquake to which the fore-shocks pointed.

Thus we have reason to believe that the increase in seismic activity along a known fault, and the tendency to uniformity in the distribution of that activity along the fault, may be heralds of the great crust-movements which cause disastrous earthquakes. The method, of course, can only be of service in countries in which earthquakes are fairly numerous and occasionally violent, and in those alone in which there exists an efficient system for the observation of earthquakes. Such conditions are satisfied at present in but one country. In the empire of Japan about a thousand earthquakes occur every year. They are so carefully studied that few, if any, escape investigation. Every few years one of considerable violence takes place. There can be no country, therefore, in which the practical prevision of earthquakes can be more readily effected.[1]

[1] *Quart. Journ. Geol. Soc.* vol. 53, 1897, pp. 1–15; Gerland's *Beiträge zur Geophysik*, vol. 12, 1912, pp. 9–15.

IS THE ORGANISM A THERMODYNAMIC MECHANISM?

By JAMES JOHNSTONE, D.Sc., *University, Liverpool*

Let us consider the chemical reactions represented by the following equations:

(1) $\underset{\text{Methane}}{CH_4} + 2O_2 = CO_2 + 2OH_2 + 213{,}800$ gram-calories;

(2) HCl, aq. + $NaOH$, aq. = $NaCl$, aq. + OH_2 + 13,700 grm.-cal.;

(3) $\underset{\text{Acetylene}}{2C_2H_2} + 5O_2 = 4CO_2 + 2OH_2 + 310{,}000$ grm.-cal.;

(4) $\underset{\text{Carbon disulphide}}{CS_2} = C + S_2 + 28{,}700$ grm.-cal.

The first two reactions are typical examples of processes which occur commonly in organic and inorganic systems. The first represents an oxidation, and the second a neutralisation. When methane burns completely in oxygen carbon dioxide and water are formed, and a certain quantity of heat is evolved. When very dilute hydrochloric acid is added to very dilute caustic soda a neutral salt, also in dilute solution, is formed. If quantities of each of the reacting substances equal to the molecular weights, in grams, represented by the formulæ take part in the chemical changes, quantities of heat represented by the numbers given on the right-hand sides of the equations are evolved. The reactions are *exothermic*. In most chemical changes the "intrinsic energy" contained in the substances before they react is greater than is the intrinsic energy contained in the products formed by the reaction, and the balance of energy appears as evolved heat. The 213,800 gram-calories of equation (1) are the heat of combustion of a gram-molecular weight of methane; and the 13,700 grm.-cals. of equation (2) are the heat of neutralisation of gram-molecular weights of hydrochloric acid and sodium hydrate.

Equations (3) and (4) represent also exothermic reactions. In the first of these acetylene burns completely in oxygen; and in the second one carbon disulphide is decomposed into its elements. When acetylene burns in this way 310,000 gram-calories of heat are evolved; and when carbon disulphide is decomposed completely 28,700 gram-calories are evolved. Now, if we calculate, from known data, the heats of combustion of the carbon and hydrogen contained in a gram-molecular weight of acetylene we shall find that it is only 256,900 gram-calories; therefore the balance of 53,100 gram-calories must be absorbed when carbon and hydrogen are synthesised as acetylene. Also when carbon and sulphur are synthesised to form carbon disulphide 28,700 gram-calories, the heat of dissociation of the compound must also be absorbed. These reactions, the combination of carbon and hydrogen to form acetylene, and carbon and sulphur to form carbon disulphide, are therefore *endothermic* reactions. In them the intrinsic energy of the final product is greater than the intrinsic energy of the initial substances.

The difference between the two kinds of chemical change—exothermic and endothermic changes—is fundamental. Nearly all substances which react with each other do so with the evolution of heat. A few reactions occur in which heat is neither evolved nor absorbed, but these are of an altogether special kind. A few reactions also occur in which heat is absorbed. These, also, are special chemical changes; they are not numerous; and the products resulting from them are unstable as a rule.

Considering further the above reactions two things are to be noted. First, exothermic reactions occur *of themselves.* Immediately caustic soda is added to hydrochloric acid neutralisation begins. Methane and oxygen do not react at ordinary temperatures (or they react "infinitely slowly") but an infinitesimal amount of energy starts the reaction which then proceeds until it has been completed. Endothermic reactions, on the contrary, do not occur of themselves: carbon and hydrogen will not react by themselves to form acetylene, nor will carbon and sulphur to form carbon disulphide. These reactions will not occur, as does the explosion of a methane-oxygen mixture, when an infinitesimal "stimulus" is applied. *In order that they may take place a compensatory energy-transformation must be set up*, and in this compensating reaction an amount of energy equal to that absorbed in the endothermic change is supplied. I have emphasised the above sentence in order that the reader may appreciate the importance that it has for our later discussion.

The second thing to notice is that the equations show that chemical reactions are *directed changes*. In all exothermic reactions, that is, in the vast majority, heat is evolved. If the reaction takes place of itself, that is, apart from intelligent ordering of the conditions under which it occurs, this heat is dissipated: it is conducted away, or radiated, and it raises the temperature of its physical surroundings to an infinitesimal degree, so that it can no longer be recovered or made use of to produce further transformations. In all physical or chemical changes of this category the heat so produced becomes *unavailable*; and the products of the changes, if they are chemical changes, possess less intrinsic energy than did the original substances. Thinking about chemical changes in general we see, then, that each one that takes place exothermically reduces the probability of the occurrence of further exothermic changes, since in it some part of the available energy of the universe has become unavailable. Every such chemical reaction tends towards stability, since the products are less likely to react with each other, or with other substances, than were the original substances.

The opposite tendency is exhibited by endothermic changes, for in such the products of the reaction possess a greater quantity of intrinsic energy than did the original substances. The reaction tends towards instability, for the substance which is formed endothermically is more likely to react (it is often explosive) than were the substances from which it was formed. In the preparation of an endothermic substance some heat is certainly dissipated, since we cannot avoid the loss of heat by conduction and radiation, and we cannot, as a rule, control perfectly the progress of the reaction. But the general result of the change is that energy remains in the available form, and can be made use of in the production of other energy-transformations.

We may now generalise these statements. All physico-chemical changes whatever, organic or inorganic, exothermic or endothermic, are said to conform to the two laws of thermodynamics. The first law states that the energy of an isolated system is constant, that is, by no conceivable change occurring within the system can the sum of potential and kinetic energy contained in it be diminished or augmented. If we couple the law of conservation of energy with the law of conservation of matter (in modern views the latter is, of course, contained in the former), we arrive at the conclusion that nothing in the universe can be created, nor can anything be destroyed. But the second law of thermodynamics states that something which is characteristic of an isolated physico-chemical system, its sum of entropy, tends continually to become augmented. Our only isolated system is the universe, and the most general statement of the second law is that the sum of entropy of the universe tends continually to a maximum.

We cannot discuss here the shadowy mathematical concept called entropy, but we may state the second law (partially, but correctly for our purpose) in saying that something is irretrievably destroyed in every physico-chemical reaction that occurs. This something is available energy. Energy, potential or kinetic, that is, energy of position or the energy of motion of entities possessing mass, which can be made use of in producing or setting up transformations, or natural phenomena, is available energy. The heat energy of one part of a system which is at a higher temperature than another part, for instance, the energy of steam in a boiler in relation to the energy of the condenser water is available. Energy which cannot be utilised to set up transformations—to do work—is unavailable: such is, for instance, the heat energy of the ocean in relation to the engines of a ship traversing it. In order that some of the energy of a system may be available some part of it must be at a higher potential than another part. If there are no differences of potential the energy is unavailable. Now, in all natural changes, or phenomena, or energy-transformations, there is an ever-present tendency for some fraction of the total energy manifested in the change to become converted into low-temperature heat. Such heat becomes conducted or radiated into the earth or into space, becomes uniformly diffused, and so becomes unavailable. This occurs in endothermic and exothermic processes. In the imaginary world of mathematical physics natural phenomena may occur in systems with perfectly elastic or perfectly rigid parts, where there is no friction, and where heat is either perfectly conducted or perfectly insulated. But in the real world all natural processes are such as involve friction and loss of heat. In all of them some energy becomes unavailable. All of them are *irreversible* processes, that is, processes which cannot be retraced. With every such irreversible process some part of the available energy of the universe becomes unavailable. With every natural phenomenon that occurs the number of phenomena that may occur in the future becomes appreciably less. "Every irreversible transformation leaves an indelible imprint *somewhere or other* on the progress of events in the universe considered as a whole."

Thus there is a *tendency or direction* in the progress of inorganic happening. Energy is continually being degraded, continually passes from the available into the unavailable form. With every such transformation the number of things that may happen in our universe in the future becomes less. The universe tends towards a limit which is the cessation of all phenomena—universal physical death. There is nothing fanciful or metaphysical in this conclusion. It is a sound deduction from the results of physical science. It is the plain outcome of our experience.

And yet it is perfectly clear that we cannot extend it, *a priori*, to the universe as a whole. We must extend universally the law of the conservation of energy—it is unthinkable that it cannot apply to all that exists. If we imagine, literally, that the sum of available energy in the universe was infinite, then the fall of available energy is asymptotic to time considered as the independent variable. But we must also regard time as infinite, that is, we must think of the universe as having, literally, no beginning and no end. Obviously, we are only juggling with words in these statements. Infinite time is really time that has as great a duration as we please to conceive. Then in the lapses of duration that lie in the past of our universe the limit to the fall of available energy must have been attained if the second law be universally true. But we look out upon an universe which is still the theatre of inorganic phenomena. The second law cannot, then, be universally true, like the first law is universally true. But it *is* true of all that comes within our experience.

Thus we come to an *impasse*, for two aspects of our experience seem to be flatly contradictory to each other. On the one hand the results of experimental physics show us an universe in which there is a progressive degradation of energy, that is, an energetic system which had a beginning and will come to an end. On the other hand experience also shows us an universe in which natural phenomena still occur, and for which we can postulate any past duration, however great. Now there appears to be only one way out from this deadlock; somewhere or other in the universe *there must be a restoration of available energy.* This is the explanation suggested by Boltzmann, and we may usefully consider it here, since it suggests at once a manner of regarding the activities of the organism which indicates that a theory of life may, after all, be possible.

Let us consider, then, a volume of a "perfect" gas equal to, say, one-tenth of a litre. Let this gas be contained in a vessel made of some perfectly non-conducting material, and let the vessel have a partition, also made of non-conducting material, dividing it into two chambers. Let the gas in the two chambers be at unequal temperatures, T_1^0 and T_2^0, T_1^0 being greater than T_2^0. Now let the partition be withdrawn so that the gases mix. In a few minutes thermal equilibrium will be established and the temperature of the moisture will be, everywhere, sensibly the same.

A perfect gas consists of a very large number of molecules moving in straight lines at very high velocities. These molecules incessantly collide with each other, and since they are perfectly elastic no energy is lost in the collisions. They must be moving in every conceivable direction and (within a certain range) at different velocities. But there is, at a definite temperature and pressure, a certain mean molecular velocity towards which the greater number of the molecules approximate. Some are moving at higher, and others at lower velocities than the mean one, and these velocities other than the mean deviate from the latter in such a way that they can be represented by a "frequency distribution" with the mode at the mean. For two gases differing only in their temperature the squares of the mean velocities are proportional to the absolute temperatures, that is, $V_1^2 : V_2^2 = T_1^0 : T_2^0$.

Since the molecules of the gas are moving with different velocities, and in every conceivable direction, the result of their collisions must be that molecules moving with speeds above the mean will tend, owing to collisions with molecules moving with speeds below the mean, to lose some of their velocity. When the two gases at different temperatures are allowed to mix the molecules of the hotter one will communicate to the molecules of the cooler one some of their velocity of movement. Thus the temperature of the cooler gas must increase while that of the hotter one must decrease. This is a progressive change requiring some time (a matter of minutes). At each momentary phase of the progressive change there is a new distribution of the molecules and of their directions and velocities of movement. Every such phase is, of course, a dynamical consequence of the preceding phase. When thermal equilibrium has been attained the mixture of molecules has a new mean molecular velocity with individual deviations from the mean represented by a new frequency distribution.

Now it can be shown that if, at the moment at which thermal equilibrium is attained, the direction of motion of each molecule were to be reversed, the series of changes through which the mixture had passed would also be reversed. It would gradually become separated into two masses of gas, each of them characterised by its original temperature. Instead of having a gas at uniform temperature in every region we should have a gas separated into two parts, in thermal contact with each other, but having different temperatures. The progressive change from two masses of gas at unequal temperatures to one mass of gas at uniform temperature would be reversed. The series of momentary phases leading from inequality to equality of temperature would be exactly followed, *but in inverse order.*

It is important to note here that the second law of thermodynamics would apparently be "violated." Heat would flow, of itself, from a body at low, to a body at high temperature. But if this were to happen perpetual motion would be possible, whereas we know (at least it is our experience) that it is impossible. Therefore we must conclude that heat cannot flow, of itself, from a body at lower to another body at higher temperature, and we seem to be justified in concluding that this imaginary simultaneous reversal of motion of all the molecules of a gas cannot take place.

Yet it may conceivably take place. At any instant many of the molecules in a decilitre of gas must be approaching each other in the same straight line, and with the same velocity. As the result of such collisions the direction of motion of these molecules must be reversed, the magnitude of their motions remaining unchanged. The number of such molecules as collide "end on" is continually changing, sometimes there are relatively many, sometimes few. The probability of any fraction of all the molecules so colliding can be calculated, and also the probability that *all* the molecules should collide end on, so leading to a reversal of the physical history of the gas. The calculation has been made by Boltzmann. It is possible that this simultaneous reversal of motion of all the molecules in a decilitre of perfect gas may occur, but in order to witness it we might have to observe the gas for a lapse of time represented (in centuries) by unity followed by a thousand millions of ciphers! The chance is thus very small. It is about equal to the chance that all the houses in London might catch fire, independently of each other, on the same day, or that all the men in London might commit suicide, independently of each other, on the same day. An insurance office would certainly disregard such risks. They would say that it was "practically impossible." So we must say that it is "practically impossible" that heat should flow, of itself, from a colder to a hotter body; that perpetual motion is a practical impossibility; and that it is also practically impossible that the second law of thermodynamics does not apply to all physical and chemical transformations. We say, therefore, that all natural phenomena have the same tendency—towards degradation of energy, or augmentation of entropy—and much of our power of so ordering naturally occurring events, or of forecasting future events, depends on our confident assumption that this tendency will continue to hold true. It is *almost* certain that the second law of thermodynamics will be valid in all our future experience as it has been valid in all our past.

"Almost certain," we say, but not logically capable of demonstration—not inevitable. The first law—that of conservation—is *a priori* certain, that is, it is unthinkable that it should not be universally true. But the second law is only a probability—a very great probability if we like. Now let us regard the decilitre of gas of our example as a "model," in a kind of way, of the universe. Like the universe we may describe it as a "collection of isolated mass points, devoid of rotatory inertia, moving in accordance with Newton's laws, and attracting or repelling each other with forces which are continuous or discontinuous functions of the distance between them." If, then, we extend the general conclusions deducible from the kinetic gas-theory to the universe itself, we might consider whether heat may flow, of itself, from colder to hotter regions—in general, whether there may not be, somewhere, a restoration of available energy. The possible form of such a speculative process would depend on the cosmology we adopt; obviously we cannot consider it in detail now. But clearly this restoration of available energy may occur in the universe, and the probability that it does so occur is of the same order as that in Boltzmann's estimate.

The probability is, as we have seen, unimaginably small. But the universe is as unimaginably great. What do we mean by saying that the universe has infinite extension and duration? We do not mean that it has, literally, no boundaries; nor that it had no beginning and will have no end. To say as much is to play with pseudo-ideas. When we say that the universe has infinite extension we mean that no matter how great, in finite numbers, it may be conceived to be, it can still be conceived as greater; and so also with its duration. That is to say, we can make the universe as big as we like, or as old as we like, while still regarding the dimensions we ascribe to it as such as are capable of mathematical treatment. Its extension and duration are "infinite" in the sense that we speak of infinitesimally small magnitudes in the theory of the differential calculus. The radius of the earth, the distance of the earth from the nearest fixed star, and its distance to stars of no parallax are to be regarded as infinitesimals of the $(n-1)$st, $(n-2)$nd, and $(n-3)$rd orders. The duration of a man's life, the age of the habitable earth, the age of the solar system, may also be so regarded. It does not matter now that the probability of a restorative of available energy is incredibly small. We can suppose the duration of the universe to be as much greater as we wish.

The universe that we know is the material universe *in which there are energy-transformations*. We may regard it as an infinitesimally small part of all that exists—the entire universe, let us say, in short. Its duration we may also regard as an infinitesimally small part of the duration of the entire universe. The latter is physically dead: the sum of its entropy has attained its maximum value. But here and there in it are regions of the magnitude of our known stellar universe—individual universes, Boltzmann calls them—but infinitesimal in their extension when compared with the entire universe. In these individual universes, for moments when compared with the duration of the entire universe, but for eternal eras (Aenonen) when compared with a human life-time, the second law of thermodynamics becomes reversed, just as it does in our imaginary decilitre of gas. Physical inertia is "normal," that is, it is the most probable condition of a dynamical system in the general sense. But in small regions of the entire universe, and for infinitesimal periods of duration, the improbable condition may obtain. It does occur in regions of a decilitre of gas, if we take these regions small enough. Here and there there are certainly small groups of molecules, in which, for a very small time, the mean linear velocity, and therefore the temperature, is greater than in adjacent regions.

Such an universe, one in which there was physical activity, as there is in our universe, would be passing from an improbable towards a more probable phase. The most probable phase would be that in which entropy had attained its maximum value; the least probable phase would be that of zero entropy. The phase of physical activity would be preceded by a phase of restoration of available energy, that is, a passage from the most probable to the least probable condition. Time would have a double sign. The passage from the improbable towards the probable conditions would occur in *our* time, that is from the past to the future. But in the period of restoration of available energy the scale of time would be reversed, and the passage from the most probable to the least probable phase would occur in time which moved from the future back into the past. Conscious organisms in such a phase of an individual universe would possess knowledge of the future but not of the past.

What this discussion leads up to is the consideration of the second law of thermodynamics as a probability only. The law states that inorganic phenomena tend in one way—towards increase of entropy. But the sign of the law may be changed, that is, events may also conceivably take place in such a way as to lead to diminution of entropy. It is more probable that entropy increases than that it should diminish.

Unless we postulate that the entire universe is passing from an initial determined state towards a final definitive state, we cannot regard the second law of thermodynamics, as it is usually stated, as true.

We can define an organism as an autonomous physico-chemical system; possessing specific form; maintaining its specific form in the midst of an environment which undergoes change, by active, compensatory adaptations of its functioning; capable of indefinite growth by accretion and dissociation (reproduction); and effecting energy-transformations without cessation (for in the general sense the organism does not die). The definition withstands criticism. We cannot here defend it in detail, and all we are immediately concerned with is the conception of the organism as a system *in which compensatory energy-transformations proceed.* We can show that it conforms to the two laws of thermodynamics, but the second law must be regarded as having a double sign.

The body of a plant or animal is a system conforming strictly to the law of conservation.[1] If the weight of an animal remains constant, there is an exact balance between the mass of the ingesta and that of the egesta. If the energy-values of ingesta and egesta be determined, a difference will be found; that is, the energy-value of the ingesta is greater than that of the egesta. But the difference will be represented by the value of the energy of the mechanical work done by the animal, by the heat conducted away from its body by conduction and radiation, and by the heat of the egesta. The animal body is therefore a machine that transforms energy just as a heat-engine does, with the limitation that we have suggested, of the second law of energetics.

It is a much more "efficient" machine than those of thermodynamics, if we like to put the difference in this way. But it is hardly accurate to speak of the efficiency of the organic mechanism in the same way as an engineer speaks of the efficiency of a heat-engine; for the animal efficiency is an adaptation. The efficiency of an overfed, sedentary man is only a small fraction of the efficiency of a soldier in perfect health and in "hard" training. The fraction of the potential energy of the food which transforms to the kinetic energy of bodily movements obviously depends on the "will" of the animal—it is an adaptation to the circumstances in which the animal places itself.

There is a certain loss in the transformation of chemical energy into mechanical work in an inorganic system. Some

[1] That is, the organism considered like other physico-chemical systems as an object in space, objectively considered. Subjectively some things—dreams, visions, some memories, hallucinations—are not conserved. We get over this difficulty by saying that the things that are not conserved, though they possess existence, are unreal. The things that are real are the things that are conserved.

fraction of this energy is dissipated as heat; a large fraction in the case of a heat-engine, and a smaller fraction in the case where chemical energy transforms directly to electrical energy, and then the latter to mechanical energy. We cannot, however, be quite sure that any part of the potential energy of the food is so dissipated in the metabolism of the perfectly healthy animal. Heat is certainly radiated away from the body of a mammal, but we must regard heat-production as a definite, purposeful activity in such an organism. It maintains its body at a certain constant temperature, which is that temperature at which its metabolism proceeds with the greatest advantage to itself. The organic system, like the inorganic one, conforms to van't Hoff's law, and it is obviously an advantage that the rate of chemical change should be independent of the temperature of the environment. Constant bodily temperature is therefore a compensatory adaptation. The warm-blooded animal is, however, the exception, for the majority are cold-blooded; that is, their temperature is identical with, or approximates very closely to, that of their immediate environment. In such animals the potential energy of the compounds taken as food transforms into the kinetic energy of the movements of the body without passing through the form of heat. Further, since it is very difficult, or impossible to demonstrate by exact calorimetric experiments, any heat production in a cold-blooded animal, we cannot say that energy is dissipated in its transformations.

In disease, or in imperfect functioning, there is, of course, more or less wasteful heat production. But we cannot demonstrate that there is an inevitable, fairly large dissipation of energy in the organic system, and so we cannot apply to it the second law of thermodynamics with all the strictness in which it applies to inorganic systems. Certainly it is misleading to compare the "animal machine" with the heat-engines of physics. The animal organism is a system in which energy falls from a state of high, to a state of low potential, as in, say, the Carnot engine. Some part of the energy of the heat-engine transforms to mechanical energy—does work against resistance—and some part is dissipated and becomes unavailable. In the animal, also, potential energy transforms to mechanical energy, but it certainly has not been shown that any large part of the potential energy taken in becomes truly dissipated, in the cold-blooded animal at all events.

This type of metabolism, the katabolic type, characterises the animal, but not the plant. The essential difference between typical animal and typical plant is that the former possesses a sensori-motor system while the latter does not. Energy of the available form is made use of by the animal (and the heat-engine) and transforms to the kinetic energy of moving bodies. Energy of radiation is said to be made use of by the typical plant organism; but it transforms, not to kinetic energy of the parts of the plant, but to potential chemical energy. The typical plant energy-transformations are to be compared with the exceptional endothermic transformations of inorganic systems.

The salient character of plant metabolism is the synthesis of carbohydrate and proteid from inorganic compounds. Water and carbon dioxide react together in such a way as to form a sugar; that is, the elements of the latter are those which have been taken into the plant system as water and carbon dioxide. These compounds possess far less intrinsic energy than does the sugar which is formed from them, so that the plant must have some source of available energy to draw upon in effecting the synthesis. This source is radiation. In the absence of light chlorophyll is not formed, and in the absence of chlorophyll sugar is not synthesised from water and carbon dioxide. More energy is represented by the radiation falling on the surface of the green leaf in unit time than is represented by the energy-difference between the sugar which is formed in unit time, and the carbon dioxide and water which are the initial phase in the transformation. Formaldehyde can be synthesised from water and carbon dioxide, and it is conceivable that formaldehyde may be "polymerised" to form a sugar. There is a certain functionality between the intensity of the incident light, the concentration of carbon dioxide round the chloroplastids, and the quantity of carbo-hydrate synthesised. It is thus probable that the formation of starch in the plant organism is a "photosynthetic" process, and that the requisite energy is obtained from that of light radiation.

But, it must be urged, repeated investigation has failed to show clearly that the radiation is the actual source of the increase of available energy due to the life processes of the plant organism. Radiation falling on an inorganic surface almost always transforms into low-temperature heat, unavailable for such a chemical transformation as that of water and carbon dioxide into carbohydrate. Further, precisely the same synthesis is effected by certain species of bacteria. Nitrifying organisms can form carbohydrate and proteid from a medium containing an ammonia salt, water, atmospheric carbon dioxide, and traces of essential mineral substances, *in the absence of light radiation.* A general theory of organic syntheses must also include these transformations, and must not regard as indispensable a source of available energy in the form of radiation.

Let us consider the system, water, carbon dioxide, and radiation as an inorganic one. If it can undergo an irreversible change it will do so, when it will attain a condition of stability. If it cannot undergo an irreversible change it will remain stable. It does not, of course, change; that is, *of itself.* The system carbon dioxide, water, and light radiation is not susceptible of transformation into the system carbohydrate. Neither will water and carbon dioxide undergo spontaneous change to form acetylene. But water can be decomposed so as to obtain hydrogen and oxygen, and carbon dioxide can be decomposed so as to obtain elementary carbon. Acetylene can then be synthesised from its elements. But obviously the synthesis is only possible when we couple the systems, incapable of themselves of further change, with other systems which do undergo change, in the course of which available energy is evolved. That is to say, we can reverse otherwise irreversible energy-changes if to the system which has attained stability we can couple a compensatory energy-transformation. Assuming, then, that the formation of sugar in the green plant is a physico-chemical reaction dependent on the transformation of the energy of radiation, we arrive at the conclusion that the living plant-cells themselves are not part of the physico-chemical system in which the potential chemical energy of sugar is being accumulated, but they are the agency which effects the compensatory energy-transformation.

Reverting now to the essential distinction between animal and plant we recall that the former is a physico-chemical system, in which potential energy passes into kinetic energy; while in the latter there is an accumulation of potential energy. In inorganic systems in which potential passes into kinetic energy the tendency is always the same, that is, the final form of the transforming energy is low-temperature heat, which becoming uniformly distributed throughout its environment also becomes dissipated. In organic systems in which potential energy passes into the kinetic form, that is, in the animal, there is no such necessary tendency, for the sensori-motor system is such that the kinetic energy resulting from the processes of metabolism can be directed. It *may* at once be truly dissipated, but it may also be transformed into available energy. An animal may uselessly dissipate its energy, with no other result than to cause mechanical friction; but it may also so use it as to create differences of potential, whereby a part, at least, of the energy transformed remains available for further change. In organic systems in which potential energy accumulates, that is, in the plant organism, the same tendency is carried much further, inasmuch as stable chemical compounds of high energy-value are formed as the result of the life-process. Thus in organic systems generally the tendency of their energy-transformations is opposed to that which characterises inorganic systems. In the latter entropy is continually augmented; in the former entropy-augmentation is arrested.

Further, the organism *is that which affects compensatory energy-transformations*; and the more we think of it in this way the more clearly do we appreciate the distinction between the organic and the inorganic system. The green plant with its environment, is the theatre of a twofold energy-transformation; on the one hand certain parts of the whole transform irreversibly into the compounds water and carbon dioxide, while, on the other hand, part, that is, the energy of radiation, transforms in such a way as to reverse the tendency to dissipation which is the result of the katabolism of the plant or its associated animal life. The plant itself, that is, something which is neither the metabolising proteid and carbohydrate, nor the carbon dioxide and water, couples together the energy of radiation with the transformed carbon dioxide and water so as to form carbohydrate. This is essentially what occurs in the synthesis, with absorption of available energy, of a chemical compound in the conditions of the laboratory. Some substance which has never been found apart from the tissues of a plant or animal is formed from the inorganic materials by the chemist who sets up a compensatory transformation. Clearly such syntheses do not prove that there is no distinction between the organic and inorganic, since they are effected by precisely the same means as that by which they are brought about in the organism. Of themselves they do not occur free in inorganic nature; they occur only as the result of *direction* conferred on physico-chemical reactions by the intelligence of the experimentalist—that is, by life.

