THURSDAY, OCTOBER 25, 1888.

EMPIRICISM VERSUS SCIENCE.

THERE is among the general public a perennial tendency to exalt and honour the man of affairs—the man whose business it is to pose as figurehead and carry through great schemes in the face of the community—at the expense of the quiet student or the scientific pioneer. And every now and then this permanent tendency is played upon by someone who ought to know better, and excited into more conspicuous vitality; sometimes taking the form of a demonstration in favour of "practice" as opposed to "theory," sometimes the form of a flow of ribaldry against scientific methods and results. Such a periodical outburst seems to have broken loose just now, and the technical press is full of scoffs at men of science, and glorification of the principle of rule-of-thumb.

It is easy for students of science to smile at the absurdities thus propounded and to take no further notice. It is only statements which have a germ of truth about them that are able really to bite and sting. And if a feeling of momentary irritation is excited by reading some piece of extra absurdity set forth for the unedification and misleading of the public, the best antidote is a return to one's own work, and silence.

It is possible, however, sometimes to carry complaisance too far. "If you make yourself a sheep," was one of Franklin's mottoes, "the wolves will eat you"; and there is sound worldly wisdom in the maxim, though it may be difficult always to reconcile it with some other precepts of a higher authority.

The only really irritating thing about these attacks is that they do not call things by their right names: if they did, the absurdity would be too glaring for anyone of importance to be taken in. So they sing the praises of empiricism and decry science under the totally false and misleading names of "practice" and "theory" respectively. Now plainly there is no real antithesis possible between theory and practice unless one is right and the other wrong or incomplete. If both are right, they must agree. If one is conspicuously right and the other conspicuously wrong, it is a very cheap and simple matter to distribute praise and blame.

Whenever there is discordance between theory and practice—a theory which says how a thing ought to be done, and the practice by which its doing has hitherto been attempted—manifestly there is something wrong with one or other of them. The blame should be applied to the error, and the error may lie equally well on either side. The practice in early steam-engines was to cool the cylinder at every stroke in order to condense the steam. It certainly did condense the steam, and was therefore successful. The self-styled "practical man" of that day would most likely have derided any small-scale laboratory experiments as futile and ridiculous, and not corresponding to the conditions of actual work. Nevertheless, that eminent theorist, James Watt, by studying the behaviour of saturated steam under various circumstances in a scientific manner, and by discovering that the pressure in any connected system of vessels containing vapour would rapidly become equal to the vapour-tension corresponding to the coldest, did succeed in introducing a noteworthy improvement into a time-honoured practice. Again, the question of the specific heat of saturated steam, whether it be zero, or positive, or negative, is a highly scientific question, first solved on the side of theory by Clausius, an eminent example of the purely scientific worker; but the fact that it is negative has an immediate practical bearing on the important subject of steam-jacketing, and fully explains the advantage of that process.

But it may be said the advantage of the steam-jacket was discovered by experience. Very likely. It is a conspicuous and satisfactory fact that progress can be made in two distinct ways. Sometimes the improvement is discovered by what may be termed blindfold experience: a certain operation turns out to be uniformly successful, and, without any further knowledge, that is sufficient justification of its performance. The observed fact that inhalations of chloroform produced temporary anæsthesia was sufficient justification of its use in surgery without any theory as to why it so acted. The motion of the planets in ellipses, according to certain laws, might have been deduced from the theory of gravitation; but historically those motions were deduced by a laborious comparison of observations. Sometimes observation is ahead of theory; sometimes theory is ahead of observation. It is mere nonsense to decry either on that account.

It is also absurd to deny that our knowledge of a fact, and our confidence in its use, and of all the conditions under which it may be used or may not be used, are enormously enhanced when one knows not only the bare fact by observation empirically, but when also one thoroughly understands the reasons and the laws connected with it. It would be justifiable to employ a successful drug even if one knew nothing of its mode of action, and could give no reason for its effects; but it is far more satisfactory to understand it exactly, and to have a complete theory of its physiological action. One can then decide beforehand, without empiricism, or a possibly fatal experiment, under what circumstances and to what constitutions it would be noxious.

The fact that lightning-conductors are often successful is ample justification for their use, but it will be far more satisfactory when, by help of laboratory experiments and theory, one understands all the laws of great electrical discharges, and can provide with security against their vagaries.

These things are truisms, but it would seem to be sometimes necessary to utter truisms.

Sometimes one hears a judgment such as this: "Yes, he is a very good man in some ways, but he is too much of a theorist." And then there is a sapient shaking of heads, as if the term "theorist" were an intelligible term of abuse. You suppose it means that the wretched man knows too much about the mode of working of things; too much about the strength of materials, too much about graphical statics, if he is engaged in building a bridge; but if you ask the meaning of the fatal term, you find it explained in some such way as that "he does not attend to details," or "he does not look after his workmen," or "he accepts rotten materials." Then why not apply some term which shall legitimately mean these things, such as careless, or lazy, or ignorant, or unbusiness-like? Probably the word "theorist" as a term of abuse is meant to euphemistically imply all these things. If so, it is a foolish euphemism.

There are certain notable theorists who are so eminent that no one is willing to stultify himself by abusing them; and inasmuch as the superabundant energy of some of these men often leads them occasionally out of their main pursuits into alien fields of activity, wherein nevertheless they frequently shine as the equals or superiors of smaller men whose life-work lies in the same fields, it is becoming customary to ingeniously attempt to exclude them from the class it is wished to denounce, and to include them in the circle wherein they are comparatively amateurs or dabblers.

At the recent meeting of the British Association the old joke was repeated about claiming Sir William Thomson as an electrical engineer instead of a physicist and mathematician. This is all very well as a joke, but the British public is too apt to take these things in sober earnest. The range of activity of a pre-eminently great man is frequently not a narrow one, and he is extremely likely to shine in whatever he takes up, even if it be only as a pastime, or as relief from more serious work. Sir Isaac Newton made an excellent Master of the Mint. Perhaps therefore, in his day, City men claimed him as essentially one of themselves. Sir William Thomson has amused himself with navigation, as well as with electrical engineering.

This outcry against theory is becoming absurd. It used to be confined to the conclusions of mathematics. It is indeed still rampant there, but it is being extended also to conclusions deduced in the laboratory. Everything done in the laboratory or the study is looked at with suspicion. The right place to study the laws of steam-engines is on a locomotive. The right place to study marine engineering is in the hold of a steamship. The only place to study lightning is in a thunderstorm.

Give out these plausible fallacies with a certain unction to a British audience, and you will evoke "loud applause." It is so easy to evoke loud applause by talking pernicious but plausible nonsense. Your British audience hates to think, and likes to have its stupidity tickled by some after-dinner sentiment, which makes it feel that, after all, no one really knows anything about anything; that whoever professes to understand a subject theoretically is *ipso facto* a quack; and that the only difference between itself and everybody else is that some people cloak their ignorance under a show of learning and mathematical formulæ. These humbugging theorists may therefore be cheaply derided. "There is a lot of arrant humbug stowed away now and then under a mathematical cloak," said a technical paper the other day.

And what of the "practical" man? Any man who talks sense and goes to the bottom of things, so as to really understand and to be able to explain what he means and how things are, is essentially a practical man. One class has no right to monopolize this adjective. A mathematician may make statements according completely with facts and phenomena, and leading to the most complete understanding of every-day truths. An empiric may utter the most glaring absurdities, utterly out of harmony with anything in heaven or earth, or under the earth. Is Prof. Stokes therefore to be styled unpractical, and Prof. (shall we say) Pepper practical?

Push the matter to an extreme, and you can enunciate sentences like these. If you want to know about steam-engines and compound locomotives, you must go, not to theorists like Rankine, or Unwin, or Cotterill, or even to Mr. Webb. The driver of the Scotch express is the man really able to give you trustworthy and practical information.

If you want to know the principles underlying the construction of ships, and why some ships go quicker than others, do not think of applying to the writings of the late William Froude with his nonsensical paraffin toys, but consult the captain of the *Umbria* or the *City of Rome*.

We have set down these sentences as a *reductio ad absurdum* of some of the claims set forth in favour of empiricism as against science, under the specious and plausible heading of practice against theory: but really they are not a whit more absurd than much that is seriously argued; and were they propounded under favourable auspices to an average British audience, they would very likely be swallowed without nausea. The experiment is almost worth trying, only it would be difficult for anyone himself faithless to avoid some suspicion of irony, which would be fatal to success.

Space may be afforded for a few more very brief extracts from some of the engineering and technical journals during the past month. The first is so choice as to need no comment:—"The world owes next to nothing to the man of pure science. . . . . The engineer, and the engineer alone, is the great civilizer. The man of science follows in his train." This doctrine is explained and illustrated by insistence on the futility of Faraday's work in connection with magneto-electricity, until taken up and realized by the practical man.

In the same paper, a week later, occurs the following:—"No one knows anything with certainty about lightning outside of the common knowledge possessed by most fairly educated people." And again, "We fail to see that what is true in the laboratory must be true out of doors."

This is interesting as an almost exact reproduction of one of the historic objections made to Galileo's unwelcome discovery of Jupiter's satellites. It was then similarly maintained that, though the telescope was all very well for terrestrial objects, it was quite misleading when applied to the heavens.

An instance of a converse proposition is told in a recent popular work on astronomy (is it Sir R. Ball's?), about a farmer and amateur astronomer, who came to the writer with a revolutionary system of astronomy, based upon a number of observations which he had taken with a sextant of the altitude of the heavenly bodies. The gentleman had thus found that the generally received opinion about the distances of the fixed stars was extremely erroneous. But on inquiry it turned out that his altitudes were all calculated on the common-sense and well-known fact that sixty-nine miles make a degree. Finding it impossible to get the gentleman to put his mind into an attitude for receiving any instruction on the theoretical subject of the measurement of angles, the representative of the orthodox clique who impose their statements on the world as something more trustworthy than common information prevailed on the gentleman to apply his sextant to determine the altitude of his own barn. This *reductio ad absurdum* was avoided, however, and the overthrow of orthodox astronomy successfully maintained, by the hoped-for convert "failing to see that an astronomical instrument had any application whatever to terrestrial objects."

A paragraph recently inserted in an electro-technical journal, with editorial sanction, styles mathematicians "the accountants of science," and goes on in a tone less comic than bitter:—"When some young shaver shoots off his school learning" (*i.e.* uses some mathematical operation or notation), "I feel inclined to reply to him in Italian, as both are as generally and completely understood in the Society of ——." Now if the subject under discussion were, say, passages in Tasso or Dante, an Italian quotation would be very natural, and persons ignorant of the language would hardly be invited, or indeed anxious, to express an opinion. Is it not equally clear that when the subject-matter is numerical magnitude and quantity, the appropriate language may sometimes have to be used?

It has always been customary, as we have before remarked, for the empiric to feel some hostility to the mathematician, especially to the mathematician who endeavours to apply his powerful and beautiful machinery to the elucidation of the facts of Nature. But only recently has it become the fashion to extend the same attitude of mistrust and dislike to the experimental worker in a laboratory. Both these hostilities probably have their root in an instinct of self-protection. Without them the empiric would be constantly suffering wounds in his self-esteem, and might lose confidence in his faith as to the universal prevalence of ignorance and the advantages of rule-of-thumb. For a man of the world professing a certain science to have to recognize a certain number of minds as immeasurably superior to his own, and their conclusions in that very science as being almost certainly correct, although flatly opposed to his own instinct and traditions: this is in many cases intolerable. He cannot away with these great theorists, neither can he in his heart contemn them; but he can do his best to deceive himself and others by extending to them euphemistic terms of abuse, and by pretending that he could do all that they do if only he thought it worth while. He may even go further, and flinging abroad a universal accusation of ignorance will easily delude a gullible public into the belief that knowledge is after all only a matter of opinion, and that what one man says is quite as good as what is said by another.

And in this procedure he is fairly secure against any retaliation from the great men. They are deeply and painfully conscious of ignorance in one sense: their knowledge sits lightly upon them; and when broadside and grotesque accusations of ignorance are hurled at them with the intention of putting them on a level with the uninstructed and, in quite another sense, "ignorant" populace, they resent it not; scarcely recognizing, indeed, the absurdity of the position.

The hostility of the "practical man" for the systematic and recondite methods of science was at one time mainly borne by mathematicians, because they it was mainly who spoke a language and thought thoughts too high for common apprehension. Since then experiment has become more exact, more illuminated by theory, more scientific and less empirical; hence it is that the hostility is now being extended to the experimentalist in his laboratory as well.

But really, it may be rather offensively suggested, what other attitude can be taken up? If a man is to be capable of getting schemes through Parliament, of impressing a jury, and generally of playing to the gallery and becoming a power in the State, he cannot, unless very exceptionally endowed, have the aptitudes and powers proper to a man of high science. And yet it will never do to allow even to himself that the scientific man is in his own line immeasurably above him. Such a reverent and submissive attitude would ruin his chance with the gallery at once. Swagger and a confident front are more than the tricks of the trade, they are the essentials to success.

We are glad to recognize, however, that the recent outburst against the methods and conclusions of pure science is the work of the camp-followers rather than of the leaders on the commercial side. There have been and are several conspicuous examples not only of the scientific man taking a high position on the commercial side, but also of the commercial man taking a high position in the ranks of pure science. This interchange of individuals, and the further *rapprochement* which the great extension of science into industrial life of various kinds has caused, and must in the future still further cause, are making it now clearly recognized how intimately pure science and the commercial applications of science are connected together, how great is their mutual dependence on each other, and how essential to the well-being of each is a close and friendly co-operation with the other.

These facts, and the friendly attitude of the leaders on both sides, render the attempt made in the rank and file to sow discord between the two great classes the more absurd, and must make it in the long run entirely futile.

THE MESOZOIC MAMMALIA.

The Structure and Classification of the Mesozoic Mammalia. By H. F. Osborn. *Journ. Ac. Nat. Sci. Philadelphia*, Vol. IX. No. 2. (Philadelphia: Published by the Academy, 1888.)

IN the elaborate memoir before us, comprising eighty quarto pages of text, illustrated by thirty woodcuts and two plates, Prof. Osborn, of Princeton College, New Jersey, gives us the result of his researches into the structure of the Mesozoic and allied Tertiary Mammals, based upon observations carried on both in America and Europe. As a rule, these Mammals are of small size, and are mainly known to us by more or less imperfect jaws and teeth; by far the greater number of specimens consisting of the lower jaw or mandible. Now, it is well known that even in groups of the smaller Mammals which are well represented at the present day, such as the Shrews among the Insectivora, or the Bats, it is almost, if not quite, impossible to recognize many of the genera, to say nothing of the species, when we have to deal only with a series of fossil or sub-fossil lower jaws from the cavern or later Tertiary deposits. And if this be so in groups with which we are well acquainted, the difficulty is of course increased many times over when we have to deal with forms having no close analogues among the existing fauna. The puzzle is further increased by the difficulty of referring such portions of upper jaws as are more rarely found to the species indicated by mandibles;

and this induces a great danger of founding species or higher groups upon the evidence of upper jaws, which cannot be decisively shown to be distinct from those founded upon the evidence of the mandibles. Prof. Osborn, as will be noticed below, has not altogether steered clear of this danger; and we consider it would be advisable in delicate researches of this nature to lay down a rule that family or higher groups should only be formed upon the evidence of homologous parts, even if genera and species have been named upon the evidence of dissimilar parts of the skeleton.

Before, however, proceeding to any detailed criticism, it will be advisable to take a brief survey of the memoir before us, and to note the scheme of classification which is proposed. The memoir begins with a survey of previous work on the subject, especial attention being directed to the labours of Sir Richard Owen in Europe, and to those of Profs. Cope and Marsh in America. On the second page (187) a table is given of all the described genera of Mesozoic Mammals, which include forms from Europe, America, and South Africa; together with certain allied Tertiary genera from North America and France, and *Thylacoleo* of the Pleistocene of Australia. We may add that since this memoir was sent to press, forms allied to those of the North American Eocene have been described by Señor Ameghino in the Tertiaries of the Argentine Republic. The next section is devoted to a detailed description of the British forms, in which certain generic terms, proposed by the author in a preliminary communication, are fully described and illustrated. We may here mention that the author tells us that the process of passing his memoir through the press occupied an unusually long period, during which certain other memoirs appeared on the subject; and that he thus saw occasion to modify in some respects several statements made in the earlier part of the work, footnotes being usually appended to this effect.

After the descriptive portion we come to what is really the most important section of the whole memoir—namely, that headed the classification and zoological relationships of the Mesozoic Mammalia. It is here observed that these forms may be divided into two large groups. "In the first group, *A*, one of the incisors is greatly developed at the expense of the others, and of the canine, which usually disappears; behind these teeth is a diastema of varying width, followed by premolars which are subject to great variation in form and number, while the molars bear tubercles. In the second group, *B*, the incisors are small and numerous, the canine is always present and well developed; the teeth usually form a continuous series, and the molars bear cusps instead of tubercles." These two groups are compared to the Diprotodontia and Polyprotodontia, among existing Marsupials, and the following scheme of classification is proposed:—

A. First Group.

I. Sub-order **Multituberculata**.

1. Family PLAGIAULACIDÆ.—*Microlestes, Plagiaulax, Ctenacodon, Ptilodus, Neoplagiaulax, Meniscoessus,* and perhaps *Thylacoleo.*
2. Family BOLODONTIDÆ. — *Bolodon, Allodon,* and perhaps *Chirox.*
3. Family TRITYLODONTIDÆ.—*Tritylodon, Triglyphus,*
4. Family POLYMASTODONTIDÆ.—*Polymastodon.*

Incertæ sedis—Chirox.

B. Second Group.

I. Order **Protodonta.**

Family DROMATHERIIDÆ.—*Dromatherium, Microconodon.*

II. Sub-order **Prodidelphia.**

1. Family TRICONODONTIDÆ.—*Amphilestes, Amphitylus, Triconodon, Priacodon, Phascolotherium, Tinodon, Spalacotherium, Menacodon.*
2. Family AMPHITHERIIDÆ.—*Amphitherium, Dicrocynodon* (*Diplocynodon*), *Docodon, Enneodon, Peramus.*
3. Family PERALESTIDÆ. — *Peralestes, Peraspalax, Paurodon.*
4. Family KURTODONTIDÆ.—*Kurtodon.*

III. Sub-order **Insectivora Primitiva.**

1. Family AMBLOTHERIIDÆ.—*Amblotherium, Achyrodon.*
2. Family STYLACODONTIDÆ.—*Stylacodon, Phascolestes, Dryolestes, Asthenodon.*

Incertæ sedis—Laodon.

