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

$$\theta\lambda^{-4}d\lambda$$

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° 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 coordinates, 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 × 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