The organism therefore exhibits *tendency* which is opposed to the direction taken by inorganic processes. But the latter is, as we have seen by considering Boltzmann's example of a physically changing gas, not necessarily always the same. It is highly probable that any physical change whatever will proceed as the second law of thermodynamics indicates, that is, in such a way that entropy will be augmented. But it may proceed otherwise: two gases at different temperatures, and in thermal contact with each other, will most probably mix so that in a short time the temperature of the mixture will everywhere be the same. The change is irreversible, we say, in the sense that it is highly improbable that it will reverse of itself; but it *may* reverse, and there *are* regions in the whole mass of gas where the temperature is higher than in adjacent regions. Yet the probability of a reversible change is infinitesimally small, and the dimensions of the regions in which the temperature departs from uniformity are also infinitesimally small.

The universe which is physically active is, both in its dimensions and its duration, very great. But the material universe that we know occupies only an infinitesimally small part of space: probably the stars that we know, that is, those which radiate light are only a small fraction of the whole material universe. To say this is the same thing as to say that it is highly improbable that any part of the entire universe is physically active—has been the theatre of a restoration of available energy, or a reversal in sign of the second law of thermodynamics. But life also is highly improbable. The total mass of organised substance on our earth is infinitesimally small in comparison with the mass of the globe. The mass of nitrogen in the chemically combined form is an infinitesimally small part of the total nitrogen of the atmosphere. Vegetation, rich as it may be on some parts of the earth, is entirely absent over other large areas, and is anywhere only a film of almost inconceivable tenuity in comparison with the bulk of the planet. If living substance is the expression of a reversal in sign of the second law of energetics, its probability of occurrence is of much the same order as that which we have already indicated in relation to physical changes.

The reader may now see the cloven hoof of Bergsonism in the above argument. Life is that which sets itself against, and tries to arrest the general tendency to inertia. In his distrust of metaphysics he may attach slight value to such a view of the organism, but, approached from the standpoint of the second law of thermodynamics as only a probability, the Bergsonian speculation may not appear to be so fantastic after all. A theory of the organism must, it seems to us, take account of the second law of energetics as having a double sign. It may be, of course, that the activities of the organism are capable of reduction to chemical and physical processes, all of which are to be regarded as special cases of the second law—in that event biology is only a department of physical chemistry, and our conception of life must be a mechanistic one. But so long as physiology fails to provide physico-chemical explanations of vital processes, and so long as another physics and chemistry than that of the second law is conceivable, then a real science of biology may be possible; and to insist on a mechanistic conception of the organism is only to dogmatise.

NOTES

The British Science Guild and the Fight for Science

We are glad to hear that the British Science Guild has decided to continue its action regarding Science and the State in spite of the pressure of business caused by the war. Indeed, the moment appears to be an auspicious one for endeavouring to impress upon both British Governments and their subjects that there is such a thing as science—which they seemed almost to have forgotten before the war. Owing to our fortunate geographical position, to our brave sailors and soldiers, and perhaps still more to our extraordinary good luck, we have hitherto escaped the immense danger which has long threatened us in consequence of our wilful indifference to the power of science in all departments of human life, including strategy. Nations, like individuals, must make up their minds as to whether they intend to be nations of sentimentalists, faddists, and indifferentists, or to use the brains that nature has given them.

The difficulties under which science labours in Britain have been much commented upon in past numbers of SCIENCE PROGRESS, and in October last we published a programme of reforms which require immediate attention. The British Science Guild has now taken up the direction of affairs in this matter, and is issuing circulars to all men of science asking for information on many points. We sincerely hope that all scientific workers will support the Guild, and, if they do not receive copies of these circulars, will write for them to the Secretary, 199, Piccadilly.

The Fools' War

The question of the cause of the war should still exercise the minds of scientific men and other trained reasoners. One of the most valuable publications on the matter is called *The Case of the Double Alliance* v. *the Triple Entente*, argued by James M. Beck, formerly Assistant Attorney-General of the United

States. It was originally published by the *New York Times*, and has since been sold by the *Times* in the form of a pamphlet for one penny. Mr. Beck is an able American lawyer, who has had much experience in disentangling important conflicting pleas; he approaches the discussion of the cause of the war from a quite impartial standpoint and furnishes a quite impartial analysis. The document is therefore a very valuable one—and it completely demolishes the excuses which the Germans have put forward in mitigation of their barbarous and wicked action. It is evident that Germany had carefully prepared her plans long before the commencement of the war, and that she used the Sarajevo crime merely as a pretext for her attack upon surrounding nations. Mr. Beck complains that German official publications have suppressed many vital documents, and sums up as follows: "(1) That Germany and Austria in a time of profound peace secretly concerted together to impose their will upon Europe and upon Servia in a matter affecting the balance of power in Europe. . . . (2) That Germany had at all times the power to compel Austria to preserve a reasonable and conciliatory course, but at no time effectively exerted that influence. . . . (3) That England, France, Italy, and Russia at all times sincerely worked for peace. . . . (5) That Germany, in abruptly declaring war against Russia for failure to demobilise when the other Powers had offered to make any reasonable concession and peace parleys were still in progress, precipitated the war." He concludes that "The German nation has been plunged into this abyss by its scheming statesmen and its self-centred and highly neurotic Kaiser, who in the twentieth century sincerely believes that he is the proxy of Almighty God on earth, and therefore infallible," and that "this detestable war is not merely a crime against civilisation, but also against the deceived and misled German people."

The first question which any man accustomed to disentangle truth from falsehood will ask is this, What on earth did Germany and Austria expect to gain by this struggle, even if victorious? The fact is that no possible gain would compensate for one-millionth part of the loss which was sure to be inflicted by the struggle itself. Before the war Germany was very prosperous in everything, and, even if she succeeds, she will not obtain more prosperity than she would have obtained had she elected to continue in the paths of peace. She reminds one of a millionaire who is making immense profits by legitimate trade, but who, by some sudden obsession or madness, is foolish enough to attempt to rob another person of his watch! Such a man is suddenly flung in a moment from the heights of prosperity into the cell of the criminal—and all for what? The frequently-stupid press of the world is very fond of talking about national ideals, aspirations, and rights. After all, what are these things? The ideals, aspirations, and rights generally come to this when analysed—that one nation imagines itself justified in seizing the goods of others. Except in a few special cases, nearly all these claims are merely fraudulent, and are put forward by designing politicians and other self-interested schemers in order to befool the stupid proletariat for their own advantage. If this is not the case, those who have caused the war must face the alternative indictment, that they themselves are fools; and those who read Mr. Beck's summary will probably decide that they are both.

But the folly has lain not only with the Germans, but with the innumerable busybodies who, though untrained in the ways of reasoning and possessing neither special knowledge nor attainments, think themselves capable of conducting all the affairs of the world. Such have been the British faddists who gave the German bandits their long-expected opportunity by refusing to allow their country to prepare adequately for its own protection and for the redemption of the guarantees which it had given to other nations, such as the Belgians. Nor will Mr. Beck's summing-up leave us without some sense of the inefficiency of our party politicians.

There is no evidence that before the war our Government had ever clearly analysed the issues, had made any adequate preparation to deal with the appalling disaster which the best military experts then declared was imminent, or had even decided upon the policy which they would adopt if a Continental war were to break out. It has been said in the French press that if Britain had firmly declared it would take the side of France and Russia in the case of an aggressive war started by Germany and Austria, this war would probably never have taken place; and Mr. Beck says, "Sir Edward Grey went so far as to tell the German Ambassador that if this was not satisfactory and if Germany would make any reasonable proposals to preserve peace and Russia and France rejected it, 'His

Majesty's Government would have nothing to do with the consequences,' which obviously meant either neutrality or actual intervention in behalf of Germany and Austria." Naturally Germany and Austria imagined that Britain was too cowardly to help the nations with which she had recently pretended to be so friendly, and therefore proceeded at once in their long meditated crime. Throughout all this, there appears to be an entire absence of foresight and preparation on the part of the British, and not a little on the part of the French—and this in spite of the most emphatic warnings from the highest experts. On the one side a deed, which if it had been performed by an individual would have been stigmatised as being one of the vilest ever perpetrated, and on the other side a degree of folly in the guardians of the world's interests which it will be difficult to find words for. And our young men are to be slaughtered by millions for the doings of these people! Ultimately the blame lies with the mass of mankind, who love to drink the strong wine of superstition, dogmas, and all kinds of falsities, and who hate the pure cold water of reason. We still worship false gods which we call ideals, and graven images which we call statesmen, leaders, and kings. No event in the history of the world has better justified the censures of the satirists from Timon to Swift and Carlyle; and the fact remains that man in the mass is certainly a very dull creature.

The Quality of the German Lie

The undoubted barbarities with which the Germans have conducted this war, at least in Belgium, have already received the condemnation of civilisation, but these people have also added another disgrace to humanity in the perfectly shameless system of lying which they have used. Of course, falsification is an actual weapon in war, often used with great effect for the purpose of fogging military movements; but the Germans go outside this sphere in their impudent falsifications, and, did we not possess a sense of humour, we might almost consider their lying to be worse than their "Kultur." Every day their armies march from victory to victory (in the same place), and when they have suffered a defeat it is always counterbalanced by a simultaneous defeat of their enemies. Thus no sooner did the British announce the naval victory of January 24 than the Germans immediately announced the sinking of three British ships, the foundering of one having been seen by a German airship! But their mendacity on the course of the war is as nothing to their mendacity regarding the causes of it, and the world's breath is taken away by their absolutely impudent statements, evidently issued on the supposition that the earth is peopled only by fools. Fortunately, however, this is not so much the case (outside Germany) as they think, and it does not need a psychologist to detect the quite simple but characteristic quality of the German lie. This is to accuse one's adversary of one's own crime. Thus if the Germans fire at attackers under cover of the white flag, they accuse the latter of having violated the conventions of war, and if they throw bombs on a civil population, they say that this was in retaliation for the same act on the part of their adversaries. Similarly as to the cause of the war, they wish to pose as a harmless and peace-loving population attacked by arrogant, jealous, and designing ruffians who wish to crush them out of existence.

Perhaps the most remarkable instance of this kind of statement was afforded by an address given by Dr. Kuno Meyer to the Clan-na-Gael in America (*Times*, December 24, 1914). "From 1896," he said, "when the first distinctly threatening note was sounded in England that Germany was the arch-rival who must at all costs be crushed, I have followed closely every step that brought us nearer to the inevitable issue, carefully noting everything in my diary. It was in the summer of 1911 that I lost all hope of peace between England and Germany." That was the time when a handful of Britons, led by Lord Roberts' National Service League, were vainly attempting to persuade the British nation to undertake general military training for the purpose of defending themselves against the attack which Germany had been obviously preparing for the last ten years. As every one knows, Lord Roberts' appeal fell entirely on deaf ears; and now, in spite of this, Prof. Kuno Meyer accuses us of having started designs to crush Germany! Surely there cannot be a single man outside Germany who is fool enough to believe such a story. At the beginning of the present war the effective army of Britain scarcely numbered four hundred thousand men, while that of Germany numbered about four millions—that is ten times as many. Does any one really believe that Britain proposed to crush Germany with a force not numbering more than one man to ten possessed by her proposed victim? This is to accuse us not only of pure brigandage, but also of lunacy. Admitting that our navy was much superior to that of Germany, yet we could hardly crush Berlin with ships of war, or indeed, in a single combat, do more harm to Germany than injure her trade. Really such a preposterous statement is deserving only of the ridicule of history and of the world in general. And the stupid *naïveté* of it is apparent from what Prof. Kuno Meyer added towards the end of his address. "That an invasion both of England and Ireland will take place sooner or later, I together with all my own countrymen firmly believe. The attack upon Yarmouth, the mines placed near Malin Head, and what happened on Lough Swilly and at Sheerness have already foreshadowed it. When Germany has obtained the great object for which she fights, the nations that now bear the yoke of England unwillingly will surely not be forgotten." This statement gives away the previous one, because it simply shows that all the Professor's countrymen had been carefully preparing beforehand for the invasion of England, and that their great object was to crush *us*. Thus Prof. Kuno Meyer directly accuses us of precisely the crime which the Germans had been secretly preparing against Britain, and, we may add, against France, Belgium, and Russia. It is just as if an armed brigand were to meet an old woman in a wood and rob her on the pretext that she was trying to rob him. Poor old England before the war was just like this hypothetical old lady, and it was only owing to the monstrous bungling of German diplomacy at the outset of war that we were saved from these secret nefarious designs.

At the same time, we do not accuse Prof. Kuno Meyer himself of intentional falsification. He is himself a gentle and very simple person who has been easily misled by falsehoods which provoke only laughter outside his fatherland. It would be interesting to ask on what evidence he relies for the truth of his statement that England first sounded a distinctly threatening note against Germany in 1896. The only event which could have made much change in the relations of England and Germany was that Heligoland had been ceded, with great generosity and equal folly, to Germany six years previously, and that the Germans, in gratitude for our kindness, immediately began to fortify it against ourselves. It is as if A should give B a pistol for a birthday present, and B should immediately begin to load it in order to shoot A! Possibly at that time this little action on the part of Germany did rather annoy Britain; but surely the fault lay not with us but with Germany. Again, in 1911, Prof. Kuno Meyer belonged to the University of Liverpool—which is not a very warlike body, and which, foolishly enough, actually refused to allow military teaching within its walls, while at about the same time it gave away two of its professorships to Germans! What fiery, warlike, or blood-thirsty symptoms Prof. Kuno Meyer ever found in the worthy citizens of Liverpool it would be difficult to see. They were like himself, gentle and very simple. On the other hand, we find in every action of the German authorities a carefully and secretly prepared organisation for robbing and murdering the nations which surround them, including Britain. Now, when at last these dull neighbours of theirs have actually begun to see through the German designs, the latter accuse them of precisely their own intentions. It is like the story of the wolf and the lamb in the fable, and is a lie that can be uttered once—but not twice.

A Converted Pacifist

The *Morning Post* of February 11 contained an illuminating article by Mr. James Sexton, the General Secretary of the National Union of Dock Labourers. "Two short years ago—nay, even less—I believed in the international brotherhood of man," he said; "I was a fervent advocate of international disarmament and questioned the wisdom of a big navy. I still retain my ideals; but I frankly recognise that under existing circumstances they are but ideals and completely out of the range of practical politics. As regards the latter point, it is scarcely necessary to say that I question the wisdom of a big navy no longer." Few of our party politicians or of our innumerable wire-pullers and members of caucuses have ventured to be quite so frank as Mr. Sexton is—and we commend him for this quality. Some years ago a large meeting of the National Service League was held in Liverpool under the Presidency of Lord Derby. The meeting was packed with working men and was even then enthusiastically in favour of universal military training for Britain. There were only a few dissentients, and Mr. Sexton was one of these. He spoke the usual words of unwisdom and sentimentality so dear to the Briton. He insisted on freedom even to be slaughtered. The word "free" seems to fill many people with a kind of delirious enthusiasm. Every one has heard of the dear old lady who found a blessedness in the word "Mesopotamia." Britannia is now also an old lady who seems to find a similar blessedness in the word "free." She never considers the exact meaning of the word. Freedom is not invariably a good thing, and, as Milton suggested, it may easily degenerate into licence. People who use catchwords, that is, who subordinate the truth to what they imagine are "principles" and "ideals," not only frequently suffer much themselves but also cause great suffering to others. Britain has been the paradise of such people, and it is largely owing to them that the long-designed and carefully-prepared brigandage of the German nation was able to find its opportunity for robbery under arms. Mr. Sexton's conversion is gratifying because it shows that we still have enough sense left to think a little if driven to it. It is to be hoped that the war will have one result (not frequently mentioned in the papers)—that it will convert Britain from being a nation of faddists to being one of honest and scientific thinkers.

Our Unspeakable Cranks

British cranks still continue to point the argument which we stated in our article on "Irrationalism" in the number for last July; and they have recently gone even beyond themselves. We have seldom seen quite such an evil production as an advertisement which appeared a little while ago in the columns of *Punch*—for the publication of which, however, we are glad to say, Mr. Punch afterwards excused himself. One would think that even our cranks would scarcely dare to hamper our armies in the field by endeavouring to discredit important sanitary measures in the field, such as anti-typhoid inoculation has proved itself to be. But persons who do not possess sufficient capacity for reasoning to apprehend the value of scientific evidence will scarcely possess enough sense to know when they are offending against the common laws of patriotism, not to say of propriety. We are glad to see, therefore, that the Medical Committee of the British Science Guild has issued a leaflet traversing the absurd statements of the British Union for Abolition of Vivisection on this subject. The Committee appeals to our press not to publish the vapourings of these people in future, because, as it points out, "those who are seeking to benefit humanity by medical research are always placed at a disadvantage in published discussions of this kind, because they cannot give a full statement of their arguments except in a lengthy scientific form, whereas anti-scientists can always pretend in a few words that such evidence has been refuted. Still further, it is disgraceful that such investigators should not only be exposed to attempts to discredit their work in this manner, but also to the insults of faddists and cranks who evidently do not trouble to make a sufficiently careful study of the matters which they pretend to condemn."

Referring to the persons whose names have been printed in connection with the British Union for Abolition of Vivisection, the Committee says that it "does not think that these persons have ever done such researches on typhoid inoculation, or indeed on pathological and sanitary problems in general, as to justify them in posing as arbiters on a question of this nature, which requires the most expert and impartial consideration." It is astonishing that there should be found enough fools in this country to keep such an association as this going by subscribing to it.

Science, War, and Agriculture

In the *Morning Post* of January 21, 22, and 23 there appeared a series of articles on the state of agriculture in this country, which have now been reprinted in the form of a pamphlet entitled *England's Food*. These articles set forth in a concise, lucid, and interesting manner the whole question of the wheat supply of the world and how the European War will inevitably increase the shortage, and consequently the price, of man's staple of existence during the ensuing two years. Statistics are given to prove this, and an excellent comparison is made between the producing capacity of England and the other European countries. From these figures we see how very far behind England is in wheat-growing, owing mainly to the fact that the farmers of this country employ methods which those of foreign countries discarded fifty years ago as completely inadequate for modern requirements. In other countries, also, notably France and Germany, the great increase in wheat-growing has been made possible by scientific research on the subject of fertilisers, and money has always been forthcoming to obtain desirable results. Many remedies are brought forward and fully and ably discussed, and a draft of an "Act of Parliament to Secure the Food Supplies of the United Kingdom" is appended.

Does not this show up the same shortcomings in England, so often emphasised in this Quarterly, namely, that scientific research is undervalued, and that through the failure of the Government to endow research properly the people in the mass must eventually suffer? In many cases such results may be indirect and perhaps a little difficult to trace; but in the case of agriculture the results are obvious and immediate—a shortage of food. The following quotation from the pamphlet shows how slow the British Government is to take up any idea. The writer says: "As the Board of Agriculture, although urged to do so, has taken no steps to induce English farmers to sow larger areas of wheat in this country during the past sowing-season there is little hope that England can greatly relieve herself from the pressure which will thus be brought upon her food-supplies, but even yet something may be done to stave off some of the effects of starvation." In another instance a deputation of women applied in the early autumn to one of the Ministers for permission to organise and train female labour to try to supply the shortage of male labour on the farms in the following spring. But they were dismissed with a smile and the assurance that the war could not last till then. But the spring has come, the war still rages, the Government of Austria insists with a firm hand that more land shall be devoted to wheat raising, and England has still done nothing. Is it not time that we should wrest some sort of a blessing out of the curses of war? It has roused England to prompt action in military matters. Can we not even yet look our defects squarely in the face, and in this matter, at least, urge the Government to act, and not only to act, but to act at once?

The Professors and the Organisation of Research

The writer of this note is certainly not the only scientific man in England whose doubts as to the wisdom of our present methods of organising scientific research have been accentuated by Sir Ronald Ross's article in *Nature* of January 17. The distinction there made between discovery and research states the problem in a new manner. We cannot organise discovery. We are unable to evoke those nutations of the intellect that are the germs of original and fruitful ideas: we can only wait for them. But we can organise minor research, although it is not apparent that we are doing so yet. The investigation that is needful in the interests of the State, or the local authority, consists in carrying out a programme. It is a matter of the application of various techniques. It is what has been called "team-work," the simultaneous exercise, towards a common object, of the techniques of various workers more or less unfamiliar with each other's methods. It must be directed by some one familiar with the object of the investigation in all its bearings.

In England this direction has been, so far, entrusted to men occupying university chairs. Now there are few busier persons. A professor in a modern university has, personally, to teach; to supervise the teaching of others; to superintend the business affairs of a laboratory; to attend numerous meetings of senate, faculties, and committees; to occupy himself with the affairs of provincial or London societies; and to write text-books. To all this the State, or local authority, may add the organisation of a programme of research, and it is characteristic of the British attitude towards scientific investigation that the official, or business man, should consider that a university professor can undertake this direction in his leisure moments. Can we wonder that it is badly done? Should not work of this nature be important enough to demand all the time and thought of one man? Should not a man who undertakes it be expected to have it continually in mind to the exclusion of all other professional concerns? If the organisation of routine scientific investigation is not considered important enough for this, can we hope to emulate German State-Kultur?

Many readers will know that the difficulty is surmounted usually by the appointment of "private assistants" by the professors. The latter are then free to engage in their own personal research, devoting only a nominal and perfunctory attention to the organisation of the investigation which has been entrusted to them. But is this fair to the assistant, who then becomes the "hanger-on" of a university laboratory, indifferently paid, and anxious to abandon his employment for some more lucrative profession? Is it fair to the personal research of the professor, for that the latter takes up public work is often due to his desire that he should be enabled to carry on scientific investigation with greater facilities than the resources of his university afford him? Above all, is it fair to the State and the local authority that the acquisition of knowledge on which future material prosperity must certainly depend should be the incidental work of men whose main professional interests are other than those of the investigation committed to them? We plead here for the recognition by the State of a distinct profession, that of scientific research apart altogether from the university organisation and traditions; as adequately remunerated as academic work, and with its own status and responsibilities.

An Assistant.

The Thomas Young Oration (delivered before the Optical Society by Sir James Crichton-Browne on January 20, the President, Dr. Ettles, being in the Chair)

The orator stated that Young was a physicist, also a physician, hydrographer, an entomologist, an actuary, a climatologist, philologist, and an Egyptologist, and while specialists in each of these departments recognise his distinguished ability and penetrating insight, it is upon his optical discoveries that his title to a high place in science must be founded. Young was the founder of physiological optics, and has placed English optical science in a position of unassailable superiority. Young anticipated by a generation Helmholtz and Kalrausch's discovery of the ophthalmometer, and to him is due the honour of the discovery of accommodation, claimed by Germany for Helmholtz and Cramer. All that was done by them was to demonstrate in 1850 by the alteration of Purkinje's images what Young had proved with far greater accuracy at the end of the eighteenth century. One of the advantages of this grievous war in which we are engaged must be that it will pull down from its pedestal and shatter for ever the notion of the German overman in science, literature, art, or ingenuity, created by German self-assertion and supported by the effusive adulation of a few professors of our own, proud of a smattering of second-rate Teutonic learning. The success of Germany in many directions has depended on its huge power of assimilation and on our slackness in this country in permitting the appropriations by her of what property belonged to us. Young's mental precocity was extraordinary. We have his own testimony that at two years of age he could read fluently, and that before he was four he had read the Bible twice through. Young's isolation and self-reliance also betrayed him into a flagrant error from which one would have thought his own consciousness of power might have saved him. He repudiated genius, and maintained that all minds are originally of equal capacity, and that all success is to be attributed to industry. It is hard to understand how Young, seeing as he must have done the bodily differences in human beings, recognising as he did the differences in the acuity of their senses and the size of their brains, could have brought himself to believe that it is possible by any amount of diligence to make a silk purse out of a sow's ear.

The orator gave a detailed account of Young's work on the accommodation of the eye, which was subsequently completed by Sir William Bowman. Young's scientific career was for a time blighted and the power he should have exerted on scientific progress paralysed by an attack in the *Edinburgh Review.*

F. W. EDRIDGE-GREEN.

The Kitasato Institute for Infectious Diseases, Tokyo

We have received a notice from Prof. Dr. Kitasato and seven of his distinguished associates informing us that they have all left the Imperial Institute for the Study of Infectious Diseases of the Home Department of the Japanese Government, and have started a new private institution with the above name at Sankocho, Shibaku, Tokyo. We wish this new institute with its very distinguished staff every success in the future.

CORRESPONDENCE

To the Editor of Science Progress

"ELEMENTARY LOGIC"

Sir,—In an interesting review of my *Elementary Logic* (in No. 35 of this Journal) Mr. Shelton asks a question which I am glad to answer. He asks why I should have written a textbook so nearly on the old lines. Three reasons appealed to me: First, that in no other way would the book have had a chance of being used at the present time; secondly, that all through Part I. the reader is guarded against taking the subject for more than it is worth; and thirdly, that the contrast between the old Logic and its successors is by this method made more visible.

I do not quite agree with him that because the old Logic fails to accomplish its purpose of being applied to actual reasoning it should be regarded as a strictly formal *science*. In so far as it is kept strictly formal it seems to me to stop short of being science—unless we mean by "science" something very unimportant. In two ways chiefly it differs from science as now best conceived: (1) It is pervaded by the notion that finally coercive proof is attainable; and (2) it is essentially something to be *learnt*, instead of being a subject in which original thought is required. Since the chief actual work of science consists in correcting, little by little, previously accepted scientific beliefs, these two qualities seem to me deeply anti-scientific. And it differs from mathematics partly in the fact that the complexities of the latter have a practical value, and partly that they have so little (if any) misleading influence.

Alfred Sidgwick.

PARTY POLITICS AND SCIENTIFIC REPRESENTATION

Sir,—Your leading article in the January number of Science Progress makes an indictment of the evils of party politics, the force of which will generally be admitted. Yet parties are a natural formation, almost a necessary formation, in a popularly elected chamber endowed with legislative or executive powers; for "party," in the words of Edmund Burke, "is a body of men united for promoting by their joint endeavours the national interest upon some particular principle in which they are all agreed." In the accomplishment of any act which requires a common effort individual members will be found ready to subordinate personal preferences on questions not of primary importance for the sake of the general purpose. Party organisation may, however, be carried to the stage when the individual is denied the use of his own judgment, and is compelled upon pain of exclusion to follow the party dictates into ways he strongly feels to be contrary to the national good. Willing or unwilling, the partisan becomes the slave of the party. The environment of present-day politics is hostile to men of broad views and independent judgment, and members of this species when they are found do not maintain their existence without a severe struggle.

What is at the root of this canker in political life? One of the causes, and not the least important, is the method of electing members of the House of Commons. In the narrow limits of the single-member constituency, entry to a parliamentary career is only possible through one or other of the organised parties. To say this is to say that the avenue to Parliament is closed to all who are not prepared to wear the party yoke. Among this class are ranked practically all our men of scientific distinction, the absence of whose voice from Parliament is the chief reason for the national neglect of science which the nation through the catastrophe of war has just begun to realise.

To enter into a detailed examination of the effects of the single-member constituency would carry me too far at this time. These effects are great and reach deep. The system is a relic of the past, framed for conditions which have long since disappeared. It is as unsuited and inadequate to the present needs of politics as a surgeon's set of instruments of one hundred years ago would be unsuited to the delicate operations of modern surgery.

The path of advance lies in the extension of parliamentary constituencies until they are of a size entitled to several representatives, and in the election of these representatives by a scientific method of proportional representation. Advocates of proportional representation do not offer it as a panacea for all political ills, but it would certainly be a mitigation of the outstanding evils of party politics, and it would provide an opportunity for the better qualities of human nature and the higher ranges of human mind which the present system of election unfortunately tends to exclude from national service. The method ensures also the more just numerical representation of the different elements of popular opinion. But one of its chief merits is the alteration both in personality and in mentality of our elected rulers which it hopes to effect.

The Proportional Representation Society exists for the purpose of effecting this reform in our political life. A short pamphlet explaining its aims and proposals will be sent to any one applying to the undersigned.