The Multituberculata, excluding *Thylacoleo,* extend in time in Europe and North America from the Upper Trias to the Lower Eocene, but the recently discovered South American forms may be of later age. In discussing the relationship of this group of families on p. 212, the author states that, admitting their Marsupial relationship, it is clear that the genera "are closely related to each other, and widely separated from the Diprotodontia by their dental structure, which is very dissimilar, and indicates that they probably branched off from the stem of the recent Marsupials at a remote period, probably the Triassic." They are accordingly regarded on the following page as a sub-order of Marsupials, characterized by the tuberculated characters of their molars. If, however, as suggested on p. 214, *Thylacoleo,* which is evidently only an aberrant and specialized Phalanger, has any sort of relationship to the *Plagiaulacidæ,* then it will be evident that this group cannot be even subordinately separated from the Diprotodonts Further observations upon the relationships of this group are given upon pp. 251 and 254, the latter section having evidently been written subsequently to the earlier sections. On the former page evidence is adduced to show that in some of these forms the first upper incisor has been lost, and the second becomes hypertrophied, whereas in existing Marsupials it is the first which always persists and becomes enlarged. There is no evidence as to the serial homology of the lower incisor. On p. 254 and the following pages, the suggestion of Prof. Cope, based on the resemblance of the molars of the Multituberculata to the aborted teeth of *Ornithorhynchus,* that these forms may be Monotremes, is discussed at some length, but without any definite conclusion being reached. We presume, however, that in writing this part of the memoir the author had come to the conclusion that the relationship of these forms to *Thylacoleo* is altogether a myth. It is, however, at first sight not very easy to believe that the general similarity in the structure of the cutting fourth premolar in the Multituberculata and the modern Diprotodontia is not indicative of a real affinity between the two; and as to the argument that the peculiar structure of the two molars is of itself sufficient to indicate the subordinal distinction of the Multituberculata, we think that a sub-order which contains such different types of molar dentition as are shown by *Macropus, Pseudochirus, Phas-*

colarctus, and *Phascolomys*, could surely also find room for the Multituberculate type. The evidence of the homology of the incisors is, however, a weighty one in the author's favour.

Prof. Osborn places the Triassic *Microlestes* with the *Plagiaulacidæ* rather than the *Bolodontidæ*, but we think the existence of a cutting fourth lower premolar ought to be proved before this view can be definitely admitted. There may also be considerable hesitation in accepting the view expressed on p. 217, that there are five premolars in the upper jaw assigned by Prof. Marsh to *Ctenacodon;* but beyond these and other small points the author's classification of this group appears to commend itself.

We cannot say the same in regard to the classification of the second group, which, as we have seen, it is proposed to split up into one distinct order, into one sub-order provisionally referred to the Marsupialia, and a second assigned with more hesitation to the Insectivora. In this group the author has, we venture to think, found differences which, if they exist at all, are by no means of the importance he attributes to them ; while at least one case occurs to us, where, to say the least, there is a considerable presumption that specimens assigned to the two sub-orders may really be referable to a single genus. Sufficient account does not, indeed, appear to have been taken of the variation in the dentition of different recent genera of Marsupials which are usually included in a single family; as, for example, *Thylacinus*, *Dasyurus*, and *Myrmecobius* among the Polyprotodonts, and *Phalanger*, *Pseudochirus*, and *Phascolarctus* in the Diprotodonts. In the case of obscure fossil forms like the present, it appears to us that there ought to be the greatest hesitation in making groups of higher value than family rank; and that even in the case of families their limits ought to be much more loosely drawn than among existing forms, where we have full evidence before us. It is, indeed, far more advantageous to keep all such obscure forms more or less closely associated until absolutely decisive evidence is forthcoming as to their right to wide separation. In the present instance, however, the author has, to put it in the mildest form, by no means adduced any such decisive evidence ; while, as already mentioned, there is a strong presumption that in certain particular cases he has widely separated closely allied, if not absolutely identical, forms.

The first so-called order—the Protodonta—is formed for the reception of the American Triassic *Dromatherium* [1] and *Microconodon;* if, indeed, the latter be really entitled to generic distinction. The grounds for the ordinal distinction of these forms are that the roots of the cheek-teeth are not fully divided; but stronger evidence than this is required before these obscure forms can be definitely regarded as entitled to constitute more than a family. And even if they belong to an order distinct from the Marsupials, there is no evidence to show that they are not Monotremes, or perhaps rather Prototheria.

The sub-order Prodidelphia is defined as including primitive Marsupials, generally characterized by the presence of four premolars and numerous molars, the latter having distinctly divided roots. It is, however, added (on p. 259) that "no definite sub-ordinal character can be assigned ; but in view of the retention of several features, and of their ancestral position, these Mammals may be distinguished from the recent Marsupials as the sub-order Prodidelphia." In our own judgment, the formation of a large group which confessedly cannot be distinguished from one already established is unjustifiable, and not conducive to any advantage. The first family of this group is the *Triconodontidæ*, in which, as shown above, our author includes a large number of genera. The genus *Triconodon*, together with the allied or identical American *Priacodon*, has, however, such a totally different *facies* from all the other forms, that we are inclined to follow Prof. Marsh in regarding it as alone constituting the family. We are, morever, rather at a loss to find the value of the characters which Prof. Osborn regards as distinctive of the enlarged family; for, whereas he states in the definition of the family (on p. 227) that the "condyle is low," on the opposite page the genus *Amphitylus* is described as having the "condyle lofty." Some very interesting observations are recorded (p. 198) as to the changing and development of the teeth in *Triconodon*, in which it is concluded, as had been previously indicated by Mr. O. Thomas, that the replacement was limited, as in modern Marsupials, to a single premolar; while it is further shown that in many instances it appears probable that the last true molar was never developed. In classing *Phascolotherium*, of the Stonesfield Slate, in the *Triconodontidæ*, the author appears to have been greatly influenced by regarding *Triconodon* as having the condyle placed low down on the mandible. We have, however, considerable doubts whether this is a character of much importance, as it varies so much in the allied *Phascolotherium* and *Amphitylus*. In considering that the whole of the seven cheek-teeth of *Phascolotherium* are true molars, the author departs very widely from the view taken by Sir R. Owen, and a great deal more evidence is required before it can be considered proved that at least the first two of these teeth are not premolars.

In making such mention as space permits of some of the other genera, we must take those included under the Prodidelphia and the so-called Insectivora Primitiva together. In this connection it appears that a great deal depends on the interpretation of the dental characters of the original genus *Amphitherium*, to which Prof. Osborn refers the fragment of a mandible figured on p. 192. It is stated, with great fairness, that when the author examined this specimen he regarded it as totally distinct from *Amphitherium*, but that comparisons of his drawings with figures led him to change his opinion. On p. 192 it is observed that "when these mutilated crowns [of the type] are compared with the perfect crowns of the newly-acquired jaw, there can be *no doubt* that they belong to the same pattern. *If this be the case,* the latter specimen is of great interest, as it enables us for the first time to fully characterize the molar dentition of *Amphitherium*." We have purposely italicized portions of the above sentences, since they show a somewhat curious instance of the author's method. Thus, in the first sentence the teeth of the new jaw are definitely stated to be of the type of those of *Amphitherium*, while in the second a provisional element is introduced; and yet subsequently this jaw is again definitely taken as affording the true structure of the *Amphitherium* molar. Far be it from us

[1] Prof. Osborn proposes to alter the spelling of this name to *Dromotherium*.

to say that this jaw does not belong to *Amphitherium*—it very probably does; but it certainly does not afford decisive evidence on which to base an extensive superstructure, and to make *Amphytherium* the type of one family, while *Amphitylus* and *Amphilestes* (regarded by Owen as closely related to the former) are referred to the *Triconodontidæ*. Then, again, exception may be taken to the interpretation of the molar structure in the jaw in question. Prof. Osborn regards the teeth as consisting of two cusps and a talon in line, approximating to the fashion of *Amphilestes;* but to us they appear to resemble those of the Upper Jurassic genus *Amblotherium*, in which the molars consist of a trilobed blade and a posterior talon. Now, *Amblotherium* is made the representative of a family which is taken as the type of the Insectivora Primitiva. Apart from the question of what *Amphitherium* really is, the molar teeth of *Amblotherium*, as already said, differ considerably from those of *Amphilestes* (Prodidelphia); but, since precisely analogous differences occur in a single family of existing Marsupials, these differences do not appear to afford grounds for even family, let alone ordinal, distinction. No definite characters are, indeed, given by which the Insectivora Primitiva (p. 235) are to be distinguished from the Prodidelphia; and if we compare the figure of the mandible of *Amphilestes*, given on p. 228, with that of *Amblotherium*, represented in Plate ix., Fig. 11, the resemblance in the contour of the posterior portion of the jaw is so close that scarcely even generic distinction could be drawn from this part. The conclusions drawn from this portion of the jaw in the different forms are indeed very remarkable. Thus we have already noticed how the low condyle is given as a character of the *Tritylodontidæ*, and yet the feature is totally wanting in the first genus, *Amphilestes*, which agrees exactly with *Amblotherium* in its lofty condyle. The alleged broad and narrow coronoids of the two forms may be in great part due to the effects of pressure. The absence of inflection in the angle of *Amblotherium* is shared by some of the forms included in the *Triconodontidæ*. Then, again, we are totally unable (after repeated examinations of the types) to see how the lower jaw, on which *Peraspalax* was founded, can be even generically distinguished from *Amblotherium*, the dental formula being, with the exception of an additional lower molar, identical; and yet the one genus is referred to the Prodidelphia, and the other to the Insectivora Primitiva. As another instance, the general similarity in the structure of the lower molars of *Spalacotherium* to those of *Chrysochloris* coupled with an analogous similarity existing between the upper ones of *Peralestes* and those of the same existing genus, suggests at all events a very considerable presumption that the two fossil genera may be identical. We find, however, *Spalacotherium* placed in the *Triconodontidæ*, while *Peralestes* is made the type of another family of the Prodidelphia, which includes the above-mentioned *Peraspalax*. Now, even if the above obvious resemblance is ignored, we totally fail to see any reason for including *Spalacotherium* in the *Triconodontidæ*, and agree with Prof. Marsh in regarding it as the type of a distinct family. If, moreover, any of these forms are to be referred to the Insectivora, we should have thought that *Spalacotherium*, with its *Chrysochloris*-like molars, and the reduction of its lower incisors to the Eutherian three, was the very one which had a claim to such a position. In regard to the new genus *Kurtodon*—the type of the *Kurtodontidæ*—we can only say that there appears to us to be no evidence that the upper jaws on which it is founded may not belong to one of the genera named on the evidence of the mandible.

Other points might be noticed if space permitted; but we have indicated enough to show that a great deal more must be absolutely proved before many of the genera admitted by Prof. Osborn can be even allowed to stand as definitely distinct; while, as to the proposed division of the Polyprotodont forms into Insectivora and Marsupialia, we have shown that in its present form it breaks down hopelessly at every point, although we are far from saying that all the known forms are certainly Marsupial. It appears, however, desirable, till we attain much fuller knowledge of their organization, to leave a large proportion of them in a single ill-defined family.

In criticizing this memoir we have not hesitated, in what appear to us to be the true interests of science, to speak freely. We should, however, be unjust if we failed to recognize the amount of labour of a very trying kind which the author has bestowed on the subject; and we especially commend the value of his observations on the Multituberculata. It is also a decided advantage to have all the American and European forms compared together by one who has had the good fortune to study so many of the types from both areas. Finally, no one can fail to be struck with the excellent illustrations with which the monograph is adorned, a large number of which, we believe, are from the author's own drawings.

EARTH SCULPTURE.

Les Formes du Terrain. Par Lieut.-Colonel G. De la Noë, avec la collaboration de Emm. de Margerie. 2 Vols. (Text, pp. 205; Plates xlix.). (Paris, 1888.)

THE origin of the features of the earth's surface must always prove an attractive subject no less to the geographer than to the geologist. The one describes and the other expounds; and the work before us is an admirable example of what may be done by the joint labours of geologist and geographer in illustrating and explaining the form of the ground.

In turning over the pages of the work, and in contemplating the many instructive diagrams and pictorial illustrations, one is prepared for a more exhaustive treatment of the origin of scenery than is really to be found in these volumes. So far as the geologist is concerned, the work is mainly a treatise on sub-aërial denudation, and with special reference to France. It is almost entirely occupied with the method of denudation by rain and rivers, and with an account of the features which they originate. We are told how different rocks are disintegrated by surface agencies, and how the broken material is afterwards transported by streams. Attention is especially called to the action of running water on rocks of varying character and inclination, to the influence of vegetation in preserving slopes at certain inclinations, and to the effect of rain in diminishing the angle of slopes. The influence of climate is dwelt upon, and it is shown how the permeable strata are characterized by dry valleys and few water-courses, while the impermeable beds support abundant streams.

The relations of disturbed strata, of anticlinals and synclinals to valley and hill, are duly noted; and it is pointed out how the flow of rivers is determined by the lie of the land when it is upraised from beneath the sea-level, and that in few cases are their courses directed by faults or fractures. The authors explain the recession of escarpments by the undermining or undercutting of softer beds and the production of landslips; and they note the influence of lateral streams in eroding these softer strata at the foot of the hills, a subject illustrated by reference to the Wealden area and other districts.

Little is said about marine denudation, for the action of the sea is essentially limited to the destruction of cliffs along its margin, and to the formation of marine platforms. Concerning great "plateaux of abrasion," or so-called "plains of marine denudation," the authors express their opinion that it would be wrong to attribute their formation exclusively to the sea, for they consider that the prolonged action of sub-aërial forces is to reduce the land to a level. Nor do the authors attribute great excavating power to glaciers. In their opinion these icy agents occupied and modified old valleys, and have not always effaced the pre-Glacial alluvial deposits; and they see little evidence of post-Glacial erosion. In these respects their observations are based on local and limited evidence; for in this country, although the main features were marked out in pre-Glacial times, there is abundant evidence of denudation by glacial action, and subsequently in times when the ice had done its work.

The authors have clearly pointed out that the topographical features are as a rule in direct relation with the geological structure; indeed, the form of the ground is one of the most important guides to the field-geologist in his delineation of the superficial distribution of the rock-masses. Nevertheless, in the explanation of the origin of our scenery, there are many points concerning the original extent of each formation, and the changes in texture which the rocks have undergone, that are but briefly, if at all, noticed in this work. In this respect, however, each country must be studied in detail before the complex history of its physical features can be deciphered.

The present work, as before stated, deals mainly with the mode in which rain and rivers sculpture the surface of the earth. It is an instructive summary of what is known on this subject, supported by original observations and by references to the principal authorities, and illustrated in a far more sumptuous manner than has ever been attempted in this country. H. B. W.

OUR BOOK SHELF.

Eclectic Physical Geography. By Russell Hinman. (New York: Van Antwerp, Bragg, and Co., 1888.)

To quote the author's preface, "The aim of this book is to indicate briefly what we know or surmise concerning the proximate causes of the more common and familiar phenomena observed at the earth's surface." The book commences with an introduction to the general laws of Nature, in which short outlines of the properties of matter and the various forms of energy are given. The earth is then treated as a planet; its relation to the sun and stars, and the nature and results of its movements, being described. Next come chapters on the atmosphere, the sea, the land, meteorology; and finally, the various forms of life. The causes of the movements of the atmosphere, sea, and land, and their respective effects, are all clearly stated. Brief outlines are given of the gradual disintegration of terrestrial rocks, and the subsequent transportation and accumulation of the products. Fossils and their teachings also receive attention. In short, nothing of importance has been omitted.

The general plan of the book bears a considerable resemblance to that suggested by the syllabus of the Science and Art Department's course of elementary physiography, and with a teacher to extend the preliminary chapter on the forms of energy, would form an admirable text-book for that subject. The order in which the subjects are taken is practically the same, and is obviously the most natural and rational.

The chapters on the forms of life and their distribution will prove of special interest to young students or general readers. There is a good outline of the development theory, and of what we know of man from prehistoric times.

The book throughout is illustrated by a great number of drawings, maps, and charts, which not only beautify but illustrate the text in a most admirable manner. The charts are drawn on three different systems of projection, each system being applied where it is most suitable; and, what is very important, the different systems are fully explained. A book like this cannot fail to impress the reader with a due sense of the importance of diagrammatic representation in facilitating description. The various sectional drawings are especially valuable in this respect.

The book thoroughly deserves the highest praise, and as an introduction to the study of science must certainly rank among the best.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Prophetic Germs.

Prof. Ray Lankester has mistaken me. When I said in my last letter of October 8 that "all organs whatever do actually pass through rudimentary stages in which actual use is impossible," I referred specially to the embryological development of the individual. This is a fact which cannot be denied. But on the Darwinian hypothesis this fact applies equally to the birth of species—which are nothing but the passing results of individual variation. If true now of all individuals, it must, on that hypothesis, have been true of them for all time.

Inheritance is no explanation of this fact. It is merely one part of the fact separately stated. Neither is "correlation of growth" any explanation of it. This, again, is a mere phrase stating in another form the very fact which it pretends to explain. All organic growths are "correlated." But with what? First, with each other; and, secondly, with some combined use, which invariably lies in the future when such growths begin. "Correlation of growth" is the law under which "prophetic germs" begin to be developed; and this prophetic character becomes all the more marked in proportion as we carry back existing forms of life to the forms which were primæval. It is a favourite idea among the disciples of Darwin that the embryological development of individuals represents in epitome the whole history of organic life. I do not see why they should object to it when it leads us to the conclusion that the whole organic world must have begun in germs which were prophetic—that all organs must have come into being before they could be used.

Argyll.

Definition of the Theory of Natural Selection.

In his Presidential address before Section D of the British Association, Mr. Dyer is reported to have said, while alluding to myself:—"He has startled us with the paradox that Mr. Darwin did not, after all, put forth, as I conceive it was his own impression that he did, a theory of the origin of species, but only of adaptations. And inasmuch as Mr. Romanes is of opinion that specific differences are not adaptive, while those of genera are, it follows that Mr. Darwin only really accounted for the origin of the latter, while for an explanation of the former we must look to Mr. Romanes himself" (NATURE, September 13, p. 476).

It is here stated: (1) that in my opinion specific differences are not adaptive; (2) that I regard Mr. Darwin's theory as explaining the origin of genera, but not the origin of species; and (3) that, consequently, biologists are virtually invited by me to accept the theory of physiological selection as a substitute for Mr. Darwin's theory of natural selection, in so far, at all events, as the origin of species is concerned.

In direct contradiction to all these statements I will now quote passages from the paper with reference to which they are made. It would be easy for me to add further quotations to the same effect under each of the three heads, but the following will be sufficient to serve the double purpose which I have in view—namely, first to correct misrepresentations, and next to furnish a basis for further remarks upon the subject. The italics have reference only to the former purpose.

(1) and (2).—"It [the theory of natural selection] is not, strictly speaking, a theory of the origin of *species*: it is a theory of the origin—or, rather, of the cumulative development—of *adaptations*, whether these be morphological, physiological, or psychological, and whether they occur in species only, or likewise in genera, families, orders, and classes.

"These two things are very far from being the same; for, on the one hand, in an enormously preponderating number of instances, adaptive structures are common to numerous species; while, on the other hand, the features which serve to distinguish species from species are, as we have just seen, by no means *invariably*—or even *generally*—of any adaptive character. Of course, if this were not so, or if species *always* and *only* differed from one another in respect of features presenting some utility, then any theory of the origin of such adaptive features would also become a theory of the origin of the species which presented them. As the case actually stands, however, not only are specific distinctions *very often* of no utilitarian meaning; but, as already pointed out, the most constant of all such distinctions is that of sterility, and this the theory of natural selection is confessedly unable to explain. . . . *In so far as natural selection has had anything to do with the genesis of species, its operation has been, so to speak, incidental: it has only helped in the work of originating species in so far as some among the adaptive variations which it has preserved happen to have constituted differences of only specific value.* But there is an innumerable multitude of other such differences with which natural selection can have had nothing to do—particularly the most general of all such differences, or that of mutual sterility; while, on the other hand, by far the larger number of adaptations which it has preserved are now the common property of numberless species. *But let me not be misunderstood. In saying that the theory of natural selection is not, properly speaking, a theory of the origin of species, I do not mean to say that the theory has no part at all in explaining such origin. Any such statement would be in the last degree absurd.* What I mean to say is that the theory is one which explains the origin or the conservation of adaptations, whether structural or instinctive, and *whether these occur in species*, genera, families, orders, or classes. *In so far, therefore, as useful structures are likewise species-distinguishing structures*, so far is the theory of their origin *also* a theory of the origin of the species which present them."

