

very slowly and those for which the rate is appreciable; but as e^{-N} varies rapidly with N when N is large, there will be but few vibrations near the border, so that it seems legitimate, for purposes of a general discussion, to divide the vibrations into the two distinct classes, quick and slow, relatively to the scale of time provided by molecular collisions.

When the material bodies are solid, the physical principle is the same, the relatively slow motions of the atoms affecting the "quick" vibrations of the ether only by raising a sort of "equilibrium tide."

The number of "slow" vibrations of the ether in any finite enclosure is finite. These quickly receive the energy allotted to them by the theorem of equipartition. Thus they form the medium of transfer of radiant energy between two bodies at different temperatures. After a moderate time the slow vibrations have each, on the average, energy equal to that of two degrees of translational freedom of one molecule; the quick vibrations have no appreciable energy, while the intermediate vibrations possess some energy, but not their full share. It is easily seen that the number of slow vibrations is approximately proportional to the volume of the enclosure, so that roughly the energy of ether must be measured per unit volume in order to be independent of the size of the enclosure. For air under normal conditions, I find as the result of a brief calculation that this value is of the order of 5×10^{-9} times that of the matter. The law of distribution of this energy will be

$$\partial\lambda - \lambda^4 d\lambda$$

until we arrive at values of λ which are so small as to be comparable with

$$\text{radius of molecule} \times \frac{\text{velocity of light}}{\text{velocity of molecule}}.$$

After these values of λ are passed, the formula must be modified by the introduction of a multiplying factor which falls off very rapidly as λ decreases, and which involves the time during which the gas has been shut up. It is easily found (*cf.* "The Dynamical Theory of Gases," § 247) that at 0° C. the spectrum of radiant energy is entirely in the infra-red; at $28,000^\circ$ C. it certainly extends to the ultra-violet, and probably does so at lower temperatures.

Finally, Lord Rayleigh asks:—

"Does the postulated slowness of transformation really obtain? Red light falling upon the blackened face of a thermopile is absorbed, and the instrument rapidly indicates a rise of temperature. Vibrational energy is readily converted into translational energy. Why, then, does the thermopile itself not shine in the dark?"

Before trying to answer this, I wish to emphasise that my position does not require the *forces* of interaction between matter and ether to be small. Considering a gas for simplicity, the transfer of energy per collision to a vibration of frequency p is found to be proportional to the square of the modulus of an integral of the form (*cf.* "The Dynamical Theory of Gases," § 237)

$$\int f(t)e^{ipt} dt,$$

where $f(t)$ is a generalised force between matter and ether. The integral may be very small either through the smallness of $f(t)$ or the largeness of p . I rely entirely on the largeness of p , because calculation shows this to be adequate. The thermopile experiment gives evidence as to the magnitude of $f(t)$, but this does not alter the fact that the integral is small for large values of p .

This being so, I am afraid I do not very clearly understand why the thermopile should be expected to shine in the dark. If the red light is a plane monochromatic wave, its energy represents only two coordinates of the ether, and has to be shared between the great number of co-ordinates, six for each atom, which belong to the thermopile. If the red light comes from a large mass of red-hot matter inside the same enclosure as the thermopile, then the thermopile will soon be raised to the temperature of this mass, and may shine in the dark. If the hot mass consists of iron, say at 600° C., the atomic motions in the iron must be sufficiently rapid to excite the red

vibrations in the ether. But if the face of the thermopile is of lampblack, the atomic motions in lampblack at 600° C. may not be of sufficient rapidity (mainly, so far as can be seen, on account of the lower elasticity of the material) to excite red vibrations except as a kind of "equilibrium tide," in which case the lampblack will not emit red radiation.

I cannot ask for further space in which to answer Lord Rayleigh's point as to the enclosure with a hole in it, but I have discussed a similar question in a paper which I hope will soon be published, in connection with Bartoli's proof of Stefan's law. I hope that this paper, and a second one which is at present in the hands of the printer, will explain my position more clearly than I have been able to in the short limits of a letter.

May 20.

J. H. JEANS.

Fictitious Problems in Mathematics.

I HAVE to thank your reviewer for so readily supplying (*NATURE*, May 18, p. 56) the example to prove his contention—and which appears (to me) to disprove it.

