

inpleasant consequences of a late supper might have led Mr. Thomson one step further, and suggested to him the probable habitat of the spirit when embodied. How brimful of meaning to Mr. Thomson, then, must be Shakespeare's well-known utterance—"We are such stuff as dreams are made of." The particular merit which he claims for himself as a discoverer is, that he has realised to himself this spirit-world "predicted of old to be in existence," become conscious of himself as a "spirit in the world of spirits," clearly distinct, "in rounded belief," as he puts it, from that other entity, the body; and he declares that any one may make this awful discovery for himself if he only has "faith," shuts himself off from the outer world, and ponders long enough and with sufficient intensity. If our author is really in earnest—and we cannot but think he is—in trying to fathom the mystery of life and of consciousness, we recommend him to approach the subject unprejudicedly from the side of physiology; for so long as a psychologist concerns himself with the phenomena of his "inner consciousness" alone, and neglects the facts of his "outer man," his work is less than half done, and he is as likely to succeed in arriving at the whole truth as Columbus would have been in discovering America, had he contented himself with studying charts and staring longingly across the Atlantic for forty years.

On the Elevation of Mountains by Lateral Pressure; its Cause, and the Amount of it, with a Speculation on the Origin of Volcanic Action. By Rev. O. Fisher, M.A., F.G.S., &c. (From the Trans. of Camb. Phil. Soc. Vol. xi. part iii.)

THIS paper is of considerable interest as bearing upon the question of the internal condition of the earth. Mr. Fisher is of opinion that the elevation of mountain chains and the phenomena of volcanoes can both be accounted for on the hypothesis that the earth is solid. He conceives that "if a sufficient loss of heat has happened since the stratified rocks were formed, to cause a slight diminution in the volume of the earth, then the outer layer will have become too large, and will have had to accommodate itself to the reduced spheroid; and the lateral pressure caused by the resulting failure of support will have given rise to those foldings which have produced mountain ranges;" and an attempt is made by the author to "estimate the lateral pressure which would arise in the outer strata of the earth under such circumstances." Referring to the results obtained by Archdeacon Pratt in India, which seem to show that the density of the earth's crust beneath mountain chains is less than in other places, the author thinks this is only what might have been expected upon the supposition that the elevation of these mountains is due to lateral pressure; for it is evident that the strata would to some extent be supported by the lateral pressure which upheaved them. Here then, he thinks, may be the origin of volcanoes:—"Diminished vertical pressure will enable the interior layers of the crust to pass into a state of fusion, and, "if from an independent cause a partial passage towards the surface is opened for molten rock containing highly heated water, the fluid will convey to a level where the resistance is less the pressure existing at a lower depth, and the force necessary to complete a passage to the surface may be furnished by the pressure of the molten rock and by the steam contained within it." But, although Mr. Fisher believes that the elevation of mountain chains and the phenomena of volcanoes are both of them the result of the same fundamental causes, yet, he thinks, it would certainly be a mistake to regard elevation as the consequence of volcanic action. He does not see how subterranean lakes of molten matter can account for the elongated form which trains of volcanoes like those of the Andes affect; nor how such lakes should have shifted about from one region to another at different geological epochs. His theory, however, offers an explanation of the elongated form

assumed by chains of volcanoes—the shifting of volcanic activity to different regions at successive periods—the spasmodic character of volcanic action, and other volcanic phenomena.

J. G.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

The Placental Classification of Mammals

A REMARK made by Prof. Allen Thomson on this subject in a late number of NATURE induces me again to draw attention to some objections I offered to the placental classification in a review of Prof. Rolleston's "Forms of Animal Life" (NATURE, vol. i., p. 81). If this system fails to satisfy so sound a critic and so accomplished an anatomist as Dr. Thomson, there must be some serious deficiencies in it. No doubt De Blainville did good service in calling attention to the wide distinction of Marsupials and of Monotremes from other mammals; but his names, *Ornithodelphia* and *Didelphia*, are inappropriate, and even misleading, and the skeletal characters of these two groups furnish quite as important, and far more available, means of diagnosis.