(3) "Let it, therefore, be clearly understood that it is the office of natural selection to evolve adaptations—not therefore or necessarily to evolve species. *Let it also be clearly understood that in thus seeking to place the theory of natural selection on its true logical footing, I am in no wise detracting from the importance of that theory.* On the contrary, I am but seeking to release it from the difficulties with which it has been hitherto illegitimately surrounded. . . . I cannot feel that I am turning traitor to the cause of Darwinism. On the contrary, I hope thus to remove certain difficulties in the way of Darwinian teaching; and I well know that Mr. Darwin himself would have been the first to welcome my attempt at suggesting *another factor in the formation of species, which, although quite independent of natural selection, is in no way opposed to natural selection, and may therefore be regarded as a factor supplementary to natural selection.* . . . And here, as elsewhere, I believe that the co-operation enables the two principles to effect very much more in the way of species-making than either of them could effect if working separately. On the one hand, without the assistance of physiological selection, natural selection would, I believe, be all but overcome by the adverse influences of free intercrossing—influences all the more potent under the very conditions which are required for the multiplication of species by divergence of character. On the other hand, *without natural selection, physiological selection would be powerless to create any differences of specific type, other than those of mutual sterility and trivial details of structure, form, and colour*—differences wholly without meaning from a utilitarian point of view. But in their *combination* these two principles appear to me able to accomplish what neither can accomplish alone—namely, a full and satisfactory explanation of the origin of species."

These quotations appear to me sufficient to prove the inaccuracy of Mr. Dyer's remarks. But I should not have taken the trouble to notice misinterpretations of so absurd a kind, were it not that I have something more to say on the subject of which they treat. For Mr. Dyer, in his address, alludes to a recent criticism by Mr. Huxley, which also deals with my "paradox," but does so in a very different manner. That is to say, the passages which Mr. Huxley devotes to this subject exhibit a much more careful consideration of the points in it to which he alludes, as well as a manifest desire to state the issue fairly. I will therefore pass on to consider the criticism as it was originally presented by Mr. Huxley, leaving behind the teratological reproduction of it by Mr. Dyer as effectually disposed of by mere quotations from my paper itself.

The substance of Mr. Huxley's criticism, in so far as it apparently applies to me, is conveyed in the following words:—"'Favourable variations' are those which are better adapted to surrounding conditions. It follows, therefore, that every variety which is selected into a species is so favoured and preserved in consequence of being, in some one or more respects, better adapted to its surroundings than its rivals. In other words, every species which exists, exists in virtue of adaptation, and whatever accounts for that adaptation accounts for the existence of the species. To say that Darwin has put forward a theory of the adaptation of species, but not of their origin, is therefore to misunderstand the first principles of the theory. For, as has been pointed out, it is a necessary consequence of the theory of selection that every species must have some one or more structural or functional peculiarities, in virtue of the advantage conferred by which, it has fought through the crowd of its competitors, and achieved a certain duration. In this sense, it is true that every species has been 'originated' by selection" (*Proc. Roy. Soc.*, vol. xliv. No. 269, p. xviii.).

Now, in the first place, I have nowhere said that "Darwin has put forward a theory of the adaptation of species, but not of their origin." I said, and continue to say, that he has put forward a theory of *adaptations in general*, and that where such adaptations appertain to species only (*i.e.* are peculiar to particular species), the theory becomes "*also* a theory of the origin of the species which present them." The only possible misunderstanding, therefore, which can here be alleged against me is, that I fail to perceive it as a "necessary consequence of the theory of selection that *every* species *must* have some one or more structural or functional *peculiarities*" of an adaptive or utilitarian kind. Now, if this is a misunderstanding, I must confess to not having had it removed by Mr. Huxley's exposition.

The whole criticism is tersely conveyed in the form of two sequent propositions—namely, "Every species which exists, exists in virtue of adaptation; and whatever accounts for that adaptation accounts for the existence of the species." My answer is likewise two-fold. First, I do not accept the premiss; and next, even if I did, I can show that the resulting conclusion would not overturn my definition. Let us consider these two points separately, beginning with the latter, as the one which may be most briefly disposed of.

I. Provisionally conceding that "every species which exists, exists in virtue of adaptation," I maintain that my definition of the theory of natural selection still holds good. For even on the basis of this concession, or on the ground of this assumption, the theory of natural selection is not shown to be *primarily* a theory of the origin of species. It follows, indeed, from the assumption—is, in fact, part and parcel of the assumption—that all species have been originated by natural selection; but why? *Only because natural selection has originated those particular adaptive features in virtue of which species exist as species.* It is only in virtue of having created these features that natural selection has created the species presenting them—just as it has created genera, families, orders, &c., in virtue of *other* adaptive features extending through progressively wider areas of taxonomic division. Everywhere and equally this principle has been primarily engaged in the evolution of adaptations, and if one result of its work has been that of enabling the systematist to trace lines of genetic descent under his divisions of species, genera, and the rest, such a result is but secondary or incidental. A wing, for example, is an adaptive structure which is formed on at least four completely different plans in different classes of the animal kingdom; and it is the function of natural selection as a theory to explain all this variety of adaptive structure, with its infinite number of subordinate variations through the different forms in each class, whether "species" or otherwise. Now, I say that such a theory is first of all a theory of the evolution of adaptations, even though it be conceded that all species exist in virtue of differing from one another in respect of adaptations, and hence that the theory becomes *also* a theory of the evolution of species, as it is *also* a theory of the evolution of genera, families, &c. Take a parallel instance. If a man were to define the nebular theory as a theory of the origin of Saturn's rings, an astronomer would tell him that his definition is much too limited. The theory is, indeed, a theory of the origin of Saturn's rings; but it is so because it is a theory of the origin of the entire solar system, of which Saturn's rings constitute a part. Similarly, the theory of natural selection is a theory of the entire system of organic Nature in respect of adaptations, whether these are distinctive of particular species only, or likewise common to any number of species. In short, it is "*primarily*" a theory of adaptations *wherever these occur*, and only becomes "*also*" or "*incidentally*" a theory of species in cases where adaptations happen to be restricted in their occurrence to organic types of a certain order of taxonomic division.

This, I think, is enough to justify my definition in a formal or logical sense. But as Mr. Huxley's criticism involves certain questions of a material or biological kind, I should like to take this opportunity of considering what he has said upon them. Therefore I will now pass on to the second head of my answer.

II. Hitherto, for the sake of argument, I have conceded that, in the words of my critic, "it is a necessary consequence of the theory of selection that every species must have some one or more structural or functional peculiarities" of an adaptive kind. But now I will endeavour to show that this statement does not "follow as a necessary consequence" from "the theory of selection."

Be it observed, the question which I am about to consider is not whether "every species which exists, exists in virtue of adaptation" common to its genus, family, order, class, or subkingdom. The question is whether every species which exists, exists in virtue of some advantageous "*peculiarity*" or adaptive advantage *not shared by its nearest allies*. In other words, we are not disputing whether it is a necessary consequence of Mr. Darwin's theory that all "species" must present "adaptations." This, of course, I fully admit. But what we are disputing is, whether it is a necessary consequence of Mr. Darwin's theory that every species must present at least one adaptive character (or combination of adaptive characters) *peculiar to itself alone*. Now, such being the question, let us consider Mr. Huxley's treatment of it.

Most obviously "it follows" from the theory of selection that "every variety which is selected into a species is favoured and preserved in consequence of being, in some one or more respects, better adapted to its surroundings than its rivals." This, in fact, is no more than a re-statement of the theory itself. But it does not follow that "every species which exists, exists in virtue of adaptation" peculiar to that species; *i.e.* that every species which exists, exists *in virtue of having been "selected."* This may or may not be true as a matter of fact: as a matter of logic, the inference is not deducible from the selection theory. Every variety which is *selected into* a species must, indeed, present some such peculiar advantage; but this is by no means equivalent to saying, "in other words," that every variety which *becomes* a species must do so. For the latter statement imports a completely new assumption—namely, that every variety which becomes a species must do so because it has been selected into a species. In short, what we are here told is, that if we believe the selection principle to have given origin to *some* species, we must further believe, "*as a necessary consequence*," that it has given origin to *all* species.

Not to perceive a consequence so necessary is said to betray a fundamental misunderstanding of the first principles of Mr. Darwin's theory. Perhaps, therefore, it is worth while to consider the matter from another and less formal point of view.

It surely is no essential part of Mr. Darwin's theory to deny that isolation (in all its kinds) may lead to the survival of new varieties, and so, in some cases, to the origin of new species, which need not necessarily present any change in the adaptive characters respectively inherited from their parent stocks. Under isolation, and the consequent absence of what Prof. Weismann has called panmixia, there is much reason to believe that new "structural or functional peculiarities" may arise (whether by direct action of changed conditions, by independent variation in the absence of panmixia, or by both these principles combined) which are without any adaptive significance; and I cannot see why it should be held to constitute any essential part of Mr. Darwin's theory to deny that such is the case. No one, I suppose, will venture to express a doubt that there are named species, both of plants and animals, which have been formed under isolation, and which experiments—such as those recently made with our severally-isolated forms of British trout—would prove to be but "local varieties," capable of being changed one into another by mere change of habitat, without any question of "selection" being so much as possible. Here it is the direct action of changed conditions which induces modifications of type sufficiently pronounced to take rank as distinct species in the eyes of a systematist; and the only difference between such a case and one where the modifications are due to independent variation is that in the former case their non-adaptive character admits of being proved by experiment. According to the general theory of evolution, there is no distinction to be drawn between a local variety and a new species, save as regards the extent to which modification may have proceeded. If, therefore, as in the case of the trout, mere change of habitat from one district of Great Britain to another (apart from any "selection") is able to induce modifications sufficient in amount to have been ranked as species by expert ichthyologists, much more may this frequently be the case under geographical isolation in larger areas, with exposure to different climates, and subject to the superadded influence of independent variation.

I have good reason to be well aware that great differences of opinion are entertained by different naturalists touching the degree of importance which should be assigned to isolation as a factor of organic evolution; and in one of the very last issues of NATURE, Mr. Wallace presents with great lucidity the view that isolation alone can never originate a new species by independent variation without the unavoidable intervention of natural selection, seeing that "at each step of the divergence" there must be "necessarily selection of the fit" from the less fit (September 20, p. 491). I will not wait to show that, if in an isolated section of a species no new peculiarities should be required to render its constituent individuals more "fit," selection need not necessarily effect any change with regard to adaptive characters; nor need I remark that even when selection is enabled to effect such a change under such circumstances, it does so *because it is assisted by isolation*, thus becoming, not *the* cause, but *a con-cause* of "the origin of species." A great deal could be said on both these points; but, for the sake of brevity, I will take my stand on the bare fact that, according to the general theory of evolution, a local variety is what Mr. Darwin calls "an incipient species"; and, on the ground of this fact, I ask where the line is to be drawn between varieties and species in respect of adaptive characters? If no answer can be given, we must take it from Mr. Huxley, as "a necessary consequence of the theory of selection," that every *variety* "which exists, exists in virtue of adaptation." Thus, to take but two illustrations from among several that might be drawn from the trout just alluded to, when two lots of "Lochlevens" were placed in two separate ponds within a very short distance of each other, and exposed, as far as could be ascertained, to parallel conditions of life, remarkable—but in no conceivable respect adaptive—differences in coloration were developed between the trout which respectively inhabited the two ponds ("British and Irish Salmonidæ," pp. 226-27, 1887). Will anyone undertake to affirm, after looking at the coloured plates, that these changes must necessarily have been due to selection? Again, in a recent communication to the *Field* (July 7), Mr. Day gives an engraving of a remarkable variation which is taking place in the gill-covers of trout which have been transported to New Zealand, and there "turned down" under nature. Premising only that, although this is a change of structure, there is no more adaptive meaning to be found in it than in those changes of colour above mentioned,[1] I will quote Mr. Day's remarks upon the subject: "It will be interesting to watch the changes occurring among these trout in their new home, and to observe whether these serrations are continued or merely temporary; for if they should become developed with time there would be still more reason for constituting them a new species than now exists among the various European races; while, should trout with serrated preopercles and interopercles be admitted as constituting a new species, we could now trace the process of development from its commencement, and show how such has been occasioned by transplanting our European trout to the warmer waters of the Antipodes."

Should it be objected that, as a matter of fact, the state of matters anticipated by Mr. Day has not yet arrived, my answer would be obvious—namely, *supposing that such a state of matters had arrived*, could the fact be reasonably held to annihilate the whole theory of natural selection? Yet this is what such a fact would *necessarily* do, if we hold it to be "a *necessary consequence of the theory*" that every species which exists, exists in virtue of having been "*selected.*" If we have not here a *reductio ad absurdum*, I do not know how one can ever hope to apply that method.

Of course I am not disputing that in general there is a very great distinction between local varieties and good species in respect of peculiar adaptive characters. In other words, I have no doubt at all that probably the great majority of of species have been originated by natural selection, either as the sole cause or in association with other causes. But the allegation which I am resisting is, that it follows as a necessary consequence *from the theory of selection itself* that *every* species *must* owe its origin to selection. And I have endeavoured to show that this allegation admits of being reduced to an absurdity. When Mr. Wallace, in the letter above referred to, expresses dissent from Mr. Gulick's view that species are frequently originated by the influence of isolation alone, he adds: "If this is a *fact*, it is a most important and fundamental fact, equal in its far-reaching significance to natural selection itself; I accordingly read the paper with continual expectation of finding some evidence of this momentous principle, but in vain." Now, supposing that Mr. Wallace had found the evidence which would have fully satisfied him, would he therefore have been logically required to abandon his own great generalization? Would he have been required to acknowledge, not only, as he says, a principle "equal in its far-reaching significance to natural selection itself," but a principle which altogether superseded that of natural selection? I say it is absurd to suppose that such would have been the case, and yet it must *necessarily* have been the case if it be "a necessary consequence" of his theory that *all* (if *any*) species are originated by selection.

It will be remembered that I am not arguing the biological question whether, or how far, species exist which do not owe their existence to selection; I am arguing only the logical question whether it is "a necessary consequence of the theory of selection" that they cannot. And I now submit that it no more follows from the selection theory alone, that "every variety" which *becomes* "a species" does so "in consequence of being in some one or more respects better adapted to its surroundings than" *its existing contemporaries*, than it does that every variety which becomes a variety does so for the same reason. If the former statement is a statement of biological fact (which, for my own part, I do not believe), the fact is one that would stand to be proved inductively as a fact: it cannot be made good by way of logical deduction "from the theory of selection."

[1] In this connection, also, it is of great importance to remember that it is only twenty years ago since the trout in question were sent to New Zealand, and their fry liberated in the waters there; for the most ardent upholder of the theory of natural selection as the *sole* cause of specific transmutation will scarcely maintain that twenty years is long enough for survival of the fittest to effect a structural change of an "unknown" adaptive character in a long-lived animal with all the waters of New Zealand to spread over.

I have thus dealt with Mr. Huxley's criticism at some length, because, although it has reference mainly to a matter of logical definition, and in no way touches my own theory of "physiological selection," it appears to me a matter of interest from a dialectical point of view, and also because it does involve certain questions of considerable importance from a biological point of view. Moreover, I object to being accused of misunderstanding the theory of natural selection, merely because some of my critics have not sufficiently considered what appears to them a "paradoxical" way of regarding it.

GEORGE J. ROMANES.

How Sea-Birds Dine.

As I have ascertained that the following fact is not well known, I send you this account in the hope that it may be of interest to naturalists and to the general public. Anyone who lives in the Western Hebrides will have often watched on a calm day the sea-birds feeding with noisy clamour in the sea-lochs and about the numerous islands. This is especially the case in August, when the shoals of small herring are very plentiful. Some years ago, when in a sailing-boat off the west coast of Mull, I caught with a hand-net a dishful of these small fry as they swam along the surface of the water. Last year, noticing from a steam-launch the birds congregated in great numbers at one spot, the idea struck me to steam to the place and try to get a share of the birds' repast. The idea was at once carried out. I stood on the prow with landing-net in hand, and the launch was steered towards the birds. As we drew near, the banqueters flew away with evident dissatisfaction at the interruption, a few of the more greedy making their last hasty dives. In another moment we were at the spot, and I saw, to my intense surprise, about 2 feet under the surface, a large reddish-brown ball, 2 to 3 feet in length and 2 feet in depth. I made a frantic swoop with the net into the ball, and brought on deck half a pailful of the sea-birds' dinner. Even as we passed we could see the great living ball sinking and breaking into pieces. This year I and others have tried the same spot with great success. Sometimes the ball has sunk too deep to be reached; sometimes there was no ball to be seen; but on the most successful day I filled a pailful in three hauls. In September we saw no ball, because, perhaps, the fish had grown too large for the birds to manage. As far as I can judge, the *modus operandi* is carried out by the divers, who surround a shoal and hem them in on all sides, so that the terrified fish huddle together in a vain effort to escape inevitable destruction. The divers work from below and other sea-birds feed from above; and, as in some cases after the birds had been at work for some time I saw no ball, I suppose not one fish is left to tell the tale. I must leave to naturalists the real explanation of the matter; but I may mention that, when disturbed by the boat, the divers seem to come to the surface in a great ring round the scene of their feast. I may also mention that once, when the boat was still 300 or 400 yards away, the birds suddenly rose and whirled about with frightened screams. I wondered what could be the cause, until I saw the round back of a porpoise rolling lazily round at the exact spot, and then rolling back again. When we steamed past there was no sign of a ball. What two delicious mouthfuls for the porpoise! COMPTON.

Loch Luichart, Ross-shire, N.B.

The Zodiacal Light.

MR. O. T. SHERMAN gives an interesting communication on the zodiacal light in NATURE of October 18 (p. 594), and asks for reference to any observations. He alludes to Cassini. The following extract from a letter by Cassini may not have come under his notice: "It is a remarkable circumstance that since the end of the year 1688, when this light began to grow fainter, spots should have no longer appeared on the sun, while in the preceding years they were very frequent, which seems to support, in a manner, the conjecture that the light may arise from the same emanations as the spots and faculæ of the sun." This does not quite tally with Mr. Sherman's notion that the maxima of the zodiacal light coincide with the minima of sun-spots. May it not rather be that, supposing sun-spots to be largely occasioned by increased influx of meteoric matter falling into the sun, which matter gets sublimed and repulsed to augment the materials forming the zodiacal light, therefore the maxima of the latter may then lag behind the maxima of the sun-spots. HENRY MUIRHEAD.

Cambuslang, October 20.

The Geometric Interpretation of Monge's Differential Equation to all Conics.

