Letters to the Editor.

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Quantum Radiation.

THE fraction $x/(e^x - 1)$, by which the quantum theory of radiation differs from the classical theory, is so important that it seemed of interest to study it for its own sake. I accordingly wrote to my brother Alfred Lodge, as a pure mathematician, asking him what he had to say about it. He directed my attention to some points which may be of interest to other students of Planck's theory as expounded in Great Britain by Dr. Jeans. First, that the function was studied by John Bernoulli and expanded in a series involving his particular numbers; and next, that it is the ratio of simple interest to continuous compound interest for the same period. Or in other words, the compound interest $h\nu$ on E, the actual basic energy, is equal to the simple interest on RT; so that E has to be reduced below the average value in order to allow compound interest to be taken on it, while the rate of interest, x, is apparently dependent on the ratio ν/T.

The expansion spoken of above runs thus:

$$\frac{x}{e^x - 1} = 1 - \frac{1}{2}x + \frac{1}{6}\frac{x^2}{2!} - \frac{1}{30}\frac{x^4}{4!} + \frac{1}{42}\cdot\frac{x^6}{6!} - \frac{1}{30}\cdot\frac{x^8}{8!} + \ldots,$$

the coefficients being the successive Bernoulli numbers. A peculiarity of this series is that there are no odd powers of x after the first; looking as if everything outside classical mechanics depended on square numbers, like the radii of Bohr orbits.

Apart from the expansion so well known to pure mathematicians, the physical suggestion is that while RT is the average energy per degree of freedom per atom, the actual individual atomic energy E accumulates continuously at compound interest, the rate of which is $x = h\nu/RT$, until some atom has attained the extra accumulation $h\nu$, which it then emits. So that $E(e^x - 1) = RTx = h\nu$.

Interest is compound until it is paid, and then begins again. Thus E is first left to grow until it equals Ee^x; then $E(e^x - 1)$ is given out, and E is left to grow again until it again equals Ee^x, when another dividend is paid.

The accumulating unit is the atom, the energy of which is RT or $3RT$ only on the average. The actual energy rises by the compound interest of thermal agitation, until an emission occurs from those which on the ground of probability have reached the critical stage: small emissions at low frequency, large emissions—if they can occur—at high frequency.

The energy E is presumably internal electronic energy, the only kind of disturbance which can affect the ether and either radiate or absorb. It is doubtless associated with some particular frequency of revolution or internal vibration. Mere molecular or mechanical energy alone would not radiate (matter alone has no link with the ether); if it did we should have the equi-partition law and its troubles. Even the internal mechanism does not radiate save in jumps or jerks. Within the atom the energy grows continuously, but it is given out spasmodically.

All this is suggestive, and may probably be put in an educational manner. I need scarcely emphasise the singular beauty of the modern theory of black-body radiation, and the fundamental way in which we are beginning to get down to the mode of interaction between matter and ether.

OLIVER LODGE.

May 9.

D. C. Miller's Recent Experiments, and the Relativity Theory.

EVIDENCE against the validity of the relativity theory was unfolded before the annual meeting, April 28, of the National Academy of Sciences by Prof. Dayton C. Miller, of the Case School of Applied Science, who, by a much-refined and improved repetition of the so-called Michelson-Morley experiment, has shown that there is a definite and measurable motion of the earth through the ether.

Prof. Miller has obtained on four occasions a small positive effect at Cleveland, namely, the equivalent of a velocity of about 2 kilometres per second at the altitude of the Case School of Applied Science, and about 3 kilometres per second on the level of the neighbouring hills. Whereas at the altitude of the Mount Wilson Observatory, in four consecutive experiments spread out over four years, he obtained with increasing precision a positive result of 10 kilometres per second, his last result this April justifies him in asserting that the result is correct to within one-half kilometre per second.

The technical details of these experiments themselves will be described shortly in special papers by Prof. Miller himself. The purpose of the present letter is to say a few words about the implications of these results from the point of view of the relativity and the ether theories.

In the first place, then, this definite result is entirely antagonistic to the Einstein relativity theory, which in fact could not be adapted to the results of Prof. Miller by any conceivable modifications, unless the very fundamental principles of Einstein's theory were given up. This, however, is as much as to say that Miller's results knock out the relativity theory radically.

In the second place, from the point of view of an ether theory, this set of results, as well as all others previously discovered, are easily explicable by means of the Stokes' ether concept, as modified by Planck and Lorentz, and discussed by the writer in a *Phil. Mag.* paper (1919).

Without entering into the mathematical details associated with this statement, we may say only that Prof. Miller's results, as obtained in Cleveland and Mount Wilson, are given immediately by the main property of such an ether, namely, to adhere almost completely to the surface of the earth, and therefore to share almost entirely its translational motion over its surface, and to have a gradually increasing velocity relative to it when we go higher and higher up.

In the third place, the result of the recent *rotational* terrestrial experiment at Clearing, Ill., near Chicago, which gave a full effect associated with the spinning motion of the earth, can be accounted for by making the natural assumption that our globe, being almost perfectly spherical and having a purely gravitational grip upon the ether, does not appreciably drag it in its rotatory motion. Also the deflexion of the light rays around the sun to the amount claimed by the Einstein formula can be easily accounted for by means of a compressible ether provided its dielectric constant is related to its density and pressure by a very simple formula published by me a few years ago in the *Philosophical Magazine*.