It admits of question whether the divisions of the higher mammals, according to the same system, are the most natural, even if the placenta were the best organ by which to define them. It is true, as Prof. Huxley observes, that the singularities which ally the elephant with the Rodentia have been a matter of common remark since the days of Cuvier, but the placental classification requires us to find still more singular ties between the elephant and the Carnivora. On the other hand the Carnivora lead down by the seals to the true Cetacea, a line of connection broken by the placental arrangement; which is equally opposed to the more doubtful analogy of the whales with the Ruminants. And the third order with deciduous zonary placentation, the isolated genus *Hyrax*, whatever may be thought of its relations to Rodentia on the one hand and to Ungulata on the other, has at least more likeness to either than to elephants and cats. Again, the different placentation of Edentata may be held only an additional proof of the looseness of an order held together chiefly by negative characters, but if we break it up, shall we obtain a more natural or convenient arrangement by placing the sloths with the Ruminants, *Manis* with Cetacea and Perissodactyla, and *Orycteropus* with Primates?

No doubt embryological characters are justly regarded as the most important for revealing true affinities between animals. But the tenacity of hereditary transmission, which gives them this value, does not appear to belong to placental structure. The placenta is more a maternal than a foetal organ, especially as to its deciduate or non-deciduate character, and should rather rank with organs like the mamma than with the yolk-sac and the amnion.

There are, moreover, many practical objections to the placental classification. The opportunities of obtaining knowledge on the subject are few, the investigation is not always easy, and it cannot be readily verified by subsequent observers.

But the most important objection to De Blainville's system is, that the perishable nature of the structures on which it is based renders it impossible to apply the criterion to fossil animals. It will probably be long before we shall have any notion of what a Sirenian placenta is like; it is only lately that we have learnt what is the real placentation of so common a creature as the rat, but we shall certainly never have the remotest idea of that of a megatherium, a Zeuglodon, or a Rhytina. So that if it be admitted—and surely no one will deny—that any classification of animals which is to be more than a mere aid to the memory, must include all known forms, recent or fossil, it follows that neither placenta, nor brain, nor any other soft part, can be of more than subordinate value in classification. On the other hand, it may be fairly maintained that there is no group of mammals, and scarcely one of the other Vertebrata, of undisputed importance, which cannot be completely defined by the characters of the skeleton.

It is, I venture to think, rather the authority of such illustrious names as Gegenbaur and Huxley than its own merits which have recommended the placental classification of mammals. If we regard the object of classification to be the setting forth of true genetic relationships, all characters must be included, and among

them the placenta has no claim to be a primary index of affinity. And if we only seek for the most practically convenient way of arranging Mammalia, it is to the bones and teeth, rather than to the maternal organs of generation, that we must look.

P. H. PYE-SMITH

Potential Energy

WHILE on the subject of Thomson and Tait's Natural Philosophy, I should like to call attention to the definition of Potential Energy, given in Art. 273, p. 189.

I think it will be found that this definition gives the wrong sign, because the potential energy in any configuration is the amount of work the forces of the system perform in *returning* to the zero configuration, the ideal position of stable equilibrium.

Thus when a spring is stretched or compressed the potential energy is measured by the kinetic energy which is generated by the work done by the elastic force of the spring by the time the spring has returned to its unstretched condition. With this change of sign the definition now agrees with that given in Art. 484.

Infinite distance being taken as the zero configuration, the potential energy is a positive quantity for such forces as electric and magnetic forces.

With this zero the potential energy for gravitating particles is negative, which is expressed by saying that the exhaustion of potential energy is positive, because as the particles approach their kinetic energy increases, and their potential energy suffers exhaustion and diminishes.

In Art. 485 we read, "The potential at any point, due to any attracting or repelling body or distribution of matter, is the mutual potential energy between it and a unit of matter placed at that point. But in the case of gravitation, to avoid defining the potential as a negative quantity, it is convenient to change the sign. Thus the gravitation potential at any point, due to any mass, is the quantity of work required to remove a unit of matter from that point to an infinite distance."

Although the gravitation potential has had its sign changed, nevertheless the potential at any point P for gravitation and for electric and magnetic forces, is defined in the same way as the sum of the quotients of every portion of the mass divided by its distance from P.

This is the Potential Function of Green, usually called by the name given by Gauss, the Potential, and is the function which satisfies Laplace's equation.

The gravitation potential is the old force function of Sir W. Hamilton and Jacobi, such that its rate of increase in any direction is the resolved part of the force in that direction on the unit of mass.

The potential, defined as the potential energy in the unit of mass is of opposite sign to the free function; its rate of decrease in any direction is the component force in that direction.

These perplexing changes of sign arise from the fact that in gravitation we have only one kind of matter, the particles of which naturally attract; hence the potential energy is negative, or it diminishes as the particles approach; it is, therefore, convenient to make a change of sign.