Yours very faithfully,

Alfred J. Gray,
Secretary.

The Proportional Representation Society,
179, St. Stephen's House,
Westminster Bridge, S.W.

ESSAY-REVIEWS

CHARACTER IN RELATION TO THE EMOTIONS AND INSTINCTS, by F. W. MOTT, M.D., F.R.S. : on **The Foundations of Character,** being a study of the tendencies of the emotions and sentiments, by ALEXANDER F. SHAND, M.A. (Macmillan & Co., Ltd., 1914, price 12*s*. net.)

OPPOSITE the title page the author gives the two following passages from the works of two of the greatest of English philosophers and students of Mind and Character :

"And this subject of the different characters of dispositions is one of those things wherein the common discourse of men is wiser than books—a thing which seldom happens.

"Wherefore out of these materials (which are surely rich and abundant) let a full and careful treatise be constructed so that an artificial and accurate dissection may be made of men's minds and natures, and the secret disposition of each particular man laid open, that from a knowledge of the whole, the precepts concerning the cures of the mind may be more rightly formed. And not only the characters of dispositions impressed by nature should be received into this treatise, but those also which are otherwise imposed upon the mind by the sex, age, country, state of health, make of body, etc. And again those which proceed from fortune, as in princes, nobles, common people, the rich, the poor, magistrates, the ignorant, the happy, the miserable, etc."—Bacon, *De Augmentis Scientiarum*, bk. vii. ch. iii.

"Ethology is still to be created; but its creation has at length become practicable. The empirical laws, destined to verify its deductions, have been formed in abundance by every successive age of humanity, and the premises for the deductions are now sufficiently complete."—J. S. Mill, *A System of Logic*, bk. vi. ch. v. 6.

No apology is necessary for giving fully these two quotations in the review of a book which essays "a full and careful treatise," and the creation of which science of character Mill states has at length become practicable. It may be stated that this work has given rise to a discussion at the Aristotelian Society, which has been printed in the form of a "Symposium : Instinct and Emotion. By Wm. McDougall, A. F. Shand, and G. F. Stout." A reference to the symposium will be made later.

Nearly twenty years have elapsed since Mr. Shand published in *Mind* an article entitled "Character and the Emotions"; in this he formulated the hypothesis that the sentiments are complex derivatives of the primitive emotions. Many eminent psychologists have adopted or partially adopted his views, among whom may be mentioned Professors Stout, McDougall, Westermarck, Sully, Caldecott, and Boyce Gibson. The work under review shows that the author in the interval that has elapsed, has spared no pains by thoughtful analysis, observation, and wide reading to develop and mature his original theories. Thus n the analysis of love and hatred he shows that "the same four emotional dispositions of fear, anger, joy, and sorrow, which are essential to the system of love, are present also in the system of hate." In his analysis of hatred the author expresses the opinion that with the progress of civilisation hatred is becoming rarer ; the knowledge of foreign countries and their abandonment of aggressive policies have diminished the hatred of foreigners. Mr. Shand did not know what German "Kultur" meant, nor was the "hymn of hate" written when these thoughts entered his mind. Much more appropriate to the present time are the following quotations : Shylock asks, "Hates any man the thing he would not kill?" "Destruction becomes the prominent end of hatred." "All means may be adopted for this end, even that of prayer." These quotations from the author's description of this sentiment are being well exemplified at the present time by a whole nation.

We find much more pleasure in reading Mr. Shand's work than those of many other psychologists ; for he has with infinite care, skill, and pains attempted the study of the tendencies of the emotions and instincts by an analysis of the characters portrayed by the great novelists, poets, and dramatists. In this part of the work we find the author at his best ; and even if the work had no other merit than that of giving apt quotations on the foundations of character, as exhibited in the portrayal of the sentiments, by the great authors, ancient and modern, and especially the great English and French authors, its value as a literary contribution to Psychology would be considerable. Again, Mr. Shand is very happy in his illustration of the emotion and instincts of animals by his references to the experiences of naturalists and hunters of repute, and in the application of the same to his theories he has shown an acutely critical mind.

When, however, the author attempts to frame a large number of systems and laws upon the foundations of character, a by no means easy task to accomplish, one has a feeling that in a natural desire to be a scientific and accurate psychologist of the schools, he sacrifices that freedom and beauty of language which we find so well exemplified in the study of character by the great novelists and dramatists, and which Lessing, in adverting to Shakespeare, admirably depicts : "He gives us a living picture of all the most minute and secret artifices by which a feeling steals into our souls ; of all the imperceptible advantages which it then gains, of all the other stratagems by which every other passion (sentiment) is made subservient to it, till it becomes the sole tyrant of our desires and of our aversions."

Mr. Shand expresses this organisation of a sentiment in psychological terms and by the law "Every sentiment tends to include in its system all the thoughts, emotions, and qualities of character which are of advantage to it for the attainment of its ends, and to reject all such constituents as are either superfluous or antagonistic."

The psychology of the foundations of character as exhibited in the motives and conduct of human beings, and "the secret dispositions of men," has been studied in all civilised countries by the great dramatists and novelists. In our own times students of humanity, such as the Russian novelists Tolstoi, Dostoieffsky, and Tourgenieff, breathe such life and action into their characters that, for the time being, when reading these authors our minds are transported to the scenes and actions their language portrays with such reality that we almost feel as if we are acting a part, and responding by an echo of our own emotions and sentiments. Mr. Shand, as before said, makes use of this source of inspiration very freely; thus, in alluding to the sentiment of avarice, he quotes in a very interesting manner characters from Molière's *l'Avare* and Balzac's novel of *Eugenie Grandet*. Avarice may be regarded as a morbid complex social product of the instinctive tendency to self-preservation ; "it is a type of character of its own, constituted of its emotions, thoughts, and volitions, which its end requires"; the passion for wealth steals into the soul, and, as it becomes more and more organised and fixed, it destroys or restricts all the nobler altruistic sentiments. Mr. Shand concludes that Balzac has added nothing essential to the type of miser as it was drawn by Molière, but he has made it more complex and human in providing it with a few stunted affections. The miser of Molière has no love even for his children ; he even suspects his children of robbing him ; but the old miser Grandet of Balzac, although he makes all the members of his household participate in his passion by industry, meanness, and parsimony, including his wife, to whom he shows little or no affection, yet the novelist makes him show a genuine affection for his only child Eugenie. Mr. Shand asks, Do we not discern from this character of Balzac that a part of the system of every great sentiment must be a social effect outside of the individual in which it has developed ? A pretty obvious conclusion. It seems to us hardly necessary to frame the following law, "The qualities that a sentiment requires for its own needs in becoming fixed tend to qualify the character as a whole," for Mr. Shand asserts this is a restatement of the law of habit ; again, it seems unnecessary to make the following law, "The qualities of a man's character, whether innate or acquired, hinder the development of all sentiments that need opposite qualities, but aid those that need the same."

It is true, as Mr. Shand says, that "there are some men whose characters are so strangely balanced that they seem to be made up of what are called contradictions—extravagance and meanness, courage and timidity, sincerity and dissimulation, frankness and reserve." May not this be due to the fact that, in the raw material of character, they have inherited two temperamental tendencies of an opposite nature, and that each has become to a certain degree organised? In fact, we hardly think the author has considered sufficiently the importance of the matter of inheritance of the raw material of character, and especially in regard to the inborn tendencies in relation to the Mendelian doctrine of inheritance, by which it is conceivable an individual may inherit from the separate temperamental tendencies of two ancestors two opposite tendencies. In chapter xiii. Mr. Shand considers the influence of temperament on character ; he makes the distinction between temper and temperament, and very properly criticises keenly and finds inadequate the traditional four temperaments. He considers the better way is to view a man's temperament in the light of his tempers—that is, the various particular dispositions which each of his emotions tend to assume : thus, in different individuals, the emotion of anger may give rise to irascible, violent, sullen, and peevish tempers.

On page 169 Mr. Shand, in discussing the influence of the natural tempers on the stability of sentiments, discusses and analyses in an interesting manner the character of Lucy Ashton in Scott's *Bride of Lammermoor*, and refers to the existence of three tempers existing in her together. The novelist says : "She, the mother, was mistaken in estimating the feelings of her daughter, who, under a semblance of extreme indifference, nourished the germ of those passions which sometimes spring up in one night—and astonish the observer by their ardour and intensity ; her sentiments seem chilled because nothing had occurred to interest or awaken them."

On page 172 the author claims that the conception of character gradually unfolded in the first and introductory book furnishes us with some tentative laws for our guidance, and that therein is indicated many lines along which further observation and research may be directed. The conclusion arrived at is that there are three principal stages in the development of character. "Its foundations are those primary emotional systems in which the instincts play at first a more important part than the emotions in them, and as instrumental to their ends are found the powers of intelligence and will to which the animal attains. But even in animals there is found some inter-organisation of these systems or, at least, some balance of their instincts, by which they are fitted to work together as a system for the preservation of their offspring and of themselves."

"This inter-organisation is the basis of those higher and more complex systems which, if not peculiar to man, chiefly characterise him, and which we have called the sentiments, and this is the second stage. But character, more or less rigid in the animals, is plastic in man, and thus the sentiments come to develop for their own more perfect organisation systems of self-control in which the intellect and will rise to a higher level than is possible at the emotional stage, and give rise to those great qualities of character that we name fortitude, patience, steadfastness, loyalty, and many others, and a relative ethics that is in constant interaction with the ethics of the conscience, which is chiefly impressed upon us through social influences. And this is the third and highest stage in the development of character and the most plastic, so that it is in constant flux in each of us, and the worth that we ascribe to men in a review of their lives, deeper than their outward success or failure, is determined by what they have here accomplished."

The second part of the work deals with Instinct and Emotion, and as already mentioned formed the subject of a symposium by three distinguished psychologists. As I was asked at the meeting to treat the subject from a physiological point of view, and as this may interest the readers of SCIENCE PROGRESS, I have added my remarks to this review in the form of an appendix. A principal criticism of McDougall regarding Shand's position in relation to the Instincts and Emotions is perhaps best illustrated by his remarks concerning Fear, to which Mr. Shand has devoted a whole long chapter. McDougall says: "By including under fear a number of emotional states which are in popular speech called fear but which are of quite different nature and are more properly called states of anxiety, Mr. Shand attempts to make the innate system of fear still more complex and comprise a still larger number of tendencies, impulses, or instincts."

"The emotion of fear (according to Shand) is said to choose intelligently whichever of these instincts is most appropriate under the circumstances as a means of securing the universal end of all fear, etc."

Mr. McDougall, having stated what he considers are Shand's views, submits that the nature of fear may be more properly stated as follows: "It is an instinct which impels the animal to seek cover and there lie hid." It is then a chain-instinct of two links; for the attainment of its end commonly requires the succession of two modes of behaviour, first, the seeking cover, secondly, the lying hid. McDougall maintains that there is essentially only one instinctive mode of behaviour, viz. concealment or flight to effect concealment. We shall later discuss this question from a physiological point of view.

In respect to Mr. Shand's contention that "fear tends to elicit anger in support of its end when its impulse is obstructed" (p. 261) McDougall, says: "If this is admitted, why should he seek any other explanation of the fact that the behaviour of fear is apt to turn, when obstructed, to the behaviour of anger."

In another place McDougall examines Shand's dictum that "an instinct has only one kind of behaviour connected with it, and when the appropriate stimulus excites it must tend to respond with this one kind of behaviour." McDougall asserts that "this dictum is an arbitrary and wholly baseless assumption and is contradicted by a multitude of facts." Every instance of the operation of what is well named a chain-instinct affords examples. Thus "when a dog chases a rabbit over rough ground, are we to say that an emotion of pursuit selects in turn from among a number of instincts that are organised in its system, running, leaping, yelping, turning to the right or left, halting, sniffing, doubling, or so on?" Now we may ask, why does the dog yelp and bark; is it not because there is a pre-organised instinctive sensori-motor mechanism over which its intelligence has not control unless the animal has been trained. The emotional disturbance of the chase is so strong that it excites the subcortical preorganised reflex mechanism of vocalisation which has become structurally organised in the long procession of ages when the dog hunted in packs. Here undoubtedly the instinct has only one kind of behaviour although in many instances it may be to its advantage when it is hunting singly.

Mr. Shand is not a physiologist, and accordingly in this otherwise admirable work he has neglected the teachings of physiology. While it must be admitted that physiologists have only lifted a corner of the impenetrable veil which hides the mysteries of the Science of Mind, nevertheless great advances have been made since Maudsley forty years ago advocated the importance of physiology in the "Method of the Study of Mind" in the following passage, p. 48, *Mental Physiology*. "The past history of Psychology—its instructive progress so to speak—no less than the consideration of its present state proves the necessity of admitting the objective method of the Study of Mind. That which a just reflection teaches incontestably, the present state of Physiology illustrates practically. Though very imperfect as a Science, Physiology has made sufficient progress to prove that no Psychology can endure except it be based upon its investigations." I, however, am in thorough agreement with the further statement of Maudsley. "No one pretends that Physiology can for many years to come furnish the complete data of a positive mental science." Still Physiology has made notable advances and in no direction more than in the bio-chemistry of the ductless glands and the influence of their internal secretions on mental and bodily functions in health and disease. This knowledge therefore cannot be neglected in any consideration of the foundations of character, for as Bacon says: "not only the characters and dispositions impressed by nature should be received into this treatise, but those alone which are impressed upon the mind by sex, age, country, *state of health*, *make of body*, etc."

It will not be out of place to further consider the question of Instincts and Emotions in the light of modern physiological knowledge; for it will probably appeal to the readers of SCIENCE PROGRESS, however elementary and imperfect it may be in solving the problems under consideration, if the knowledge so acquired rests upon the sure basis of direct observation.

SOME PHYSIOLOGICAL ASPECTS OF THE QUESTION OF INSTINCTS AND EMOTION

Mr. McDougall finds fault with Mr. Shand for saying that the appetite for food in the new-born infant is aroused by internal rather than external stimulation, which is the feeling or impulse which accompanies or controls the search for and absorption of food; but neither Mr. Shand nor his critics have paid any attention to the important advances made in our knowledge of biochemical stimuli, the result of internal secretions or hormones in the preservation of the individual and the species. And if Mr. Shand, when speaking of an internal rather than external stimulation, is willing to accept a qualitative bio-chemical change in the blood as the exciting cause of desire to gratify the appetites, then physiologists will agree with him. But can this bio-chemical change excite a sensori-motor instinctive mechanism apart from feeling, if we admit that feeling does not exist in the lower medullary centres? To answer this question it is necessary to see what physiological evidence there is of bio-chemical changes in the blood playing an important part in exciting instinctive protective motor reaction. Mr. Shand cites the act of sucking in the new-born infant as being aroused by internal stimulation. The important part that bio-chemical changes play in the vital processes of reproduction has been proved by physiological experiments. Thus Goltz showed that at the proper time a bitch that had become pregnant after a large part of the spinal cord had been removed gave birth to a litter of puppies. Paul Bert long ago showed that the mammary glands of a goat, that had had all the nerves divided, increased in size and prepared for lactation during pregnancy. Physiologists have shown that the corpus luteum, the tissue which occupies the place of the ripe ovum after its discharge from the Graafian follicle, liberates an internal secretion which acts as a bio-chemical stimulus in the blood to the mammary glands and the uterus; it also excites certain cells in the pituitary gland to active proliferation; it causes enlargement of the thyroid and parathyroid glands, increasing their function; and it increases the quantity of lipoid substances in the cortex of the suprarenal glands. There is thus proved to be a bio-chemical association of the ductless glands and the reproductive organs. In the testis, as well as in the ovary, there are really two glands, the one generative, producing germs which escape by a duct, the other ductless (interstitial), the cells of which secrete a bio-chemical hormone, which (from the embryonic state onwards) passes into the blood and is carried to all the cells of the body, making dominant the corresponding male or female secondary sexual characters. It is also the source of vague unconscious sexual desires which at puberty become so strong as to cause a complete mental revolution of the individual.

Now McDougall, in referring to the sucking infant, says the odour of the milk is the first stage in the act of sucking, and it is this which excites an instinctive search; but we know that an anencephalous monster is capable of the act of sucking. Seeing that it has no great brain, can we assume that it is excited by the odour of milk? Why should not a bio-chemical change in the blood occasioned by the entirely changed conditions of life of the child after birth be the exciting cause? In support of this influence of the condition of blood in the medullary centres acting as an exciting cause, we know that an animal with the medulla oblongata separated from the brain above and from the spinal cord below evinces by respiratory movements of the nostrils and the larynx that a condition of anoxaemia suffices automatically to excite the motor mechanism of respiration in the medulla. When a person loses a large quantity of blood he feels the pangs of thirst; this feeling is not removed by moistening the mouth, but it is by the introduction of a large quantity of saline solution subcutaneously or by the bowel, or by transfusion of blood. The normal mode of relief of this concentrated condition of the blood necessitates entrance of fluid into the body by the mouth, and the feeling of dryness of the mouth which is relieved by fluid has become part of an organised associative memory between concentrated blood and the nervous centres in the bulb and cortex, which are connected with the sensori-motor mechanism of the mouth, tongue, and throat. The same applies to the sinking feeling at the epigastrium, indicative of a bio-chemical deficiency in the blood as the result of a lack of food entering the organs of digestion.

The appetites connected with the preservation of the individual, hunger, thirst, and the desire for fresh air, can therefore be shown to be dependent upon bio-chemical stimuli arousing instinctive medullary sensori-motor mechanisms controlled and supervised by cerebral activities, affective, cognitive, and conative, with increasing variety and refinement as we rise in the animal scale.

With the appetites depending upon the organic needs is associated the emotion of disgust which is excited by the sense of smell and of taste; these senses stand like sentinels to the respiratory and alimentary tracts. It is a remarkable thing that nearly all acrid and bitter plants are poisonous, and excite the feeling of disgust and nausea; likewise do foul odours and noxious gases. The feeling excites the emotive expression of disgust, and the desire to avoid a recurrence of the experience. The emotion of disgust is a powerful instinctive protective feeling, dependent upon a sub-cortical preorganised sensori-motor mechanism, as was shown by the experiments made by Goltz upon his decerebrate dog. This animal, which could never learn to recognise the man who fed it, but invariably growled and bit and resisted when he took it out of his cage to put the food in its mouth, which it then ate, was given meat containing some bitter substance, such as quinine or strychnine, yet could not be induced to swallow it, not even when given no food for some time. A normal dog was given this meat, and, as Goltz says, he looked wistfully at his master as much as to say, "I don't like it, but I'll swallow it to please you." Here the intelligence of the animal controlled the powerful instinctive protective reaction. A number of physiological experiments teach us that there is a preorganised medullary protective mechanism against injury (apart from pain, which may be regarded as a psychical adjunct), which enables an animal to adapt its behaviour in accordance with a previous perceptual experience, how to avoid pain under a like condition, and so protect its body from injury. There is always a conscious judgment of value in the behaviour of an animal based upon the individual experiences of the animal, but may there not be an instinctive unconscious judgment of value structurally organised in the medullary protective reflex sensori-motor nervous mechanism? This proposition seems to be of importance in relation to one subject in dispute between Mr. Shand and Mr. McDougall. Thus the former asserts that there may be a multiplicity of instincts within an emotional system, and there may be an organisation of the same instinct in different emotional systems; it is upon these grounds that Mr. Shand differentiates the emotional disposition and the instinct. Mr. McDougall will not accept Mr. Shand's views, and affirms that they are erroneous because they proceed from a radically false conception of instinct, for he regards each instinct to be correlated with a specific emotion. Now instinct is essentially dependent upon sensori-motor mechanisms organised by habitual activities for preservation and propagation in the evolution of the species by natural selection. These habitual actions are dependent upon structural organisation in the spinal and medullary centres, and it appears that there is physiological evidence in support of Mr. Shand's view with regard to the emotion of fear giving rise to several instinctive modes of behaviour, and not, as Mr. McDougall says, one mode of behaviour, namely, concealment.

Sherrington has shown experimentally that a decerebrate cat, kept alive by artificial respiration and suspended in such a way that the limbs hang loose, will instinctively when injured behave in such a manner as to protect itself from injury by flight, for it makes diagonal cyclic movements of the limbs as in running away, for flight under normal conditions is associated with fear; it however also shows signs of anger, for it turns its head towards the side of the injury, snarling, showing its canine teeth, and howling. It may be supposed that the former behaviour would be associated with the feeling of fear, the latter with anger. But in the decerebrate animal the one motor reaction to injury does not appear to interfere with the other; the animal having no brain has no judgment of value to make, and therefore makes no choice in its behaviour of flight and fight. If one mode of behaviour interfered with the other's operation, in all probability the less effective would cease. The following experiment supports this conclusion. In the spinal dog, that is an animal with the spinal cord severed high up, Sherrington has shown that a conflict takes place between the scratch reflex and the reflex to pain, in which the latter prevails. To produce the scratch reflex, an excitation of the hair in the forepart of the body is produced, similar to that caused by a parasite; this causes the hind limb on that side to be drawn up to scratch the part irritated. All the while the rhythmical movements of the hind leg are taking place, the animal is supporting itself on three legs. Now if the pain reflex of withdrawal from injury by a sharp prick (such as a thorn would cause), is excited in the other resting hind foot, the scratch reflex immediately ceases, in order that one hind leg can rest on the ground, thus allowing the injured hind limb to be drawn up. There is a choice here and a mechanism of unconscious judgment of value in the protective reflexes. Mr. Shand cites fear as the emotional system containing several instincts; thus there is the instinct of concealment, the instinct of flight, and even the instinct of fight when the other two are impossible. Mr. McDougall contests this view, and says that flight and concealment are one instinct, for flight has the same end, namely, concealment; but from a physiological standpoint they are different. Concealment may be effected by immobility, in which case there is a distinct diminution of liberation of muscular energy, and concealment by immobility from a physiological point of view is surely not the same instinct as the extraordinary liberation of energy required to escape pursuit successfully.

Here again I do not think psychologists have given sufficient attention to physiological experiments. We may assume that the perception of danger arouses the emotion of fear, and of fright (extreme fear), which acts on the vaso-motor centre, inhibiting its action and causing a depressor effect, so that the large internal arteries are dilated, and the blood flowing away from the skin and muscles causes coldness and pallor of the skin with erection of hairs; the muscles lose their tone and are deprived of energy to contract. This paralysis of fear leads to insensibility which under favourable conditions might lead to concealment, but certainly could not lead to successful flight and concealment. For successful flight requires the same conditions for the production and liberation of muscular energy as fight, and an animal may acquire by experience a conscious judgment of value between flight and fight that under certain conditions it exercises to its advantage.

Nature has provided a remedy to this paralysing effect of fear independent of the will and consciousness, but probably not independent of the feeling of fear; it is the provision of a bio-chemical stimulus to the whole sympathetic nervous system and plain muscle fibre. Elliott and Cannon have shown experimentally that when an animal is frightened a stimulus passes down the splanchnic nerves to the medullary substance of the suprarenal gland, causing an increase of adrenalin to pass into the circulation; this has a powerful vaso-constrictor effect and raises the blood pressure. The biological significance of this has been so well expressed by Elliott in the Sydney Ringer Lecture that I will quote a passage in support of my argument.

"Morphology therefore tells the same tale as physiological analysis. The adrenalin cells and the sympathetic nerves belong to a common system, whose first duty is that of sustaining the activities of the circulatory muscles. As the animal develops its muscular efficiency, learns a hundred new functions, and with a constant body temperature becomes independent of its environment, the sympathetic system becomes more and more complex, and is split up into manifold possibilities of delicate adjustment. All these are but refinements of means for the one great end—to enable the animal to move more swiftly, to catch its prey, and to fight. Fighting power rises with the rise of blood pressure, reserves of sugar to feed the muscles are hurried up from the liver on the call of the circulating adrenalin, the daily routine of digestion is checked by intestinal inhibition, and the various segments of the bowel are cut off from one another by the closure of the sphincters. Cannon has reiterated this view and, further, shown very neatly how the clotting of blood by adrenalin, which had been observed by myself and others, is a further elaboration to check leakage in any chance wound of the body during action.

"By that curious antithesis of the emotions upon which Charles Darwin laid stress, the machinery employed to prepare for fight may, with the cowardice of civilisation, be set as powerfully in motion only to express fright. And in this the adrenalin is equally, or perhaps even more, exhausted."

It is difficult to follow Elliott in this latter argument, for if it requires prolonged fright to exhaust the supply of adrenalin, its escape into the circulation of an animal that relies upon flight is as necessary for preservation as it is in one that relies upon fight. Carnivora may, under certain conditions, rely upon flight just as herbivora may upon fight for preservation.

The raw material or inborn tendencies of the foundations of character are a complex dependent upon species, sex, and race, which have been fixed by the laws of evolution and have structurally organised, and are therefore capable only of comparatively little variation ; grafted on this stock are the inborn dispositions and tendencies dependent upon racial amphimixis and the more mutable and less stable individual dispositions and tendencies derived by ancestral variations and modifications. These "characters of dispositions impressed by nature," and the study of which Bacon found wanting, form the raw material of individual character. The same primitive emotions, instincts, appetites, and desires are common to all the higher animals. These are the source of spontaneous attention and of all habitual actions connected with the preservation of the individual and the species, poetically expressed by Schiller in the following lines :

"Durch Hunger und durch Liebe
Erhält sich die Weltgetriebe."

The stimulus to action is fundamentally bio-chemical, and if there is one fact more firmly established than another in regard to bio-chemical influence of hormones in respect to the emotions and instincts connected with the preservation of the species, it is in relation to the sexual glands. There is abundant experimental evidence and observation showing that, from birth onwards to puberty, an internal secretion of the sexual glands escapes into the blood which excites and makes dominant the corresponding secondary sexual bodily and mental characters ; and that without this internal secretion these distinctive characters of body and mind are modified or wanting.

At puberty, coincident with the dawn of the generative function, there is a complete mental revolution; there is a vague longing and desire, accompanied by an attraction for the opposite sex; it is insistent and so strong that "Love is blind." Indeed, this fact explains what Maudsley in his *Mental Physiology* surmised long ago before the great importance of the internal secretions were known. "The passion of love has its source in the unconscious life, and can no more be explained in consciousness than the feelings of hunger or thirst; it makes an elective affinity of the organism which oftentimes enslaves consciousness and overpowers volition."

But if "Love is blind" it is also true that "Love adds a precious seeing power to the eye," which is apparently a paradox; but the latter is not a biological antithesis to the former, for the latter implies a perceptual instinct of sexual attraction necessary to materialise the bio-chemical stimulus and effect preservation of the species. This perceptual instinct is shown by the fact that the language of love is universal; it cannot be concealed, it appeals by mute eloquence in expression, gesture, attitude, and eye in a manner more enticing and forcible than by any spoken language.

Much more might be said regarding the study of the Mind through Physiology and Pathology, which is applied Physiology, when injury or disease performs an experiment on the human being; but it must not be forgotten, as it is so apt to be both by physiologists and psychologists, that the divisions in all knowledge are artificial; that they should be accepted and used, as Bacon says, rather "for lines to mark and distinguish than sections to divide and separate, in order that solution of continuity in sciences may always be avoided."

PLAGIARISM IN SCIENCE, by the EDITOR: on **Essays on the Life and Work of Newton,** by AUGUSTUS DE MORGAN, edited, with Notes and Appendices, by PHILIP E. B. JOURDAIN, M.A. [Pp. xiii+198.] (London: The Open Court Publishing Co., 1914, price 5*s*. net.)

AN extremely interesting little book, giving the views of that distinguished mathematician and clever writer, Augustus de Morgan, on certain points in the life of Newton, and especially on the famous Newton-Leibniz controversy. Mr. Jourdain's editing is excellent, and his footnotes supply much information which is lacking in De Morgan's essays. The part of the work which will chiefly interest the reader to-day is that which deals with De Morgan's opinions on the controversy just mentioned.