The man who set that example did so in order to test (*inter alia*) whether the pupil knew that, for any friction to arise, both the surfaces must be rough; your reviewer originally wrote:—"What the average college don forgets is that roughness or smoothness are matters which concern *two surfaces not one body*." The italics are your reviewer's; and this is the statement which I called (and still call) in question.

It is no part of my book to uphold the verbiage in which the example is couched; by chance, in my former letter, I explained in anticipation the terms used in it. I do not see, however, why your reviewer applies the favourite word inaccurate to these terms. Perfect smoothness may not occur in nature; still, in considering the pendulum, I probably begin by assuming no friction on the axis of suspension, and, if I try afterwards to apply a correction for this friction, I probably make an assumption which is inaccurate. Friction = pressure \times a constant is inaccurate, statically and dynamically.

C. B. CLARKE.

As I take it, the mathematician's "perfectly rough body" means a body which never by any chance slips on any other body with which it is placed in contact, similarly the "perfectly smooth body" is supposed never to offer any tangential resistance to any other body which it touches. The inconsistency of this nomenclature is evident when we imagine the two bodies placed in contact with each other, as in the case of the perfectly rough plank resting on the smooth horizontal plane. The subsequent course of events cannot at the same time be compatible with the assumed perfect roughness of the one body and the assumed perfect smoothness of the other. The coefficient of friction between two bodies depends essentially on the nature of the parts of the surfaces of both bodies which are in contact as well as on their lubrication, and neither body can be said to have a coefficient of friction apart from the other. It is equally incorrect to speak of perfect smoothness or perfect roughness as attributes of a single body. Moreover, this misleading language is quite unnecessary; it is very easy to frame questions in a way that is free from objection. For instance, "A man walks without slipping along a plank which can slip without friction on a horizontal table." Or again, "A sphere is placed in perfectly rough contact with the slanting face of a wedge whose base rests in perfectly smooth contact with a horizontal plane."

G. H. BRYAN.

A New Slide Rule.

IN the article which appeared on p. 45 of *NATURE*, May 11, describing the Jackson-Davis double slide rule, you notice one little fault in the rule sent for examination.

We desire to exonerate the designer of the instrument, Mr. C. S. Jackson, from responsibility for the very obvious fault to which you allude, viz. that the scale on the feather edge is divided into inches and sixteenths, and that the continuation scale which is read below the ordinary slide

is in millimetres. The rules can be supplied with the plain scales either in inches or millimetres, and in the specimen submitted to you the mix up is the result of accident, and not perversity.

JOHN DAVIS AND SON.

All Saints' Works, Derby. May 20.

THE LOWER VERTEBRATES.¹

"EVERYTHING comes to him who waits"! Certainly the patience of many has been sorely tried by the long advent which has preceded the appearance of this last volume of the Cambridge Natural History. Students of the lower vertebrates will be naturally predisposed to accord it a favourable reception, inasmuch as its predecessors have presented such a high standard of excellence. If in some respects a closer acquaintance reveals some cause for complaint it will be admitted that, surveyed as a whole, both authors and editors alike are to be congratulated on having produced a work of sterling merit.

The groups dealt with in this volume are not only of the highest scientific interest and importance, but they present more than ordinary difficulties to be investigated, and these difficulties are materially increased when stern necessity compels the several contributors to condense their work within the smallest possible limits. Happily this task has fallen on the right shoulders, and all must admire the way in which it has been performed.

The first chapter of this book has been written by Dr. S. F. Harmer, and deals with the Hemichordata, a group which includes creatures of the existence of which the layman has never heard! Yet their importance in the scheme of evolution is of the highest, inasmuch as they bridge the gap for us between vertebrates and invertebrates.

The true nature of these worm-like and tubicolous animals has been determined only after the most laborious and painstaking research, in which Dr. Harmer, the author of this chapter has borne a very conspicuous share. Though the vertebrate affinities of the worm-like *Balanoglossus* were first hinted at by Kowalewsky in 1866, it was not until 1886 that this relationship was really demonstrated: a triumph achieved by Bateson. Forming at first a branch by itself of the vertebrate phylum, *Balanoglossus* has since lost something of its unique character by the discovery that certain other tubicolous forms—*Rhabdopleura* and *Cephalodiscus*—would have to be promoted to share this position, though to the ordinary observer nothing could be less like a vertebrate in appearance! This advance in our knowledge was made by the author of this chapter; and he has now still further extended the boundaries of this group so as to include *Phoronis*, an animal hitherto referred both to the Gephyrea and to the Polyzoa.