In the general case of which electrical and magnetical phenomena may be taken as the type, like particles repel, unlike attract, and the potential energy increases as the particles approach.

These definitions and conventions of signs are, of course, in accordance with those given by Thomson and Tait; the proper signs and names are given also in Briot's "Théorie Mécanique de la Chaleur," but in all the other French books there is great confusion; for instance, in the "Théorie Mécanique de la Chaleur" of Verdet, the potential goes by Green's name, the potential function, but has its sign changed, while the potential energy is called the potential, after Clausius. This also seems to be the nomenclature adopted by the Germans.

It is very necessary that all doubt as to the meaning and value of these important functions should be set at rest; the system adopted in Thomson and Tait's "Natural Philosophy" leaves nothing to be desired.

A. G. GREENHILL

St. John's College, Cambridge, March 6

Development of Barometric Depressions

I LEAVE to those who are equal to it the task of reconciling and discussing "J. K. L.'s" propositions in reference to Indian

meteorology, which appear to be these:—1, "The rainfall in the Himalayas" (instanced by him in proof that rainfall is not the cause of depression), "probably causes a very great depression" (meaning, I now suppose, the great Asiatic depression really due to the rarefaction of the air in Central Asia); 2, "but certainly not any currents such as I have described" (viz., currents in accordance with Buys Ballot's Law, having the lowest pressure on their left); 3, "the circuit of the wind in the region of the Himalayas is, so far as we know, in exact accordance with Ballot's Law."

My complaint was that the critic had ignored, not, of course, Part II. of my book, but certain propositions in Part I., as "distinctly enunciated" as those on which he comments, and inseparable from them, though not yet fully discussed.

I will now close, as far as my part is concerned, a discussion, for the opening of which I was responsible, but which has, contrary to my intention, become rather personal than scientific. The question, however, really at issue between us I believe to be one of some interest in meteorology. "Does the fact that precipitation in certain cases, and especially in the warmer regions of the globe, fails to produce baric depression, disprove, or render improbable, the theory (based on substantial evidence) that the depressions which occur in Western Europe are results of precipitation?"

March 10

W. CLEMENT LEY

A Safety Lamp

THE article in this week's NATURE on "Foul Air in Mines, and how to live in it" calls to mind a contrivance made use of by the watchmen of Paris in all magazines where explosive or inflammable materials are stored, and suggests the idea that the same may possibly be of service to our miners.

The Paris *Figaro* says, "Take an oblong vial of the whitest and clearest glass, put in it a piece of phosphorus about the size of a pea, upon which pour some olive oil, heated to the boiling point, filling the vial about one-third full, and then seal the vial hermetically. To use it, remove the cork, and allow the air to enter the vial, and then re-cork it. The whole empty space in the bottle will then become luminous, and the light obtained will be equal to that of a lamp. As soon as the light grows weak its power can be increased by opening the vial and allowing a fresh supply of air to enter. Thus prepared the vial may be used for six months."

4, Moreton Place, S.W.

B. G. JENKINS

Beautiful Meteor

I ENCLOSE a description of meteor, apparently of unusual brilliancy, recently seen by my assistant at Parsonstown, thinking that it may perhaps be interesting to some of your readers.

Carlton Club, London, March 12

ROSSE

"Observed an intensely brilliant meteor. It was first seen in the region about Lepus, whence it moved with a slow and steady motion across the heavens to the S.E. horizon, where it gradually disappeared in a bank of cloud at about 9^h 5^m 19^s, Greenwich mean time, having occupied seven or eight seconds in moving over 50° of a great circle. The time given may be a few seconds wrong, as it was noted by an ordinary watch. The head was intensely brilliant, of a bluish white colour, and lighted up the whole sky.

"Its brightness was maintained during its entire visibility, and may have been as great as the moon at quadrature. Apparent diameter of the head 42'. It was followed by a very narrow tail about 3° in length and of a reddish hue. It did not leave any phosphorescent train behind it, but at the latter part of its course it threw out some reddish luminous masses, that gradually faded away. Its apparent course was in a great circle through β Canis Majoris to a point near the S.E. horizon, in azimuth S. 28½° E., and altitude 8½°. For β Canis Majoris the azimuth was S. 20° 52' 4 W., and altitude 16° 43' 3".

"Observatory, Birr Castle, March 8"

WHILE travelling last night, at about twenty minutes to nine o'clock, as we were descending a tolerably high hill, about 5 miles from this city, our road leading S.S.W., I found myself very favourably circumstanced for seeing a beautiful meteor which was