It is often stated very unwisely that questions of priority have no importance for science. This is, in fact, just the opposite of the truth. Men of science, as is well known, derive little or no pecuniary benefit from their studies; but they, like other men, do at least like to obtain the credit which their work is likely to give them among their fellows. This is only what is to be expected; and, certainly, no man of science who has spent, let us say, years over making an important discovery likes to see that discovery attributed to some one else who has perhaps jumped into the arena just at the moment of victory, and who then claims, for such small help, to share the laurels with the victor. It is not to the benefit of science that miscarriages of credit should be easily allowed—and yet they often occur. Nothing is more easy than scientific plagiarism. A man has been working, let us say, for a long time at a special theme, and has nearly brought it to completion. He speaks of his researches and mentions his conclusions to his friends before he completes his work for publication, and the more punctilious and careful he is the longer will such completion be delayed. In the meantime, however, another person who has equal or better opportunities for the investigation hears of this work, hurries through a few experiments or calculations, and brings out the whole idea as his own. Cases of this kind abound in every branch of science, and have a paralysing effect. Thus, in one well-known case, a most important method for the diagnosis of certain diseases was discovered by two workers but could not be perfected by them for want of material. One of them, however, mentioned the method at a Congress. A third person who was present at the Congress possessed much material, and was able to verify and complete the method in a few weeks and to publish it immediately. The result has been that this third person's name has been attached to the discovery and that the names of the original inventors have been forgotten in connection with it, except among the students of scientific history. It is supposed to be an honourable thing for a man of science to be open regarding all his work, but, as will be seen from this example, he may lose much by being so. In this case, the worker who obtained the credit for the discovery behaved quite honourably and mentioned that he had received the hint at the Congress, and the credit fell to him without his seeking for it in any way.

But in other cases the second or third party is not so innocent, and makes a deliberate effort to obtain credit really due to another; and it often happens that a mere hint of A's researches is sufficient to set B upon the track, and if B is an unscrupulous person he can pretend that he has made the whole discovery independently of A, and, as the world is not too much interested in scientific work, B may succeed in this without giving much evidence in favour of his claims. The Newton-Leibniz controversy is perhaps the most famous one of this class, not excepting that of Harvey and the Italians. As every one knows, the controversy raged over what may perhaps be described as the most important discovery ever made—namely, that of the Calculus. There is no doubt that Newton framed the skeleton and muscles of the Calculus when he was about twenty-three years of age—namely, from 1664-66. On the other hand, Leibniz seems scarcely to have commenced his investigations on this subject until 1673 (when he was in England), and did not complete his notation until two years later. The notation of Leibniz is certainly different from that of Newton, and the former advanced the Calculus in many important directions; but the question is whether he did not receive the hint of the whole great system from Newton's work. De Morgan seems to conclude otherwise, and attacks Newton and his friends for attributing plagiarism to Leibniz. But we must confess that his essays on the subject are far from convincing. They are written in a rapid style but with so many parentheses and in such involved language that we often have a difficulty in following him. Besides that, he gives no consecutive history of the facts and nothing like a sufficient analysis of the respective contributions of Newton and Leibniz to the Calculus. Moreover, he appears not to have made a sufficient study of Newton's manuscripts, for on p. 33 of the present book he makes much capital out of the idea that Newton did not develop a proper algorithm for his fluxions until 1677, whereas we learn from the editor's note on the same page that Newton uses his dots as early as 1665, which completely upsets De Morgan's contention. Still further, De Morgan does not assure us on the vital point (which is quite possible) that, apart from all letters, manuscripts, or public documents, Leibniz may have obtained the outline of Newton's system from the conversation of other mathematicians, and that such hints may have sufficed to set him at work on the subject during his visit to England in 1673. It would then have been an easy task for him to advance the study and not a difficult one to invent the famous notation which, quite deservedly, stands to his credit. As a matter of fact, the *onus probandi* lies upon those who maintain that Leibniz's work was quite independent of Newton's. Admittedly, Leibniz did not begin his studies until many years after Newton had invented and employed his in the solution of many difficult problems and had probably shown the method to numbers of mathematical friends. De Morgan's essays are always interesting, but not too convincing. He attacks Newton on several points, and indeed undertakes the not very generous task of impugning the high character of a great but dead man ; and he does so, not in one essay, but in several. In no case, we think, has he proved his case. Even in Newton's quarrel with Flamsteed, we cannot be sure from the written documents that the former had no justification. The truth is that in these quarrels the most important points are often not put into writing at all, are never published, and can therefore never be known to posterity. It is better to let them remain in obscurity ; but the question as to the discovery of the Calculus is on another plane and still requires attention. We should like to see a much superior analysis to that of De Morgan, accompanied by photographs of parts of the various manuscripts, a sound consecutive history of the facts, and an impartial judgment based upon them. De Morgan also ventured to criticise Newton for having abandoned science so largely when he went to the Mint. De Morgan did not appear to know that in some men of great genius a curious psychical revulsion against past studies appears to set in between the ages of forty and fifty—possibly due to the very intensity of the labours undergone in the past. Huxley was another case and nearly abandoned his anatomical studies in middle age.

We are thankful to the publishers and the editor for the book—though we scarcely endorse their estimate of De Morgan's appreciations. In the meantime, Mr. Jourdain's book (with a good portrait of Newton) will serve as a necessary guide for further, and, let us hope, more reliable, analyses than these essays prove to be.

MATHEMATICAL TEXTBOOKS, by AMATEUR : on **Elementary Theory of Equations,** by LEONARD EUGENE DICKSON, PH.D., Professor of Mathematics in the University of Chicago. [Pp. iv + 183.] (London : Chapman & Hall, 1914, price 7*s*. 6*d*. net).

IT may be of advantage that amateurs should occasionally be allowed to review academical works on science, especially on mathematics. The amateur generally comes to a subject without sufficient previous instruction but with an open mind—and possibly with some experience of affairs outside the theme which he makes his hobby. He is therefore peculiarly qualified to judge whether textbooks really fulfil one of the objects for which they are usually written. Really, they are written for two objects—first for ready reference by teachers and other experts, and secondly for the information of those who may be suddenly called upon to explore a subject because it is related to some other branch of work in which they are engaged. Now no one is better qualified than the amateur to judge as to whether a new book fulfils the latter condition. Again, scientific textbooks should really be works of art as well as of science. The various facts or propositions should be set out in the most carefully ordered arrangement, without redundancy, and yet without so much condensation that the reader cannot easily follow the meaning. In mathematics, Euclid is the classical example of the textbook which is both science and art, and the proof of this is that it remains our textbook to the present day in spite of frequent attacks which have failed to dislodge it from its position.

Prof. Dickson's little book is an excellent one in the class to which it belongs. It is well but not too well condensed ; it covers the field usually covered by these books ; the author's meaning is seldom in doubt ; important propositions are not often relegated to examples, and so on. In spite of the Preface, however, from which we gather that the author has designed his book not only for specialists, it will be more useful to them than to those who occasionally dip into the theory of equations. But this is not the fault of the author so much as that of academical custom in the writing of these mathematical textbooks. Really, such books on the "Theory of Equations" are quite wrongly named. They should be called books "On the Theory of Rational Integral Equations" only, and it would generally be right to add the words "Of Low Degree." They seldom make any reference whatever to transcendental equations, or even to rational integral ones of high degree but of few terms, or to other equations which can easily be solved without being put into the rational integral form. Yet such equations are of frequent occurrence in, let us say, chemistry, physics, or statistical work. The labourer in these fields, who may not be himself a very expert mathematician, when he is investigating some problem is often confronted and sometimes defeated by such equations ; yet when he "looks up" a textbook on the subject he can get no help whatever, and, instead of finding assistance, is supplied only with purely academical though beautiful theorems, such as those on the square root of minus one. In fact, these academical works would seem to be designed principally for examination purposes, and contain scarcely any business-like dealing with the whole theme. There is no wide survey of algebraic equations in general ; little reference to the literature, and no list of previous textbooks ; and, while problems which have been solved are triumphantly given, those which continue to foil the human intellect are too frequently left quite unmentioned—much to the discomfiture of the non-expert reader. At the same time, these books really contain much matter which properly belongs to other branches of mathematics, or, indeed, which are of very little practical importance. After all, our main concern with an equation is to solve it, and as quickly as possible ; but our textbooks too often fail to help us in this respect, and give us endless invariants and complex numbers in place of the bread which we seek.

Is it not time that some change be made ? For example, Prof. Dickson's book commences very wisely with a chapter on the graph of an equation. After that, the humble amateur will think that he should have proceeded at once to the analysis and solution of cubic and quartic equations, and then to that of numerical equations of any degree, with at least some survey of the classes of equations mentioned above. Instead of this, Prof. Dickson's second chapter plunges at once into complex numbers, and his fifth chapter rushes at once upon the theorems of Gauss and Cauchy on the existence of a root, this being called the fundamental theorem of algebra—a doubtful proposition, since much work was done on algebra by Descartes, Newton, and a few other inferior individuals before this theorem was invented. The seventh chapter deals with symmetric functions, and the solution of numerical equations is not considered until the ninth and tenth chapters are reached. Surely it would be better to remove the academical parts of the subject to the end of the work, or even to remove them entirely into general algebra or double algebra, and to replace them by a sufficient study of equations other than rational integral ones of low degree. This, of course, can easily be done without impairing the proof of the really useful propositions.

Nevertheless, Prof. Dickson's book is more practical than some on the same subject. It is pleasant to see that he recognises the advantage of Newton's method over Horner's for the solution of numerical equations, in that the former applies as well to non-algebraic as to algebraic equations—in spite of De Morgan's advocacy of the latter method. But neither this book nor the recent one of Burnside and Panton make any mention of Michael Dary's beautiful method (further studied by Heymann and others). The development of roots in convergent infinite series is left altogether untouched, possibly because Newton said that such series were not useful in the solution of equations. There is also no reference to the analysis of equations by considering them as the intersections of two independent curves, and we are still supposed to use Sturm's theorem where an equation can be analysed in two minutes in this way.

The whole subject of mathematical textbooks needs revision in the interests of men of science who are frequently required to make calculations for the study of their own subject, even in biology, but who have no time to make themselves expert mathematicians in general. After all, what we want is a working knowledge of the Calculus, of the management of series, and of the solution of equations, and as regards all of these the existing academical textbooks give us little help, unless we are prepared to spend months of study over them. Each book seems to be copied from the previous one, and to be meant merely for schoolboys and undergraduates, and not for men who have their work in the world to do.

FACT AND FANCY IN HÆMATOLOGY, by H. C. ROSS: on **The Biology of the Blood-Cells,** by O. C. GRUNER, M.D., Lond. [Pp. xii + 392 and 83 Illustrations, 7 in colour.] (John Wright & Sons, Ltd., Bristol, price 21*s*. net.)

ONE of the first instruments bought by the medical student is a microscope, and the first operation he does is usually to prick his own finger, or preferably that of his laboratory neighbour, and examine the blood. Thus from the outset he becomes an authority on the blood-cells and their biology. The examination of the blood is so easy and the cells look so pretty, that the subject is one of great interest to the medical man throughout his career. Should he, by chance, drift to become a professor of pathology, it will be expected of him to write a treatise on the blood at least once in his life-time. On an average these textbooks appear about once a quarter, and have done so for nearly half a century; moreover, nearly every one of them bears a dedication or acknowledgment to Pappenheim.

The book under review is one of these textbooks, although it is modestly styled a companion to other textbooks; but it does contain some novel points which make it differ from most of them. It is clearly and pleasantly written; the author has a charming style in those few paragraphs where he can depart from Pappenheim and embark on description. There are no chapters on or paintings of malaria parasites or trypanosomes; there is no description of how to make a blood-count or stain a film; and from this work it is impossible to make a lightning diagnosis of osteoarthropathy or mental aberration. But there is an excellent glossary of hæmatological terms which must have involved immense work; and the bibliography is complete.

The subject-matter of the book itself is occupied in describing how certain cells of the body go to form other cells, and the other cells to form other cells, and at last the other cells form the dear old blood-cells with which we have been so friendly from our youth. Every one of them has a name, not, alas, Tom, Dick, or Harry, but those found in the glossary at page 337.

As in so many of these books, there is no line of demarcation between fact and fancy. Assertions are made right and left. Certain cells come from the bone-marrow—so it is alleged ; and others come from the spleen. True, in some of the assertions evidence *pro* and *con* is discussed ; but as a rule many of the old theories are stated as point-blank facts—especially if they have emanated from Pappenheim or Heidenhain, who were arch-speculators. All this sounds very fine and builds up a pretty book, especially in the introduction describing the blood-cell factories ; but when we come to analyse it we are forced to the conclusion that most of it is the outcome of deduction based on observation through the microscope of dead structures artificially stained. The number of experiments described is very small, most of the book is merely morbid and physiological anatomy—or rather histology—and speculations based on them. Peripheral blood-cells are killed by some force such as heat, or some reagent, and are then stained in their flattened and contracted state. Bone-marrow or spleen or other tissue is treated in a similar way after having been shredded with a razor. Because the cells or parts of them in one class of slide resemble those in the other, it is deduced that the one class of cell developed into the other. No one has followed the development in the living tissues ; but, never mind, it is all swallowed, especially if it agrees with the ideas of Pappenheim. The red-cells come from the normoblasts in the bone-marrow ; but has any one ever seen a living cell come from the bone-marrow and undergo transformation ? Hyaline lymphocytes are talked about without granules, and leucocytes are said to divide by multiple mitoses within their so-called nuclei. But has the author made these cells divide in the living state by means of the auxetics to which he refers ? The biology of the blood-cell means the birth, reproduction, and death of the living cell, yet *in-vitro* staining is mentioned in the same breath with deductions from fixed films ; is there then no difference between life and death ? The blood-platelets are still described as extruded nuclei of red-cells or as precipitates, in spite of published proof to the contrary ; Kurloff's bodies in the guinea-pig are still described as evidence of degeneration when it has been proved that they are parasites. Degeneration is a term still glibly used although there is no definition as to what it means to the living cell.

Dr. Gruner's enthusiasm is so encouraging and his powers of research are so great that we may expect better things of him. Let him shroud his Pappenheim and other authorities, and take to common-sense experiments in a laboratory. When he has worked out his subject according to his own thoughts and originality, giving due regard to the differences between life and death and between theory and fact, it is to be hoped that he will write another book in his charming style.

The book is elegantly bound and printed, the plates and photomicrographs are beautifully reproduced ; the zoological names are not printed in italics, nor are the generic names spelt with a capital initial.

REVIEWS

MATHEMATICS

Constructive Text-book of Practical Mathematics. Vol. iii., Technical Geometry, by H. W. MARSH. [Pp. xiv + 244.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1914. Price 5*s.* 6*d.* net.)

IT would be easy to misjudge this book. If indeed it is regarded as a book, it would be impossible to judge it leniently: it is only by considering it as a text subsidiary to the course of instruction at the Pratt Institute that one can give any estimate of it. There is no difficulty in discerning the author as a man who has worked out with great persistence, and perhaps ingenuity, a system of his own; but the printed matter which he places before us conveys to the reader little more than a grotesque travesty of his earnest and devoted labours with the 2,000 students who, he tells us, have passed through his hands. The author's system of teaching is heuristic, and this system has always failed, apart from strong personality in the teacher, to achieve results of value in geometry. At the best there are but few geniuses in the world at any one time who can, as Pascal is said to have done, discover the system of geometry for themselves; when the heuristic system is applied with students who are not Pascals, it degenerates into a system of suggestion in which the students attempt to supply answers which they think will please their instructor.

The book begins with a preface, which is perhaps its best part. Then follows a section in which the "work-book" is dealt with, a section which is of value for the student of the Pratt Institute, but worthless to the student of geometry. We have then shorthand contractions by which axioms are referred to—thus "Thru Pt 1 ||" denotes Playfair's axiom. Thereupon 120 definitions succeed, which introduce the ten books in which the subject is developed in skeleton form, or, as the work goes on, with enunciation only. Each book is prefaced by a contracted list of contents of the enunciations; some of these are intelligible, others are not. Here is one which some readers might like to guess:

Ea Leg M P Hyp and Adj Seq.

The plan of the author seems to be to get the student to make certain measurements of figures and then to form a guess at a theorem; when guessed successfully, the proof of the theorem is roughed out. The method of proceeding is unsound, as it subordinates reason to measurement. One feature of interest is that while all figures in plane geometry are omitted, representations in two dimensions are yet given of solid figures. There are good reasons for omitting figures altogether if the author is, as a French geometer puts it, convaincu que des notations convenablement choisies permettent de suivre une démonstration mieux qu'une figure qui détourne forcément l'attention; but enough has been given to show that this can hardly be the conviction of the author of *Technical Geometry*, nor does this apology for the absence of figures constitute a defence of the figures which exist.

It seems unnecessary to say more of a production which cannot be regarded as a serious contribution to the solution of the problems involved in the presentation of geometry. One quaint feature deserves notice—the author has a pleasant habit of quoting from the great writers. In this way many an ugly blank in his pages is filled by encouraging aphorisms from Bacon, Emerson, Aristotle, Dante, and Chaucer.

C.

Models to Illustrate the Foundations of Mathematics. By C. ELLIOTT. [Pp. viii + 116.] (Edinburgh: Lindsay & Co. Price 2*s*. 6*d*. net.)

THE title of this book hardly expresses its contents adequately. The book deals with the logical bases of mathematics, and "is intended to assist in bringing some modern views on the Foundations of Mathematics within the scope of school work." The models themselves are mentioned only accidentally, the body of the text consisting of an exposition of mathematics from the standpoint of the ideas of correspondence, classification, multiplexes, etc. Although it is not stated how far the author has been successful in familiarising children with the subject-matter of the book, the writer possesses such a firm grasp of his material and presents it with such clearness that we are quite willing to believe that pupils have enjoyed and benefited from such instruction. Evidence of success would not, however, convince us of the wisdom of such a course of instruction; a good teacher rises superior to the system which he adopts. It is, however, unwise to dogmatise as to the standpoint from which mathematics may in the future be expounded. Newton and Euler would doubtless be amazed at much they would find in schools to-day, and many a mathematician of the present time would rub his eyes very hard indeed if introduced into a class in which the writer of this book was teaching the foundations of his subject to a class of children. The one thing certain is that the minds of children in the seventeenth century were very much the same as they are in the twentieth, and Boole's dictum holds good for the twentieth as it will for the thirtieth that "a premature converse with abstraction is fatal to a virile growth of the intellect." It is the support of such an authority as Boole on such a subject as this that leads me to expect that the development of mathematical teaching will not follow the lines so lucidly laid down in this tract.

C.

Leçons de Mathématiques Générales. Par L. ZORETTI, avec une Préface de P. APPELL. [In-8 (23-14) de xvi + 753 pages, avec 205 figures. Cartonné.] (Paris: Gauthier-Villars, 1914. 20 fr.)

IT is always profitable to read the better class of French mathematical text-books, because the spirit in which they are written is so different from our own; and such books are particularly interesting at present when, to judge from M. Zoretti's preface, French mathematical teachers and writers are faced with the same perplexities and doubts as ours. The author modestly says that his book *ne veut être qu'un essai*; we should describe it as an attempt to cover the range of mathematics which should be included in a liberal modern education. The book differs in many respects from those brilliant *traités d'analyse* with which we are familiar, though it covers much of the ground usually occupied by such treatises: it is wider in extent, as it includes the analytical portions of mechanics, but it is more restricted, inasmuch as there is no attempt made to discuss number, limits, or continuity. In such a book the choice, as well as the arrangement of the matter, must have caused the author much thought: he tells us that he acted upon two principles, (1) that of including everything that is *utilisable*, even though it is used but once, and assembling about this nucleus all that was necessary or even desirable for the demonstration of the indispensable theorems; (2) that of rejecting systematically everything else. Unfortunately these two principles do not establish everlasting canons which will guide future writers. They constitute at best an involuntary confession of the impossibility of laying down a cast-iron programme. It is the function of authors to guide the footsteps of those who direct our studies; but the utilisable will never prove a trustworthy lodestar. As M. Appell tells us in an introductory preface, a solid mathematical basis is indispensable for every serious scientific study, theoretical or practical; and the value of this book is that it attempts to provide this basis for all, not being addressed to specialists.

The range of subjects covered by the book is necessarily wide: an English student finds here information that he would have to seek in a series of four or five text-books in his own language. The variety of the contents naturally suggests the question whether a student derives more good from isolated treatises or from the encyclopædic treatment. Every student will have his own answer to this question, but indirectly, by the sections into which he divides his book, the author seems to indicate his sympathy for the English method of treatment. These are: I. Geometry and Analytical Geometry; II. Algebra, Theory of Functions, Derivatives and Applications; III. Integral Calculus and Applications. In the first part, Geometry is mainly the geometry of vectors, and includes the composition of a system of localised vectors, while Analytical Geometry is almost restricted to the study of the forms of the equations of planes, lines, conics, and quadrics, and does not include tangents or tangent planes, which are reserved for Part II. In the third part we have almost an embarrassment of riches; for besides the usual applications, we find chapters on elliptic functions, Fourier series, and a short treatise on ordinary and partial differential equations. But it is perhaps in the second part that the author must have had his chief struggle to select the really utilisable. The following summary of topics selected in their order gives some notion of the contents—complex numbers, binomial theorem, determinants, the infinitely small, the infinitely great, series, functions, derivatives, variation of functions, construction of curves, expansion in series, applications of the differential calculus to curves, to surfaces, kinematics, solution of equations, numerical calculation and graphics. The impression left is something that reminds one of the collector into whose soul, side by side with the virtue of order, had crept the spirit of acquisitiveness; he could not bear to part with anything, and on his death left many well-packed drawers which were carefully labelled "useless." M. Zoretti's second part might have been more simply labelled "utilisable." We cannot help regretting that the general plan of the book has suffered from the desire to exhibit in it anything which could be of use, *ne serait-ce qu'une fois*. Every part of the book exhibits great skill in treatment, and in particular we would select the last chapter of the second part, which every teacher should read and study; it is real practical mathematics, the very best statement of this important branch of the subject.

The object of a mathematical treatise should be to treat mathematics from the mathematical standpoint, and to allow illustrations to group themselves in their natural place and in their fitting proportions, but above all the first object is to teach mathematics. It is idle to suppose that there is one mathematics for engineers and physicists and another for mathematicians. It is only by learning mathematics that any one, be he physicist or chemist, can hope to use mathematics as a tool. M. Zoretti is right in setting forth to give a treatise suitable for all; he is right also in not giving us a treatise over-elaborated with detail, and written with that repellent meticulousness which some mathematicians regard as essential to any statement of their subject. I think, however, that he would have produced a more readable, and therefore a better book, if he had grouped his chapters around certain central ideas and not attempted a comprehensiveness which has been acquired at the expense of that quality of luminous power in the exhibition of which his countrymen have so often set their English brethren such an excellent example.

C.

Projective Geometry. By G. B. MATHEWS, M.A., F.R.S. [Pp. xiv + 349, with diagrams.] (London: Longmans, Green & Co., 1914. Price 5*s*.)

THE publication of this book commemorates the centenary of the conception of the subject. For although Poncelet did not publish his *Propriétés Projectives* before 1822, he tells us that the ideas of the subject were conceived in his brain in a prison in Russia after the retreat from Moscow. In that most unlikely place he demonstrated the greatness of the human spirit by achieving an intellectual triumph more lasting in its results than the political success and failure of the millions who were engaged in that Russian campaign of 1813-4. In the hundred years which have succeeded, Projective Geometry has been elaborated by German, French, English, and Italian mathematicians, until it is now perhaps the most perfect monument in Pure Logic which has been raised by the genius of man.

It is one of the few omissions in Mr. Mathews' book that he does not briefly unfold the story of the development of Projective Geometry, placing before us the stages by which the subject was first freed from the metrical conditions under which Poncelet conceived it, and then describing the parts which von Staudt, Laguerre, Cayley, and others played in bringing within its sphere the wider realms which it has conquered in the last sixty years.

In the English language we have in Projective Geometry a translation of one volume out of three of Reye's *Geometrie der Lage*, the first volume of a brilliant book by Messrs. Veblen and Young, the second volume of which is awaited with eager expectancy, and two admirable tracts by Dr. Whitehead upon the axioms. But this is the first book in the language which gives a serious account of the real scope of Projective Geometry.

The book is not one of those which follow the syllabus of examinations; examinations will, if English mathematicians wake up, follow it. But whether they follow it or not, no student will in future have to be satisfied with the jumble of disconnected theorems and haphazard results which have hitherto masqueraded as Projective Geometry in English text-books.

Mr. Mathews has not started with the first verse of the first chapter of the first book of geometry as taught by the abstract logical school, and in this he is wise. He does not, however, shirk the genuine difficulties of the subject, as those who follow him will find when they arrive at Chap. VII. and tackle the Fundamental Theorem of Projectivity. In the elementary portions of the subject, which are covered by the first eighteen chapters, the author shows a wise discretion in avoiding the duplicity or quadriplicity of which the various theorems are susceptible. Students of the subject should, however, follow his precept and not his example, and state these theorems completely, as without much practice they will find it impossible to reach the true attitude of mind which, in geometry, is of more importance than knowledge of facts. After this Mr. Mathews offers us an account, original often and brilliant always, of von Staudt's theory of complex elements: then he discusses the theory of casts and establishes homogeneous co-ordinates upon a projective basis. From this point the author gives a little too much play to his analysis. It is always with regret that one sees the domains of geometry annexed into the spreading kingdom of algebra. Some readers would have been pleased if it had been possible to keep up the geometrical illusion a little longer. We are not complaining that geometrical methods are excluded, for this is not the case; rather we lament that the superior strength of analysis is brought so early into play.

In the last hundred pages of the book we have discussions of such important subjects as projectivities in space, quadric-surfaces, null-systems, and skew involutions. Line geometry has a chapter to itself, while projective problems are discussed in Chap. XXXII. and their solutions by geometry and algebra compared. Throughout the later chapters the analytical theory of linear transformations is continually kept before the reader's attention.

In conclusion Mr. Mathews is to be congratulated upon having written a book which cannot fail to have a wide and lasting influence upon the progress of geometrical knowledge in England. If the book receives the distinction of translation into foreign tongues, England will pay back a portion of the heavy debt which she owes to continental mathematicians who have done so much work in the field of Projective Geometry.

C.

Plane Geometry. Part I. By G. St. L. Carson, M.A., M.Sc., and D. E. Smith, Ph.D., LL.D. [Pp. iv + 266.] (Messrs. Ginn & Co. Price 2*s*. 6*d*.)

There are signs that teachers of geometry after a long period of wandering are returning to saner ways of regarding their subject. The book before us bears testimony to the new attitude. Its authors are mathematicians versed in the philosophy and history of the subject; they also bring to their task the ripe experience of a long connection with practical teaching. The first part of the geometry which lies before us treats of triangles and rectilinear figures, and is to be followed by a second part in which the other portions of the school course in geometry will be included; enough is given here to allow us to judge the scheme of the authors.

The book may be divided roughly into two portions—(1) the introduction, in which first notions in geometry are discussed, and (2) the formal portion, in which the subject is deduced from postulates. A very wise step has been taken in separating the two sections. It would have been perhaps still better to have carried out the principle more completely and to have excluded some of the large number of numerical examples given in the second part, which can add little to what has been effected so thoroughly in the earlier portion. In the introductory sections the authors give full play to their invention: here we find the map, the photograph, the pantograph, hand mirrors, gothic windows, cricket-balls, doll's gymnasia—in short, the delightful variety of the Christmas grotto. But throughout the authors show that they know their audience, particularly by choosing as the keynote of their introduction to geometry the problem of the hidden treasure. How can the utility of the subject be better demonstrated than by showing its intimate connection with a topic which occupies at some time or other the imagination of every boy and girl? I hope that many teachers will be encouraged by Carson and Smith's example to devote a period at least early in their course in geometry to reading and working out the Gold Bug, the greatest of all short stories of treasure-trove. I am confident that a term, or even a year, spent over the problems proposed in the introduction will redeem the subject from the charge of dullness. Even teachers of geometry have to remember Herbart's maxim—Seien sie niemals absolut langweilig !