Although our knowledge of the Tunicates—those "common objects of the sea-shore," known as the "sea-squirts"—has been accumulating for something more than two thousand years, it was not until the middle of the eighteenth century that any real progress in the study of these creatures was made. And yet a century passed before the appearance of Kowalewsky's epoch-making work, which showed conclusively the astonishing fact that these shapeless jelly-bags were really kith and kin of the vertebrates—but degenerates!

No other group of animals is so all-embracing in the nature of the phenomena it displays. As the author remarks, "They demonstrate both stable and

variable species, monophyletic and polyphyletic groups. They exhibit the phenomena of gemmation and of embryonic fission, of polymorphism, hibernation, alternation of generations, and change of function. They have long been known as a stock example of degeneration; but in fact they lend themselves admirably to the exposition of more than one 'chapter of Darwinism.'"

Prof. Herdman has made this group peculiarly his own, and the editors are to be congratulated in having secured him to write this chapter. Nowhere else will the student find so complete and altogether admirable a summary of this most difficult and puzzling group of animals.

In dealing with amphioxus Prof. Herdman has been hampered by lack of space. This seems evident, not from the absence of any essential facts in his account, but from the condensed fashion in which the facts are presented. To the majority of those who will use this book this is perhaps of no great moment, but others, we imagine, will fail to appreciate the full

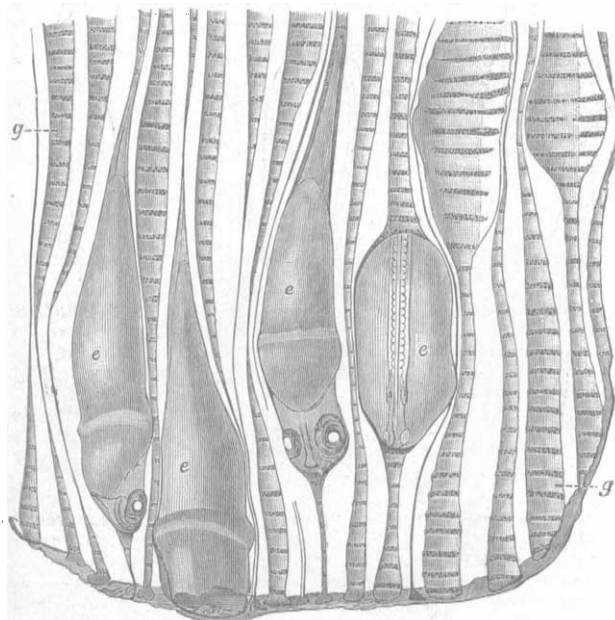


FIG. 1.—Embryos of *Rhodens umurus* in the gill-cavities of *Unio*. *e*, Embryos; *g*, inter-lamellar cavities. From the "Cambridge Natural History."

importance of some phases in the life history of this "weed in the vertebrate garden."

The remarkable ciliated condition of the embryonic and early larval stages is, for example, all too lightly passed over. Attention is not called to the importance of the fact that in the free-swimming, ciliated larva we have a connecting link between vertebrates and invertebrates. His reference to the existence of cilia is of the briefest. He remarks simply, that "the embryonic stages being passed through during the night . . . the larva hatched in the early morning," and then, on the next page, continues, "The epiblast cells become ciliated all over the surface, so that the embryo rotates within the thin covering which still surrounds it." Passing on to describe the metamorphosis of the embryo he goes on to say that "When it has (developed) about five pairs of mesoblastic somites, it breaks out of its covering, and becomes a free swimming larva." Probably no living biologist knows more of amphioxus than Prof. Herdman. Thus, then, this lack of emphasis of a really important feature must be attributed to the fact that he had to

¹ "Hemichordata, Ascidians and Amphioxus, Fishes." By Drs. Harmer, Herdman, Bridge and G. S. Boulenger. The Cambridge Natural History, vol. vii. Pp. xvii+760. (London: Macmillan and Co., Ltd., 1904.) Price 17s. net.