NEITHER the note of Prof. Asutosh Mukhopadhyay in NATURE of the 11th inst. (p. 564), nor that of Lieut.-Colonel Allan Cunningham in the number of August 2 (p. 318), has satisfied me that the criticism implied in my short note (June 28, p. 197) on the Professor's first note (June 21, p. 173) is unfounded. Permit me, therefore, to develop that criticism a little more at large.

I have not yet had an opportunity of referring to the papers of the Professor in the Proceedings of the Asiatic Society, but from what I can gather as to their contents from his notes in NATURE, I am in no way disposed to underestimate the accuracy or the value of his results. It is only to his claim to find in them "*the* true interpretation of Monge's differential equation to any conic" that I demur.

To my apprehension the interpretation in question is a *truism*, not a *truth*. What has been put into the question as a *definition* emerges afterwards, as might have been anticipated, as an *interpretation*. If the Professor has given a definition of *aberrancy*, independent of a conic and its known properties, of course I am wrong; but I gather from his note that by *aberrancy* he merely means (if I may thus express it) *deviation from conicity*. Whatever measure of *aberrancy*, then, he adopts for curves generally, must necessarily become zero for a conic, which has, from the very meaning of the words, no "deviation from conicity."

The difference, as I conceive it, between an interpretation properly so called and an interpretation that is a mere *truism*, may be clearly illustrated by the case of the circle. The Professor tells us that "the differential equation of all circles $(1 + p^2)r - 3pq^2 = 0$, means that the angle of aberrancy vanishes at every point of every circle." If thus read, what I have said above applies, and the interpretation is but a *truism*. It admits, however, of a different reading. For it is easy to show that $(1 + p^2)r - 3pq^2 = (1 + p^2)^3 \frac{d^2\phi}{ds^2}$, where s, ϕ are the usual intrinsic co-ordinates of the curve, so that the differential equation is equivalent to $d^2\phi/ds^2 = 0$. Now $d\phi/ds$ is the measure of the curvature of a curve, defined as the rate of change, per unit of arc, of the inclination of the tangent to a fixed direction, a *definition which is quite independent of the circle*; and $d^2\phi/ds^2$ is the rate of change, per unit of arc, of the curvature. Hence the equation $d^2\phi/ds^2 = 0$, being true at every point of every circle, expresses the *truth* that in a circle there is no change of curvature from point to point—or, in other words, the property that the curvature of a circle is the same at every point. I submit that this, rather than the Professor's, involving the notion of *aberrancy*, has a right to be regarded as the true interpretation of the equation.

In like manner, the true interpretation of the differential equation to a conic, if it ever is discovered, will express that some magnitude or concept connected with a curve, and defined independently of the particular curves, the conic sections, vanishes at every point of every conic.

Even admitting the Professor's interpretation, I agree with Colonel Allan Cunningham that it has no prerogative right over others of the same character to be called *the* interpretation of the equation. To go no farther, any number of "aberrancy curves" may be imagined; as, for instance, the locus of the focus, instead of the centre, of the osculating conic, for which it will be true that "the radius of curvature of the aberrancy curve vanishes at every point of every conic"; for in fact, in this case the aberrancy curve degenerates into a single point, and to say that the radius of curvature vanishes, or that the curvature is infinite, at every point of a curve, is, to my apprehension, only a roundabout, and not very instructive, way of saying that the curve becomes reduced to a single point.

Harrow, October 13. R. B. H.

A Shadow and a Halo.

THE following notices of anthelia may be interesting to the readers of NATURE. Frances Ridley Havergal thus described a sunset on the Faulhorn: "At one juncture a cloud stood still, apparently about two hundred yards off, and we each saw our own shadow gigantically reflected on it, surrounded by a complete rainbow arch, a full circle of bright prismatic colours, a transfiguration of our own shadows almost startling; each, moreover, seeing only their own glorification" ("Swiss Letters and Alpine Poems").

Tennant, in his book on Ceylon, states that this curious phenomenon, which may probably have suggested to the early painters the idea of the glory surrounding the heads of beatified saints, is to be seen in singular beauty at early morning in Ceylon. When the light is intense, and the shadows proportionally dark, when the sun is near the horizon, and the shadow of a person is thrown on the dewy grass, each drop of dew furnishes a double reflection from its convex and concave surfaces; and to the spectator the shadow of his own figure, but more particularly the head, appears surrounded by a halo as vivid as if radiated from diamonds.

S. T. Coleridge described the phenomenon thus:—

"Such thou art, as when
The woodman winding westward up the glen
At wintry dawn, where o'er the sheep-track's maze
The viewless snow-mist weaves a glist'ning haze,
Sees full before him, gliding without tread,
An image with a glory round its head:
The enamoured rustic worships its fair hues,
Nor knows he makes the shadow he pursues."

Benvenuto Cellini saw, probably, this phenomenon, and supposed it peculiar to himself. F. Robertson cites it as a proof of inordinate vanity. He says: "Conceive a man gravely telling you that a vision of glory encircled his head through life, visible on his shadow, especially on the dewy grass at morning, and which he possessed the power of showing to a chosen few" ("Life and Letters of F. Robertson," vol. ii. p. 192).

Bardsea, October 22. EDWARD GEOGHEGAN.

I HAVE frequently, on the South Downs, seen a halo round the shadow of my head, as described in your last number by Mr. A. S. Eve. I have noticed that the further off the shadow, the brighter is the halo. I have also observed, when looking at my shadow in the sea, that rays of light appear to surround the shadow of my head. CHARLES CAVE.

Ditcham Park, Petersfield, October 22.

On the Grass Minimum Thermometer.

THE average readings of the self-recording grass minimum thermometer for every month during the past three years have been compared with the average minimum *damp* bulb temperatures, obtained from the means of hourly readings, and the following figures show the corrections to be applied to the latter in order to obtain the former:—January $-0^\circ.3$, February $+0^\circ.3$, March $-0^\circ.3$, April $-0^\circ.8$, May $-0^\circ.2$, June $-1^\circ.1$, July $-1^\circ.1$, August $-0^\circ.9$, September $+0^\circ.2$, October $+1^\circ.4$, November $+1^\circ.9$, December $+0^\circ.4$.

The grass minimum is nearly a degree below the damp bulb minimum in the wet season, and nearly 2° above it in the driest month. The comparison between the minimum air temperature and the minimum on grass does not measure the terrestrial radiation, although the difference is to some extent influenced by radiation. Moreover, the epochs of the two minima need not coincide—*e.g.* in Hong Kong the early morning hours are more cloudy than the evening hours.

During the daytime in summer the thermometer, exposed an inch above the short grass, shows as a rule temperatures rising to 120° or 130°, especially in calm weather; but even when it is not perfectly calm, the force of the wind is not felt so near the ground, from which the air rises laden with minute particles of dust, which are observed adhering to the cloth of damp bulbs and other objects cooled by evaporation, and which may occasionally be smelt in the air. At night such minute particles would of course tend to return to the ground, and the unhealthy character of the ground-fog during early morning hours in tropical countries may be intensified by this circumstance.

Hong Kong Observatory, September 10. W. DOBERCK.

ON THE ELECTROMOTIVE VARIATIONS WHICH ACCOMPANY THE BEAT OF THE HUMAN HEART.

THE observation of these variations is extremely easy, the only requisite being a sufficiently sensitive capillary electrometer.[1]

[1] The electrometers I used were made by Mr. Dean, glass-blower, 8 Cross Street, Hatton Garden.

The successful issue of the observations is so certain that they can be best described in the form of directions to a person who should be desirous of seeing them for himself, followed by the prediction of what will be observed by him.

§ I. Two vessels of salt solution are to be prepared, and connected with the capillary electrometer by electrodes. The various extremities of the observer are to be dipped into the salt solution, while the capillary column is watched. Electrical variations, apparently synchronous with the heart's pulse, will be observed with certain combinations rather than with others, and the results (on a normal person with the heart pointing to the left) will be as follows:—

Connect with electrometer—	
1. Left hand and right hand	Electrical variations
2. Left hand and left foot	Little or no variations
3. Left hand and right foot	Little or no variations
4. Right hand and left foot	Electrical variations
5. Right hand and right foot	Electrical variations
6. Right foot and left foot	No electrical variations
	will be apparent.

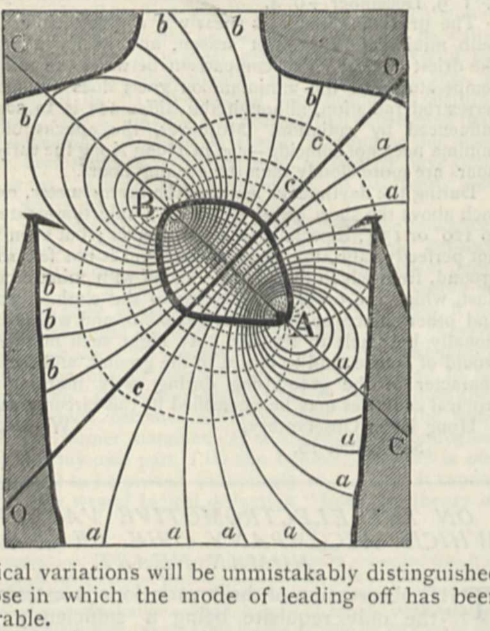
Further observations may be made with the mouth used as a leading-off point in connection with each of the four extremities. To lead off from the mouth a silver electrode coated with silver chloride is kept under the tongue. The results will be as follows:—

Connect with electrometer—	
7. Mouth and left hand	Electrical variations
8. Mouth and right hand	Little or no variations
9. Mouth and left foot	Electrical variations
10. Mouth and right foot	Electrical variations
	will be apparent.

Finally, it is possible to add to the evidence obtained, by using the rectum as a lead off by means of a silver electrode. This, if tried, would give with

11. Rectum and mouth	Electrical variations
12. Rectum and left hand	Little or no variations
13. Rectum and right hand	Electrical variations
14. Rectum and left foot	Little or no variations
15. Rectum and right foot	Little or no variations.

These will have been the results; the cases in which the mode of leading off has been favourable to the production



of electrical variations will be unmistakably distinguished from those in which the mode of leading off has been unfavourable.

The explanation of these facts is most shortly given in the diagram. C C is the axis of any current which must be produced if at any time the apex and base of the ventricles differ in potential. O O is the line of zero potential at right angles to C C.

a a a are equipotential lines round a supposed focus A. *b b b* are equipotential lines round a supposed focus B. Any lead off from two superficial points *a a* or *b b* is unfavourable. Any lead off from two points *a b* is favourable to the manifestation of electromotive differences originating at the heart. This will have been demonstrated by the experiments directed to be made.

§ II. On a quadruped (dog, cat, rabbit) the results will come out somewhat differently. The heart occupies an approximately median position, so that the asymmetry observed on man does not hold good with the above-named animals. In these the current axis will be along a median longitudinal line; the line of zero potential will be at right angles to it, *i.e.* transverse.

This can be verified by trial with very little trouble. A quadruped is led off by the various extremities and orifices immediately after death before the heart has ceased to beat; or a dog may be trained to stand quiet with his feet in dishes of salt solution (I have a large and well-disposed dog who will stand thus by the hour). However the test be made, the results will come out as follows:—

Connect with electrometer—	
1. Left paw [1] and right paw	Little or no electrical variations
2. Left paw and left foot	Electrical variations
3. Left paw and right foot	Electrical variations
4. Right paw and left foot	Electrical variations
5. Right paw and right foot	Electrical variations
6. Right foot and left foot	Little or no electrical variations
	will be apparent.

Extending the observations to mouth and rectum, the results will be thus:—

7. Mouth and left paw	Little or no electrical variations
8. Mouth and right paw	Little or no electrical variations
9. Mouth and left foot	Electrical variations
10. Mouth and right foot	Electrical variations
11. Mouth and rectum	Electrical variations
12. Rectum and left paw	Electrical variations
13. Rectum and right paw	Electrical variations
14. Rectum and left foot	Little or no electrical variations
15. Rectum and right foot	Little or no electrical variations.

§ III. Upon these two proofs may be piled a third proof of the correctness of the facts and of their explanation. Cases of *situs viscerum inversus* are to be found; the viscera of such people are situated as those of a normal person seen in a mirror; *i.e. inter alia*, the heart points to the right. I have examined two such cases, with results exactly as anticipated, viz. the favourable combinations, 4, 5, and 7, of a normal subject (§ I.) are unfavourable in the case of *situs inversus*, while the unfavourable combinations, 2, 3, and 8, are favourable. Combinations 1, 9, and 10 are favourable, and 6 is unfavourable in both cases, there being the notable peculiarity as regards 1 that the variations are reversed in direction in each of the two cases. The significance of this point will be obvious to the reader who has followed the facts up to this point: in both cases we have a favourable combination, but a reversal of points *a* and *b*.

§ IV. As regards the character and direction of each cardiac variation, it will be found to be composed of two phases, the first short, sharp, and difficult to read as regards direction, the second comparatively prolonged and easy to read. The second phase clearly indicates negativity of the heart's base, the first phase less clearly negativity of the heart's apex—facts which testify that the contraction begins at the apex and ends at the base of the ventricles. The auricular contraction does not affect any electrometer I have used.

[1] "Paw" is used as an abbreviation for anterior extremity; "foot" for posterior extremity.

If I may venture to forecast the manner in which these statements may receive from independent sources that verification which any statement requires before it can be accepted as a correct representation of fact, I should say that as regards § I. no contradiction will arise unless the first case tested should happen to be that of a person with the heart occupying an unusually median position, when the favourable and unfavourable cases, though still distinguishable, may be less so than if the heart occupied its usual oblique position pointing to the left. In any case, however, the variation will be found more marked with a favourable than an unfavourable combination. As regards § II., the statements made can be verified as soon as tested upon a recently killed cat or upon a properly educated dog. The verification of § III. only requires that a suitable case should be discovered. As regards the character of the variation, it is probable that its diphasic character may be overlooked at the first glance, but (in a favourable case) this character will soon be apparent. As regards direction, that of the second phase will be determined without much difficulty, but that of the first will be found very difficult to seize. I was not able to make up my mind about it until I had obtained successful photographs of the movements on a quick-travelling sensitive plate.

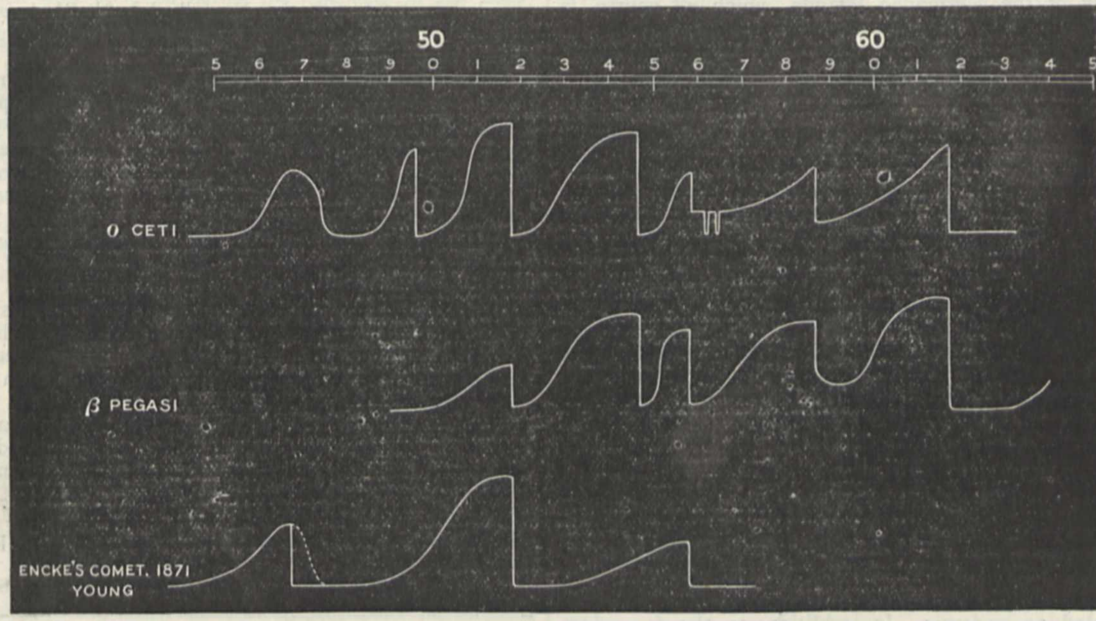
AUGUSTUS D. WALLER.

THE MAXIMUM OF MIRA CETI.

I AM anxious to call the attention of observers to the present spectrum of Mira, which arrived at its maximum brilliancy on the 15th inst. I pointed out recently (NATURE, May 24, p. 79) that stars of the group to which Mira belongs are sparse meteorite-swarms like comets, and that, when variable, the variability is produced by collisions between two swarms, the centres of which are nearest together (periastron passage) at maximum.

Broadly speaking, then, we may regard variables of this class as incipient double stars, or condensing swarms with double nuclei, the invisibility of the companion being due to its nearness to the primary, or to its faintness. It is obvious that variability will occur mostly in the swarms having a mean condensation, for the reason that at first the meteorites are too far apart for many collisions to occur, and that, finally, the outliers of the major swarm are drawn within the orbit of the smaller revolving one, so that it passes clear.

The present maximum of Mira tests my hypothesis, and its brightness is such that a small telescope and a Maclean's spectroscopic eye-piece are all that are necessary to see in how striking a manner the test is borne. The two brightest bands now visible are at λ 517 and λ 546, precisely where these are seen in the brightest comets. The former is the brightest carbon fluting seen in the spectrum of the Bunsen flame, or spirit-lamp,



and the other, at 546, is the citron carbon fluting beginning at 564, but modified by the masking effects of the manganese absorption fluting at 558, and also that of lead at 546.

The blackness of the spaces between the bright flutings shows that there can be very little continuous spectrum from the meteorites, and therefore that the absorption is that of the light of the carbon flutings.

The mean spectrum of Mira is that of a star like β Pegasi, which I have shown to consist of bright carbon flutings, and dark flutings of magnesium, manganese, iron, lead, and barium. In β Pegasi, as in Mira under mean conditions, the carbon is somewhat faint, but in α Herculis it is very bright. The general effect of the conditions of maximum of Mira therefore seems to be that of changing its spectrum from one like that of β Pegasi to one like that of α Herculis.

I observed that the principal carbon fluting at λ 517 was somewhat brighter on the 14th than on the 17th inst. In variable stars of this class the proof is now complete that the increase of luminosity is accompanied by cometary conditions, and that it is due to the increased radiation of carbon.

In the accompanying figure the spectrum of Mira is compared with that of β Pegasi and Encke's comet. In some comets the carbon fluting is cut off at 546, exactly as it is in Mira. The observations of Mira were made by myself at Westgate, those of β Pegasi by Mr. Fowler at the Astronomical Laboratory at South Kensington.

J. NORMAN LOCKYER.

FLORA OF THE KERMADEC ISLANDS.

UPWARDS of thirty years ago Sir Joseph Hooker published an account of the botany of Raoul or Sunday Island, one of the Kermadec Group (Journal of the Linnean Society, i. pp. 125–29), founded upon a small collection made by McGillivray and Milne, naturalists attached to H.M.S. *Herald*. This collection consisted of forty-two species, of which twenty were flowering plants, and the rest ferns and lycopods: and the most interesting circumstance connected with it was "the identity of most of the flowering plants, and all but one of the ferns, with those of New Zealand."