The various ways in which the book differs from the classical treatment of Euclid are interesting. The first variation we note is the use of hypothetical constructions. This seems to be sanctioned by modern usage, but we confess that it is a shock to find a construction employed in the proof of Prop. iv. which has to wait for its demonstration until Prop. xxv. A second innovation consists of the statement of a largely increased number of postulates. Thus there are three postulates of perpendicular and three of parallels ; proofs are given in small type of five of these postulates. It is not easy to divine the gain in extending over these five the ægis of the postulate. The same result would probably have been attained by stating them as propositions and distinguishing their proof by asterisks or other device. Elementary geometry can be simplified by assuming the principle of symmetry ; this assumption is made tacitly by Euclid, and its open employment would get over some of the difficulties which modern authors surmount only by the use of hypothetical construction and new postulates. A third difference from the classical method is created by the abandonment of the *reductio ad absurdum* proof. It is doubtful whether this represents a gain ; this method of proof is of great value in analysis, and to the beginner it presents a welcome variety, however boring it may become to a teacher who hears it for the thousandth time. One outstanding feature of the book is the historical sketch with which the volume concludes : it is excellent in every way, and will lighten the labours of many a youthful traveller in his geometrical journeys.

The book is a valuable stage in the evolution of a sound system of instruction in geometry. The success of the authors has been achieved by their adherence to the spirit of Euclid, for, whatever variations may be introduced into geometry, it is Euclid who will probably always supply geometrical writers with the theme. It is books like the present one that will render it possible for schoolmasters to take their better senior students through the Elements at the close of their school course. In this way Euclid's masterpiece may return into school work in its proper place, and be studied as a classic deserves to be by minds which are able to appreciate its greatness.

C.

PHYSICS

A Textbook of Physics. By J. H. POYNTING, Sc.D., F.R.S., etc., and SIR J. J. THOMSON, O.M., M.A., F.R.S., etc. : Vol. iv. Electricity and Magnetism. Parts I. and II. Static Electricity and Magnetism. [Pp. xiv + 345, with Illustrations.] (London : Charles Griffin & Co., 1914. Price 10*s*. 6*d*.)

THE volume before us is, like its three predecessors, a model of good writing and clear exposition. No attempt is made to overload its pages with experimental details—a procedure which renders the reading of many "Handbooks" a weariness to the flesh—yet the account of experimental work is adequate to the purpose which the authors have in view, the establishment of fundamental principles, and the attainment by the student of a physical as well as a mathematical grasp of the main concepts. Considerable space is devoted to the development of the ideas of quantity of electrification and potential, apart from any particular law of force or any mathematical formulæ. The gain to the student in comprehension is enormous. When he is made familiar with an experimental arrangement by which one can, as it were, "ladle" multiples and submultiples of a definite charge into a body, he begins to feel almost as intimate with a unit of electricity as with a pint of water. If we had any criticism at all to urge on this part of the work, it is that, considering the analogy drawn between potential and level, the definition of potential given on page 74 of Chap. VI., a definition notorious as a stumbling block to the student, is not consonant with the spirit of this chapter, and might well be replaced by some such phrase as: "the potential is a scalar quantity whose gradient in any direction is equal and opposite to the field intensity in that direction."

One of the noteworthy features of this volume is the full treatment accorded to certain parts of the subject which hitherto have been ignored or but briefly mentioned in the majority of English text-books. We have here very adequate accounts of experimental methods which have been employed to measure the specific inductive capacities of different types of material, of the electrification produced by heating or straining certain crystals, of the measurements carried out in recent years by Curie, Wells, Townsend, and Pascal on the susceptibility of paramagnetic and diamagnetic bodies, and of Langevin's and Weiss's developments of Ampère's molecular hypothesis. A point of departure from usual methods of exposition consists in the early introduction of the student to the close connection between the phenomena of electricity and magnetism and those of light. There is also as complete a development of Maxwell's expressions for the stress in an electric field as can be obtained without the use of advanced analysis; and as an addition to it a chapter dealing with Quincke's and Kerr's work on the elastic strains in a dielectric accompanying an electrostatic field in it. Thus is the danger of the student confusing Maxwell's hypothetical "displacement" or "electric strain" with an elastic strain of the usual type considerably obviated. The volume is to be heartily recommended, not only for the mass of information contained in it, but also for the attitude which will be induced in the mind of the thoughtful student by a study of its pages.

J. R.

The Spectroscopy of the Extreme Ultra-Violet. Monographs on Physics. By Dr. T. LYMAN, Ph.D. [Pp. v+135.] (London: Longmans, Green & Co., 1914. Price 5*s*. net.)

THIS book deals with the investigations which have been carried out in the region of the spectrum extending beyond the wave-length 2,000 Ångström units, and called the "Schumann region," on account of the extensive contributions to our knowledge of it made by Victor Schumann. Observations of the first "octave" of the ultra-violet, 4,000 to 2,000 A.U., are possible with apparatus in which lenses, prisms, etc., are made of quartz or uviol glass, and from which it is not imperative that air should be excluded. Above this "octave," however, it becomes necessary to replace quartz by fluorite, especially the clear, colourless variety, and to mount the apparatus in a vacuum chamber. Recognising this fact, and being possessed of unrivalled powers of manipulation, Schumann constructed a vacuum spectograph with which he was able to reach a limit estimated to be about 1,000 A.U. Owing, however, to insufficient data concerning the dispersive power of fluorite, accurate measurements of wave-lengths could not be made. At Harvard University, Dr. Lyman has carried out a series of researches with a vacuum spectrograph, in which the prism has been replaced by a concave grating of radius one metre approximately, with lines ruled on speculum metal, about 15,000 to the inch. By this modification, absorption of the light is much reduced, accurate measurement of wave-lengths becomes possible, and manipulation rendered less difficult. In fact the author has reached the line 905 A.U., thus entering well within the third "octave" of the ultra-violet spectrum.

After a brief account of earlier work and some reference to the life and researches of Schumann, Dr. Lyman devotes a chapter to the details of his own spectrograph, its construction and manipulation, and also to the preparation of suitable photographic plates with an emulsion which must be almost gelatine-free, as gelatine has a considerable absorbing power in the region considered. There follow chapters on the absorption and emission by several gases and solids of light of this extremely high quality.

The author is not hopeless concerning still further extension of the limit reached toward higher frequences, and of meeting, as it were, the school of investigators who are seeking to produce X-rays of longer and longer wave-length. The main difficulty seems to be connected with the use of the "windowless" discharge tube, from which the discharge tends to spread into the receiver of the spectograph and so fog the plate. Reduced sensitivity of the Schumann plates, and decrease of reflecting power in the speculum grating, may also be responsible for the failure of very high quality radiation to impress itself on the sensitive surface, while small traces of impurity in the low-pressure hydrogen in which the discharge takes place may exercise considerable absorption.

Dr. Lyman draws attention to the importance of research in this region, in connection with the photo-electric effect and Einstein's explanation of it in terms of the "quantum" hypothesis. He refers to the strong bacteriacidal action of such light, and points to the conclusion that interplanetary space is germ free, if sun-spots are accompanied by jets of burning hydrogen projecting through the solar envelope. Hydrogen, in fact, seems to emit light of this type in great abundance; and among the useful tables of emission spectra at the end of the book, the longest is that concerning this gas, containing, as it does, some 400 lines between 1,675 and 1,228 A.U.

This little work is not only a valuable record of research for the specialist in this branch of physics, but is also to be recommended to the general reader seeking to keep in touch with the progress of physical science at all points.

J. R.

The Theory of Heat Radiation. By DR. MAX PLANCK; translated by M. MASIUS, M.A., Ph.D., from the second German edition. [Pp. xiv + 225, with 7 illustrations.] (Philadelphia: P. Blakiston's Son & Co.)

THE present volume is a much-needed translation of a work which has already had a deep influence on the development of physical theories. Many English physicists, not too intimate with the German language, will welcome a translation of a really important book as an aid to the comprehension of a new and somewhat difficult line of reasoning.

But apart from its exposition of the now famous "Quantum" theory of the author, the book is valuable for the careful introduction to radiation phenomena, and to the well-established laws of Kirchoff, Stefan, and Wien, given in the earlier chapters. Physics manuals are frequently compelled by exigencies of space to be brief, even on points where the most careful elucidation is vital to a correct understanding on the part of the beginner; for that reason might this work be put in the hands of any intelligent student, if only to read the earlier pages which do not deal with the author's special hypothesis.

But of course it is the author's development of the "Quantum of Action" theory which constitutes the most interesting feature of the volume. That theory, although imperfectly developed at present, has nevertheless provided a formula for the density of radiation under given conditions which has been applied with signal success to measurements on specific heats and the photo-electric effect, and to the calculation of the natural unit of electricity. That a physical "model" of the theory is still wanting, that on questions as to the "atomicity" or "continuity" of energy there should be diverse opinions among the experts, is only to be expected in view of the recent introduction of the hypothesis and the far-reaching consequences of its adoption. Not the least of these consequences will be the abandonment (except as a good working summary of average effects) of the laws of mechanics and electrodynamics as hitherto laid down. At bottom Planck's views constitute the first attempt, promising success, which will take account in our fundamental equations of that irreversibility which is such a marked characteristic of all natural phenomena, and whose existence would be unsuspected by any one guided solely by our classical mechanics and electrodynamics.

The author is well aware of the present imperfections of his theory, and, in a conservative endeavour to mitigate somewhat the blow dealt at established views, he has, in this second edition of his book, receded somewhat from the position which he occupied in his earlier papers and in the 1906 edition of the present work. He now succeeds in obtaining his well-known radiation formula by assuming that the atomic and subatomic mechanisms which are responsible for exchange of energy by radiation, emit discontinuously or in "lumps," but absorb continuously; this is in contradistinction to his earlier views, which postulated both discontinuous absorption and emission. He still maintains that his hypothesis, whether in the earlier or more recent form, does not commit one to an assumption of an atomic structure of energy itself. The newer presentation does to some extent meet certain objections launched at the older, and the author believes it to be less "revolutionary." This may well be doubted; at all events it is more abstruse, and rests, even more than his first exposition, on a new view concerning general theorems in probability, which may prove vague and unsatisfying to people little acquainted with the modern methods of that most "slippery" of subjects.

The translation is well done, and the translator has added a useful bibliography of important papers and a demonstration of certain mathematical formulæ used in the text, but not likely to be familiar to the general reader.

J. R.

The Electron Theory of Matter. By O. W. RICHARDSON, M.A., D.Sc., F.R.S. [Pp. vi + 612.] (London: Cambridge University Press, 1914. Price 18*s*. net.)

THIS century has already produced considerable alterations in the centre of interest of many branches of science; in none more than in Electricity. Twenty year ago the "electron" was experimentally unknown; it had made its appearance as a postulate in certain papers of Lorentz dealing with the mathematical theory of dispersion, etc. The genius of Faraday and Maxwell had impressed on student and teacher alike the doctrine that in the medium alone were the essential phenomena to be found, the conductor being merely a more or less unimportant boundary to the medium. Atomic views of electricity could not flourish in such a mental atmosphere, despite the strong suggestion of atomicity conveyed by Faraday's own work on electrolysis.

With the immense change in the point of view occasioned by the researches of the past two decades, it was natural that the presentation of the subject to the student should suffer some alteration ; even the elementary text-book now presents in its first chaper the "electron theory" as a young and formidable rival to the senile and decaying "single-fluid" and "double-fluid" theories. For the serious student bent on an honours degree (to be followed perhaps by a period of research work) it has been a time of transition, a most uncomforable sort of time indeed for any one who seeks "dry land" on which to rest, without fear of being dislodged by an unexpected wave. The procedure with regard to him has generally been to allow him to study the subject for two or three years in the old, orthodox manner, and then to correct his views by placing in his hands certain recent books, which, assuming a fairly wide knowledge of the older material on the part of the student, proceed to give an account of theory and experimental work developed in recent years.

Professor Richardson's book is, however, not of this type. It is really an attempt, and an excellent one, to write an advanced text-book of electricity in which the electron is taken as a fundamental postulate from the outset. All our old familiar electrostatic friends, Coulomb, Gauss, Laplace, Poisson, are presented in the light of this new concept. Physical reality is given to all those terrible crevasses, "long-narrow" and "short-wide," in which we used to flounder painfully seeking for the distinction between magnetic "force" and magnetic "induction." A clear conception of the behaviour of a dielectric becomes possible. Indeed, before one quarter of the pages have been passed the author has succeeded in presenting all the older material fully and yet succinctly, in terms of the electron. Lorentz's equations for the electromagnetic field are next dealt with, and the greatest care taken to explain the exact physical meaning of the symbols involved in them.

It is impossible in a brief notice to deal adequately with the wide range covered in the remaining pages. A very complete account of electron motion, uniform and accelerated, is followed by a chapter on the Aether, and the difficulties raised by the absence of the second order effects of the earth's motion on the speed of light. This leads naturally to an exposition of the principle of Relativity, and some account of the new dynamics which is founded upon it. The electron theory of Magnetism, spectroscopic phenomena, Radiation, and the Planck Quantum hypothesis are fully dealt with. Two excellent chapters on electronic conduction bring the student well within range of the most recent research on this part of the subject. Indeed the "up-to-dateness" of the book is evidenced by the introduction of a final chapter on Gravitation, containing the recent speculations of Einstein on that subject and the supposed effect produced by a gravitational field of force on the paths of light-rays.

The book is based on a course of lectures delivered to graduate students by the author when lecturing at Princeton University. For any one with the necessary mathematical equipment (and how can one proceed very far nowadays in this subject without such equipment ?), this book will serve as an admirably compact and yet very complete account of the subject, leading him directly to many points where research, theoretical and experimental, is proceeding.

J. R.

(1) **On Acquired Radio-Activity.** By Sir Wm. Crookes, O.M., LL.D., D.Sc. (From the *Phil. Trans.*, A, vol. 214 [Pp. 433-445], with two plates.)

(2) **On the Spectrum of Elementary Silicon.** By Sir Wm. Crookes, O.M., etc. (A paper read before the Royal Society, June 25, 1914.) [Pp. 12.]

(1) In this paper Sir Wm. Crookes gives an account of attempts made for some years past to impart radio-activity to such subjects as diamond, garnet, ruby, quartz, gold, platinum, and various phosphorescent substances by prolonged bombardment in a high vacuum by cathode rays or by exposure to radium emanation.

After bombardment by cathode rays no activity could be recognised either by photographic or electrical means, even if the cathode stream had acted long enough to discolour the surface in the case of diamonds.

Exposure to radium emanation conferred temporary activity on all the substances which had been tried, due, apparently, to the condensation of the emanation on the surface ; but this transient activity could be completely removed by washing in dilute acids.

If, however, instead of exposing the substances to emanation by placing an open bottle containing radium bromide near them in a vacuum, one placed them in the bottle and covered them with the powdered radium bromide and left them so for several months, there were in the case of glass and diamond discolorations of the surface, and the objects acquired a permanent activity. This activity could be removed from glass, quartz, and other substances, except diamond, by boiling in dilute nitric acid, or by immersion in a mixture of fuming nitric acid and potassium chlorate. In the case of diamond the activity could not be so removed ; it has persisted practically unchanged in the case of one diamond for close on ten years. Only by grinding away the discoloured layers of the surface could the acquired radio-activity be removed.

It is not quite clear that the author attributes this activity really to the diamond. It might obviously be due to radium E and polonium getting so far below the surface of the diamond by process of recoil that the reagents used could not reach them. The writer of this notice is aware of experiments (the results of which will shortly be published) in which platinum foil exposed to thorium emanation in an electric field in the usual manner still retains permanently about 2 per cent. of its original activity even after prolonged and repeated heating in a furnace at 1,200° C.

(2) This paper gives an account of experiments carried out by Sir Wm. Crookes in the spectrum of silicon. He suggests that some discrepancies between the results of past observers have been due to impurities in the specimens of silicon used. After many failures to secure a sufficiently pure specimen he obtained three samples from the Niagara Falls Carborundum Company giving on analysis 99·56, 99·86, 99·98 per cent. of silicon, the impurities being titanium, iron, and aluminium. With these samples, and using a spectrograph designed by himself and previously described (*Roy. Soc. Proc.*, vol. 72, p. 295), he has carefully measured the lines of silicon between 3,807 and 2,124 A.U., being enabled in fact by the purity of his specimens to correct the lines given by other less pure samples, and to clear up doubtful points where impurity would interfere with certainty of identification. Where the plates used were insufficiently sensitive, eye observations and measurements could be made by a special piece of apparatus which could be fitted to the spectrograph, being in fact a kind of pantagraph enlarger.

The results are tabulated, the measurements of six previous workers being printed in parallel columns for comparison.

J. R.

CHEMISTRY

A Manual of Practical Physical Chemistry. By FRANCIS W. GRAY, M.A., D.Sc. [Pp. xvi + 211.] (London: Macmillan & Co. Price 4*s*. 6*d*. net.)

THIS little book is a good example of how difficulties may be surmounted. The difficulties lie in the fact that in some laboratories the time allotted to physical chemistry in a student's course is either too short or too broken to be of much educative value to him.

In the present work allowance is made for the available time being broken; and the experiments, numbering nearly forty, are designed to occupy each only from one to a few hours.

Of course, such a method is bad, as doubtless the author would be one of the first to allow; but it cannot very well be helped under present conditions, under which the student comes to be fed with knowledge, like a goose being fattened, with the sole object of reaching that lamentable pathological condition, "the degree standard." And any one who tries, as the present author does, to make the best of this bad job, earns the thanks of his fellow-sufferers.

The Theory of the Solid State. By PROF. WALTHER NERNST. Based on four lectures delivered at University College, London, in March 1913. [Pp. viii + 104.] (London: University of London Press. Published by Hodder & Stoughton, 1914. Price 2*s*. 6*d*. net.)

THOUGH small in size, this book is one of the most important physico-chemical publications of the year. The problem of the solid state is viewed from the standpoint of thermo-dynamics and the new statistical mechanics, a mode of treatment which has been pursued with extraordinary vigour and success during the last five or six years. Considering the large and varied amount of work expended upon the problem, the variety and complexity of the phenomena and the profound nature of the subject in itself, it is indeed no small tribute to the author's well-known power of exposition to find the whole thing so clearly epitomised in a little over one hundred pages. Not only does the reader obtain a reasonably comprehensive and proportioned view of the whole, but there is at the same time the atmosphere of authoritativeness which always manifests itself when the writer has been himself one of the pioneers in the investigation of the subject which he expounds.

The book is a reproduction of the four lectures delivered by Prof. Nernst at University College, London, in 1913, which awakened very considerable interest at the time; and it is with great pleasure that we welcome the present somewhat belated publication. It must not be thought that we are here given a review of the quantum theory in its entirety, although the quantum theory and the Nernst Heat theorem form the groundwork of the whole treatment. Prof. Nernst has kept closely to the particular subject in hand, and has thus been able within small compass to deal with certain parts of the subject in considerable detail. A notable example is the very excellent account given of the experimental methods worked out in the Berlin laboratory for the determination of specific heats over short temperature ranges down to extremely low temperatures.

For many years the solid state of matter and the properties of solids had proved a rather unproductive field. The early discovered law of Dulong and Petit remained a purely empirical one until Boltzmann showed its significance from the standpoint of the classical statistical mechanics. But this was only a partial success, for the "law" was, after all, not generally valid. It was not until Einstein took the bold step of discarding the principle of equipartition of energy (as Planck had already done in the case of radiation) and applied Planck's quantum theory to the energy content of solids that the property of specific heat became one of the first importance for the theory of the solid state. It is now well established, both experimentally and theoretically, that the "law" of Dulong and Petit is not really a law at all in the sense that the "constant" involved in it is a function of the temperature. Einstein's work was, however, itself incomplete, and Nernst gives a very clear account of the extension of Einstein's theory undertaken by Lindemann and himself and later the more important contribution made by Debye.

Perhaps the most suggestive part of the book is to be found in the three concluding sections as pointing the way to further investigation of many widely varying phenomena, such as the molecular weight of solid compounds, the behaviour of substances at very low temperatures in respect of specific heat, thermal expansion, compressibility, conduction of heat and electricity, magnetic susceptibility, thermo e.m.f. and the Thomson effect. The relation of the quantum theory to the electron theory is indeed one of the most vital of all, and already since the delivery of these lectures a very considerable amount of work has been done in this direction (cf. Lindemann, *Phil. Mag.* January 1915). It is a remarkable fact that in spite of the amazing rapidity with which the quantum theory has entered into the fundamental problems of physics and chemistry we are still absolutely in the dark as regards a clear physical conception of the quantum itself. This very fact, however, should prove the great incentive for further research.

W. C. McC. LEWIS.

A Textbook of Inorganic Chemistry. In nine volumes. Edited by J. NEWTON FRIEND, D.Sc., Ph.D. Volume I. Part I.: An Introduction to Modern Inorganic Chemistry, by J. NEWTON FRIEND, H. F. V. LITTLE, and W. E. S. TURNER; Part II.: The Inert Gases, by H. V. A. BRISCOE. [Pp xv + 385, with frontispiece, plate and 88 other illustrations. Crown 8vo] (London: Charles Griffin & Co., Ltd. Price of Vol. I. 10*s*. 6*d*. net.)

THE volume before us is the first of a series of nine, which is intended evidently to form the standard work in English upon inorganic chemistry. It is, of course, not the first which deserves the title, but it so very clearly marks the fundamental change which has been made in the last twenty years in regard to method of treatment and point of view, that its appearance is significant. This is the conclusion come to on inspection of the first volume; it remains to be seen, of course, whether the later volumes will pursue the same ideal. It is rather a remarkable fact that British scientists have never taken at all kindly to the preparation of treatises on the grand scale. Such work seems in the past to have appealed more particularly to the Teutonic mind, though the utility of a considerable amount of such work has been rendered of somewhat doubtful value through the absence of the one essential factor, the exercise of the critical faculty. A merely massive compendium which includes, or strives to include, everything that has been published on a particular subject, is rather a burden than an assistance to the development of a science. The present volume is fairly complete as regards facts, and a particular point has been made in the matter of literature references. Above all, it does appear to have been written with a real appreciation of proportion and critical selection. It must have been obvious to the compilers that their chief competitor would be Abegg's *Handbuch*, and indeed, in all fairness, as it seems to the reviewer, the general scheme of the present work has been influenced—and rightly influenced—to a certain extent by that admirable publication. But Abegg's book has not yet been completed, and parts of it already require revision. The present work, if brought out promptly, should take the premier place as far as English-speaking people are concerned.

The general plan of the present textbook is the fairly obvious one of division according to the groups of the Periodic Table. Vol. I. deals with Group O, the inert gases, and likewise includes in its first part an introduction of some 300 pages dealing with the physico-chemical aspect of inorganic chemistry in general. This introduction has on the whole been well written. Chapters I. and II. deal with stoichiometrical relationships and general properties. Chapter III. is devoted to molecular weights and their determination for vapours, dissolved substances, and pure liquids. In the writer's opinion the results obtained from drop-pipette measurements, in view of the work of Kohlrausch and Lohnstein, scarcely deserve even the short account given, and much the same may be said of Walden's capillary relationships. Chapter V., on chemical change, is a useful epitome of such subjects as types of reaction, thermo-chemistry, chemical affinity, equilibrium and chemical kinetics—but why, oh why, is the term "heat-tone" (Wärmetönung) employed here and elsewhere; it even finds its way into the index! Part of Chap. VI. is devoted to a short account of the electrolytic dissociation theory which is in every way admirable.

In dealing with complex salts in solution one misses a reference to Jacques' book, but probably this appeared too late to mention. One very good feature is the account of an accurate atomic weight determination (in Chap. VII.), illustrated by the work of Richards on Lithium and Guye on Nitrogen. In the section on specific heat we have a brief account of Einstein's application of Planck's quantum theory to the problem, which serves to indicate the general up-to-dateness of the work as a whole, though possibly a good deal more might have been said on this particular point. For example, a reference might have been made to Boltzmann's deduction of the Dulong-Petit law, as it was this work which, by its very incompleteness, paved the way for the application of the quantum idea to the problem. Chap. VIII. gives a good account of the principle of the periodic table, its imperfections as well as advantages. In this connection mention might well have been made of the work of Biltz, who has shown that the periodicity in properties can be related (approximately at least) to the periodicity of a more fundamental quantity, namely, the characteristic vibration frequency of the atoms upon which "properties" to a large extent depend.

The most serious omission in the whole of the introduction is the absence of any discussion of electromotive force, and its bearing upon the quantitative measurement of chemical affinity. As the other volumes appear we shall certainly expect some information upon the electrometric properties of the various metals, decomposition potentials, affinity and its variation with temperature, pressure, and concentration, oxidation and reduction processes, molecular weight determinations by means of e.m.f., hydrolysis and electrolytic dissociation by the same means. There is only a passing reference to electromotive force as a means of determining transition points, and since it was considered necessary to have an introduction at all, one is at a loss to know why this very important mode of investigation has been omitted from the discussion. It would have been a good thing too if the introduction had paved the way for the application of such thermodynamical principles as Nernst's heat theorem, which we shall expect to see applied later on in dealing with the affinity of the various elements for one another, *e.g.* silver and iodine.

With Part II. we pass to the subject proper of descriptive inorganic chemistry. The portion dealt with here includes the group of inert gases, helium, neon, argon, krypton, xenon, and niton. There is little room for comment except to say that Mr. Briscoe has done his work well. Perhaps something more than a brief reference to Onnes' isothermals would have been an advantage especially from the point of view of continuity of state, for it is evident that the inert gases by their molecular simplicity afford the most trustworthy experimental data for investigating and testing equations of state. It is easy, however, to offer suggestions of this kind. It is quite a different matter to really keep the balance between what ought to be included and what must be excluded, and Part II. is exceptionally good from this standpoint.

W. C. McC. Lewis.

GEOLOGY

The Deposits of the Useful Minerals, Their Origin, Form, and Content. By Professors F. Beyschlag, J. H. L. Vogt, and P. Krusch. Translated by S. J. Truscott. In three volumes. Vol. I. Ore-Deposits in General—Magmatic Segregations—Contact Deposits—Tin Lodes—Quicksilver Lodes. [Pp. xxviii + 514 with 291 Illustrations.] (London: Macmillan & Co. Price 18*s*. net.)

This great work, which Mr. Truscott has now translated into English, first appeared in 1909, but the third volume has not yet been published. It will rank as the finest presentation yet made of the science of ore-deposits from the Continental standpoint. Our knowledge of ore-deposits is now based upon a secure scientific foundation, and is illuminated by a series of great text-books such as this. In its English dress the book, of course, challenges comparison with the most recent production of the American school, Lindgren's *Ore-Deposits*. The latter is a smaller book than that of Beyschlag-Vogt-Krusch; but its treatment of the philosophy of ore-deposits is on a larger scale, and is perhaps of a more illuminating character than that of the Continental work. The American work excels in the thorough application of physico-chemical principles to the investigation of ore-deposits. The book under review is supreme in the detail and acumen with which individual occurrences are discussed, and in the thoroughness with which the economic application of the material to prospecting and mining is kept in mind. European occurrences of ore-deposits are naturally treated in great detail, and in regard to the theoretical interest and importance of many of these little-known bodies the book is of great value in bringing them to the notice of English-speaking investigators. This point has a topical interest just now in view of the question whether the central European countries can obtain from their own areas sufficient of the metals they need for warfare, since the overseas supply has been completely cut off.

We may say that, in general, the translator has done his work excellently. The style is formal and dignified, but owing to an exceedingly close rendering of the original German there are frequently long, laboured, and heavy sentences, in which the translator has not sufficiently divested his text of the German idiom. This tendency is aggravated in some places by the very sparing use of commas (*e.g.* pp. 171, 196). The translator's preface is occupied with a discussion of the exact English equivalents of certain German terms used in the description of ore-deposits. *Gang*, for example, may be translated either as "lode" or "vein." The translator falls in with British and Colonial practice rather than American in choosing "lode" as the equivalent. He points out that the German *Ader* more nearly expresses what we mean by vein—that is, an occurrence of lesser magnitude than a lode. *Seifen* is translated "gravel-deposits" in preference to "placers." *Sahlband* is translated as "gouge," and *Gangtonschiefer* as "flucan." With this choice of English equivalents we are in general agreement.

