

names also are incorrectly printed, and the references given at the end of the chapter on mine illumination mostly refer to ventilation. On p. 681 the student is taught to load a hole "with nitroglycerine by pouring from a tin cup upon the fuse with its cap and covering the mass with water." Evidently the Coal Mines Regulation Act has no analogue in a country where, as the authors point out, "each new camp, untrammelled by tradition to keep it in the rut of prejudice, displays its genius for organisation and absorbs the latest devices, tried and true."

*The Practical Photographer.* (Library Series.) Edited by Rev. F. C. Lambert. No. 16, *Pictorial Composition*. Pp. xx+64. No. 17, *Animal Photography*. Pp. xxiv+64. (London: Hodder and Stoughton, 1905.) Price 1s. net.

IN the first of these books the editor gives an interesting account of the pictorial work of Bernard Alfieri, illustrating it with six excellent reproductions of this well-known worker's studies. Among the other sections of the book, which are written by various authors, those on the principles of composition, by Arthur Burchett, and some notes on composition in landscape, by Horace Mummery, will be found of great practical value. In these the pen and ink sketches showing the several methods of producing balance in pictures call for special attention. Other articles, such as that on the arrangement of the foreground, are well worth perusing. Numerous well reproduced illustrations, serving as examples of good and bad composition, accompany the text. The second of the above books appeals to another class of photographers, for, with the exception of the editor's article on the pictorial work of Viscount Maitland, it is devoted to the photography of animals. Like the former book, numerous authors have contributed to the text, and a very wide range of points of view is included. It is written on the same practical lines, and is accompanied by fifty-five well selected illustrations. Both volumes will add to the value of this useful library series.

*Determination des Espèces minérales.* By L. M. Granderye. Pp. 184. (Paris: Gauthier-Villars, n.d.) Price 2.50 francs.

IN this little book, which is a publication of the "Encyclopédie scientifique des Aide-Mémoire," the author has apparently attempted to devise a royal road for the determination of a mineral species. For this purpose he has compiled a number of lists of the more common minerals arranged according to physical characters, viz. crystal-system, colour, structure, density, &c., and has supplemented these with some instructions on blowpipe analysis and chemical examination in the dry way. Such lists are certainly of great value for determination purposes, but, as regards the more common minerals, at any rate, it would be a mistake to encourage the student to rely upon any methodical scheme of determination to the neglect of an acquisition of a thorough knowledge of the characters of the individual species. For many minerals, especially with imperfectly crystallised specimens, we fear these tables would prove an uncertain guide in the absence of any observations of the optical characters or of chemical examination in the wet way. In Brush and Penfield's standard work on determinative mineralogy it is true that no account is taken of the optical characters, but sufficient importance is given to chemical tests in the wet way. The tables are not altogether free from errors and misprints; thus a saline taste is attributed to sodalite, rhodonite is described as a carbonate, and the density of wolframite is given as 5.5 on one page and 7.5 on another. The book concludes with a list of 600 minerals with their principal characters, viz. density, hardness, &c.

LETTERS TO THE EDITOR.

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The Dynamical Theory of Gases and of Radiation.

I AM glad to have elicited the very clear statement of his view which Mr. Jeans gives in NATURE of April 27. In general outline it corresponds pretty closely with that expressed by O. Reynolds in a British Association discussion at Aberdeen (NATURE, vol. xxxii. p. 534, 1885). The various modes of molecular motion are divided into two sharply separated groups. Within one group including the translatory modes, equipartition of energy is supposed to establish itself within a small fraction of a second; but between the modes of this group and those of vibration included in the other group, equipartition may require, Mr. Jeans thinks, millions of years. Even if minutes were substituted for years, we must admit, I think, that the law of equipartition is reconciled with all that is absolutely proved by our experiments upon specific heat, which are, indeed, somewhat rough in all cases, and especially imperfect in so far as they relate to what may happen over long intervals of time.

As I have already suggested, it is when we extend the application of the law of equipartition to the modes of æthereal vibration that the difficulties thicken, and this extension we are bound to make. The first question is as to the consequences of the law, considered to be applicable, after which, if necessary, we may inquire whether any of these consequences can be evaded by supposing the equipartition to require a long time for its complete establishment. As regards the first question, two things are at once evident. The energy in any particular mode must be proportional to  $\theta$ , the absolute temperature. And the number of modes corresponding to any finite space occupied by the radiation, is infinite. Although this is enough to show that the law of equipartition cannot apply in its integrity, it will be of interest to follow out its consequences a little further. Some of them were discussed in a former paper,<sup>1</sup> the argument of which will now be repeated with an extension designed to determine the coefficient as well as the law of radiation.

As an introduction, we consider the motion of a stretched string of length  $l$ , vibrating transversely in one plane. If  $a$  be the velocity of propagation,  $\xi$  the number of subdivisions in any mode of vibration, the frequency  $f$  is given by

$$f = a\xi/2l \dots \dots \dots (1)$$

A passage from any mode to the next in order involves a change of unity in the value of  $\xi$ , or of  $2lf/a$ . Hence if  $e$  denote the kinetic energy of a single mode, the law of equipartition requires that the kinetic energy corresponding to the interval  $df$  shall be

$$2le/a \cdot df \dots \dots \dots (2)$$

In terms of  $\lambda$  the wave-length, (2) becomes

$$2le/\lambda^2 \cdot d\lambda \dots \dots \dots (3)$$

This is for the whole length of the string. The longitudinal density of the kinetic energy is accordingly