In 1885, Mr. J. T. Arundel presented to the Kew Herbarium a collection of fourteen species from Meyer, a small rocky islet about a mile and a half north of Sunday Island. Poor as it was, it contained half-a-dozen plants not previously known from the group, though they are all included in the collection referred to below.

Since then, no further light has been thrown on this insular flora, until the quite recent appearance (Transactions of the New Zealand Institute, xx. pp. 151–81) of a paper by Mr. T. F. Cheeseman, Curator of the Auckland Museum, New Zealand, a copy of which was kindly forwarded to the writer. Mr. Cheeseman was permitted, through the kind offices of Mr. Percy Smith, the Assistant Surveyor-General of New Zealand, to accompany the expedition despatched last year for the purpose of formally annexing the group to the colony of New Zealand. If Mr. Cheeseman has not succeeded in exhausting the botany of the Kermadec Islands, which, of course, is hardly probable, the undiscovered species cannot materially affect the question of the origin of the vegetation. But before giving the results of his investigations, it will be useful to indicate the position and extent of the islands.

There are four islands lying at great distances apart, between 29° 10′ and 31° 30′ S. lat., and stretching in a south-west and a north-east direction, like New Zealand itself, the nearest point of which is between 500 and 600 miles distant. Raoul or Sunday Island is the largest and the farthest from New Zealand, being twenty miles in circumference, and about 640 miles from Auckland, and a little less than that distance from Tonga. Macaulay, the next in size, is sixty-eight miles to the south-west of Sunday Island; and Curtis and L'Espérance, still farther to the south-west, are little more than rocks. The expedition failed to land on the last-named island, and the visit to Curtis Island was of very brief duration, hence the botany relates almost exclusively to Sunday and Macaulay Islands.

The group is of volcanic origin, and the greatest elevation in Sunday Island is 1720 feet, while Macaulay nowhere reaches quite half that height.

Altogether Mr. Cheeseman collected 115 indigenous vascular plants, eighty-four being phanerogams and thirty-one cryptogams, and only five of them were regarded as endemic. In addition to the foregoing, twenty-six species of naturalized plants, chiefly European weeds, were observed or collected.

Of the 115 indigenous species, no fewer than eighty-five are also found in New Zealand, though only fourteen of these are absolutely confined to the two localities. Forty-four species are found in Norfolk Island, forty of which also occur in New Zealand, and only two are apparently confined to Norfolk Island and the Kermadecs. Forty species extend to Lord Howe's Island, but thirty-four of these are also in New Zealand, and none of the peculiar plants of Lord Howe's Island reach the Kermadecs. Seventy-six of the species are common to Australia, sixty-three of them being also in New Zealand, and none of them otherwise peculiar to Australia. Lastly, forty-seven are found in Polynesia, and thirty-one of these also occur in New Zealand.

The foregoing data, as Mr. Cheeseman observes, point unmistakably to New Zealand as the source of the greater part of the flora of the Kermadec Islands. How the plants reached these islands is an interesting question. Mr. Cheeseman is prepared to admit a former north-western extension of New Zealand; but, after a careful examination of the evidence, he arrives at the conclusion that the Kermadec Islands have always been isolated, or, at least, have not formed part of any other land since the Secondary period. Spores of the ferns may have been conveyed by winds; and ocean currents and birds, it may well be conceived, have operated in stocking the islands with flowering plants. Most of the birds are New Zealand species, and the presence of Kauri logs, of different dates and brands, stranded on various parts of the beach, is convincing evidence of the direction of ocean currents. Moreover, the composition of the flora strongly supports this theory.

Sunday Island is the only one of the group on which there is anything approaching arboreous vegetation, and this, with the exception of a small area of the crater, is clothed with forest from the sea-shore to the tops of the highest peaks. The prevailing tree is *Metrosideros polymorpha*, one of the most characteristic trees of Polynesia, especially of the smaller islands, reaching the Sandwich, Marquesas, and Pitcairn Islands; but this particular species does not occur in New Zealand nor in Australia.

Next to the *Metrosideros* in abundance and conspicuousness is a palm, which Mr. Cheeseman thinks may be identical with the Norfolk Island *Rhopalostylis Baueri* (*Areca Baueri*). In some places this grows gregariously, forming large groves.

Ferns are everywhere abundant, varied, and luxuriant; and the endemic tree-fern, *Cyathea Milnei*, is very plentiful, and handsome withal, rising to a height of 50 or 60 feet. Prominent among the New Zealand trees are *Corynocarpus lævigatus*, *Myoporum lætum*, *Melicope ternata*, *Melicytus ramiflorus*, and *Panax arboreum*. *Cordyline terminale*, the widely-spread Polynesian "Ti," and *Pisonia Brunoniana*, *Pittosporum crassifolium*, *Coprosma acutifolia*, and *C. petiolata*, natives of New Zealand, are other elements deserving of notice.

The herbaceous vegetation includes no plants with very conspicuous flowers, but there are two orchids—namely, *Acianthus Sinclairii*, a native of New Zealand, and *Microtis porrifolia*, which also inhabits both New Zealand and Australia.

Macaulay Island was entirely covered with a beautiful sward of natural grass, supposed to be composed of a species of *Poa* and an *Agrostis*, but in the absence of flowers they were indeterminable.

Students of botanical geography will find much more that is interesting in Mr. Cheeseman's valuable paper, from which I have extracted the principal facts.

W. BOTTING HEMSLEY.

DIGITI MINIMI DECESSUS.

[*Sent by a Correspondent.*]

THE following lines appeared in the *Guy's Hospital Gazette* of October 13. The correspondent who sends them to us suggests that they may fitly find a place in NATURE, *à propos* of the controversy on "Prophetic Germs."

"*Man is losing his little toe, ... and can do without it.*"
—MR. CLEMENT LUCAS, in his opening lecture.

If thou must go, thou feeble, foolish digit,
Fain would I speed thy slow, degenerate way!
I daily feel a disagreeable fidget
Whenever I've occasion to display
Thy doubtful outline, and thy form chaotic
(Born of a taste in boots, perhaps erotic).

Thou art a shock to my æsthetic sense,
And offerest no kind of recompense
In way of use; of every function shorn,
Except to act as basis for a corn.
When thou art gone I'll still maintain my grace,
Still walk erect wherever I may be;
Still I'll belong to the athletic race,
Waltz with the fair, and kick mine enemy!
So *pace* Schopenhauers, and *pace* Mallocks
When I've acquired a hypertrophied hallux,
To monodactyle type thus simplified,
Life shall be simpler too, and so—beatified.

* * * *

When future science forgets thee in thy prime,
Methinks a great mind from a northern clime
May then discuss thy remnants, and declare
He finds a true *prophetic organ* there!

F. G. H.

NOTES.

We lately (Sept. 6, p. 437) printed an account of the formation of the Australasian Association for the Advancement of Science. If we may judge from the newspaper reports which have now reached this country, the first general meeting of the Association seems to have been remarkably successful. The session began at the Sydney University on Tuesday evening, August 28. Lord Carrington opened the proceedings with a short speech, and then an address was delivered by Mr. H. C. Russell, the President. On the following day the sectional meetings began, and their work went on during the remainder of the week. About 110 papers were sent in by students of various branches of science, and a considerable number of them will be published in full in the first volume soon to be issued by the Association. The members had an opportunity of taking part in several pleasant excursions, and much hospitality was shown to visitors by leading citizens. At the time of the meeting there were about 850 members, and it is confidently anticipated that next year this number will be largely increased. The next meeting is to be held in Melbourne, and Baron Sir Ferdinand von Müller, the Government Botanist of Victoria, is the President-elect. In 1890 the Association will meet in New Zealand.

The following is the list of names to be submitted, at the annual meeting (November 8) of the London Mathematical Society, for the new Council:—For President, J. J. Walker, F.R.S.; for Vice-Presidents, Sir J. Cockle, F.R.S., E. B. Elliott, and Prof. Greenhill, F.R.S. The Treasurer and Hon. Secretaries remain unaltered. The other members are: A. B. Basset, Dr. Glaisher, F.R.S., Messrs. J. Hammond, H. Hart, J. Larmor, C. Leudesdorf, and S. Roberts, F.R.S., Captain P. A. Macmahon, R.A., and Dr. Routh, F.R.S. It is proposed that the vacancies caused by the withdrawal of Lord Rayleigh, Sec.R.S., and the lamented recent death of Arthur Buchheim, shall be filled up by Messrs. Basset and Routh, as above.

H.M.S. *Jackal*, which has been engaged, under the direction of the Scientific Committee of the Scottish Fishery Board, in a cruise of physical investigation in the North Sea, recently returned to Granton. The course was along the east coast to the Orkney and Shetland Islands, and then to Bergen, Copenhagen, and Kiel. The physical work was carried on by Dr. Gibson, of the Chemistry Department of the Edinburgh University, assisted by Dr. Hunter Stewart and Mr. F. M. Gibson; and owing to the exceptionally favourable weather a large number of stations were formed at various parts of the route, at which series of temperature observations were taken, the density and alkalinity of the water determined, and samples preserved for analytical examination. Dr. Gibson had interviews with most of those conducting scientific fishery work in the countries visited, including Mr. Buch of Bergen, Dr. Paulsen, Lieut. Drechsel, Dr. Pettersen, and Mr. Feddersen of Copenhagen, and Prof. Karsten of the Kiel Commission; and we understand these conferences may result in closer co-operation between the various countries, in regard to the method and scope of scientific fishery investigations.

The members of the International Commission of Weights and Measures have finished their session at the Pavillon de Breteuil, Paris. The making of standard metres is progressing, and next year they will be distributed to the various Governments. The guarantee of the Bureau extends to the thousandth of a millimetre and the ten-thousandth of a gramme.

There are now on the books of the Institution of Civil Engineers 1614 members, 2499 associate members, 458 associates, 19 honorary members, and 939 students, together 5529, being an increase at the rate of $3\frac{3}{4}$ per cent. during the past twelve months.

A specimen of the sword-fish (*Xiphias*) was captured some days ago in Long Reach, Milton Creek, Sittingbourne, by a bargeman. The fish measured 5 feet 2 inches from end of tail to tip of sword.

An Agricultural and Industrial Exhibition was opened at Mysore by the Maharajah on the 17th inst.

At a recent meeting of the Bombay Natural History Society, the idea of starting a Zoological Garden in that city was mooted by Mr. H. M. Phipson, the Honorary Secretary of the Society, and was warmly taken up. It was stated that the Society has been compelled to refuse large numbers of valuable specimens of animals offered to it. All that is asked from the Government is that they shall grant a site, and it is hoped that they may see their way to do so.

Dr. J. C. Cox lately described, at a meeting of the Linnean Society of New South Wales, two very remarkable female figures, modelled in wax, obtained in an aboriginal camp at Miriam Vale, near the head of the Calliope River, Rockhampton. These figures are said to be the only examples of plastic art ever discovered among the Australian aboriginals.

In the Report of the Superintendent of the Adelaide Botanic Garden for the past year it is stated that the insect-powder plant (*Pyrethrum cinerariæfolium*, Trevir.), *roseum*, and *carneum*, Bibrst.), and the cheesemaker (*Withania coagulans*, Dun.), which were introduced into the Garden a few years ago, have found a congenial climate there, and have prospered wherever they were planted in the colony. Eland's Boontges (*Elephantorrhiza Burchellii*, Benth.), which has also been recently introduced, does fairly well. In winter nothing remains of this plant but the roots, which contain tannic acid. A number of cuttings from the Daira grape, a valuable species which comes from Almeria, have thriven wonderfully in the Garden. There are now in the palm-house 180 species and varieties of palms. The Museum of Economic Botany attached to the Garden has been enriched during the past year by 1795 articles, amongst the more remarkable of which was a collection sent by the Sultan of Johore, one of the specimens being a sample of sugar prepared from the cocoa-nut.

Students of the Caucasian languages will be glad to learn that the second volume of Baron Uslar's work, "The Ethnography of the Caucasus," has been published at Tiflis. It contains his "Tchetchen Language," and, in an appendix, several articles on the epics of the Caucasian mountaineers, on the study of the Caucasian languages and their alphabets, as also a translation of Schiefner's "Tchetchensche Studien," and a collection of Tchetchen proverbs and tales about Nasr-eddin, by J. Bartolomei.

In connection with the discussion on "Valency" at the Bath meeting of the British Association, referred to in last week's Nature, Prof. Meldola read a paper on the constitution of the azonaphthol compounds, in which he drew attention to the fact that the properties of these important colouring-matters could only be satisfactorily explained by admitting that they contained oxygen in the tetravalent condition.

THE vapour-densities of the chlorides of chromium have, for the first time, been determined by Profs. Nilson and Pettersson, of Stockholm. The interest attaching especially to the chromic chloride, hitherto known as Cr_2Cl_6, in view of the recent re-determinations of the densities of the corresponding chlorides of aluminium and iron, gives more than secondary importance to the work of the Swedish chemists. Readers of NATURE will remember that these recent experiments by the indefatigable workers just mentioned, and by Prof. Victor Meyer and his co-workers at Göttingen, upon the composition of the molecules of the chlorides of aluminium and iron, resulted in the conclusion that the double formulæ, Al_2Cl_6 and Fe_2Cl_6, must be abandoned in favour of the simpler formulæ, $AlCl_3$ and $FeCl_3$. This, of course, meant that our old notions as to the tetrad nature of these elements were incorrect, and that in reality they behave as triads. Profs. Nilson and Pettersson now clinch the matter by showing that chromium, which in many respects so much resembles aluminium and iron, behaves in precisely the same way. Chromic chloride was fortunately obtained in beautiful laminated crystals of almost perfect purity. The minute traces of absorbed moisture were readily eliminated by gently warming in a current of dry carbonic acid gas; when this was accomplished the requisite quantity was weighed out into a small platinum capsule in those experiments which were conducted in the platinum density apparatus, and in small pieces of ignited porous tubing when the porcelain apparatus was employed. The chloride was found to vaporize very slowly indeed at 1065° C., precluding the possibility of taking densities below that temperature; however, at this comparatively low temperature, the density was 6·135. Now $CrCl_3$ corresponds to a density of 5·478, while Cr_2Cl_6 must of necessity require a number twice as great, and hence cannot exist in the gaseous state. On increasing the temperature to 1190°, the value of 5·517 was obtained, which remained practically constant up to nearly 1300°. Over 1300° the molecules of $CrCl_3$ commence to break up into those of $CrCl_2$ and free chlorine. This is a most decisive result, and one which cannot possibly lead to any other conclusion than the adoption of the formula $CrCl_3$. It is only fair to mention that Messrs. Friedel and Crafts on carrying out vapour-density determinations of aluminium chloride by Dumas's method for 250° above its boiling-point (183°), have very recently obtained results which appear to indicate that this chloride may condense to the double molecule Al_2Cl_6 at these comparatively low temperatures. However this may be, there can be no doubt in the cases of iron and chromium that the triad formula is the only one compatible with experiment, and we shall be very glad to see the doubt in case of aluminium completely cleared up by further experiments. The determinations in the case of the lower chloride of chromium, $CrCl_2$, have been made under great experimental difficulties. This substance is the most difficultly volatilized of any yet submitted to vapour-density determinations. It required the most intense heat of the hottest procurable furnace, and even then was only very slowly converted into vapour. It was obtained perfectly pure by reduction of the chromic chloride utilized for the former experiments, by gently heating in a stream of hydrogen. At the lowest observable temperature, 1300°–1400° C., the density was found to be 7·8, considerably lower than the number required by Cr_2Cl_4. On further increasing the heat to 1600°, the density gradually diminished to 6·2, showing that at some still higher temperature one would finally attain the value 4·25 corresponding to $CrCl_2$. Hence chromous chloride again resembles ferrous chloride, the only difference being that the former is much more difficult to vaporize.

AN exceedingly useful and handy *résumé* of results in the "modern geometry of the triangle" is published in the just issued Proceedings of the Association Française pour l'Avancement des Sciences, Congrès de Toulouse, 1887. It is entitled "Premier Inventaire de la Géométrie du Triangle," by M. E. Vigarié. A second "Inventaire," which the author proposes to draw up, will be occupied with the extensions to certain (as Harmonic) quadrilaterals and polygons, and to space figures.

WE have received Part 3 of "A Catalogue of the Moths of India," compiled by E. C. Cotes, First Assistant to the Superintendent, Indian Museum, and Colonel C. Swinhoe. Of the first two parts, dealing respectively with Sphinges and Bombyces, we have already given some account (NATURE, vol. xxxvii. p. 386). The present part deals with Noctues, Pseudo-Deltoides, and Deltoides.

THE Trustees of the Australian Museum, Sydney, have issued Part I. of a catalogue of the fishes in the collection of the Museum. It relates to recent palæichthyan fishes, and has been compiled by Mr. J. Douglas Ogilby.

A REMARKABLE book on "The Butterflies of the Eastern United States and Canada, with especial reference to New England," by S. H. Scudder, of Cambridge, Mass., U.S.A., is about to be published in monthly parts. It will be completed in twelve parts, the first of which will appear in November. The preparation of this elaborate work was first announced by the author in 1869. Since that time he has had it always in hand, and during the last eight years he has devoted to it undivided attention. According to the prospectus which has been issued, Mr. Scudder has not only availed himself of the personal aid of a host of willing friends and correspondents, who have confided to him their voluminous field notes and numerous specimens, but he has carefully gleaned every fact of value from the natural history journals and other publications, and supplemented all by his thirty-five years' experience in the field. It is claimed that no systematic work on butterflies has ever appeared in any language comparable with it in the complete elaboration of a single limited fauna, in attention to every stage of life, in thorough and excellent illustration of every period of the butterfly's existence, and in careful detail of all structural features. The book will contain seventeen plates of butterflies, six of eggs, eleven of caterpillars, two of the nests of caterpillars, three of chrysalides, two of parasites, thirty-three of structural details in all stages of life, nineteen maps and groups of maps to illustrate the geographical distribution of the butterflies, and three portraits of early naturalists of America—in all, about two thousand figures on ninety-six plates, of which forty or more will be coloured. The printing of the plates was begun three years ago, and is now nearly finished.

A THIRD edition of Mr. R. Milne Murray's "Chemical Notes and Equations" (Maclachlan and Stewart, Edinburgh) has been issued. The book is intended for the use of students. In this edition a section on the electrolysis of salts has been introduced, and some additions have been made to the descriptive part of the work.

THE latest number (No. 3, vol. iii.) of the Journal of the Bombay Natural History Society contains, amongst other papers: unscientific notes on the tiger, by J. D. Inverarity; butterflies and ants, by Lionel de Nicéville; on the Lepidoptera of Karachi and its neighbourhood (part 2), by Colonel Swinhoe; notes on some bees and wasps from Burmah, by Captain C. T. Bingham; notes on the origin of the belief in the bis-cobra, by G. A. Da Gama. Mr. Da Gama says that the term bis-cobra is not of Oriental origin, but is a contraction of the Portuguese *bicho-de-cobra*. The early Portuguese settlers in India named the animals they met with from their most prominent features.