The translator throughout betrays some unfamiliarity with petrological terms. He uses the term "sodium" where "soda" is meant in many compounds such as "soda-augite-syenite." No doubt the huge compound petrological terms on p. 250 and elsewhere are taken from the original German. Mistakes and misprints are commendably few. The atomic weight of platinum is given as ranging between 14 and 19 (p. 83) instead of the specific gravity as obviously meant. Aluminium is referred to as a mineral on p. 152, whilst on the same page there appears to be a misstatement as to the amount of thorium in the rare earths. The term "ilmenitite" has frequently been misspelt (pp. 252–3), as also has "pyrrhotite" (p. 301), and "limonite" (p. 218).

This volume contains a general discussion of ore-deposits, which occupies the first half of the book. The second half contains the detailed descriptions of the ore-deposits due to magmatic segregation, contact-metamorphism, and the tin and quicksilver groups of the ore-bodies classified under lodes, irregular cavity fillings, and metasomatic deposits. Early in the book, in connection with form and graphic representation, the ore-deposits are classified as *syngenetic*, originating with the country-rock, and *epigenetic*, arising from later processes; but these terms are abandoned in the more extended discussion of classification that occurs later on. The close connection between ore-deposits and igneous rocks is accepted as axiomatic, and the authors finally adopt a classification with four main groups on a genetic basis, which also brings into prominence the degree of closeness of the connection of the deposits with their original igneous source. The four groups are: (1) Magmatic Segregations, due to concentration of metalliferous minerals in a molten magma, which represent ore-deposits nearest to their original source; (2) Contact-deposits, formed by endomorphic and exomorphic contact-metamorphism at the margins of intrusive igneous masses; (3) Fissure cavity-fillings, irregular cavity-fillings, and metasomatic deposits, which represent different phases of the activity of aqueous solutions emanating at some distance in time and space from an igneous intrusion; and (4) Ore-beds, sediments more particularly, but including also some doubtful bedded deposits. In these the ore material is remote from its original source, and generally has no direct connection at all with igneous rocks. This grouping does not possess the sharp physico-chemical definition of the classification developed by Lindgren, but it is doubtful whether the latter can be applied in many cases.

No treatise on ore-deposits, not even Lindgren's, has yet given a full account of the valuable microscopic methods of investigation of ore-deposits. In this book there is no adequate treatment of this subject. The microscopic criteria which give the order of succession of the minerals in igneous rocks are frequently not applicable to ore-deposits. In the latter the complete inclusion of one mineral within another is not evidence for the priority of the included mineral, as is the case in igneous rocks, since it may have grown by metasomatic processes within its host. Many points similar to this require to be formulated in order to establish the microscopic study of ore-deposits on a more secure basis.

From the association of Vogt in the authorship of this work, it was to be expected that the ore-deposits due to magmatic segregation would be very fully treated. We can support the hypothesis of magmatic origin in respect to the iron-ore and sulphide deposits in connection with gabbros, but when huge masses of iron-ore are derived by magmatic differentiation from a granite we feel that the hypothesis is being unduly strained. The magmas from which such opposite poles as granite and iron-ore rock were derived must have had an unusual chemical composition before differentiation, so unusual that most petrologists would not be able to recognise them. In accordance with these extreme views on magmatic segregation even the great pyrite deposits, such as Rio Tinto, are assigned to a magmatic origin. The opinion of Prof. J. W. Gregory, supported by the recent work of Finlayson, that these deposits are much more likely due to metasomatic replacement along zones of faulting and shearing, would, however, be upheld by most mining geologists.

The illustrations are numerous and excellent. Mr. Truscott is to be congratulated on this translation which makes available to English-speaking students the latest and finest Continental work on ore-deposits. We shall look forward with pleasure to the continuation of his work.

G. W. T.

The Rare Earths: Their Occurrence, Chemistry, and Technology. By S. T LEVY, B.A., B.SC., A.I.C. [Pp. xiv + 346, with 11 illustrations.] (London: Edward Arnold, 1915. Price 10*s*. 6*d*. net.)

THIS work forms a valuable supplement to our standard text-books in chemistry and mineralogy, and at the same time is of technical importance as regards the trade in incandescent gas-mantles. The prospector will learn from it the variety of minerals on which his attention should be fixed, and tests are given, as in the case of monazite (p. 91), which will be of service to him in dealing with mixed materials in his camp. More emphasis might perhaps be laid on the high specific gravity of many of the valuable materials, a property which leads to their natural concentration. The chapter on monazite sands, however, calls attention to this and affords a good example of the author's appreciation of the practical side of his inquiries. He illustrates an electro-magnetic method of extracting monazite, the mineral becoming held down on a revolving belt if the field is sufficiently strong, and falling off into a receiver when carried beyond reach of the attraction. Good descriptions are given of the deposits that are worked commercially. Mineralogists will note the limited locality of Cerite; the double discovery of Baddeleyite, the oxide of zirconium, in 1892 in different quarters of the globe; and the fact (p. 2) that the rare earths generally were held to be restricted to Scandinavia and the Urals down to 1885, when mineralogical investigation became stimulated by technical demands. The historical introductions to each group of earths contain matter of much human interest, such as the capture of Giesecke's specimens, subsequently named Allanite, by an English privateer, and the use of pencils of compressed zirconia in 1867 for the illumination of the Hôtel de Ville in Paris.

The table on p. 113 shows how far we have travelled from the knowledge available in our boyhood. Didymium has disappeared or bifurcated into Praseodymium and Neodymium; the original term Ytterbia, chosen by Gadolin from the locality Ytterby, and shortened by Ekeberg into Yttria, now reappears, somewhat unfortunately, as the earth of an element with an atomic weight nearly twice that of Yttrium. This resuscitated name is, however, also threatened (p. 205), and Mr. Levy already speaks of "the old ytterbium," meaning Marignac's element, which was separated from erbium in 1878. Dysprosium dates only from 1906, and Celtium from 1911.

The question of isotopes naturally arises here and there. Thorium is thus chemically identical with ionium, the parent of radium (p. 253); the end product of disintegration in the thorium series may possibly be an isotope of lead (p. 107). Mr. Levy brings the inquiry as to whether this isotope or bismuth arises down to a paper by Soddy and Hyman in 1914; but he is naturally unable to include the work of Holmes and Lawson in December of that year. These authors conclude (*Phil. Mag.* vol. xxviii. p. 840) that uranium passes into a stable isotope of lead, while the end product of thorium is unstable, and does not accumulate in geological time.

The penultimate vowels of eudialyte and eucolite have got mixed on pp. 14 and 15; "eucolyte" appears on p. 50, where eudialyte is spelled correctly; but otherwise slips of any kind seem extremely rare in Mr. Levy's excellently printed manual. Lavenite (pp. 19 and 51) should be Låvenite or Lovenite. M. Urbain is responsible for the spelling of Lutecium (p 205), which has stood the test of seven years in this French but unclassical form. It is curious that two of the old ways of dealing with Lutetia introduced a "c," but not where modern Parisians place it.

In conclusion, we know of no book that so well covers the field selected by Mr. Levy.

GRENVILLE A. J. COLE.

PALÆONTOLOGY

An Introduction to the Study of Fossils (Plants and Animals). By HERVEY WOODBURN SHIMER, A.M., P.H.D. [Pp. xiv + 450, with 175 illustrations.] (London: Macmillan & Co., Ltd. Price 10*s.* net.)

IT is somewhat surprising to learn from the preface of this well-illustrated little volume that the students attending the author's palæontological lectures at the Massachusetts Institute of Technology too often regard fossils "merely as bits of stone, differing only in form from the rocks in which they are embedded," and that they also fail to realise that these same fossils contain in themselves evidence of the former existence of a more or less unbroken chain connecting the animals and plants of to-day with those of long past epochs of the earth's history. To remedy this state of affairs, and at the same time to awaken a real interest in palæontology, Prof. Shimer has come to the conclusion that the best method is thoroughly to explain the general structure of the chief groups of living plants and animals, and then to show the extent of our knowledge of the extinct representatives of each. The results of such a method of teaching are embodied in the present volume, which may be regarded as a botanical and zoological text-book supplemented by concise reviews of the past history of each group. In the relatively large amount of space devoted to living forms and the condensation of the palæontological aspect of the subject, the work appears, indeed, to be unique; and the only question is whether this plan has not been carried to excess.

Be this as it may, the general treatment of the subject is excellent, albeit from an American point of view, which may render the work less acceptable to English students than would otherwise be the case, since many of the geological formations and horizons are referred to by unfamiliar local names without any indication of their European equivalents.

The volume commences with the lowest plants and ends with the highest animals; and it is satisfactory to note that in referring to the Piltdown fossil man (*Eoanthropus dawsoni*) in his concluding pages the author is thoroughly up to date. In a few instances, however, this can scarcely be said to be the case. On p. 378, for example, we are told that there are only two existing genera of egg-laying mammals (Monotremata); and on the same page the author follows Dr. Gidley in associating the extinct Multituberculata with the Marsupialia, thereby ignoring the views of Dr. Broom, who considers that their relationships are clearly with the Monotremata. Then, again, it is distinctly startling to be informed (p. 399) that no fossil dolphins are known; while the statement that the mammal-like anomodont reptiles of the Permo-Trias are mainly restricted to New Mexico and South Africa (p. 356) is decidedly misleading, in view of their abundance in Russia. As a minor matter, the statement (p. 345) that there is only one species of the African ganoid fishes of the genus *Polypterus* is scarcely accurate. It may be added that in recognising only nine orders of reptiles the author differs widely from the views of his compatriot, Prof. Williston, who, in his recently published *Water Reptiles*, admits no fewer than fifteen. None of these, however, is anything more than one of those slips which are, unhappily, bound to occur in works of this comprehensive nature; and it may be confidently affirmed that Prof. Shimer's volume is in every way entitled to a place among the best class of elementary biological text-books, more especially as the mode of treatment of the subject serves to emphasise the too often forgotten fact that palæontology is nothing more than the botany and zoology of past epochs, and not a science by itself.

R. L.

ZOOLOGY

Textbook of Embryology. Edited by WALTER HEAPE, M.A., F.R.S. Vol. i. **Invertebrata.** By E. W. MACBRIDE, M.A., D.SC., LL.D., F.R.S. [Pp. xxxii + 692, with 468 Illustrations.] (London: Macmillan & Co., Ltd. Price 25*s*. net.)

IT is a difficult task for one man to compress within the limits of a single volume a subject of so wide a scope as the embryology of the invertebrates, more especially when this includes not only the Invertebrata but also the Protochordata, those borderline forms usually included with the Vertebrata in the term Chordata. Prof. MacBride has accomplished this task successfully, and produced a book that will be welcomed for its utility. The subject matter of such a work might have been presented in one of two ways—either in an encyclopædic manner, in which as many statements as possible are collected under the one heading, or under the type system, in which the general principles are propounded and illustrated by reference to one or a few types. The author has, and we think wisely, adopted the latter plan, and has been guided in the choice of his types by those that have been fairly satisfactorily or recently worked out and that may be obtained in temperate latitudes. This treatment appeals particularly to the student, and one who works through this book will get a thorough and sound knowledge of his subject.

Perhaps because it is hardly possible for one person to be equally versed in all the invertebrate groups, we find that their treatment varies somewhat, and the best chapters are those at the end dealing with the Echinodermata and Protochordata, in which Prof. MacBride's own researches are already widely known.

While in entire agreement with the author's desire to keep the literature lists as short and up-to-date as possible, it would appear that brevity in some cases has been secured at the cost of excluding good recent work. In the pages dealing with the Hydrozoa no reference has been made to Kühn's papers on budding (1909) and the formation of gonophores (1910), which are more recent than those of Götte, nor to his last paper on the development of the Hydrozoa (1913), which is of great value because of the very full bibliography it contains. The chapter on the Platyhelminthes might well have included a reference to Bresslau's papers on the development of Rhabdocœla and Alloiocœla (1904) and the Acœla (1909). Mention might have been made of Herber's paper on *Anodonta cellensis* (1913) in the chapter on molluscs. It is also to be regretted that there was not time to include even in footnotes the work of Schleip on two species of *Clepsine* and von Wenck on *Macrobiotus*, both of which appeared in the first half of 1914. The all too short account of the Tardigrada is based on Erlanger's work on *Macrobiotus macronyx* in 1895. This investigation was mainly carried out on whole mounts, and the illustrations are very diagramatic. *M. lacustris* has been studied by von Wenck, who mainly relied on sections prepared with an improved technique, and who consequently has been able to give a much more satisfactory description of the early stages.

The introductory chapter is very concise, but perhaps some account might have been given of the apparent organisation of the ovum in some cases even before fertilisation and of the rôle played by its cytoplasm in determining the fate of the cells derived from its different parts. This is a subject that has received much attention of late years, especially from a number of American biologists. Another point, connected with this, concerns the origin of the germ cells, and here we think it would have been advisable to indicate that in certain cases these cells can be recognised at a very early period in segmentation before definite germ layers have been established. This might have been done in the first page of the introduction, where two instances are cited to show the errors that may arise from an examination of an incomplete series of developmental stages. In each case the germ cells were first stated to arise from the ectoderm, whereas they are now said to be derived from the cœlomic epithelium. One cannot help recalling that even in four vertebrates it has been shown that the germ cells, although long stated to be derived from the cœlomic epithelium, are in reality migrants into this layer, and may be recognised before they take up their definite position. The remainder of the chapter is well worth reading, and Driesch's attitude towards the law of biogenetics is effectively criticised.

Like the introduction, the summary also repays reading, although we fear that the Hormone theory, enticing as it appears, has been too readily accepted and strained beyond its legitimate bounds.

In spite of the small criticisms that have been put forward above, mostly matters of opinion, the book on the whole reaches and maintains a very high level, and the need for such a work has long been felt by the student of biology in this country. It has the outstanding merit of being concise, easily followed, and readable throughout. Much interest is added to it by the incorporation of the principal results of experimental work; these are not only important in themselves, but shed light on many morphological questions. The printing and illustrations are of a very high quality. Indeed, Prof. MacBride is to be congratulated on producing a book that will be indispensable to the senior student and any one else who wishes to become familiar with the facts and teachings of invertebrate embryology.

C. H. O'D.

The Germ-cell Cycle in Animals. By ROBERT W. HEGNER, Ph.D. [Pp. x + 346, with 84 illustrations.] (New York: The Macmillan Co., 1914. Price 7*s.* 6*d.* net.)

THE title of this book perhaps does not indicate quite clearly its scope. Germ-cell cycle is a term employed by the author "to indicate all those phenomena concerned with the origin and history of the germ cells from one generation to the next generation," and not merely those concerned with maturation. It is, in other words, a study of the continuity of the germ-plasm. The suggestion of such a continuity we owe to Jäger, but it was first brought into prominence by Weismann, and it is his name that is usually associated with the idea. Broadly stated, the theory maintains that the developing ovum gives rise to two series of descendants, one of which forms the soma, or body, and the other the germ cells of the succeeding generation, and that these two lots of cells are separated one from the other from the beginning. The ideal condition would be realised if at the two cells resulting from the first segmentation division should be the mother cell of the body cells and the mother cell of the germ cells. Such a condition, whether realised or not, has never yet been observed. The nearest approach to it is found in the fly *Miastor*, where the first definite cell to be segmented off is the primordial germ cell; but the rest of the egg, instead of being a single cell, is a syncytium with eight nuclei. Thus, although absolute morphological continuity has not yet been shown, a near approach has been made both in this fly and again in *Ascaris*, where in the 32-cell stage it is possible to pick out one cell that gives rise to all and nothing but the germ cells.

In these and other animals, a modified part of the cytoplasm or a peculiar differentiation in the chromatin either within or without the nucleus is observable at an early period, sometimes before fertilisation, and this differentiated part is passed to the germ cells. Thus it is possible in a number of cases to follow the line of descent of the primordial germ cell from the ovum, and such a line is often termed the Keimbahn. The structural differentiations which seem to settle this line are spoken of as the Keimbahn determinants, and both the line and determinants are fully dealt with in this volume.

The earlier workers on the germ cells, and particularly those who were interested in the cytological phenomena underlying heredity, laid practically the entire stress on the nuclei, and in particular upon the chromosomes. It is undoubtedly true that these structures play a large part in heredity, *e.g.* the X and Y chromosomes in the determination of sex, but the evidence brought together by Dr. Hegner shows quite clearly that the cytoplasm of the ovum is also extremely important. Indeed, the behaviour of certain inclusions in the cell, which have received a multitude of names but are perhaps most generally included in the term mitochondria, indicates that they influence strongly the future of the cell in which they occur. As the author points out, the belief is gaining ground that the phenomena of heredity are due to the interaction of nucleus and cytoplasm, and not, as was formerly held, entirely to the activities of the former.

In the last decade more and more attention has been paid to the history of the germ cells during the early segmentation stages, and in consequence a large literature has grown up. That this work has not been unproductive is clearly shown by the present book, which aims at setting forth the most recent advances in this field of research. The accounts are well and concisely written by one who has himself been an active investigator of such problems, and the illustrations are throughout good. The long literature list and indices add much value to a very useful book.

C. H. O'D.

a. **A Textbook of General Embryology.** By PROF. WILLIAM E. KELLICOTT. [Pp. v + 376, with 168 illustrations.] (London: Constable & Co., 1914. Price 10*s.* 6*d.* net.)

b. **Outlines of Chordate Development.** By PROF. WILLIAM E. KELLICOTT. [Pp. v + 471, with 185 illustrations.] (London: Constable & Co., 1914. Price 10*s.* 6*d.* net.)

ALL students of biology must have felt the want of a textbook of embryology wherein they could find the facts and theories of this most important branch of their study clearly set out in an elementary manner. Perhaps in no other part of biology is the need of a reliable work more great, and this in a field that has progressed markedly in recent years and which lends itself admirably to a discussion of many important theories. As a rule embryology is relegated to the end of a general textbook, and then treated but indifferently with scant regard for modern work. Here are two volumes that go far towards remedying this state of affairs, and in both the requirements of the student appear to have been constantly borne in mind.

a. The title of the first is misleading, for it is rather an introduction to embryology than a general embryology. About two-thirds of the book is concerned with the accounts of cells, cell division, maturation, and fertilisation, and the remaining part to cleavage, the processes of differentiation, heredity, and sex determination, the blastula, gastrula, and the formation of the germ layers. Embryology is generally preceded by, or, at any rate, accompanied by, studies in general biology, and much of the ground covered in the first part of the book is dealt with in the ordinary textbook, and might, we think, have been considerably shortened with advantage. It deals largely with problems from a cytological point of view. The room thus gained could have been profitably devoted to histogenesis, organogeny, fœtal membranes, cœlom formation, and many other important embryological problems. It is just these last questions that are not usually well treated in most elementary books. This, however, is but a criticism of the scope and title of the present volume.

The various chapters are easy to follow and well illustrated, and the serious student who wishes to follow up any point in greater detail will derive much help from the short bibliography given at the end of each of them. The tables here and there throughout the book are useful.

b. This book in part fills up the gap noted in the preceding, and gives an account of the development of Amphioxus, the frog, the chick, and the early development of a mammal, including also a brief account of the germ cells and fertilisation. It is a valuable addition to the elementary textbooks, and is of use to the student of biology and to the student of medicine as an introduction to the more detailed study of human embryology. Again, the author is to be congratulated on the clearness of his style and the aptness of the illustrations. Prof. Kellicott is in no way to blame for the extraordinary muddle in the volume sent for review. No less than eight pages (pp. 122, 123, 126, 127, 134, 135, 130, and 131) are reintroduced at various points, to the exclusion of eight pages of reading matter, with the result that about twenty-six pages of the book are quite useless. It is hard to see how such carelessness could be confined to one copy, and it is to be hoped that the publishers will take steps to see that similarly marred copies are withdrawn from circulation.

The two volumes read together form a sound introduction to the cytological aspects of the earliest stages of embryology and the latter stages in the chordate types mentioned.

C. H. O'D.

Water Reptiles of the Past and Present. By PROF. S. W. WILLISTON. [Pp. vi + 251, with 131 illustrations.] (Chicago: The Cambridge University Press, 1914. Price 12*s*. net.)

THE first vertebrates were undoubtedly marine forms perhaps allied to the primitive cartilagenous fish. After the great step forward had been taken and the Amphibia and Reptiles had become land dwellers with habits and structure adapted to their environment, some of them returned to the water either fresh or salt and consequently had once again to become adapted to changed surroundings. The same return to the water has also taken place in certain of the mammals, and the whales and porpoises of to-day are undoubtedly the highly modified descendants of land-dwelling mammals. Prof. Williston furnishes a very instructive and interesting account of the various branches of the class Reptilia both recent and extinct that have come to live in the water either partially or completely.

It would perhaps be natural to suppose that the necessary structural adjustments could be made by the re-assumption of some of the features of their remote marine fish-like ancestors. Indeed a glance at the spirited restoration of an Ichthyosaur on p. 108 will suffice to show how readily one could accept such an explanation unless the details of its structure were more closely studied. Their discoverer Scheuzer held them to be fish, and the name Ichthyosaur was later given to the genus to indicate that they were intermediate between fishes and reptiles. One fact however appears to be consistently borne out by anatomical and palæontological inquiries, and that is that structures that have once been definitely lost in the past history of a race cannot be reacquired and consequently when wanted again their place has to be taken by new structures or a modification of existing ones. Such new organs may resemble superficially those of remote predecessors, but a closer examination will show them to differ anatomically. This resemblance brought about by the similar needs or environment of the organisms is termed homoplasty or convergence and is a very interesting phenomenon. At times the convergence is not limited to one or two organs, but the whole body form of the animal is influenced, as for example the fish-like appearance of the Ichthyosaurs, whose general shape is again reproduced by the Dolphins. This likeness does not in any way indicate a relationship between the forms possessing it, but is due to a similarity of habits and surroundings. Many striking examples of convergence are illustrated in this work.

Although dealing only with the modifications adapting reptiles to partial or complete aquatic life, it is remarkable how many different forms come within the scope of its pages. With some of the modifications, exemplified by turtles and crocodiles, we are already familiar, and the many others, mostly fossil, are to be found in this book. The descriptions are clear and easily followed, and the introductory chapters call attention to the main points of reptilian structure and classification relevant to the subsequent pages. The accounts are quite up to date, and even although it is meant to have a more or less popular appeal we think the value of the book would have been augmented by the addition of a short bibliography. The short historical introductions to some of the chapters are very interesting, especially the account of the vicissitudes undergone by the first known fossil skull of a Mosasaur. The book forms a useful addition to the few volumes that make the results of recent palæontological research accessible to the general reader. It is well illustrated and printed.

C. H. O'D.

Reptiles and Batrachians. By E. G. BOULENGER, F.Z.S. [Pp. xiv + 278, with 202 illustrations.] (London: J. M. Dent & Sons. Price 16*s*. net.)

THIS book gives an account of the external appearance and habits of a number of the most interesting reptiles and batrachians (the amphibia of most authors). More than two-thirds of the book is devoted to the reptiles and so covers practically the same ground as Ditmar's *Reptiles of the World*. It is not so full as the latter, but introduces a certain number of new observations. The descriptions are arranged systematically, but little attention is given to the main points in the anatomy or the relationship of the various groups. It is difficult to reconcile the statement on p. 111 that the largest snake yet recorded, a python, "is just under thirty-three feet in length" with the one on the following page, that the largest boas "may measure thirty-five feet in length."

The last part of the work deals in the same systematic way with the batrachians. The section dealing with the transformation of the water-living Axolotl into the terrestrial adult Amblystoma, in particular the author's own experiments thereon, makes very interesting reading. A short note on the possible bearing of these experiments on the position of the Urodela with persistent gills would have added to the interest. It is stated on pp. 271 and 272 that *Siphonops*, an apodous batrachian from S. America, does not lay eggs but brings forth young alive; this, however, is not accurate in as far as *S. annulatus* is concerned. This species lays eggs in a string which undergo development on or beneath the surface of damp ground.

The 176 illustrations from photographs by Mr. Berridge, F.Z.S., form a very good series, portraying many of the animals described in the text.

C. H. O'D.

Flies in Relation to Disease: Non-Bloodsucking Flies. By G. S. GRAHAM-SMITH, M.D. [Pp. xvi + 389, with 32 text figs., 27 plates and 20 charts. Second, revised and enlarged edition.] Cambridge Public Health Series. (Cambridge: at the University Press, 1914. Price 12*s*. 6*d*. net.)

THE first edition of this useful work contained a large amount of information, including much that was original, relating to the bionomics and carriage of disease by non-bloodsucking flies, and, in fact, summarised our knowledge of this subject up to the end of 1912. In the new edition accounts of researches carried out by the author and other workers during the year 1913 are incorporated, and are presented in the form of an Appendix; the latter consists of 87 pages and is illustrated by 3 plates and several charts. The new matter is thus distinct and easily accessible; it is, moreover, arranged in the same order as in the preceding chapters, and each subject is provided with a reference to the page on which it was previously mentioned.

Interesting observations by the author on the measurements of blow-flies are given which show that a considerable degree of variation occurs in regard to the dimensions of the head, thorax, and wings, and as a result of several experiments, three of which are detailed, it is apparent that "the size of the fly depends on the quality and quantity of the food of the larva, and the time during which it feeds." Further information is given regarding the range of the flight of flies, and attention is drawn to recent papers by Froggatt and others on the changed habits of Australian blow-flies. These show that in different parts of the world different species of flies may be responsible for similar diseases, and that great changes may take place in the habits of certain species within comparatively short periods of time.

The question of the hibernation of the house-fly is referred to, and attention is directed to Copeman's investigations; from observations on allied species the author favours the theory that this period is passed in the pupal stage. Bahr's work on dysentery in Fiji, omitted in the first edition, now receives adequate notice, and is followed by an account of some important researches conducted by the author on summer diarrhœa and its relations to meteorological conditions and probable carriage by flies. This subject occupies a considerable portion of the Appendix and is provided with a number of charts demonstrating the relations between bright sunshine, soil temperatures, and deaths from epidemic diarrhœa in Birmingham, Manchester, and Cambridge. Greig's investigations, proving the existence of human cholera-carriers and their connection with flies, are discussed, and the interesting observations of Patton, Cragg, and Mitzmain on the habits of certain non-biting, hæmatophagous Muscids are noted. These flies, being unable to pierce the skin themselves, rely for their supply of blood upon the wounds caused by biting flies ; therefore they may be regarded as probable factors in the dissemination of disease. Additional information, in regard to recent work, is given on Myiasis and on the various parasites and enemies of adult flies and larvæ.

The bibliography has been increased by over one hundred references, and constitutes a valuable appendage to the work. H. F. C.

Some South Indian Insects and Other Animals of Importance, Considered Especially from an Economic Point of View. By T. BAINBRIGGE FLETCHER, R.N., F.L.S., F.E.S., F.Z.S. [Pp. xxii + 565, with 50 plates and 440 text-figures.] (Madras : Printed by the Superintendent, Government Press, 1914. Price 9*s*. (6 rupees).)

MUCH of the matter composing this volume has been derived from information supplied by the records and collections prepared chiefly by the official entomologists of Madras. These were formed both before and during Mr. Bainbrigge Fletcher's tenure of office as Government Entomologist to this province, and were overhauled prior to his departure so that they should be in order for his successor. The information obtained proved larger than had been anticipated and was published mainly with the idea of providing a basis for future work.

The contents may be divided into two sections. The first, comprising twenty-three chapters, contains matter of an introductory nature and general accounts of insect pests, etc. ; the second, forming the main and larger portion of the work, provides information regarding the more important economic species met with in Southern India. This part is not arranged in chapters, but in more or less continuous sections according to the natural orders in which the species are grouped.

