

the ground in a rude circle, while at the centre are large blocks which probably formed the central dolmen. "There are two entrances to the enclosure, a northern and a southern, and on the east side of the latter is a large detached mound. Four hundred yards west of the main enclosure is a still larger mound, known as Gib Hill, connected with it by a low rampart of earth, now nearly worn away." Buxton and Matlock lead Mr. Firth to make some quotations from Erasmus Darwin's poetical references to them in his "Botanic Garden: Economy of Vegetation," and "Loves of the Plants." Dr. Darwin knew and loved the scenes he described, whatever opinion may be held as to his possession of the divine afflatus. There are a few other references to people and scenes of especial interest to the scientific world, but the book will not be valued for these so much as for its bright narrative of literary and historical centres of Derbyshire, and its fine illustrations.

*The Tower of Pelée. New Studies of the Great Volcano of Martinique.* By Prof. Angelo Heilprin. Pp. 62+xxii plates. (Philadelphia and London: Lippincott, 1904.)

PROF. HEILPRIN'S latest volume on Martinique is chiefly remarkable for the beautiful photographic plates with which it is illustrated; they give an excellent idea of the features of the great tower of solid lava which for nearly three years has been the centre of interest in the crater of Pelée. One of these plates, however (No. xi), seems to have been accidentally printed upside down. In the accompanying text there is an account of the author's fourth visit to the volcano in June, 1903, and a good deal of somewhat discursive matter regarding the lessons to be learnt from the recent eruptions. The number of points which are still unsettled concerning the mechanism of the explosions and the concomitant phenomena is very large, and the author shows a wise caution in dealing with some of them. He advances the opinion that the tower of Pelée is a volcanic core of ancient consolidation, and not an extrusion of solidified new lava, as the French observers believe. We cannot believe this is at all likely to obtain general acceptance.

J. S. F.

*Experimental Researches on the Flow of Steam Through Nozzles and Orifices.* By A. Rateau. Translated by H. Boyd Brydon. Pp. iv+76. (London: Constable and Co., Ltd., 1905.) Price 4s. 6d. net.

THE laws of flow of steam are of much importance in the design of turbines. A clear sketch is given of the theory, and then an account of an excellent experimental research to determine the values of the constants. Amongst previous experiments, those of Napier are English, not American as the author states. The novelty in M. Rateau's method is the use of an ejector condenser for condensing the steam. The rise of temperature, which is easily measured, gives the quantity of steam condensed. The errors of the method, especially that due to entrained water, are carefully examined. Convergent nozzles and a thin plate orifice were used. The results are compared with those by Hirn on air, and close agreement is found. In a note, the complex phenomenon of the discharge of hot water just on the point of evaporating is examined.

The translation is clear. It is, however, a defect, for English readers, that the principal formulæ are left as given by the author in foreign units. The book is essentially one for practical use, and it would have added much to the convenience of engineers if other formulæ than the one on p. 6 had been given in English units.

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*Introductory Mathematics.* By R. B. Morgan. Pp. vi+151. (London: Blackie and Son, Ltd., 1905.) Price 2s.

IN Mr. Morgan's "Introductory Mathematics" the view of the author is that as soon as a boy knows decimal and vulgar fractions he should begin a mixed course of elementary practical mathematics comprising algebra, geometry, and squared-paper work, developed as a whole in mutual dependence, leading up through the manipulation of formulæ to the solution of problems involving simultaneous simple equations and giving a knowledge of the fundamental facts of geometry with a training in practical applications such as the plotting of graphs and of figures to scale, and the finding of simple areas and volumes. This scheme, ignoring the old water-tight compartment system, is a good one. The chapters on algebra and geometry usually alternate, and the work progresses on natural and easy lines, with illustrations of every-day interest. The author might with advantage have carried the idea still further and have brought in computations from quantitative experimental work in the laboratory, involving the use of the balance and measuring flask, and perhaps an investigation of the action of forces at a point. There are some minor defects, such as an occasional lack of precision in a statement, bad perspective in several of the figures, the use of a graph to give a forecast of population fifty years hence, &c. But the treatment of the subject as a whole is very satisfactory; there is a good collection of exercises, and the book is well suited to its purpose.

#### 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.]

##### The Dynamical Theory of Gases and of Radiation.

LORD RAYLEIGH, in a letter which appears in NATURE of May 18, opens up the general question of the applicability of the theorem of equipartition to the energy of the ether. As the discussion has arisen out of my "Theory of Gases," may I, by way of personal explanation, say that although I was fully alive to the questions referred to in this letter when writing my book, yet it seemed to me better not to drag the whole subject of radiation into a book on gases, but to reserve it for subsequent discussion? Since then I have written two papers in which questions similar to those raised by Lord Rayleigh are discussed from different aspects, but as neither of these papers is yet in print, I ask for space for a short reply explaining how my contentions bear on the special points raised by Lord Rayleigh's letter.

May I, in the first place, suggest that the slowness with which energy is transferred to the quicker modes of ether-vibration is a matter of calculation, and not of speculation? If the average time of collision of two molecules in a gas is a great multiple  $N$  of the period of a vibration, whether of matter or of ether, then the average transfer of energy to the vibration per collision can be shown to contain a factor of the order of smallness of  $e^{-N}$ . The calculations will be found in §§ 236-244 of my book. It is on these that I base my position, not on a mere speculation that the rate of transfer may be slow. Lord Rayleigh's example of a stretched string, say a piano wire, will illustrate the physical principle involved. If a piano hammer is heavily felted, the impact is of long duration compared with the shortest periods of vibration, so that the quickest vibrations are left with very little energy after the impact, and the higher harmonics are not heard. If the felting is worn away, the impact is of shorter duration, the higher harmonics are sounded, and the tone of the wire is "metallic."

The factor  $e^{-N}$  is so small for most of the ether-vibrations as to be negligible. There is no sharp line of demarcation between those vibrations which acquire energy

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  $5 \times 10^{-9}$  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  $\lambda$  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  $\lambda$  are passed, the formula must be modified by the introduction of a multiplying factor which falls off very rapidly as  $\lambda$  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  $0^\circ$  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  $600^\circ$  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  $600^\circ$  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