Thus, the *nag* they called, on account of its hood, *cobra-de-capello*; the *Daboia*, on account of its carpet-like skin, they called *cobra-de-alcatifa*—that is, the carpet-snake. From old Portuguese writings he believes that the mangoose is the bis-cobra; and from the crawling motion of that animal the Portuguese had an idea that the *bicho-de-cobra* was a lizard. In fact, in a work of the Jesuit father De Souza, published in 1710, though probably written twenty years earlier, the mangoose is described as "that poisonous reptile, *bicho-de-cobra*." The name mangoose gradually usurped the place of *bicho-de-cobra*, but among the natives the idea of a poisonous lizard called bis-cobra remained, and it has been handed down with terrible stories of its poisonous powers.

THE South London Microscopical and Natural History Club has published its seventeenth Annual Report. The Report includes abstracts of some interesting papers read at the meetings. The Committee say that during the past year there was a uniformly good attendance of members.

WE have received the third number of the series "Insect Life," issued by the Entomological Division of the United States Department of Agriculture. The object of this series is to exhibit the economy and life-habits of insects, especially in their relation to agriculture. Among the contents of this number are notes on the Rocky Mountain locust; a report on injury done by "roaches" to the files in the Treasury at Washington; further notes on the hop-plant louse (*Phorodon humuli*); and a paper suggesting steps towards a revision of Chambers's index to the described Tineina of the United States and Canada, with notes and descriptions of new species, by Lord Walsingham.

SOME time ago the Colorado Ornithological Association was formed, and through the efforts of its members a comprehensive list of the birds of Colorado, numbering about 350 species and sub-species, was soon prepared. This Society has now transformed itself into an organization with wider aims, and has assumed the name of the Colorado Biological Association. The objects of the Association in its new form are the detailed investigation and recording of the fauna and flora of Colorado, recent and fossil. The Association hopes to become the highest authority on all matters connected with the biology of the State, both from the scientific and the economic points of view, and through its Secretary and referees will place itself at the service of the scientific and general public in answering all questions within the scope of its investigations, and in identifying specimens that may be submitted for this purpose.

THE general Report, by Prof. Egoroff, on the observations made in Russia and Siberia during the eclipse of the sun of August 19, 1887, under the direction of the Committee of the Russian Physical and Chemical Society, is now published (in Russian) in the Journal of the Society (vol. xx. 6). Seven stations were provided with observers and instruments (at Wilno, Nikolsk, Tver, Petrovsk, Vyatka, Krasnoyarsk, and the Bay of Possiet), but only at three of them—Petrovsk, Krasnoyarsk, and Possiet—could the eclipse be observed in detail. Fourteen excellent photographs were taken at Krasnoyarsk, and of these two are reproduced in M. Egoroff's Report, as also several drawings of the corona which were made by hand at Polotsk, Vladimir, and places in Siberia. Various observations with regard to the position of the protuberances and the shape of the corona are given in the Report, and its general conclusions are as follows:—(1) The corona is not a merely optical phenomenon: it has a real existence, and it maintained its shape not only during the whole of the eclipse at each spot where it was observed, but also at spots as far distant from one another as Polotsk and Possiet (distance, 6000 miles). (2) The corona of 1887 is a representation of those coronæ which correspond to a minimum of spots on the sun. The like were observed in 1867 and 1878. Its peculiarities are interesting in connection with the question as to the structure of the sun and its corona. Mr. Norman Lockyer, in his work on "The Chemistry of the Sun," expresses regret that he could not see, in 1886, while in Grenada, those *panaches* on the poles of the sun which he had carefully studied in 1878. The photographs of M. Hamontoff (Krasnoyarsk) prove that those currents existed, and that they were well seen on August 19, 1887. (3) There is a correlation between the distribution of the rays of the corona and the position of protuberances. (4) The brilliancy of the light of the corona is of the same order as that of the full moon (as shown by several photometric measurements, and also by the visibility of α Leonis in the rays of the corona). (5) The spectrum of the corona was an uninterrupted one, with feeble Fraunhofer lines. Bright lines were not seen, except for a moment at Petrovsk, where M. Stonaewicz saw the green bright line; the cloudiness of the sky, which resulted in a great amount of reflected light, probably prevented the bright lines from being seen. (6) Polarimetric measurements require a bright sky; under other conditions false conclusions might be arrived at. (7) Both atmospheric pressure and temperature are lowered during the eclipse, the minimum coming at a later time than the middle time of the full eclipse.

A GREAT number of meteorological observations having been made during the eclipse at various places in Russia and Siberia, Prof. Hesehus now sums them up in the same issue of the Journal of the Russian Physical and Chemical Society (xx. 6). It appears from the curves which he has drawn after having availed himself of observations made at twenty-five different stations, that the eclipse resulted in lowering the atmospheric pressure by about 0·2 mm., the minimum being reached a few minutes (about five to ten) after the time of the full eclipse. The fact is best explained by the condensation of vapour in the atmosphere. The temperature was lowered by an average of 1°·6 C. in the shade—the minimum being reached ten minutes after the full eclipse; and by about 8°·6 in the sun's rays—the minimum being attained in this case three minutes after the full phase of the eclipse. The force of the wind also was reduced, probably on account of the condensation of vapour in the atmosphere. The data as to the influence of the eclipse on the magnetic needle are contradictory. The influence of the eclipse on plants and animals was well pronounced. The *Acacia armata* folded its leaves, while the *Nicotianæ* and *Mirabilis jaloppa* opened their flowers. In the marshy spots of Siberia, such as Turinsk, the mosquitoes made their appearance, as they usually do in the evenings. The well-known facts as to the uneasiness and fear which are felt by higher animals were confirmed. On the whole, the Physical Society expected more important results when it organized meteorological observations at so many stations provided with physical instruments, but the weather was unfavourable to the work of the observers. Hilger's spectrograph for photographing the ultra-violet parts of the spectrum of the corona with the view of detecting traces of carbon and carboniferous compounds, could not be used on account of the weather.

THE same periodical contains a record of Prof. Mendelejeff's impressions during his balloon ascent at Klin. The Russian chemist saw the corona from his balloon for only twenty seconds. His view of the sun was unfortunately obstructed by a cloud.

THE Meteorological Council have published Part 5 of "Contributions to our Knowledge of the Meteorology of the Arctic Regions." The four previous parts contained principally the meteorological results furnished by the Franklin search expeditions which wintered to the eastward of longitude 120° W. between 1848–58, but also included the results available from the date of Sir W. E. Parry's expedition in 1819. Part 5 relates to the region of Behring Strait, and to the search expeditions in that direction between 1848–54. The whole series has been discussed in a uniform and most complete manner by Mr. R. Strachan, and all the available information relating to the physical phenomena, and to the movements of animals and birds, has been thoroughly exhausted. The work contains most valuable data for scientific inquiry, and for use in any future expeditions to those remote regions.

The additions to the Zoological Society's Gardens during the past week include two Toque Monkeys (*Macacus pileatus* ♂ ♀) from Ceylon, presented by Mrs. Ellen Hodson; a Moustache Monkey (*Cercopithecus cephus* ♂) from West Africa, presented by Mr. Andrew Allen; a Common Otter (*Lutra vulgaris* ♀), British, presented by Mr. John Crisp; a Japanese Deer (*Cervus sika* ♂) from Corea, presented by Capt. H. C. Eagles, R.M.L.I.; three Virginian Opossums (*Didelphys virginiana* ♂ ♀ ♀) from North America, presented by Mr. G. F. Whateley, R.N.; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. George Berry; a Collared Mangabey (*Cercocebus collaris*) from West Africa, a Grey Ichneumon (*Herpestes griseus*) from India, two Cockateels (*Calopsitta novæ-hollandiæ*) from Australia, four Snow Geese (*Chen albatus*) from North America, a Larger Hill-Mynah (*Gracula intermedia*) from Northern India, deposited; four Radiated Tortoises (*Testudo radiata*) from Madagascar, purchased; an Indian Swine (*Sus cristatus*) from India, a Nilotic Trionyx (*Trionyx ægypticus*) from the River Nile, received in exchange.

OUR ASTRONOMICAL COLUMN.

The Ring Nebula in Lyra.—Prof. Holden reports that this object, as seen with the great Lick refractor, shows far more detail than had been detected either by Lassell with his 4-foot reflector, or by the Washington observers with the great 26-inch refractor. With these telescopes thirteen stars had been seen in an oval outside the ring, and one star had been seen within it. The 36-inch Lick telescope shows twelve stars within the ring or projected upon it, and renders it obvious that the nebula consists of a series of ovals or ellipses: first the ring of stars, then the outer and inner edges of the nebulosity, next a ring of faint stars round the edges of the inner ring, and last a number of stars situated on the various parts of the nebulosity and outer oval.

Comets Brooks and Faye.—The following ephemerides are in continuation of those given in Nature, vol. xxxviii. p. 576:—

1888.	Comet 1888 *c* (Brooks). R.A. h. m. s.	Decl. ° ′	Comet 1888 *d* (Faye). R.A. h. m. s.	Decl. ° ′
Oct. 29	16 47 54	0 14·1 S.	7 53 14	8 22·3 N.
31	16 52 12	0 59·0	7 55 34	7 58·4
Nov. 2	16 56 26	1 42·0	7 57 48	7 34·6
4	17 0 33	2 23·2	7 59 53	7 11·0
6	17 4 36	3 2·7	8 1 50	6 47·5
8	17 8 35	3 40·6	8 3 39	6 24·3
10	17 12 30	4 17·0	8 5 21	6 1·3
12	17 16 21	4 51·8 S.	8 6 55	5 38·7 N.

Comet 1888 *e* (Barnard).—The following ephemeris for Berlin midnight is by Herr A. Berberich (*Astr. Nach.*, No. 2861):—

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Oct. 28	5 40 6	3 48·7 N.	0·3370	0·1498	6·0
30	5 32 12	3 17·5			
Nov. 1	5 23 39	2 44·6	0·3317	0·1214	7·1
3	5 14 24	2 9·9			
5	5 4 28	1 33·4	0·3265	0·0949	8·2
7	4 53 50	0 55·3			
9	4 42 31	0 16·0 N.	0·3214	0·0716	9·4

The brightness at discovery is taken as unity.

American Observatories.—Prof. W. W. Campbell has been appointed to the position in the Observatory of Ann Arbor which was held by Mr. J. M. Schaeberle previous to his appointment as assistant at the Lick Observatory.

The Observatory at Iowa College, Grinnell, Iowa, possesses a fine equatorial of 8 inches aperture by the Clarks, and strong efforts are being made to obtain a transit-instrument and chronograph, and sidereal and mean clocks, so that a time service may be maintained.

The Carleton College Observatory, Northfield, Minnesota, is now a very well equipped institution, with transit and prime vertical instruments, besides the old equatorial of 8¼ inches, and the new one of 16 inches aperture, the 30-foot dome for which is already in its place. A standard time service has been organized, and standard "Central" time—that is, time six hours later than Greenwich mean time—is distributed to nine railways, embracing in all more than 12,000 miles of road. The charge of this department has been given to Miss C. R. Willard. Dr. H. C. Wilson, late of Mount Lookout, Cincinnati, is Assistant Professor of Astronomy at Carleton College, and Prof. W. W. Payne, editor of the *Sidereal Messenger*, is Director of the Observatory.

Messrs. Fearnley (the Director of the Christiania Observatory) and Geelmuyden have published zone observations of the stars between 64° 50′ and 70° 10′ north declination, made at the Observatory. The volume is a large one of 319 pages. The observations are preceded by an introduction giving an account of the work.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 OCTOBER 28—NOVEMBER 3.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 28

Sun rises, 6h. 50m.; souths, 11h. 43m. 49·0s.; sets, 16h. 38m.: right asc. on meridian, 14h. 12·8m.; decl. 13° 22′ S. Sidereal Time at Sunset, 19h. 8m.

Moon (at Last Quarter October 28, 2h.) rises, 22h. 8m.*; souths 6h. 12m.; sets, 14h. 6m.: right asc. on meridian, 8h. 40·1m.; decl. 19° 31′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	7 40	12 12	16 44	14 41·6	17 23 S.
Venus.....	9 35	13 38	17 41	16 7·7	21 48 S.
Mars......	12 6	15 47	19 28	18 17·0	24 58 S.
Jupiter....	9 49	13 57	18 5	16 26·4	21 13 S.
Saturn....	23 33*	7 0	14 27	9 28·1	15 51 N.
Uranus...	5 12	10 41	16 10	13 10·1	6 47 S.
Neptune..	17 46*	1 32	9 18	3 59·4	18 48 N.

* Indicates that the rising is that of the preceding evening.

Oct.	h.	
29	4	Saturn in conjunction with and 1° 16′ south of the Moon.
Nov.		
1	0	Mercury in inferior conjunction with the Sun.
1	21	Venus in conjunction with and 1° 31′ south of Jupiter.
3	12	Mercury in conjunction with and 4° 50′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·4	81 16 N.	Oct. 31,	2 9 *m*
Algol	3 0·9	40 31 N.	,, 30,	20 29 *m*
λ Tauri	3 54·5	12 10 N.	,, 30,	4 38 *m*
			Nov. 3,	3 30 *m*
R Canis Majoris	7 14·5	16 12 S.	Oct. 31,	0 51 *m*
			Nov. 1,	4 7 *m*
U Monocerotis	7 25·5	9 33 S.	Oct. 31,	*M*
S Cancri	8 37·5	19 26 N.	,, 29,	23 42 *m*
U Ophiuchi	17 10·9	1 20 N.	Nov. 1,	18 12 *m*
β Lyræ	18 46·0	33 14 N.	,, 1,	20 0 *M*
T Vulpeculæ	20 46·7	27 50 N.	Oct. 29,	20 0 *M*
			,, 30,	21 0 *m*
Y Cygni	20 47·6	34 14 N.	,, 29,	3 0 *m*
			Nov. 1,	3 0 *m*
δ Cephei	22 25·0	57 51 N.	,, 2,	1 0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near ν Arietis	43	22 N.	Slow; brilliant.
,, 30 Tauri	56	10 N.	Slow; brilliant.
,, β Tauri	78	30 N.	Swift.

ON THE ORIGIN AND THE CAUSATION OF VITAL MOVEMENT.[1]

I.

AMONG the phenomena of life the movement of masses, or mechanical work, takes a prominent place. It is the most accessible of all the vital processes to our sensual perceptions, so universally distributed, and so bound up with most of the activities of organisms that it might almost be designated the incarnation of life.

In saying this it must be understood that vital movement is by no means exclusively confined to animals—that it is not, as was once believed, a special animal function; on the contrary, it is an attribute of *all* living matter, as well of the lowest creatures as of the most highly developed plants, so that, however extraordinary it may appear, the activity of our muscles which enables us to transform sensation into action finds an analogue in the plant. Our conviction of the inter-connection and profound unity of all living things has thus a physiological foundation, based as it is not merely on the community of derivation and of structure of living things, but also on the proof of similar activities.

If a division of the morphological from the physiological is in any way permissible, it may be said that the unitary conception of life for which our age is distinguished rests in a higher degree on the knowledge of vital *processes* than is commonly recognized, and in fact is just as much founded on physiological experience as on that of the *forms* of the organism.

From the traditional conception of life, which scarcely contained more than that everything between life and death is the antithesis of the not living, it is a long road we have had to travel to attain to the modern conception of the *real* unity of life; and a remarkable road, since it bears witness to the confident anticipation of victory, in face of all impediments raised up by science itself. Movement, and nothing less, had been placed at the summit of that antithesis, which physico-chemical research in the animal and vegetable kingdom had revived with the discovery that the plant transformed kinetic into potential energy, and the animal the latter into the former. While the animal made use of oxygen to generate heat and perform work through the metabolism of its substance, the plant made use of the heat in reducing and synthetic processes for the accumulation of potential energy in the form of its own consumable substance and the expired oxygen.

With whatever unassailable correctness this conception comprehends life as a whole, affording a pleasing solution of its antithesis by referring animal activities to nourishment by the plant, the latter to the products of the combustion of the animal body, and both in the last instance to the forces of the sun as original source of all life, yet *this* did but cast up the sum-total of the processes of life, and did but express more intimately than before that which divides the most highly-developed branches of the animal and vegetable kingdom, in which the divergence of forms and arrangements is greatest. For *by the side of* this distinction there exists even between man and the most highly elaborated plant a connection of a kind quite other than the symbiotic interdependence through the medium of light, air, and food, a community, however, which is not disclosed until we go back to the ultimate elements of organization.

As in the animal synthetic processes are not wanting, without which it could not even produce a molecule of the colouring matter of its blood, so in the plant we are acquainted with dissociations and combustion, and also with evolution of heat and movement of masses; not that by this I refer to those coarser movements which are referable to turgescence, but primitive movements, which we find first in the smallest elementary organisms, of which all living beings are made up.

We have almost in our own persons lived to see the old anticipation of a single kingdom of living things become gradually an established truth through the discovery of the cell. After the ground-lines of the construction of plants and animals out of originally similar *nucleated* cells had been established by Th. Schwann, and since Darwin's immortal work enabled us to derive everything that ever lived or will live from one single cell, we have come to realize that every single organism renews in itself the work of past ages, and again builds itself up from a germ similar to that from which its most ancient ancestors started.

This conviction has become so firmly implanted in our generation that now we scarcely feel the gaps which still exist in our actual knowledge, and almost unjustly under-estimate that which the investigations of our contemporaries yet add to the cell-theory, as if it were mere work of repetition. And yet it has been very extensive and decisive—for example, the recent researches upon the intimate structure of the cell-nucleus—since nothing less results from it than that the reproduction of the cell by fission takes place identically, down to the most minute details, in all animals and plants.[1]

Now, if the *shaping* of the cell and all the *fashioning* of forms is an *activity*, and if Morphology, "since it has made the *arising* of form more its study than the describing of what is already completed," has become part of Physiology, it might be possible and conceivable that research directed to *all* activities and going beyond the *visible* form to the chemical components of the structures and the transformation of substance and force, should observe great differences in processes where all our morphological experience would only have shown identity. We were near enough to this point; for if it were true, as was long assumed, that that which is the bearer and the seat of the most essential of all vital processes *in* the cell is completely formless, it is not easy to see why the form should be so determinant of function.

We have hope that this is not so, and will endeavour to show in Movement the functional as well as the morphological unity of all living matter.

As I have already said, there is an elementary kind of movement in the cell, carried out by the cell-body—that part of the cell which, in contradistinction to nucleus, membranes, and various inclosures, has been designated protoplasm. The protoplasm moves itself, as in the case of certain free-living Protozoa, like the long-known Amœba, like the so-called sarcode—in many cases better comparable to the movement of the pseudopodia of Rhizopods. The resemblance of the latter to what was formerly called the sap-current in many plant-cells, led Ferd. Cohn[2] to interpret plant protoplasm as sarcode, an idea actively supported by Max Schultze,[3] the best authority on pseudopodial movement. It is not necessary to say here how widespread protoplasmic movement is, for there cannot be a cell that does not present it at some stage of its existence. Doubt on this subject can only exist in regard to the smallest of all organisms, those of fermentation, of putrefaction, and of pathogenic activity which are too small for observation. But even in these, from the movement they perform as a whole, we have grounds to infer the existence of a protoplasm.

It is proved that protoplasmic movement does not follow external impulses or currents, but is a spontaneous activity. It may go on in opposition to gravity, and overcomes frictional resistance, as shown by the mass itself moving forward on surfaces of every kind, and being able to drag heavy bodies along with it. It is proper mechanical work.