The opening chapters relating to the structure, classification, metamorphosis, means of defence of, and communication amongst, insects are purposely brief but interestingly written and are quite suitable for a book of this nature. The economic aspect of the subject may be said to commence with the useful account of tropisms and their practical importance. Insects in their less obvious relations to plants—insectivorous plants, flower fertilisation and symbiotic relations—are then briefly considered and are followed by chapters on symbiosis and parasitism, and the balance of life. A few pages are devoted to a general consideration of insect pests, in which, by estimating an average of 10 per cent. of all crops to be destroyed by these creatures, the author shows that the annual loss suffered in Madras, by damage to crops alone, is in the region of 200,000,000 rupees. The various methods adopted for the control of crop pests are conveniently arranged under four headings—agricultural, mechanical, insecticidal, and special; under that relating to insecticides a well-illustrated account of the several types of spraying machines and their usage is included. Chapters containing matter of a general character on the more important groups of insects attacking crops (caterpillars, grass-hoppers, crickets, termites, bugs, beetles, and flies) follow and provide information regarding the habits of these pests, their varied modes of attack, and the means of controlling their ravages. Those on insects damaging stored products, on household pests and on insects in relation to disease are treated on somewhat similar lines. Beneficial and useful insects now receive attention and are succeeded by an account of certain other animals of economic importance occurring in Southern India. All kinds of animals other than insects, from elephants to the minute eelworms, are considered in the latter and are treated from the point of view of their economic status. The second part of the volume opens with two useful lists of Indian plants. One of these contains the names of the commonly-grown crops, arranged alphabetically, and under each, with page references, those of the various insects known to attack it; the other is a list of allied plants and shows the commoner plants and trees grouped under their natural orders. Over 350 species of insects of economic importance and two species of mites, considered in a systematic and detailed manner, occupy the remainder of the text. These are ascribed to fourteen natural orders and each species is treated under six headings—references, distribution in S. India, life-history, food-plants, status from an economic point of view, and control. The references, including synonyms, are limited to the original description and to those of a more important or accessible nature, and the control methods given are such as usually are found to be efficacious. Every species considered is depicted, either by means of a text-figure or coloured plate, and frequently other stages than the adult are shown—often in such a way as to demonstrate their economic importance. Descriptions of the various insects are thus dispensed with, and the method adopted should not only prove more agreeable to the reader, but should also enable him to identify at least the more obvious species. The index is well arranged, but does not include plants, since they are already shown in order in the body of the book.

While the illustrations are, on the whole, good, the coloured plates deserve especial commendation. These latter have all been reproduced from original drawings made at the Agricultural Research Institute, Pusa, and many have already appeared in various departmental publications; the majority of the text figures have been prepared, under the author's supervision, by artists inexperienced in this work, and although a few are somewhat crudely executed, they are, in the main, highly creditable.

This volume should prove of considerable value to those residents of Southern India who are closely affected by the economic aspect of the subject, and indeed to all who are interested in this branch of entomology. H. F. C.

The House-Fly (*Musca domestica*, **Linn.**)**: Its Structure, Habits, Development, Relation to Disease, and Control.** By C. GORDON HEWITT, D.Sc., F.R.C.S. [Pp. xv + 382, with 104 illustrations and 1 map.] Cambridge Zoological Series. (Cambridge: at the University Press, 1914. Price 15*s*. net.)

DR. GORDON HEWITT'S work on the house-fly is so well known that a further contribution from his pen relating to this obnoxious insect is most acceptable.

The contents of this well-produced volume include the whole of the original matter composing the author's monograph on the house-fly (1907-9); but, owing to the recent extensive additions to our knowledge of the subject, the preparation of an entirely new work has been necessitated. The book is divided into six parts, embracing twenty-seven chapters, and possesses an extremely useful bibliography covering thirty-seven pages and containing several hundred references. The first part, after a brief historical sketch, introduces us to a minute and well-arranged account of the anatomy of this insect and to a consideration of its habits and bionomics. Among the sections which constitute the latter subject, those relating to the range of flight and feeding habits receive more particular attention. Part II. is devoted to the breeding habits and life-history of the house-fly and to the structure of the mature larva. The first-named subject is treated primarily from an historical point of view, and the more important observations of various workers in this connection since the time of de Geer are discussed. As a summary of these results, a list of the substances on which flies breed is given, no less than forty-one being enumerated. The natural enemies and parasites of this insect receive attention in the third portion of the book, and a very interesting account of that important parasitic fungus *Empusa muscæ* is given. Among the many other species of flies which occasionally visit or frequent houses, those of special economic interest are considered in Part IV., and accounts are given of the Lesser House-Fly (*Fannia canicularis*, Linn.), the Latrine Fly (*Fannia scalaris*, Fab.), the Stable Fly (*Stomoxys calcitrans*, Linn.), the Blow-Flies (*Calliphora* spp.) and Sheep Maggot Fly (*Lucilia cæsar*, Linn.), the Cluster Fly (*Pollenia rudis*, Fabr.), etc. The last two portions of the work (Parts V. and VI.) deal respectively with the relation of house-flies to disease and control measures. Considerable space is devoted to the former subject, in which, besides their connection with various maladies, a discussion of the modes of dissemination of pathogenic organisms by flies and particulars of some miscellaneous experiments regarding the carriage of micro-organisms, pathogenic and non-pathogenic, are given.

The illustrations, most of which have been drawn by the author, are well reproduced and decidedly enhance the value of the book. Four of the figures (1, 48, 79, and 80) are coloured, and form three really beautiful plates, the first forming the frontispiece and portraying the female house-fly.

This volume is not intended as a popular treatise on the subject, since one or two works of this nature are already in existence; it is intended more especially for the use of scientific workers, and to such it should prove of much value.

H. F. C.

BOTANY

Transpiration and the Ascent of Sap in Plants. By HENRY H. DIXON, Sc.D., F.R.S. [Pp. viii+216.] (London: Macmillan & Co., Ltd., 1914. Price 5*s*. net.)

THE problem presented by the rise of sap in plants has so many points of contact with other branches of science besides that of plant physiology that a wide circle of readers will welcome this recent addition to Macmillan's Science Monographs, in which Professor Dixon presents a discussion of this problem, to the solution of which his own researches have contributed so largely. Even those who may be unwilling to concede that the theory elaborated by the author in conjunction with Dr. Joly finally disposes of this much-debated problem, will at least admit that the questions involved have been placed on a new footing, that the results of the investigations made by Professor Dixon and those who have worked along the lines indicated by him have added materially to our knowledge of plant physiology, and that the explanation of the ascent of sap here advanced is supported by a remarkably convincing body of experimental data. It may be noted that the author, doubtless because the ascent of sap is the main question at issue, hardly deals with the other part of the subject as represented in the title of the book—namely, transpiration itself. Recent researches have greatly extended and modified our knowledge of this process, though it is doubtful whether they can as yet be brought to bear directly on the question of the rise of sap. Still, a consideration of modern work on transpiration would not have been out of place in a book which, as it is, should more accurately have been entitled simply "The Ascent of Sap in Plants," leaving room in the series for a "monograph" on modern physiological and ecological researches in transpiration, a critical resumé and discussion of which would form an extremely useful publication from many points of view.

The early workers in plant physiology recognised the connection between the flow of water upwards in the stems of plants and the absorption of water from the soil by the roots on one hand and its escape as water vapour into the atmosphere from the leaves on the other; but though Hales and others made experiments on the pressure set up by this flow and the amount of water exhaled by the leaves, the ascent of the sap was ascribed by them to the vital activity of the plant. Vitalistic explanations of this phenomenon persisted long after it had been shown that water still rises in cut shoots and in entire plants whose roots had been killed by heat, that the current still flowed when a considerable length of the stem was killed by heat, and that poisonous solutions were carried up for a length of time more than necessary to ensure the death of all the living cells of the wood. These experiments are generally considered to have disposed finally of the vitalistic explanations that the rise of sap might be due to the osmotic pressure set up in the absorbing root-hairs, or to the "pumping" action of the living cells in the wood, which were supposed to repeat the root-pressure effect at different levels in the stem by absorbing water and afterwards returning it under pressure into the vessels. Some writers, however, still regard these "killing" experiments as unconvincing, explaining the stoppage of the water flow as due to the blocking of the vessels by products of the dead cells, and so on. Meanwhile, various physical explanations were suggested, such as capillarity, differences between the pressure of the outer atmosphere and the lower pressure of the gases within the plant, the passage of the water within the walls and not in the free lumina of the vessels, etc., but these were easily disposed of by experiment or by simple resort to established physical facts which had been overlooked by the theorists.

The problem was attacked from a quite new point of view in 1894 by Professor Dixon and Dr. Joly, and the present work gives an account of the theory then outlined and of the earlier and more recent experiments carried out to test its validity and to illustrate its applicability. This theory rests essentially upon the discovery made in 1846 by Donny that a column of water possesses great tensile strength; Donny himself considered that the presence of dissolved air reduces the cohesion considerably, but Dixon and Joly found that this was not the case, and that the tensile strength of the sap of plants is even greater than that of water, so great that, according to the most recent determinations, it amounts to more than 200 atmospheres. Since resistance to a current of water moving through the wood of a plant at the velocity of the transpiration stream is approximately equal to a head of water equal in length to the wood traversed, the tension applied to the upper end of the water columns, which will be able to raise the transpiration stream in a tree, must equal the pressure produced by a head of water twice the height of the tree—that is, in a tree 100 metres high a tension of 20 atmospheres—and the cohesion or tensile strength of sap, amounting as it does to at least 200 atmospheres, is obviously in no way taxed by this tension. Since the transpiring cells of the leaf normally remain turgid during transpiration, the osmotic pressure keeping them distended must on this theory correspond in magnitude to the tensions necessary to raise the sap; and this is invariably the case, the pressures developed being indeed far in excess of those demanded by transpiration. Briefly, on this theory the flow of water up the highest tree is to be regarded as due to the evaporation and condensation produced by the difference between the vapour pressure in the soil spaces and that obtaining around the leaves, the column of tensile water flowing under the action of this difference from end to end of the plant.

Many of the experimental methods and results given in this work throw light upon other problems in plant physiology than that with which the author is particularly concerned; in this connexion particular mention should perhaps be made of the methods devised for extracting cell-sap and determining its osmotic pressure, and for investigating the conditions under which water flows in the vessels of the wood.

F. CAVERS.

Cocoa. By C. J. J. VAN HALL. [Pp. xvi + 515, with 140 Illustrations and 1 Map.] (London: Macmillan & Co., 1914. Price 14*s.* net.)

DR. VAN HALL'S position as Director of the Institute for Plant Diseases and Cultures at Buitenzorg and his long experience in tropical agriculture would alone give weight to his sound and emphatically expressed opinions regarding the methods of cultivation of any useful plant grown in the tropics. He has, however, had unusual opportunities for mastering the subject with which he deals, having taken a large share in developing the cocoa plantations in Java, where the decay of the coffee-culture about 1880, owing to the ravages of the leaf-disease fungus Hemileia, was the direct cause of the starting of cocoa-growing. Cocoa has been extensively planted in the old coffee fields, but the cocoa in its turn was badly attacked by two insect pests, with the result that the crop was much reduced. The planters in Java have had to resort to many expedients, and have been successful in overcoming the difficulties which threatened to wipe out cocoa-culture there as completely as coffee-culture was wiped out in Ceylon.

The present work is based on the author's intimate knowledge of cocoa-culture in the East; but he has incorporated in it so much information about the growing of this valuable crop in other lands that its range is monographic, if not encyclopædic, in completeness. Following upon introductory chapters dealing with the history of the cocoa industry, geographical distribution, climatic conditions, soils, chemistry of cocoa, botanical characters of the cocoa plant and its varieties, the cultivation of cocoa, fermentation and drying, and diseases, the longest chapter of the book is devoted to a valuable survey of the cocoa-growing countries. In the case of each country passed under review, the author gives not merely statistics, but interesting data regarding climate, soils, methods of culture and curing, diseases and enemies, labour conditions, etc.

The enormous increase in the world's production and consumption of cocoa is strikingly shown in a series of tables. The most remarkable increase in production is shown by the Gold Coast, where the output rose from just under thirteen to nearly forty million kilogrammes between 1908 and 1912, the total world output rising during this period from 194 to 230 million kilogrammes. That is, from being last on the list of the seven great cocoa-producing countries the Gold Coast has leapt to the foremost position; the others remained practically stationary during these five years, their output in 1912 being as follows (in millions of kilogrammes): Eucador 35·5, San Thomé 35·5, Brazil 30, San Domingo 20·9, Trinidad 18·9, Venezuela 12·5. The greatest increase in the amount of cocoa imported for local manufacture during recent years has taken place in the United States, which has replaced France as the most important cocoa-manufacturing country, while England has fallen from second to third place.

The brief concluding chapter on the cocoa and chocolate industry contains some interesting and little-known facts, such, for instance, as the importance of cocoa-butter as an increasingly valuable and variously used by-product of cocoa manufacture, and the use of the shells or cuticles of the cocoa-fruit as a substitute for tea in Ireland and Switzerland and as a cattle-food and manure elsewhere.

F. CAVERS.

The Coco-nut. By EDWIN BINGHAM COPELAND, Professor of Physiology and Dean of the College of Agriculture, University of the Philippines. [Pp. xiv + 212, with 23 Illustrations.] (London: Macmillan & Co., 1914. Price 10*s*. net.)

THIS book is based upon the author's experience in organising and conducting courses of instruction in coco-nut physiology and coco-nut culture at the College of Agriculture of the University of the Philippines, and its aim is the same as that of these courses—namely, to give the knowledge and advice which will qualify a person for the practice of coco-nut raising.

Throughout the book the author insists upon the importance of general principles which apply not only to coco-nut culture but to tropical agriculture as a whole. The behaviour of the coco-nut, and of every other cultivated plant, is intelligible in the light of the knowledge of its physiology, and in no other way; but this common-sense or scientific view is not often so consistently kept to the front as in the case of this book. The natural result is that, although at first sight the book may seem to deal too exclusively with the coco-nut industry of the Philippine Islands in so far as details are concerned, it contains everything that a planter anywhere in the tropics really needs most to know. The author wisely omits various matters which are already sufficiently dealt with in the standard works by Ferguson, Prudhomme, Smith, and others—statistics on the coco-nut industry of different countries and on the commerce in coco-nut products, estimates of the cost of establishing and maintaining plantations, etc.

The first two chapters, dealing with the physiology of the coco-nut and with climate and soil, contain much that is new, being based upon the author's own investigations, and form a model introduction and foundation such as we should like to see extended to many other cultivated plants. These two chapters represent a really good ecological study of the coco-nut—*i.e.* of its physiology in relation to its environment; and there can be no better method of attacking the problems of cultivation than by studying, on modern lines, the *ecology* of each plant that is to be cultivated—a fact that practical cultivators are at last beginning to appreciate.

The chapter devoted to diseases and pests is considerably the longest in the book; and here again the ecological view-point adopted by the author makes much that he has to say concerning the coco-nut applicable to the warfare against plant-disease in tropical agriculture generally. He points out, for instance, that whereas in the temperate regions the spread of pests is checked by winter, and there occur extensive forest formations made up of one or a few species, in the tropics the climatic conditions controlling plant communities and the climatic opportunities for epidemics make it impossible for such pure formations to exist in nature; hence, if men will plant and maintain forests of coco-nuts, or any other single tree in the tropics, it will prove possible only by much greater precautions against disease than are required by any crop of temperate lands.

In the remaining chapters the author deals with the selection and treatment of seed, field culture, and coco-nut products. Throughout he calls attention to points which require investigation, but have hitherto received little or no attention despite their importance. For instance, he emphasises the need for detailed and systematic experiments in the breeding of coco-nuts. While acknowledging the valuable work done in Madagascar under the auspices of the French Ministry of Colonies in the study of varieties, races, and strains, he points out that at present nothing is known as to thoroughly distinct varieties that can be trusted to breed true, or as to the suitability of different varieties for different climatic or soil conditions.

F. CAVERS.

APPLIED PHYSIOLOGY

Report of the Committee on Standards and Methods of Examining the Color Vision. Reprinted from the Transactions of the Section on Ophthalmology of the American Medical Association. Chicago, June 1914.

THE Committee commences its report with a theoretical exposition and classification of colour-blindness on a modified form of the trichromatic theory. It is curious that so many committees appointed for a practical object adopt a theoretical basis which, in the case of the trichromatic theory, is different in each case, and quite irreconcilable with each other or with the true facts of colour-blindness.

The Committee give a detailed account of the methods in use in foreign countries. This is very complete and well done, and contains much valuable information. An error, however, is made in stating that the British Admiralty use Holmgren's test in addition to Edridge-Green's lantern and spectrometer. The British Admiralty only use Edridge-Green's methods.

The final section deals with the methods in use in the United States, Canada, and the Public Health Service. Twenty-five railroads use a lantern in all cases, six railroads use a lantern in special cases, twenty-one railroads do not use a lantern in any case. The lantern used in these cases must be of a very defective character, as an examination of the replies of the examiners shows that very few of those rejected by the lantern failed to be detected by the very inefficient wool test. As is well known, over 50 per cent. of dangerously colour-blind persons rejected by Edridge-Green's lantern will pass the wool test, even in its most approved form, with the ease and accuracy of a normal-sighted person. The Committee has not recognised that a dangerous dichromic or trichromic can pass the wool test with ease. It has, however, recommended that a lantern test should be used in all cases in addition to the wool test, in order to detect (1) those with a shortening of the red end of the spectrum, and (2) those with a central scotoma for red and green.

It is to be hoped that the next Committee appointed on colour vision will base its conclusions on actual fact. It will be interesting to note how long it will take before such a very simple fact as the extreme inefficiency of the wool test is universally recognised.

ARCHÆOLOGY

The Roman Cemetery in the Infirmary Field, Chester. Part I. By R. NEWSTEAD. (Annals of Archæology and Anthropology, vol. vi. No. 4.) [Pp. 52, illustrated with plates and sketches.]

IN order to appreciate fully the results of the recent excavations in the Infirmary Field at Chester, of which Prof. Newstead has given so clear a report, it is necessary to bear in mind a few facts concerning the Roman occupation of the site. The Roman fortress of Deva was established about the year 50 A.D., and the twentieth legion remained in garrison there for about three centuries. Whether the fortress was originally constructed of earth or of stone we cannot tell; what appears to be certain is that about the year 200, or perhaps later, it was fortified with a strong stone rampart, portions of which still remain. The present walls of Chester are, of course, mediæval, and enclose a larger area than did the walls of the Roman fortress; it is only on the north and east that the two coincide. In the year 1887 the remarkable discovery was made that the core of the north wall was composed of Roman inscribed tombstones. In the course of the next few years about 150 of these sepulchral relics were taken out of the wall and removed to the Grosvenor Museum, which now contains one of the richest collections of such stones in the country. It was obvious that the Romans, in constructing their northern wall, had torn up, as building material, the gravestones of an adjoining cemetery.

Now the Infirmary Field, where Dr. Newstead's excavations were carried out, lies just outside the north-west corner of the Roman fortress, though within the bounds of mediæval Chester. Further, not a single inscribed tombstone has been discovered in the cemetery that has just come to light. It is at least *possible* that some of the tombstones which formed the core of the north wall may have been taken from this very site.

For more than half a century sepulchral remains have been turned out at intervals on this area, now known as "Lady Barrow Hey"; but it was in May 1912, when excavations were being made for the foundations of a new infirmary, that the series of discoveries commenced which are recorded in the report before us. With commendable foresight the Infirmary Board immediately requested Dr. Newstead, in conjunction with Dr. John Elliott, to report on objects of archæological or anthropological interest, and down to the early months of 1914 they kept an assiduous watch upon the site. Since then they have made the minute examination of the relics, the results of which are embodied in the present report.

Within an area about sixty feet square, on the site of the isolation wards of the new infirmary, they found traces of over thirty burials of men, women, and children, the orientation of the bodies being either north and south or east and west. The relics found in the tombs, as well as the form of the tombs themselves, leave no doubt that we have here part of the burial-ground where the Roman legionary soldiers, their wives, children, and freedmen were laid to rest. Though the burials were by inhumation in all cases, yet there is a curious variation in the forms of the graves; presumably a difference of rank is indicated. In nineteen cases the bodies were simply laid in rectangular trenches cut in the native clay ; in other cases the graves were formed of Roman tiles ; two consisted of roughly formed cists ; one was constructed of solid masonry ; and in one case the body had been covered by a thick sheet of lead. The occurrence of a number of nails suggests that wooden coffins were used.

It is impossible in a brief review to do justice to the minute care and skill with which the details of these interments and their accompanying relics have been recorded and classified. The illustrations alone are a speaking record. A clear, large-scale plan shows the relative positions of the graves ; a number of sketches from Prof. Newstead's own pen depict the relics, and the various bones of the skeletons that have been preserved ; and admirable photographs are added of the tombs and the pottery. The present report, which is entitled Part I., deals mainly with the details of the tombs and the relics ; we may perhaps assume that the forthcoming second part will discuss anthropometrical details ; we believe, for example, that the remains of a young woman found in the stone-built tomb were typical of a Mediterranean race.

The relics are of the class usually occurring in the case of burials by inhumation, including pottery, glass flasks, lamps, objects of bronze, coins, beads, rings, bones of animals, and so on. The pottery and coins, as affording the best materials for determining dates, are very fully discussed. The general conclusion is to the effect that the site was in use as a burial-ground towards the close of the second century, though some relics may point to a much earlier period.

We have said that the excavations produced no inscribed stones. There is, however, one inscription which has given information of some importance. On the base of a tiny glass vessel, only two and a half inches high, occurs the stamp

PATRIMO VECTIGAL.

No less than twenty-five glass *ampullae*, bearing this or a similar legend, have been found elsewhere in the Western Empire, twelve in Rome itself, and four in Britain. Fortunately, the completeness of the present stamp throws welcome light on the others, though it is not yet possible to give a satisfactory explanation of the inscription. We can only say that it has been suggested that the stamp had some imperial significance, and may have freed the contents of the flask from taxation or duty.

We may mention one other discovery, as it coincides in point of time with a similar discovery elsewhere. At one point, below the level of the burials, and therefore prior to them in point of date, there was found, imbedded in the undisturbed boulder clay, a circular domed "furnace," about forty inches in diameter, with a "hob" or "feed-hole" on one side. Such furnaces, or—as we prefer to call them—"ovens," of varying sizes, have been found in many Roman forts in Britain, and it is generally assumed that they were used for baking bread. The layer of charcoal, which occurs elsewhere, was also present at Chester. Only a year ago, while the Balkerne Gate of the Roman town at Colchester was being excavated, a similar oven was found in the floor of the southern guard-chamber. I have no details to hand, but the description given to me last January by the excavators on the spot seemed to correspond exactly with the description of the example found at Chester.

Perhaps we may hope that in his next report (Part II.) Dr. Newstead may be able to institute a comparison, not only between these two ovens, but also between the tiled graves found at Chester and those to be seen in the basement of the museum at York ; and, generally, between the Chester tombs and their contents and those found on other sites in Britain, as well as those recorded in the German Limes Reports.

F. A. BRUTON.

STATISTICS

Report on the English Birthrate. Part I. England north of the Humber. By ETHEL M. ELDERTON. [Pp. viii + 246, with 2 diagrams and 20 plates.] (London: Dulau & Co., 1914. Price 9*s*. net.)

THE science of eugenics has suffered somewhat from the undue dogmatism of many of its exponents. The application of the principles of heredity to mankind is no doubt a matter of great importance, but unfortunately there are at present no generally accepted principles to apply. The rival theories of Neo-Darwinians, Neo-Lamarckians, and Mutationists are to a large extent mutually destructive, and this is a fact which many writers on eugenics (most of whom are Neo-Darwinians) have ignored. But some of the literature on the subject is valuable, especially the series of memoirs which have proceeded from the Eugenics Laboratory of London University, to which the present volume is an addition. The subject is certainly a topical one, for we have all had our attention forcibly called to the disproportion between the French and German populations (a disproportion which of course did not exist in 1870), and this memoir like its predecessors contains a large amount of carefully collected information and is a monument of industry.

The investigations cover Cheshire and the six northern counties, the three ridings of Yorkshire being treated, however, as distinct counties. This area contains rather more than one-third of England's population, but statistics are not given for the region as a whole, which is an unaccountable omission. The birthrate is calculated throughout on the number of married women between the ages of 15 and 55 (including legitimate births only, of course), not on the usual basis of total population. The period covered is from 1851 to 1906. The full facts are secured from the 1901 census, but it has been found possible to bring most of the computations down to 1906, and in a few cases data from the 1911 census are given. In Durham and Northumberland the birthrate rose considerably between 1851 and 1876, and in the other counties it was fairly steady during that period. From 1876 to 1906 the rate fell in all the counties, but in Durham and Northumberland the fall was not much more rapid than the previous rise. The changes in the "potential birthrate," which varies with the age of married women between 15 to 55, have been trivial. The actual fall is mostly of a serious character. Thus during the thirty years the birthrate in Lancashire fell from 24 (per annum per 100 married women between 15 and 55) to 17. The bulk of the book (pages 20 to 194) is taken up with a detailed consideration of the individual counties and individual registration districts. The only district of any importance which shows a rise from 1876 to 1906 is Liverpool, which contains, of course, a considerable number of Roman Catholic Irish. Owing to the heterogeneous character of the registration districts, it is not easy to find in what classes of the community the fall has been most marked. The data are incomplete, but it appears that the fall is large among workers in cotton and woollen factories, and where married women are employed, but much less among miners and agriculturists. There is also some evidence that the fall is least among people described as socially inferior, and it is this differential fall in the birthrate upon which the authoress lays great stress. The investigations were not of a purely statistical character, but include reports by medical men and others which prove (as was to be expected) that the fall is due to artificial limitation and not to decrease of natural fertility. The reports also prove the disgraceful prevalence of the sale and use of abortifacients, which as Miss Elderton says should be brought more within the reach of the law. As to the cause of limitation, the authoress thinks that it is largely due to the Factory Acts, which reduced or destroyed the economic value of children, and she attaches much importance to the notorious Bradlaugh trial in 1877. Her remedy is the endowment of the well-born child.

Miss Elderton has as usual done her statistical work well, but many of the tables would have been more valuable if they had included the actual numbers as well as the percentages and averages. The worst instance of this omission is in the detailed study of Bradford on pp. 224 to 231. Such figures as are given appear to mean that in this town large net families (number of children surviving) belong predominantly to mothers who are healthy in spite of unfavourable surroundings (a truly hopeful fact, if it be a fact), but the tables are so inadequate that it is impossible to be sure.

We think the authoress exaggerates greatly the importance of the differential nature of the fall. Man differs from other animals in "inheriting" a complex social environment which affects him from (and even before) birth. And these social environments differ greatly, of course, in different classes and even between different members of the same class. Thus, although it is probably true that the "less desirable" classes really have a slight inborn inferiority, they are certainly much less inferior than appears on the surface. This immense source of error is not discussed by Miss Elderton.

The book has a number of printers' errors and there is no index.

A. G. THACKER.

MEDICAL

A Text-Book of Insanity and other Mental Diseases. By C. A. MERCIER, M.D., F.R.C.P., F.R.C.S. [Pp. xx+368. Second Edition.] (London: George Allen & Unwin. Price 7*s*. 6*d*. net.)

AN authoritative text-book on Mental Diseases is worthy of notice, for it needs to be written by a medical man of large and long experience with the insane, and who himself should be a physiologist, a pathologist, and a psychologist of repute and standing. Too rarely is such a combination of qualifications found, but it is not too much to say that the author of this manual fulfils all these requirements, being, in addition, a recognised master of metaphysical subtleties. The text-book under review is lucid, original, and informing, but it is also lacking in some essentials, and to these deficiencies we shall refer later.