The amount of additional evidence for the reality of Prof. Miller's beautiful results afforded by his tables

showing the relations of the observed azimuths of drift to the sidereal time is very remarkable. These tables indicate a motion of the solar system in a direction and with a velocity in good accordance with the independent results obtained by Dr. Strömberg and others.

LUDWIK SILBERSTEIN.

Washington, D.C., April 30.

Phylogeny as an Independent Science.

UNDER the title "Phylogeny as an Independent Science," E. W. M. gives in NATURE of December 20, 1924, a critical review of my "Geschichte der Organismen." Some points in this review cannot be passed without comment.

Almost at the beginning, the reviewer sums up the tenor of the book in the words: the author "treats of the evolutionary history of every phylum both in the animal and in the vegetable kingdom!" He has thus neither learned from the title, preface, and contents of the book, nor communicated to the reader of the review, the fact that "Geschichte"—that is, history—is not identical with "evolutionary" history. In my mind, the history of organisms tends to follow three courses, namely: (1) a simple account according to the chronology of the fossils and to the recent changes of fauna and flora; (2) showing the superiority of differentiation and centralisation (integration, Herbert Spencer), in contrast with the inferiority of differentiation alone; (3) phylogenetical connexion, the latter being the hypothetical part of the history. Thus the reviewer apparently did not understand that this mode of treatment leads, among other things, to securing, for his part, in good cases phylogeny more dependent than hitherto on empirical chronology, and to gain perhaps here or there an adaptation to each other of isolated phylogenetical hypotheses.

Furthermore, the reviewer enumerates two themes, of which he laments the absence of any attempt at explanation in the book. Well, the law of recapitulation is by no means explained in detail, but "the value and the limitations of the evidence from fossils" is discussed in a distinct manner in what to me is one essential respect, namely, that, on the average, the number of fossils diminishes at an accelerated rate when we go down to the older geological formations, and that, where it completely vanishes in pre-Cambrian times, we are still in a period approximately recent relative to the periods of origin of life, as well as of forming the chief branches of the genealogical tree.

Concerning the phylogeny of the Tunicata, the reviewer says: "Not a word is mentioned of Lang's significant theory of their derivation from Ctenophora . . .," etc. This theory, however, is mentioned in the book, pages 305-6, in the chapter on the Ctenophora, in some twenty lines, where I say that it seems to me "somewhat too distinct." Moreover, the more recent view of Wilhelmi, perhaps not yet known to the reviewer, is recorded and adopted. The same theory is briefly mentioned again on page 630, in order to compare the hypothetical place of the creeping ctenophores with that of the dipnoid fishes and that of the Bennettit plants.

The reviewer remarks, "how ill-founded is Franz's comparison of the cystid and the echinoid because both have a spherical shape." The book, however, says that probably the echinoids were derived "from cystids with five short arms." Therefore, according to my meaning, these cystids, besides the ancestors of those known as fossils, were not spherical, as also in the greatest number of fossils the arms are only broken off.

With this, to be sure, I do not wish to deny that the reviewer's objection, that the sucker of attachment of asterid larvæ is identical with the attachment of the stalk of the crinoids, can be right; and here again, if I may add in short a phylogenetical consideration, I should not believe Bury's and the reviewer's phylogeny of the Echinodermata to be persuading, nor should I mean that F. Mueller's and the reviewer's interpretation of the Nauplius (ancestral) is more suggestive than mine (modified metatrochophora). As to the Echinodermata, I do not understand how it can be overlooked that by far the greatest probability is in favour of the origin of the radiometry of this great phylum in fixation, since fixation has effected all other radiometry, or, at any rate, nearly all others, in the animal and vegetable kingdom, and since Balanoglossus, the creeping allied form to the echinoderms, is not radial. There is a striking parallelism: annelids→brachiopods, enteropneusts→echinoderms, if we assume that the non-radiometry of the brachiopods has caused lower vitality or victoriousness and the less modulation of these animals also fixed, compared with the echinoderms. To present such ideas, connected with each other, though in many cases partially hypothetical, and surely to be in future again adapted to the considerations presented from other points of view, is to be one of the tasks of the "history of organisms."

As to the Nauplius, the identification, though only approximate, of a living marine larva with a Cambrian fossil would be strongly against the tendencies of the book, and to the intentions of the author. Moreover, I see no essential resemblance between the four-segmented Nauplius and the multisegmented Cambrian Marrella, without regard to the question whether this interesting form, perhaps nearly the missing-link between branchiopods and trilobites, could have been mentioned in the book.

V. FRANZ.

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I AM sorry if I misunderstood the objects aimed at by Prof. Franz's book, "Die Geschichte der Organismen." I admit that the German word "Geschichte" is capable of being understood in two senses, namely, (1) a general descriptive account, and (2) evolutionary history. Since Prof. Franz holds the chair of phylogeny in his university, I understood "Geschichte" in the latter sense. All I can say is that if he intended it to be understood in the former sense, a task of such gigantic dimensions could not be attempted in a work of the size of his book. I gather that he intended to bring phylogenetic hypotheses into relation with fossil discoveries; in this aim I am in entire sympathy with him, however little success may have attended his efforts.

The object of my review was thus to point out that phylogenetic theory must remain a matter of personal taste, until the foundations on which it should rest are discussed and defined. These foundations are, as it seems to me, three, namely:

(1) When a number of closely allied species or genera are compared together, the more specialised amongst them have been evolved from the more generalised.

(2) When a close succession of allied fossil forms has been discovered in the same locality, becoming gradually changed as we pass from older to younger beds, this indicates a true evolutionary series.

(3) When the same larval form is found in the life