The cause of the movement can only be an internal one, residing in the contractile substance itself, and can only consist of chemical processes taking place within the peculiar pasty, slime-like mass. Yet the question had to be put whether these processes were not first set up by something coming perhaps from the outside, for the movement changes, sometimes stops or takes place more slowly, or occurs but partially, and may by many means be artificially aroused or diminished.

At this point experimental physiological research had to step in, attacking the problem in the same way as it had long before done in the case of the most highly-developed contractile structures, the muscles. A muscle behaves so far just like protoplasm that its contraction does work, which can only depend on chemical transformations of its own substance, during which potential is converted into kinetic energy; but it differs in that a distinct impulse from without is needed to set the game going. In normal conditions it receives the initiating impulse from its nerve, and nothing else appears able to take its place, since nothing that might otherwise act upon it, such as the motion of

[1] "On the Origin and the Causation of Vital Movement (*Ueber die Entstehung der vitalen Bewegung*)," being the Croonian Lecture delivered in the Theatre of the Royal Institution on May 28, 1888, by Dr. W. Kühne, Professor of Physiology in the University of Heidelberg.

[1] The most complete exposition of these important later discoveries on the reproduction of the cell is to be found in the book of W. Flemming, "Zellsubstanz, Kern und Zelltheilung," Leipzig, 1882. Cf. the "Kurze historische Uebersicht" (p. 385), with the quotations from the works of Schneider, Strassburger, Bütschli, Flemming, O. Hertwig, and the researches of Auerbach, Balbiani, van Beneden, Eberth, Schleicher, Balfour, and others.

[2] "Nachträge zur Naturgeschichte des Protococcus pluviatilis," *Nova Acta Acad. Leopold. Cæsar.*, vol. xxii, Part 2, p. 605 (1850).

[3] "Ueber den Organismus der Polythalamien," Leipzig, 1854.

the blood or changes in its constitution, disturbs its repose. But if we let electric currents traverse the muscle, or if we suddenly change its temperature, or act upon it mechanically or chemically, contractions result which do an amount of work out of all relation to the insignificant impulse; the means employed only set going the process peculiar to the muscle; and this is what is meant when we term them *stimuli*, and the faculty of muscles to react to them irritability.

Now, is protoplasm irritable in this sense? Experiments on objects of every kind have answered this affirmatively, and, more than that, have even shown a striking agreement with the irritability of muscle. Of the above-mentioned agents, besides rise of temperature, which ultimately sets all contractile cell-substance in maximal contraction—a heat tetanus[1] which disappears with cooling—the electric current has shown itself the most efficient, the stimulus which most surely excites muscles of every kind as well as all nervous matter, and has thence become the most indispensable instrument of physiology.

I may be permitted to adduce an example because it illustrates what is typical and essential.[2] It is the case of the fresh-water Amœbæ. Every time these organisms, moving like melting and rolling drops, are subjected to an induction shock, they contract almost to a sphere, and assume the spherical form completely if the shocks follow each other at short intervals, being by this means fixed for a longer time in this condition. Feebler shocks, which singly have no effect, become effective by summation when applied in quick succession, just as in the case of muscle. If the movements of the animal by itself are sluggish, on electrical stimulation they are strengthened and accelerated. Thus the stimulation increases the natural movement, and if increased stimulation brings about repose, it is only the apparent repose of prolonged maximal contraction, like that of our muscles when we hold out a weight for some time at arm's length. All protoplasm behaves in this way from whatever source derived. Larger masses which cannot contract to one sphere (as in many plant-cells, or those great cake-like giant masses of the plasmodium of the Myxomycetes) form several such spheres in part connected by thread-like bridges. Everywhere the taking on of a figure with smallest surface is the result of stimulation and the expression of augmented contraction.[3] That which was outstretched becomes shorter and in like measure thicker, just as a muscle swells when it shortens itself.

Since protoplasm, which either does not move at all spontaneously or so slowly that we cannot perceive it, reacts in the same way to stimuli, we must in the case of ordinary movements infer the existence of processes originating them either in the interior, *i.e.* automatic stimuli, or of external processes which had at first escaped us. Whoever sees for the first time the action of any one of the simpler independent Protozoa cannot avoid the idea that psychic activity in the strictest sense of the term lies behind it, something like will and design. He sees the elementary being seeking and taking up food, avoiding obstacles, and when touched by foreign objects energetically drawing back, so that he infers sensation also. Possibly he has struck the correct solution—at least we could not refute him—but we should put his deduction to a hard proof if we showed him the same phenomena in the colourless cells of his own blood, or in the protoplasm of a plant-cell; and if we placed him before the rhythmically contracting cells from the beating heart of a bird's egg incubated barely a couple of days, he would certainly wish with us that the search were for a more material cause, and hope that among them some chemical or physical cause might be found to set up the process. Biology cannot indeed yet claim to have established such causes in explanation of the automatism of protoplasm, but no one will blame the science for continuing the search for them.

Some causes are already excluded, *e.g.* light, although there are a few micro-organisms whose movements are excited by it.[4] Fluctuations of temperature may also be left out of account. On the other hand, oxygen has a notable influence.[5] Withdrawal of the vital air stops all protoplasmic movement, though without killing the cell-body, as is seen from the fact that after the loss of automatism electrical stimulation can supply its place, and that the normal movements return on readmitting the air.

We might thus consider oxygen the prime mover in automatism, and processes of oxidation its essence, did we not remember that many objects need very prolonged withdrawal of the gas to come completely to rest. This might, however, depend upon the difficulty of removing the last traces of oxygen completely, or it may be that these cannot be removed by the means adopted, but must remain until consumed by the protoplasm itself.

Since protoplasm is of pap-like softness, and may be in a state of rest or motion at any spot, its exterior limits are just as capable of change as everything within it is capable of quitting its position and taking up any other. Thus the movement cannot become more ordered until obstacles confine and direct it. Between the perfected organization of contractile substance in muscle and that of protoplasm capable only of unordered movement, we meet a succession of significant steps by means of which we can see how the ordering was attained. The first step would seem to consist in the uncommonly widespread flagellar and ciliary motion, in which an elastic structure, affixed on one side to the contractile mass, is drawn down or bent by its movement, straightening out again in the rhythmic pauses of repose. A further step, at which the contraction can only take place along an axis, consists in the arrangement of the protoplasm in fine strips wholly or partially surrounded by elastic walls, or again in elastic fibrils being embedded in protoplasmic processes. In this case we have actual primitive muscles before us, of which the most elegant examples are known in the Infusoria among the Vorticellæ and Stentores. The movement of these structures is quite like that of muscle. The strips lengthen and thicken, and they may also be contracted in quick twitches or in a prolonged tetanus, the relaxing, like the stage of diminishing energy of all muscles, always proceeding more slowly than that of the increasing energy *before* the maximum.

The muscles of the unicellular Infusoria, no longer doubtful in a physiological sense, show us muscle as a constituent of the cell, and differentiation, without the production of new cells specially endowed for the purpose, taking place in *one* cell to the extent of elaborating contractile elements determinate in form and precise in work. It is very noteworthy that side by side with these muscular strips provided with highly regulated movement, other protoplasm persists, which continues uninterruptedly its ordinary unordered movements, while no such unrest is to be remarked in the muscles. On the contrary, these latter are only used from time to time, apparently for attaining distinct objects. We get the impression that the automatism has, as it were, been lost by this portion, so that it must wait for stimuli to reach it from other parts of the cell. If oxygen really applies the first spur to the protoplasm, it has no direct power over the primitive muscle, so that compared with the protoplasm the muscle is endowed with a diminished irritability.

It has often been said that protoplasm presents the complete set of vital phenomena—assimilation, dissimilation, contractility, automatism, resorption, respiration, and secretion, and even reproduction by dividing. Leaving reproduction on one side, as now disputed, and on good grounds, we can assent to the assertion, and examine which of those functions remain for the products of differentiation. In the case of the muscle, we find it to be all of them with the exception of a single one; for, while it undoubtedly takes part in nutrition as in respiration and carries on a chemical exchange, all of which are indispensable for contractility, *i.e.* for its work, and since secretion generalized signifies merely the throwing off of broken-down products, it is wanting *only* in automatism, that faculty of reacting to certain stimuli, which remained reserved for protoplasm. In this there is nothing opposed to the assumption that protoplasm as opposed to muscle possesses elementary *nervous* properties.

The above is sufficient to show the transition to the very highly developed motor apparatus which distinguishes the animal kingdom from almost its lowest stages—I mean the bi-cellular apparatus, which consists of separate cells united only for *one* purpose, one of which presents the exciting nerve, the other the obedient muscle.

From past experience we know that division of the nerve, or, more correctly speaking, removal of the nervous cell substance,

[1] W. Kühne, "Untersuchungen über das Protoplasma und die Contraktilität," Leipzig, 1864, pp. 42, 66, 87, 102.

[2] Kühne, *ibid.* p. 30.

[3] Th. W. Engelmann, five years later, confirmed the passage of protoplasm, especially of Amœba, to the spherical form on stimulating; cf. his "Beiträge zur Physiologie des Protoplasmas," *Pflüger Archiv*, vol. ii. 1869, p. 315, and "Handbuch der Physiologie, herausg. von L. Hermann," vol. i. p. 367.

[4] Engelmann, "Ueber die Reizung des contraktilen Protoplasma durch plötzliche Beleuchtung," *Pflüger Archiv*, vol. xix. p. 1.

[5] Kühne, *l.c.*, pp. 50, 67, 88–89, 104–106. The cessation of the so-called sap-stream in the cells of *Chara* on excluding the air by oil was observed as far back as 1774 by Bonaventura Corti; and further by Hofmeister in *Nitella* under the influence of reduced atmospheric pressure. Cf. Engelmann in "Handbuch der Physiol. von Hermann," vol. i. Part 1, p. 362.

condemns the muscle to rest. The stimuli then start from the nerve-cell, to them the muscles react by doing work, and they are conveyed to the muscles through the continuation of the cell which the nerve-*fibre* presents. We need not yet trouble ourselves how the excitation of the nerve-cell arises, whether through external—sensory—stimuli, or through an enigmatical psychic act, or through chemical influences; certain it is that these were, before the division of the nerve, the sole impulse to the muscle's movements. But what the muscles lack we can supply artificially, and more; we can put the nerve-remnant in such manifold states of excitement as it never before experienced from its cell-body, so that the muscle is compelled to undergo many kinds of movement quite new to it, and we can attain the same result by direct stimulation of the muscle.

In the circle of these experiences arose the controversy, not yet quite ended,[1] as to muscular irritability; properly, the question whether it was, in general, possible to stimulate anything artificially that is not nerve—that is, to set free the activity peculiar to a non-nervous structure by the means at our command.

Haller, who was the first to occupy himself minutely with the stimulation of muscle, and introduced the term irritability, decided, but only incidentally and by the way, that the stimulus could strike also the ramifications of the nerve in the muscle, and he was far from interesting himself in the question in the modern sense, or from suspecting the point of view from which the independent irritability of muscle would later on be questioned. We ought not to blame him much for the latter, since even to-day it is not easy to understand the motives of an opposition now continued for more than a century. At the outset, if I am not mistaken, the teaching of the Animistic, or, as it might now be called, the Neuristic school, led to the conception that not only the excitation and regulation of the various functions, but the actual endowment of the several tissues with their respective activities, was the work of that everywhere predominant and distinctly animal contrivance, the nervous system.

In connection with this, there seems to have arisen the view of the ubiquity of nerves—that is, of so fine a penetration of the parts with nerve radiations, that, especially in muscle, not the smallest particle free from nerve could be demonstrated, a view which, on the strength of microscopic research, is coming up again at the present day in a constantly new dress, and finds energetic adherents,[2] but, as we shall see, to be refuted, especially by experiment. If we disregard this, we shall find the tendency to consider only nerves as excitable, in some degree founded on the differentiation which transferred automatism to the nervous matter, robbing all the remaining tissues of irritability, so that they only retained the faculty of reacting to the stimulated nerve with which they were bound up. This was as much as saying it was impossible artificially to replace the nervous stimulus, or that, if we did succeed, we were strictly imitating it, in which case, indeed, we should have come unawares upon the solution of the problem of motor innervation. Against such arguments it availed nothing to point out the excitability of nerveless sarcode, as was often done in favour of irritability; for, just as it was formerly useless, because the real genetic connection of sarcode and muscle was not known, so to-day it would have to be rejected, because automatic protoplasm can also be correctly considered nervous.

A non-irritable muscle would strike us as strange enough, and, against all expectation, different from the nerve, when we consider that the nerve-fibre, although incapable of being affected by all the natural stimuli which excite its ganglion-cells, free, that is, from automatism, is artificially excitable at every spot by the most different agents. However, we have no further need of such considerations, since the question of irritability lies within a region where, instead of speculation, observation and experiment have become decisive.

[1] Cf. J. Rosenthal, "Allgemeine Physiologie der Muskeln und Nerven," Leipzig, 1877, p. 255.

[2] J. Gerlach, "Ueber des Verhalten der Nerven in den quergestreiften Muskelfäden der Wirbelthiere," *Erlangen Phys. Med. Soc. Sitzber.*, 1873. "Das Verhältniss der Nerven zu den willkürlichen Muskeln der Wirbelthiere," Leipzig, 1874. "Ueber das Verhältniss der nervösen und contraktilen Substanz der quergestreiften Muskels," *Archiv Mikrosk. Anat.*, vol. xiii. p. 399. A. Foettinger, "Sur les terminaisons des nerfs dans les muscles des insectes," *Archives de Biol.*, vol. i., 1880. Engelmann, *Pflüger Archiv*, vol. vii, 1873, p. 47; vol. xi., 1875, p. 463; vol. xxvi. p. 531. In these publications it is sought to prove that the motor nerves pass either into the interstitial nucleated substance of the muscle (therefore into the sarcoglia) or into the layers of the "Nebenscheiben." This latter view is opposed by, among others, A. Rollett, in his thoroughgoing exposition of the structure of muscle (Vienna, *Denkschriften der k. Akad.*, vol. xlix. p. 29), and W. Kühne (*Zeitschr. f. Biol.*, vol. xxiii. p. 1.

As a matter of fact, the older statements, long considered a good basis for opposing irritability, are incorrect, as, for instance, that an excised piece of muscle in which no nerves could be seen with the lens did not twitch on stimulating it.

We can show you a little piece, 3 millimetres long, from the end of the sartorius muscle of the frog, in which the best microscope discovers no traces of nerves, easily made recognizable by osmium-gold staining (Fig. 1). Such a piece, transversely cut off, twitches, as we know, at each effective muscular stimulus. Pieces which can be obtained free from nerves from many other muscles behave in the same way, as, for instance, pieces from the delicate muscles of the pectoral skin of a frog (Fig. 2).

Further, the assertion was incorrect that everything that excited the nerve made the muscle twitch, and *vice versâ*; for we see here a sartorius suspended in ammonia vapour, contracting

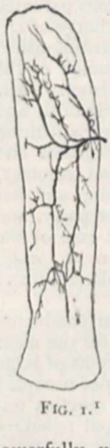


FIG. 1.[1]

FIG. 2.

powerfully, while a nerve entirely submerged in liquid ammonia appears wholly unstimulated, for it does not rouse the thigh muscles from their repose.

Conversely, we see a thigh whose nerve dips into glycerine in maximal contraction, and, on the other hand, a muscle in contact at its excitable end with the same glycerine remains at rest, yet it twitches if I dip it in up to its nerve-bearing tracts.[2]

These are old experiments,[3] and it is admitted they have overthrown the earlier opinion. But they have not been deemed sufficient to prove muscular irritability, because the ultimate endings of the nerves might have an irritability other than that of their stems. This is the only objection still raised. One could wish no other were conceivable, for this one admits of refutation.

(*To be continued.*)

THE HEMENWAY EXPEDITION IN ARIZONA.[4]

DR. JACOB L. WORTMAN, of the United States Army Medical Museum, has just returned from Arizona, where he has spent the winter and spring attached to the Hemenway South-Western Archæological Expedition under the direction of Frank Hamilton Cushing, which was mentioned in the March number of the *Naturalist*, and he confirms the importance as well as the genuineness of the discoveries of Mr. Cushing. The Expedition is thoroughly equipped and well organized, and its investigations have been conducted in a vigorous and scientific manner, with special reference to the many details which go to make collections of this character of value to the scientific student. Not only have the ruins been carefully surveyed and mapped, but each specimen has been labelled with great care, in

[1] The drawings, Figs. 1, 2, 3, 5, 8, are taken from the papers of Dr. K. Mays, "Histophysiologische Untersuchungen über die Verbreitung der Nerven in den Muskeln" (*Zeitschr. Biol.*, vol. xx. p. 449), and "Ueber Nervenfasertheilungen in den Nervenstämmen der Froschmuskeln" (*Zeitschr. Biol.*, vol. xxii. p. 354); Figs. 9–13 are from the author's papers in *Zeitschr. Biol.*, vol. xxiii. pp. 1–148, Plates A–Q.

[2] The experiments were performed during the lecture by projecting on the wall images of the preparations enlarged some thirty times.

[3] Kühne, "Ueber direkte und indirekte Muskelreizung mittelst chemischer Agentien," *Müller's Archiv. f Anat.*, 1859, p. 213.

[4] Reprinted from the *American Naturalist*, June 1888. The writer is Mr. Thomas Wilson, of the Smithsonian Institution.

such a manner as to indicate exactly where found, together with all such other facts in connection with it as will be of use to the student.

The Expedition has for its object the study of the ancient civilization of the south-west, and if the results of the first year's work can be taken as an index of what it will accomplish, we may confidently look for a solution of this perplexing question. Already a large and valuable collection illustrative of the culture of these prehistoric people has been secured, and it is a matter of congratulation that it has been so collected that the scientific student can get all out of it that it can be made to tell.

Mr. Cushing's ethnological training has been in such a direction as to give him a peculiar fitness for the position which he occupies, having spent six years or more in studying the social institutions, customs, habits, religion, and language of the modern Pueblo Indians, and this thorough knowledge of these is indispensable to the proper interpretation of the facts gathered by the Expedition. The anthropological work is in charge of Dr. Herman Ten Kate, a native of Holland, son of the distinguished artist of that name. Dr. J. L. Wortman, the Anatomist of the Army Medical Museum of Washington, is his assistant. Mr. Adolph Bandelier, whose knowledge of the early Spanish and Mexican records is well known, is connected with the Expedition as historian. Mr. Chas. A. Garlick is the civil engineer and topographer. Mr. Fred. Hodge is the draughtsman and secretary, while Mr. Yates is the photographer. Mrs. Cushing and her sister, Miss Margaret Magill, are also members of the party, and have rendered important aid in the classification and care of the specimens. Miss Magill's artistic talents have been of special service to the Expedition by reason of her clever sketches and drawings of the specimens *in situ*.