The first edition, brought out a dozen years ago, was designed for the student, but, as the present preface suggests, this new edition is intended also for the instructed, and we recognise this must be so, for the author is frequently tempted to reflect upon the ignorance of the alienist and the uselessness of the psychologist, and he proceeds to repair these imperfections in a dogmatic, cynical, albeit original fashion. "A knowledge of text-book psychology is of no more value to the student of insanity than a knowledge of cuneiform inscriptions." "The nature and varieties of attention, the association of ideas, imagination, and the relation of thought to language are no concern of the alienist, the analysis of sensation or the nature of apperception are useless acquisitions, and the sooner he forgets them the better will he understand the disorders of mind from which the patient suffers"—"to understand the disorders of mind it is necessary to forget all the teaching of psychologists,"—"a psychological analysis of the disordered mind has never hitherto been attempted," and "self-estimation is a faculty unknown to the psychologist." These are overstatements, for Bain—upon whose work Dr. Mercier must have browsed psychologically in his earlier days—deals fully with all the emotions of self: self-esteem, self-complacency, self-pity, self-interest, and self-humiliation. Stanley Hall also refers to self-estimation in his two volumes upon Adolescence; Kant based upon it the radical principle of evil, and Spinoza defines it at full length. Naturally, and because of the high position the author holds among psychologists, the most interesting chapter in the book is the third, which deals with the analysis of mind. This chapter is the fundamental factor in the book, and it is full of originality, ingenuity, and suggestiveness. He divides the mind into certain primary faculties; on page 52 they are seven, on the next they are five, but they are sub-divided into four evolutionary levels, "so that altogether there are twenty-five pigeon-holes or compartments, in one or more of which every disorder of mind can be placed."

Dr. Mercier states somewhat categorically that in practice any one of these faculties may alone be subject to disorder, apparently in some way insulated or separated from the others which remain normal. We have always held and taught that the mind is an indivisible whole, although it may be convenient theoretically to abstract it into faculties, but these faculties are fictitious, they are a sham, and they reduce the mind to a flux of descriptive literature. The highest level of "objective" thought is described by the author as "wisdom," but fancy the "wisdom" of a bank clerk or of a bricklayer's labourer being alone affected! Moreover, the cortical structure of the brain forbids a "natural history" classification of the mind. In only one portion of the brain cortex, and that the motor area, are there, apart from the granules, any cell areas which can in any sense support the "faculty" division. It is the whole mind which feels, which wills, which thinks. We have no belief in "faculty psychology." Our experience of mental disorders forbids its validity, and we hold that the mind must be taken wholly in its cognitive, affective, and conative attitudes, as one of these is meaningless without the other. We also think that the classification which the author adopted from Herbert Spencer in the first edition to be the more preferable analysis whereon to base the motives for normal conduct, as also to study their departures in insanity, viz. the impulses, instincts, and desires which are directly or indirectly self-preservative, and in these a chain of mutual dependencies occur which are sufficient to explain both conduct and character. We believe it is true to state that the plotting in tabular form of the constituents of mind and their sub-division into evolutionary levels has not hitherto been attempted in an analysis of mind; but the method, although original, is neither convincing nor final. These fractional representations of the mind are frequently referred to in the text, but we think they are unprofitable as an analysis, and are a mechanical setting of a dynamic psycho-physiological process which, with a "purpose," guides the individual in peril and helps him to avoid disaster. The author urges that the composition of the emotions is not a matter of any concern to the alienist, but we maintain that in the emotions, the appetites, and impulses we have the clue to all intellectual and voluntary processes, and all qualities of character must finally depend upon these. The classification of insanity adopted by the author is one that we have not infrequently found from experience in teaching to be a source of bewilderment to students. Forms and varieties and symptoms are discussed. "I shall here call the symptom insanity by the name of the form of insanity and the different diseases that I include by the title of kinds of insanity," but as the author states, classification of insanity has always been a stumbling-block. We find Idiopathic insanity—yet with an assignable cause. We have Dissolute insanity for Insanity of dissolution. Alcoholic insanity is partly described under no less than three scattered separate chapters or headings. We do not agree with the interpretation of the term Anoia, which is used as the equivalent of Dementia rather than of Amentia. The author states "Dementia may, as a form of insanity, be of any degree, from the slight blunting of intelligence and feeling and the slight diminution of conduct that we all experience at the end of a tiring day . . . to Coma." Then, "the degrees of Anoia are practically infinite. They range from the trifling decadence of intelligence, feeling, and conduct that is exhibited by any one after an enfeebling illness, or at the end of a tiring day," also "thus understood Anoia of some degree is present in every case of insanity, and it is Anoia that constitutes insanity."

Only confusion and embarrassment result from this misleading description. The term Anoia has had a definite meaning ever since its first application in mental nomenclature by John Mason Good a hundred years ago, and it is unjustifiable from any standpoint to pervert its original use. We referred to the deficiencies of this manual. There is too little about treatment in the volume, no mention is made of the method of psycho-analysis, yet no modern psychiatrist—a term which the author would deride as against his own of "alienist"—would attempt modern mental therapeutics to-day without using the method of "free association." No description is given as to lumbar puncture, and no details are given of the Wasserman reaction and the two methods of administering neo-salvarsan or salvarsanized serum are omitted. The subject of heredity is barely discussed, and Mendelism finds no place, although from the Eugenic standpoint this is vital, and although also the Cambridge School has related valuable findings in regard to human heredity relating to left-handedness, the musical temperament, brown eyes, and features, and the shape of hands and fingers. There is no index to the volume ; and to open the book to ascertain the author's views upon points of special interest is analogous to a visit to Selfridge or some other self-contained emporium in search of underclothing, only to find oneself helplessly groping in the motor department !

The book, however, is the product of a very able, experienced, and clear thinker, who has a right to express himself in *ex-cathedra* statements upon the subject of his life-work. Dr. Mercier is always interesting when discussing the meaning of terms, whether insanity be a symptom or a disease, how it can best be classified, its relation to crime, the basis of conduct, and the psychological origin of abnormal actions and their relationship to the law.

The book contains several errors of printing, type, and orthography. "Dulness" is spelt throughout in the American fashion. "Agoraphobia" is Agorophobia, p. 112 ; "any" appears for "my," p. 341 ; "Armentarium" for Armamentarium, p. 81 ; "triponema," p. 264, for treponema, and a series of words seem to be meaningless "by and large," pp. 88 and 90. "Insanity" is commenced upside down, p. 300 ; "in" is used for "is," p. 18, line 12 ; and there are others.

Nevertheless, the book will be read by all students of psychiatry with appreciation, although certainly not with full agreement.

BOOKS RECEIVED

(*Publishers are requested to notify prices*)

(In the January number a review was published of *Interpretations and Forecasts*, by Victor Branford. The name of the Publisher was quoted as "H. K. Lewis" in error for "Duckworth & Co.")

Practical Field Botany. By A. R. Horwood, F.L.S., Member of the Ecological, Conchological and other Societies; Author of "Plant Life in the British Isles," etc. Illustrated with 20 Plates and 26 Figures in the text. London: Charles Griffin & Co., Ltd., Exeter Street, Strand, 1914. (Pp. xv + 193.) Price 5*s*. net.

The Chemistry of Colloids, and some Technical Applications. By W. W. Taylor, M.A., D.Sc., Lecturer in Chemistry at the University of Edinburgh. London: Edward Arnold, 1915. (Pp. viii + 328.) Price 7*s*. 6*d*. net.

A Course of Pure Mathematics. By G. H. Hardy, M.A., F.R.S., Fellow and Lecturer of Trinity College and Cayley Lecturer in Mathematics in the University of Cambridge. Second Edition. Cambridge: at the University Press, 1914. (Pp. xii + 442.) Price 12*s*. net.

A New Analysis of Plane Geometry, Finite and Differential. With Numerous Examples. By A. W. H. Thompson, B.A., sometime Scholar of Trinity College, Cambridge. Cambridge: at the University Press, 1914. (Pp. xvi + 120.) Price 1*s*. net.

Practical Physical Chemistry. By Alex. Findlay, M.A., Ph.D., D.Sc., Professor of Chemistry and Director of the Edward Davies Chemical Laboratories, University College of Wales, Aberystwyth. With 104 Figures in the text. Third Edition, Enlarged. Longmans, Green & Co., 39, Paternoster Row, London; Fourth Avenue and 30th Street, New York; Bombay, Calcutta, and Madras, 1914. (Pp. xvi + 327.) Price 4*s*. 6*d*. net.

The Extra Pharmacopœia of Martindale and Westcott. Revised by W. Harrison Martindale, Ph.D., F.C.S., and W. Wynn Westcott, M.B. (Lond.), D.P.H. Seventh Edition. In two volumes. London: H. K. Lewis, 136, Gower Street, W.C., 1915. (Pp. Vol. I. xl + 1113, Vol. II. viii + 469.) Prices Vol. I. 14*s*. net, Vol. II. 7*s*. net.

Science and Religion. By Seven Men of Science: Sir Oliver Joseph Lodge, F.R.S., D.Sc., LL.D., Prof. John Ambrose Fleming, M.A., D.Sc., F.R.S., Prof. W. B. Bottomley, M.A., Ph.D., F.L.S., F.C.S., Prof. Edward Hull, LL.D., F.R.S., John Allan Harker, D.Sc., F.R.S., Prof. G. Sims Woodhead, M.A., LL.D., M.D., F.R.C.P., F.R.S.E., Prof. Silvanus P. Thompson, B.A., M.D., LL.D., D.Sc., F.R.S., Speakers in Browning Hall during Science Week, 1914. With Portraits and a Suggestion from John Edward Stead, D.Sc., D.Met., F.R.S., F.I.C. London: W. A. Hammond, Holborn Hall, E.C. (Pp. 138.) Price 1*s*. net.

A Textbook of General Physics for College Students: Electricity, Electromagnetic Waves, and Sound. By J. A. Culler, Ph.D., Professor of Physics, Miami University: Author of "General Physics for Colleges: Mechanics and Heat." Philadelphia: J. B. Lippincott Co. (Pp. x + 321.) Price 7*s*. 6*d*. net.

American Permian Vertebrates. By Samuel W. Williston, Professor of Paleontology in the University of Chicago. The University of Chicago Press, Chicago, Illinois. With Illustrations and 38 Plates. (Pp. v + 145.) Price 10*s*. net.

Prehistoric Times, as Illustrated by Ancient Remains and the Manners and Customs of Modern Savages. By the late Rt. Hon Lord Avebury, D.C.I. (Oxon.), LL.D. (Cantab., Dubl. et Edin.), M.D. (Würzb), F R.S., V.P.L.S., F.G.S, F.Z.S., F.S A., F.E S., Trust. Brit. Mus., Assoc. Acad. Roy. des Sci. Brux., etc. Seventh Edition. Thoroughly Revised and Entirely Reset. London: Williams & Norgate, 14, Henrietta Street, Covent Garden, W.C., 1913. With Illustrations. (Pp. 623.) Price 10*s*. 6*d*. net.

Nerves. By David Fraser Harris, M.D., C.M., B.Sc. (Lond.), D.Sc. (Birm.), Professor of Physiology in the Dalhousie University, Halifax, Nova Scotia; Author of "The Functional Inertia of Living Matter," "National Degeneration," "Sleep." London: Williams & Norgate. (Pp. viii + 254.) Price 1*s*. net.

The Determination of Sex. By L. Doncaster, Sc.D., Fellow of King's College, Cambridge. Cambridge: at the University Press, 1914. With 22 Plates. (Pp. xi + 172.) Price 7*s*. 6*d*. net.

Roger Bacon: Essays Contributed by Various Writers on the Occasion of the Commemoration of the Seventh Centenary of his Birth. Collected and Edited by A. G. Little. Oxford: at the Clarendon Press, 1914. (Pp. viii + 425.) Price 16*s*. net.

Dante and the Early Astronomers. By M. A. Orr (Mrs. John Evershed). London: Gall & Inglis, 31, Henrietta Street, W.C., and Edinburgh. Illustrated. (Pp. xvi + 507.) Price 15*s*. net.

A History of the Indian Medical Service, 1600-1913. By Lieut-Colonel D. G. Crawford, Bengal Medical Service, Retired List. In two volumes. London: W. Thacker & Co., 2, Creed Lane, E.C. Calcutta and Simla: Thacker, Spink & Co., 1914. (Pp. Vol. I. xiv + 529, Vol. II. 535.) Price 28*s*. net.

An Examination of Some Recent Studies of the Inheritance Factor in Insanity. From *Biometrika*, a Journal for the Statistical Study of Biological Problems, Vol. X. Nos. 2 and 3, November 1914. By David Heron, D.Sc. Cambridge University Press. (Pp. 37.)

Our Knowledge of the External World as a Field for Scientific Method in Philosophy. By Bertrand Russell, M.A., F.R.S., Lecturer and Late Fellow of Trinity College, Cambridge. The Open Court Publishing Co., Chicago, 122, South Michigan Avenue; London: 149, Strand, W.C., 1914. (Pp. vii + 245) Price 7*s*. 6*d*. net.

Hazell's Annual for 1915. A Record of the Movements of the Times. Revised to November 25, 1914. Giving the Most Recent and Authoritative Information on the Topics of the Day. With Copious Index. Edited by T. A. Ingram, M.A., LL.D. London: Hazell, Watson & Viney, Ld. Thirtieth Year of Issue, 1915. (Pp lxix + 623) Price 3*s*. 6*d* net.

Not many of our annual books of reference condescend to pay much attention to scientific affairs, and we must therefore specially commend "Hazell's Annual" for its excellent abstracts on the Australian Meeting of the British Association last year, on Anthropology, Astronomy, Geography, Geology, and Medicine and Surgery. The "Annual" also even gives information regarding the Nobel Prizes—while another one we know mentions only the Peace Prize, and does not even refer to the scientific prizes, which some will think infinitely more important.

Mendelism and the Problem of Mental Defect. III. On the Graduated Character of Mental Defect and on the Need for Standardising Judgments as to the Grade of Social Inefficiency which shall Involve Segregation. Being a Lecture delivered at the Galton Laboratory, February 10, 1914. By Karl Pearson, F.R.S., Galton Professor. With Frontispiece and 23 Diagrams. London: Dulau & Co., Ltd., 37, Soho Square, W., 1914. (Pp. 51.) Price 2*s*. net.

Directions for a Practical Course in Chemical Physiology. By W. Cramer, PhD., D.Sc. Second Edition. Longmans, Green & Co., 39, Paternoster Row, London; New York, Bombay, and Calcutta, 1915. (Pp. viii + 102.) Price 3*s*. net.)

The Analysis of Sensations and the Relation of the Physical to the Psychical. By Dr. Ernst Mach, Emeritus Professor in the University of Vienna. Translated from the First German Edition by C. M. Williams. Revised and Supplemented from the Fifth German Edition by Sydney Waterlow, M.A. Chicago and London: The Open Court Publishing Co., 1914. (Pp. xiv + 380.) Price 6*s*. 6*d*. net.

The Differential Essence of Religion. By Theodore Schroeder. Reprinted from the *New York Truth Seeker*, October 31 and November 7 and 14, 1914. (Pp. 28.)

A Statistical Study of American Men of Science. By J. McKeen Cattell, Professor of Psychology, Columbia University. Reprinted from *Science*, N.S., vol. xxiv. No. 621, pp. 658-65, November 23; No. 622, pp. 699-707, November 30; No. 623, pp. 732-42, December 7, 1906; and under the title, "A Further Statistical Study of American Men of Science," vol. xxxii. No. 827, pp. 633-48, November 4; No. 828, pp. 672-88, 1910. (Pp 59.)

Science and Education. Vol. iii. University Control. A Series of Volumes for the Promotion of Scientific Research and Education Progress. Edited by J. McKeen Cattell, Professor of Psychology, Columbia University. Together with a Series of 299 Unsigned Letters by Leading Men of Science holding Academic Positions, and Articles by J. Jastrow, G. T. Ladd, J. McKeen Cattell, and others. The Science Press, New York and Garrison, N.Y., 1913. (Pp. viii + 484.)

The Principle of Relativity in the Light of the Philosophy of Science. By Paul Carus. With an Appendix containing a Letter from the Rev. James Bradley on the Motion of the Fixed Stars, 1727. Chicago: The Open Court Publishing Co., 1913. (Pp. 105.) Price 4*s*. net.

The Mechanistic Principle and the Non-Mechanical. An Inquiry into Fundamentals, with Extracts from Representatives of Either Side. By Paul Carus. Chicago: The Open Court Publishing Co., 1913. (Pp. 125.) Price 4*s*. net.

Dialogues Concerning Two New Sciences. By Galileo Galilei. Translated from the Italian and Latin into English by Henry Crew and Alfonso de Salvio, of Northwestern University. With an Introduction by Antonio Favaro, of the University of Padua. New York: The Macmillan Co., 1914. (Pp. xxi + 300.) Price 8*s*. 6*d*. net.

All About Leaves. By the late Francis George Heath, Author of "Nervation of Plants," "Our Woodland Trees," "The Fern World," etc. With 80 Photographs from Nature and 4 Coloured Plates from Drawings by Miss M. Schroeder. London: Williams & Norgate, 14, Henrietta Street, Covent Garden, 1914. (Pp. ix + 228.) Price 4*s*. 6*d*. net.

The Principle of Relativity. By E. Cunningham, M.A., Fellow and Lecturer of St. John's College, Cambridge. Cambridge: at the University Press, 1914. (Pp. xiv + 221.) Price 9*s.* net.

What is Adaptation? By R. E. Lloyd, M.B., D.Sc. (Lond.), Major, Indian Medical Service; Professor of Biology, Medical College, Calcutta. Longmans, Green & Co., 39, Paternoster Row, London; Fourth Avenue and 30th Street, New York; and Bombay, Calcutta, and Madras, 1914. (Pp. xi + 110.) Price 2*s.* 6*d.* net.

The Year-Book of the Scientific and Learned Societies of Great Britain and Ireland. A Record of the Work done in Science, Literature, and Art during the Session 1913–14, by numerous Societies and Government Institutions. Compiled from Official Sources. Thirty-first Annual Issue. London: Charles Griffin & Co., Ltd., Exeter Street, Strand, 1914. (Pp. vi + 376.) Price 7*s.* 6*d.* net.

The Mirror of Perception. By Leonard Hall, M.A. London and Redhill: Love & Malcomson, Ltd., 1914. (Pp. 29.)

The Indian Museum, 1814–1914. With 11 Illustrations. Calcutta: Published by the Trustees of the Indian Museum and Printed at the Baptist Mission Press, 1914. (Pp. xi + 136 + lxxxi.)

Algebraic Invariants. Mathematical Monographs, No. 14. Edited by Mansfield Merriman and Robert S. Woodford. By Leonard Eugene Dickson, Professor of Mathematics in the University of Chicago. First Edition. First Thousand. New York: John Wiley & Sons. London: Chapman & Hall, Ltd., 1914. (Pp. x + 100.) Price 5*s.* 6*d.* net.

A Catalogue of Current Mathematical Journals, etc., with the Names of the Libraries in which they may be found. Compiled for the Mathematical Association. London: G. Bell & Sons, Ltd., Portugal Street, Kingsway, and Bombay, 1913. (Pp. 39.) Price 2*s.* 6*d.*

ANNOUNCEMENTS

MEETINGS OF SOCIETIES

Royal Society. Ordinary Meetings, 4.30 p.m., April 22, 29, May 6, 13, 20, June 3, 10, 17, 24. Election of Fellows, 4 p.m., May 6.

Royal Meteorological Society. Meetings, 7.30 p.m., April 21; 4.30 p.m., May 19, June 16.

Institution of Mechanical Engineers. General Meeting, 8 p.m., April 23.

Chemical Society. Meetings, 8.30 p.m., May 6, 20, June 3, 17.

Physical Society. Meetings, 5 p.m., April 23; 8 p.m., May 14; 5 p.m., May 28; 8 p.m., June 11; 5 p.m., June 25.

Optical Society. Meetings, 8 p.m., April 15, May 13, June 10.

Zoological Society. Meetings, 5.30 p.m., April 13, 27, May 11, 25, June 8.

Sociological Society. Meeting, 5.15 p.m., April 27.

NEW FELLOWS OF THE ROYAL SOCIETY

The Council of the Royal Society recommended the following for election during 1915 as Fellows :—

Prof. Frederick William Andrewes
Prof. Arthur William Conway
Mr. Leonard Doncaster
Mr John Evershed
Dr. Walter Morley Fletcher
Prof. Arthur George Green
Mr. Henry Hubert Hayden
Dr. James Mackenzie
Prof. John Cunningham McLennan
Dr. Arthur Thomas Masterman
Prof. Gilbert Thomas Morgan
Dr. Charles Samuel Myers
Mr. George Clarke Simpson
Mr. Alan A. Campbell Swinton
Mr. Arthur George Tansley

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

NOTICE

THE EMOLUMENTS OF SCIENTIFIC WORKERS

It is proposed to undertake an inquiry regarding the pay, position, tenure of appointments, and pensions of scientific workers and teachers in this country, and the Colonies. The Editor will therefore be much obliged if all workers and teachers who hold such appointments, temporary or permanent, paid or unpaid, will give him the necessary information suggested below. The figures will be published only in a collective form, and without reference to the names of correspondents, unless they expressly wish their names to be published. The Editor reserves the right not to publish any facts communicated to him. Workers who are conducting unpaid private investigations must not be included. The required information should be sent as soon as possible, and should be placed under the following headings :

(1) Full name
(2) Date of birth. Whether married. Number of family living
(3) Qualifications, diplomas, and degrees
(4) Titles and honorary degrees
(5) Appointments held in the past
(6) Appointments now held, with actual salary, allowances, fees, and expected rises, if any. Whether work is whole time or not
(7) Body under which appointment is held
(8) Conditions and length of tenure
(9) Pension, if any, with conditions
(10) Insurance against injury, if any, paid by employers
(11) Family pensions, if any
(12) Remarks

Scientific workers may also write to *The Secretary*, *British Science Guild*, 199 Piccadilly, for the Circulars issued by the Guild asking for information on cognate points.

SCIENCE PROGRESS

IN THE TWENTIETH CENTURY

A QUARTERLY JOURNAL OF SCIENTIFIC WORK AND THOUGHT

NO. 33. JULY 1914

CONTENTS

EDITOR

SIR RONALD ROSS, K.C.B., F.R.S., N.L., D.Sc., LL.D., M.D., F.R.C.S.

LONDON

JOHN MURRAY, ALBEMARLE STREET, W.

Price 5/- net

VOL. IX

Bausch and Lomb MICROSCOPES

Over 95,000 have been sold and are in use all over the World.

B H 6

The Instruments illustrated here are our New Models, as supplied to H.M. Government Departments, the Medical Schools and Colleges in Great Britain, India, Australia, Africa, etc. They have many special features which particularly commend them for laboratory use, viz.: solid construction, accuracy of all details, large stage (will take full size Petri dish) completely covered with vulcanite (patent), protecting the metal from damage by stains and reagents. They may be used at any angle, having inclination joint giving a movement of 90°.

F F 8

Catalogue No.	Objectives.			Eye-pieces.		Nosepieces.	Abbe Condenser.	Prices.		
	Dry.		Oil Immersion.					£	s.	d.
BH 1	16 mm.	4 mm.	—	7·5 ×			—	5	14	6
BH 2	16 mm.	4 mm.	—	7·5 ×		Circular Double	—	6	9	6
BH 3	16 mm.	4 mm.	—	5 ×	10 ×		—	5	19	6
BH 4	16 mm.	4 mm.	—	5 ×	10 ×	Circular Double	—	6	14	6
BH 6	16 mm.	4 mm.	—	5 ×	10 ×	,, ,,	1·20 N.A.	8	4	6
BH 8	16 mm.	4 mm.	1·9 mm.	5 ×	10 ×	Circular Triple	1·20 N.A.	13	9	6

Price of F Models, outfits 1, 2, 3, and 4, same as BH Models.

FF 6 (Outfit as BH 6) **£7 15 0**

FF 8 (,, ,, BH 8) **13 0 0**

Attachable Mechanical Stage, No. 2114, **£3 3 9** extra.

Inspection cordially invited at our New Showrooms, or Descriptive :: Price List "A. 5" (Microscopes) sent post free. ::

Also particulars of Microtomes, Photo-Micrographic and Projection Apparatus, Centrifuges, Ganong's Apparatus for Plant Physiology, Viscosimeters, Precision Laboratory Glassware, &c., on application.

BAUSCH & LOMB OPTICAL CO. ROCHESTER, N.Y. B-L Z S

Trade Mark.

BAUSCH & LOMB OPTICAL CO.,

37-38 Hatton Garden, London, E.C.

Or through all Dealers.

Telephone: Holborn 2640.

Telegrams: "Optibalop, London."

BAUSCH & LOMB OPTICAL CO. ROCHESTER, N.Y. B-L Z S

Trade Mark.

SCIENCE PROGRESS

IN THE TWENTIETH CENTURY

A QUARTERLY JOURNAL OF
SCIENTIFIC WORK AND THOUGHT

NO. 34. OCTOBER 1914

CONTENTS

EDITOR

SIR RONALD ROSS, K.C.B., F.R.S., N.L.,
D.Sc., LL.D., M.D., F.R.C.S.

LONDON

JOHN MURRAY, ALBEMARLE STREET, W.

Price 5/- net

VOL. IX

NEW MODEL

FS and FFS

Bausch and Lomb

MICROSCOPE

WITH SAFETY SIDE FINE ADJUSTMENT.

FFS MODEL.

BH MODEL.

Catalogue No.	Objectives. Dry.		Oil Immersion.	Eye-pieces.	Nosepieces.	Abbe Condenser.	Price. £ s. d.	Prices of BH, F and FF Models. Outfits as same Nos. in FS and FFS.	£ s. d.		£ s. d.
FS 1	16 mm	4 mm	—	7·5×		—	6 4 6	BH 1	5 14 6	F 1	5 14 6
FS 2	16 mm	4 mm	—	7·5×	Circular Double	—	6 19 6	BH 2	6 9 6	F 2	6 9 6
FS 3	16 mm	4 mm	—	5× 10×		—	6 9 6	BH 3	5 19 6	F 3	5 19 6
FS 4	16 mm	4 mm	—	5× 10×	Circular Double	—	7 4 6	BH 4	6 14 6	F 4	6 14 6
FFS 6	16 mm	4 mm	—	5× 10×	" "	1·20 N.A.	8 5 0	BH 6	8 4 6	FF 6	7 15 0
FFS 8	16 mm	4 mm	1·9 mm	5× 10×	Circular Triple	1·20 N.A	13 10 0	BH 8	13 9 6	FF 8	13 0 0

NEW ATTACHABLE MECHANICAL STAGE, No. 2116, £3 0 0

We have sold over 98,000 Microscopes, which are in use all over the world.

Inspection invited at our New Showrooms, or List "A.S. 5" sent post free.

N.B.—All our instruments being made at our own factory in Rochester, N.Y., there will be no delay in delivery, and we have just received large stocks.

TRADE MARK. **BAUSCH & LOMB OPTICAL CO., 37-38, Hatton Garden, London, E.C.** OR THROUGH ALL DEALERS. TRADE MARK.

SCIENCE PROGRESS
IN THE TWENTIETH CENTURY
A QUARTERLY JOURNAL OF SCIENTIFIC WORK AND THOUGHT

NO. 35. JANUARY 1915

CONTENTS

EDITOR

SIR RONALD ROSS, K.C.B., F.R.S., N.L., D.Sc., LL.D., M.D., F.R.C.S.

LONDON
JOHN MURRAY, ALBEMARLE STREET, W.

Price 5/- net

VOL. IX

SCIENCE PROGRESS
IN THE TWENTIETH CENTURY
A QUARTERLY JOURNAL OF SCIENTIFIC WORK AND THOUGHT

NO. 36. APRIL 1915

CONTENTS

EDITOR

SIR RONALD ROSS, K.C.B., F.R.S., N.L., D.Sc., LL.D., M.D., F.R.C.S.

LONDON

JOHN MURRAY, ALBEMARLE STREET, W.

Price 5/- net

VOL. IX