The locality in which explorations have so far been conducted comprises the Gila and Salt River Valleys, situated for the most part in South-Western Arizona. They are fertile tracts of large extent, and there can be little doubt that they were once occupied by a thrifty and prosperous people, whose history remains unwritten. The Rio Salado (Salt River) is the principal tributary of the Gila, and affords abundant water to irrigate its valley, a tract including a half a million acres, or more. The land for the most part is covered with cactus, sage brush, grease wood, and mesquite trees, but when cleared and brought under irrigation is made to produce abundantly almost any and all the crops of civilized husbandry. Fruits and cereals grow in profusion, and the land is said to be well adapted to the growth of cotton and tobacco. The land rises from the river at a gentle slope, a fact which is of great importance to a system of irrigation. At the upper or north-western end of the valley, however, the river is bordered upon the south by a mesa which slopes away to the Gila, no mountains intervening between the streams at this point. Water brought from the Salt River upon this mesa can be made to flow a distance of twenty miles to the south, or into the Gila, and will irrigate a tract many miles in extent. This these ancient people did, and, scattered over this plain from the Salt to the Gila are to be found the ruins of their villages, towns, and cities, long since crumbled into dust, and now overgrown with a thick mesquite forest.

Their houses were for the most part built along the main irrigating canals, and are now indicated by irregular truncated mounds, of various dimensions, thickly strewn with fragments of broken pottery. Excavating these mounds, the foundations or ground plans of the buildings were discovered. Some of them were large, often several hundred feet square, and, according to Mr. Cushing, three or four stories in height. They were constructed usually of adobe bricks, but in some instances they inclosed the adobe between rows of upright posts wattled with cane or willow. Each house would contain from two to five hundred rooms, and is thought by Mr. Cushing to have been the house of a clan. A considerable grouping of the communal houses constitutes what Mr. Cushing has called the cities of Los Muertos, Los Hornos, Los Guanacas, Los Pueblitas, Los Acequias, &c. They are not built with the regularity of our modern cities. Los Muertos (the city of the dead) can be traced for three or four miles, and includes some forty or fifty of these great communal structures that have been so far unearthed, but if systematic search be continued double or quadruple this number will probably be found.

A characteristic feature of each of these cities, and one which probably led Mr. Cushing to designate them as such, is a ruin of much greater dimensions than any of the rest, which is invariably surrounded by a strong outside wall, inclosing a considerable space or yard. This inclosed space around the large building or temple is supposed to have been for the purpose of protection in times of war, when pressed by an enemy, and the large building itself served not only as a store-house for a reserve supply of provisions, but also, if we are to judge from the remains and implements, was the abode of the ruler or chief priest of the people of the town.

While no accurate computations have been attempted, it is supposed, taking into consideration the number of towns or cities known to have existed in the Gila and Salt River Valleys that the population could not have been less than two hundred thousand. There is every reason to believe that these places were not successively, but simultaneously occupied, especially when we remember that they constructed large irrigating canals for a distance of fifteen or twenty miles, which with their rude implements must have been a gigantic undertaking. Their irrigating system was extensive and complete, and covered almost, if not quite, all the cultivable parts of the two valleys. The present inhabitants of the soil have taken advantage of these ancient waterways, constructed at such expenditure of prehistoric labour, and they now run many of their irrigating canals in these ditches. These ancient canals were constructed with care. A cross-section exhibits a series of terraces widening towards the top, so that a large or small quantity of water could be accommodated and a good depth secured. After the canals were dug they were puddled and then burnt, probably by filling them with brush and then setting it on fire, so that they almost equalled terra-cotta in durability. Mr. Cushing is of opinion that they were not used for irrigation alone, but for navigation as well. There are indications that they used rafts made of reeds (balsas) for navigating these canals, and this appears more probable from the heavy materials that have been brought from a distance. It seems certain that they floated the pine timber used in their building operations down the Salt and Gila Rivers from the distant mountains: it is too much to suppose that they carried this material upon their backs for a distance of a hundred miles.

The burial customs of these people were peculiar, and consisted of two methods, viz. cremation and interment. In the case of the priestly class the body was wrapped in cotton cloths and deposited beneath the floor of the house. Generally the bodies were laid along the east wall of the building, with head to the east, although this custom was not invariable. When a person of this clan died, a grave was dug in the floor, a foot and a half or two feet deep, and the body placed therein; it was then covered with adobe mud and packed firmly around the corpse. When this covering dried, and the soft parts and wrappings disappeared, the skeleton would be found inclosed in a rude sort of sarcophagus. In numerous instances, two, and more rarely three, skeletons were found in one grave. In all such cases of double or triple burial the skeletons indicate that it was male and female, or one male and two females. Buried with each cadaver was a food vessel and a water jar, and sometimes several of each, often highly decorated. That they were wrapped in cloths, presumably of cotton, is evident from the impressions of the cloth made upon the soft adobe covering. Fragments of this material were found and preserved, notwithstanding its decomposed condition.

Connected with each communal structure is what Mr. Cushing aptly terms a pyral mound, since the bodies of the common class were burned and their possessions destroyed upon this spot. The ashes and fragments of the charred bones were collected and placed in a burial urn, which had been previously "killed," and the whole buried in close proximity to the spot. The accumulations of this charred and fragmentary material now make mounds of sizable dimensions, which in itself would indicate a long period of occupancy. In the case of the pyral burials everything was broken and destroyed, while in the priestly burials the accompaniments were always whole. In one case of the priestly burials not only were the usual accompaniments present, but a quantity of arrow-points, spear-heads, and a large stone knife, together with numerous turquoise ornaments and materials for inlaying, were found deposited in the grave. This individual Mr. Cushing identified from his paraphernalia as belonging in all probability to the priesthood of some war order, and this seems more probable when we come to examine the skeleton, for he had sustained a fracture of the arm, and one knee was stiff from anchylosis, no doubt the scars of hard-fought battles.

Of the priestly burials something like four or five hundred were unearthed in the various towns, while many more of the cremated remains were found in the vicinity of the pyral mounds. The skeletons, as a rule, were so frail that comparatively few could be preserved. Of the whole number about one hundred good skulls, and probably fifty tolerably complete skeletons, were collected. These were so frail that Dr. Wortman was compelled to use a goodly supply of shellac varnish to keep them from falling to dust. Silicate of soda was tried, but it was not found so good as the ordinary shellac dissolved in alcohol.

The objects which go to make up the collection are various, and consist of those of ornament and utility. Numerous shell carvings, some of which had been beautifully inlaid with turquoise, were found, while a very few copper ornaments in the shape of bells and ear-rings were also dug up. Their tools consist almost entirely of stone, and were, for the most part, polished, though such implements as potters' stones, rasps, mauls, metates, &c., were never polished. Their stone axes and hatchets are of the ordinary pattern, and are generally well polished; they are of various sizes and shapes, and some of them were no doubt used as picks in digging up the hard cement and gravel in the construction of their irrigating canals. Stone hoes, knives, and arrow-heads were also found in abundance.

The collection of pottery is large, and, according to Mr. Cushing, resembles that of Zuñi manufacture more than any other people. It is often highly decorated with quaint and unique patterns, in various colours, and some fragments exhibited a fine glaze, which indicates a high state of the ceramic art.

That they were acquainted with metals there can be but little doubt, although they do not appear to have made use of it except in the way of ornament. Some places in the neighbouring mountains seemed to indicate that they mined for ore, which they smelted in crude ovens. Whether this was copper or the precious metals is now difficult to determine, but that they were accustomed to bring these ovens or furnaces to a very high heat is indicated by the slag in their immediate vicinity.

It is perhaps premature to attempt to decide who these people were, to whom they were related, and what became of them. I think it fairly settled by these discoveries that they were the ancestors of the modern Pueblos. Whether or not they were in any way connected with the ancient people of Mexico and Yucatan the future alone can decide. It seems certain, however, that one part of them went north to found the later Pueblo civilizations which are now represented by the Zuñis of to-day.

If historical evidence is worth anything, and if we can trust the ordinary evidences of archæology, then these ruins are beyond question pre-Columbian, and may be as much as a thousand years old.

Mr. Cushing's final Report will be awaited with interest by all who are in any way interested in the subject. The archæological specimens have been shipped to Salem, and the skeletons will go to the Army Medical Museum in Washington.

SELF-REPRODUCING FOOD FOR YOUNG FISH.

IN a very interesting Report of the United States Consul at Marseilles on the above subject, he says that every person interested in the artificial propagation of fish, particularly those of the genus *Salmonidæ*, knows the great care which is necessary to carry the young fry through the period immediately following the absorption of the umbilical sac, and to bring them to such a stage of maturity that they can be safely turned loose in open ponds and streams to shift for themselves. The mere hatching of the eggs presents no difficulty, but with the commencement of artificial nutrition the serious part of the work begins, and it is usually only a small percentage of the swarms which are hatched that reach the maturity of yearlings. During the intervening months it has been customary to feed the young fish on curdled milk, coagulated blood, finely hashed meat and liver, grated yolk of eggs, macerated brains of animals, &c., the preparation of which, and the constant feeding of the little creatures, involves constant and costly labour. Besides, none of these forms of nutriment have been found entirely satisfactory; they are artificial, and different from the living organic food which Nature provides. A plan invented by Mr. F. Lugrin, of Geneva, and practised since 1884 with the greatest success in the piscicultural establishment at Gremaz, in the province of Ain, in Eastern France, seems to overcome all these difficulties. The apparatus at Gremaz occupies a gently-sloping piece of ground, about six acres in extent, watered by three springs, which collectively yield about 500 gallons of water a minute. The tanks are about 120 feet long, 12 feet wide, and 5 feet deep. On account of the gravelly nature of the soil, the walls and bottoms of some of the tanks are lined with cement. The tanks are divided by sliding gates of wire gauze sufficiently fine to prevent the passage of the fry. Mr. Lugrin spreads upon the bottom of these tanks a material impregnated with the elements necessary to produce spontaneously a limitless number of *Daphniæ*, *Cyclops*, *Limnæ*, as well as larvæ of various *Ephemeræ* which form the natural aliment of trout and other *Salmonidæ*. This producing material is of trifling cost. The water in the tanks, which is from 2 to 3 feet deep, is left undisturbed for a few weeks, and is then found to be peopled with myriads of the species above named. With a fairly abundant propagation of these organisms, 20,000 young fry and 3000 fish one year old can subsist and thrive for a whole month in a tank of the size of one of those at Gremaz. These 23,000 fish and fry will eat from 600 to 800 pounds in a month, and each tank at Gremaz will produce from 650 to 900 pounds of *crevettes* (freshwater shrimps), to say nothing of the myriads of other species which are produced at the same time. Trout raised by this method have the flavour and firmness of wild fish. One great advantage of Mr. Lugrin's system is, that once a tank is prepared it is permanently productive.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The lecture-lists for this term contain no considerable innovations in the physical and chemical teaching. The usual systematic courses are to be given at the University Museum, and at Balliol, Christ Church, and Trinity. We may notice especially the following lectures:—

Prof. Pritchard, Recent Speculations on the Structure of the Stellar Universe, Spherical Astronomy, and the Theory of Errors; Prof. Price, Optics; Mr. Walker, Double Refraction treated Mathematically; Mr. Baynes, Theory of Gases, and Practical Electrical Measurements; Prof. Odling, 5-Carbon and 6-Carbon Compounds; Mr. Vernon Harcourt, Volumetric Analysis.

In the Biological Departments two new Professors have just entered on their offices. Prof. Green is giving two courses of lectures on Geology, and improving the Museum collections, and Prof. Vines has begun a systematic course of Elementary Botany. The Morphological Laboratory is in charge of Dr. Hickson and Mr. Latter; and Mr. Mitchell lectures on the Geographical Distribution of Animals. Prof. Burdon-Sanderson is lecturing on Elementary Physiology, and Mr. Gotch has a more advanced course. Dr. Tylor's subject this term is Race, Language, and Civilization.

An important statute has just past Convocation, which introduces the biological sciences into the Pass Examinations of the University for the B.A. degree. It is expected that the change will be of great use, especially to medical students, who cannot afford the time required to read for an Honour Examination in Natural Science.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 15.—M. Des Cloizeaux in the chair.—On the deformation of the images of stars seen by reflection on the surface of the sea, by M. C. Wolf. An attempt is here made to calculate the extent of this deformation, attention to which has lately been drawn by M. Riccò. The calculation shows that the difference in the angular heights of the object and its image increases towards the zenith, at first rapidly, then slowly, attaining its maximum at the zenith, for which it is double the depression of the horizon. A luminous band stretching from the apparent horizon to the zenith of the observer, and subtending an angle of 90° 19'·2, would give an image terminating at the nadir, and with an angular extent of not more than 90° – 19'·2.—On the latent colours of bodies, by M. G. Govi. The experiments here described with the bi-iodide of mercury, minium, and some other substances exposed to the light of the incandescent vapour of sodium—that is, the nearly pure yellow light D—tend to show that ordinary diffused or transmitted light does not give us the true colour of bodies. To obtain this true, but *invisible* or *latent* colour, a special process of illumination is needed. In general, solar or diffused light, not containing all the visible coloured radiations, is incapable always of showing us the true colour of bodies; further, the light given by incandescent bodies containing all the visible radiations is insufficient to disclose this true colour, which can be discovered only by means of a complete continuous spectrum without absorption bands or rays, or by simple radiations from incandescent gases. In such lights the true colour is that which is diffused or transmitted with greatest intensity, or else the blend of those so diffused or transmitted. This is somewhat analogous to the dichroism or polychroism of certain substances, as, for instance, the alcoholic solution of chlorophyll, which may seem green, brown, or red, according to its degree of concentration or its thickness on the path of the white light traversing it.—On the observations of stars by reflection, and on the measurement of the flexion of Gambey's circle, by M. Périgaud. The experiments here described with the modified form of Villarceau's mercury bath, lately submitted to the Academy, have enabled the author, as he anticipated, to obtain good images of reflected stars. Thus have been easily obtained within a period of five or six weeks about three direct and six reflected observations of about a hundred stars of all altitudes from 25° above the southern to 25° above the northern horizon. A calculation of the flexion of Gambey's circle yields a value practically identical with that given by Villarceau.—On the luminous ligament in the transits and occultations of Jupiter's satellites, by M. Ch. André. In a recent communication (*Comptes rendus*, cvii. p. 216) the author showed that one of the chief causes of uncertainty in these observations was due to the formation in the focal plane of the telescope, and, within a certain distance of the geometrical contact, to a luminous connection or "ligament" between the images of the satellite and the planet. A method is here explained by means of which the possible errors due to this phenomenon may be avoided.—Observations of Brewster's neutral point, by MM. J. L. Soret and Ch. Soret. The neutral point of atmospheric polarization situated below the sun has rarely been observed since its existence was first determined by Brewster. The authors have now been able accurately to observe it on the summit of Rigi (1800 metres) on the mornings of September 23 and 24, the height of the sun above the horizon being from 20° to 35°. They were able at the same time to determine the distance of the neutral point above the sun (Babinet's neutral point).—On some double phosphates of yttria and of potassa or soda, by M. A. Dubois. These phosphates have been obtained by causing the amorphous phosphate of yttria to react, by the dry process, on the sulphate of potassa (H. Debray's process, extended by Grandeau to the chief groups of metallic oxides); and also by making the pure yttria react at a high temperature on the metaphosphates and pyrophosphates of potassa and soda.—On the alkaloids of cod liver oil (continued), by MM. Arm. Gautier and L. Morgues. Having already determined the volatile alkaloids, butylamine, amylamine, hexylamine, and hydrodimethylpyridine, the authors here describe the two fixed bases accompanying them. These are named *aselline*, from *Asellus major*, the large cod; and *morrhuine*, from *Gadus morrhua*, the common cod; the latter being especially remarkable for its physiological properties. The respective formulas are, $C_{25}H_{32}N_4$ and $C_{19}H_{27}N_3$.—On propylphycite, by M. Ad. Fauconnier. Under this name, Carius described, in 1865, a body with the formula $C_3H_8O_4$, which Claus afterwards declared to be the glyceric aldehyde, unknown in a pure state. From the author's further researches it now appears that propylphycite is nothing but glycerine itself.

STOCKHOLM.

Royal Academy of Sciences, October 10.—Species Sargassorum Australiæ descriptæ et dispositæ a Prof. T. G. Agardh.—On persulphocyanacid and dithiocyanacid, by Dr. Klason.—On a scientific tour in Russia, Germany, and Holland, by Dr. S. Arrhenius.—On a magnetic field balance, by Dr. Ångström.—Baron Nordenskiöld exhibited an edition, from 1560, of Mercator's large map of the world, lately discovered by himself.—On a new arseniate mineral from Mossgrufvan, in Nordmark, by Hr. Sjögren.—On the anatomical structure of *Desmarestia aculeata*, Lam., by Miss E. Söderström.—On a class of transcendents, which originate through iterated integration of rational functions, by M. A. Jonquière, of Bern.—On aceto-propyl-benzol and aceto-kumol and their derivatives, by Prof. Widman.—The electrical and thermic conductibility of specular iron, by Hr. H. Bäckström.—Contributions to the knowledge of the thermo-electricity of crystals, by the same.—Determination of the magnetic inclination in Stockholm, Sundsvall, and Östersund, by Hr. P. A. Siljeström.

AMSTERDAM.

Royal Academy of Sciences, September 29.—M. de Vries read a paper on sterile plants of maize or Indian corn.—M. Van Bemmelen discussed the contents of a paper of M. Bakhuis Rozeboom, on the combinations of calcium chloride with water in solid and fluid condition.—M. J. A. C. Oudemans read a paper on levels becoming unfit for use by the diminished mobility of the bubble, in consequence of the precipitation of granular corpuscles against the interior surface of the glass. He demonstrated that this evil could be obviated by (1) constructing the levels of kali-glass, and not of natron-glass; (2) taking care that no water should be able to penetrate into the interior of the instrument; and (3) employing, instead of sulphuric ether, petroleum ether for the filling.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Fatal Illness of Frederick the Noble: Sir M. Mackenzie (Sampson Low).—The Senses, Instincts, and Intelligence of Animals: Sir John Lubbock (Kegan Paul).—Lectures on the Ikosahedron and the Solution of Equations of the Fifth Degree: F. Klein, translated by G. G. Morrice (Trübner).—Text-book of Practical Logarithms and Trigonometry: J. H. Palmer (Macmillan).—Experimental Mechanics, 2nd edition: Sir R. S. Ball (Macmillan).—Examples for Practice in the use of Seven-figure Logarithms: J. Wolstenholme (Macmillan).—The History of Australian Exploration, 1788–1888: E. Favenc (Turner and Henderson, Sydney).—A Manual of the Vertebrate Animals of the Northern United States, 5th edition: D. S. Jordan (McClurg, Chicago).—Outlines of Natural Philosophy, enlarged edition: J. D. Everett (Blackie).—The British Moss Flora, Part xi.: R. Braithwaite (published by author).—The Theory and Practice of Absolute Measurements in Electricity and Magnetism, vol. i.: A. Gray (Macmillan).—Mathematical Examples: J. M. Dyer and P. Prowde-Smith (Bell).—The Student's Pestalozzi: J. Russell (Sonnenschein).—Journal of the Royal Microscopical Society, October (Williams and Norgate).—Journal of the Royal Statistical Society, September (Stanford).—Annalen der Physik und Chemie, 1888, No. 10; Beiblätter zu den Annalen der Physik und Chemie, 1888, No. 9 (Barth, Leipzig).—Bulletin of the American Geographical Society, vol. xx. No. 3 (New York).—Bulletins de la Société d'Anthropologie de Paris, Tome xi. (3 Série) Fasc. 1 and 2 (Masson, Paris).

CONTENTS.