$$2e/\lambda^2 \cdot d\lambda \dots \dots \dots (4)$$

In each mode the potential energy is (on the average) equal to the kinetic, so that if we wish to reckon the whole energy, (4) must be doubled. Another doubling ensues when we abandon the restriction to one plane of vibration; and finally for the total energy corresponding to the interval from  $\lambda$  to  $\lambda+d\lambda$  we have

$$8e/\lambda^2 \cdot d\lambda \dots \dots \dots (5)$$

When we proceed to three dimensions, and consider the vibrations within a cube of side  $l$ , subdivisions may occur in three directions. In place of (1)

$$f = a/2l \cdot \sqrt{(\xi^2 + \eta^2 + \zeta^2)} \dots \dots \dots (6)$$

where  $\xi$ ,  $\eta$ ,  $\zeta$  may assume any integral values. The next step is to ascertain what is the number of modes which corresponds to an assigned variation of  $f$ .

If the integral values of  $\xi$ ,  $\eta$ ,  $\zeta$  be regarded as the

<sup>1</sup> "Remarks upon the Law of Complete Radiation," *Phil. Mag.*, xlix. p. 539 June, 1900.

coordinates of a point, the whole system of points constitutes a cubic array of volume-density unity. If R be the distance of any point from the origin,

$$R^2 = \xi^2 + \eta^2 + \zeta^2;$$

and the number of points between R and R+dR, equal to the included volume, is

$$4\pi R^2 dR.$$

Hence the number of modes corresponding to df is

$$4\pi(2l/a)^3 f^2 df,$$

or in terms of  $\lambda$

$$4\pi \cdot 8l^3 \cdot \lambda^{-4} d\lambda \dots \dots \dots (7)$$

If e be the kinetic energy in each mode, then the kinetic energy corresponding to dλ and to unit of volume is

$$32\pi e \lambda^{-4} d\lambda \dots \dots \dots (8)$$

Since, as in the case of the string, we are dealing with transverse vibrations, and since the whole energy is the double of the kinetic energy, we have finally

$$128\pi \cdot e \cdot \lambda^{-4} d\lambda \dots \dots \dots (9)$$

as the total energy of radiation per unit of volume corresponding to the interval from λ to λ+dλ, and in (9) e is proportional to the absolute temperature θ.

Apart from the numerical coefficient, this is the formula which I gave in the paper referred to as probably representing the truth when λ is large, in place of the quite different form then generally accepted. The suggestion was soon confirmed by Rubens and Kurlbaum, and a little later Planck (*Drude Ann.*, vol. iv. p. 553, 1901) put forward his theoretical formula, which seems to agree very well with the experimental facts. This contains two constants, h and k, besides c, the velocity of light. In terms of λ it is

$$E d\lambda = \frac{8\pi c h}{\lambda^5} \frac{d\lambda}{e^{ch/k\lambda\theta} - 1} \dots \dots \dots (10)$$

reducing when λ is great to

$$E d\lambda = 8\pi k \theta \lambda^{-4} d\lambda \dots \dots \dots (11)$$

in agreement with (9). E dλ here denotes the volume-density of the energy of radiation corresponding to dλ.

A very remarkable feature in Planck's work is the connection which he finds between radiation and molecular constants. If N be the number of gaseous molecules in a cubic centimetre at 0° C. and under a pressure of one atmosphere,

$$k = \frac{1.013 \times 10^6}{273N} \dots \dots \dots (12)$$

Though I failed to notice it in the earlier paper, it is evident that (9) leads to a similar connection. For e, representing the kinetic energy of a single mode at temperature θ, may be identified with one-third of the average kinetic energy of a gaseous molecule at that temperature. In the virial equation, if N be the total number of molecules,

$$\frac{2}{3} p v = \sum \frac{1}{2} m V^2 = 3 N e,$$

so that

$$e = p v / 2 N \dots \dots \dots (13)$$

If we apply this to one cubic centimetre of a gas under standard conditions, N has the meaning above specified, v = 1, and p = 1.013 × 10<sup>6</sup> C.G.S. Accordingly, at 0° C.

$$e = 1.013 \times 10^6 / 2 N,$$

and at θ°

$$e = \frac{1.013 \times 10^6 \times \theta}{2 \times 273 N} \dots \dots \dots (14)$$

Introducing this into (9), we get as the number of ergs per cubic centimetre of radiation

$$\frac{64 \cdot \pi \cdot 1.013 \cdot 10^6 \cdot \theta \cdot d\lambda}{273 \cdot N \cdot \lambda^4} \dots \dots \dots (15)$$

θ being measured in centigrade degrees. This result is eight times as large as that found by Planck. If we retain the estimate of radiation used in his calculations, we should deduce a value of N eight times as great as his, and probably greater than can be accepted.

A critical comparison of the two processes would be of interest, but not having succeeded in following Planck's reasoning I am unable to undertake it. As applying to all wave-lengths, his formula would have the greater value if satisfactorily established. On the other hand, the reasoning which leads to (15) is very simple, and this formula appears to me to be a necessary consequence of

the law of equipartition as laid down by Boltzmann and Maxwell. My difficulty is to understand how another process, also based upon Boltzmann's ideas, can lead to a different result.

According to (15), if it were applicable to all wave-lengths, the total energy of radiation at a given temperature would be infinite, and this is an inevitable consequence of applying the law of equipartition to a uniform structureless medium. If we were dealing with elastic solid balls colliding with one another and with the walls of a containing vessel of similar constitution, energy, initially wholly translational, would be slowly converted into vibrational forms of continually higher and higher degrees of subdivision. If the solid were structureless, this process would have no limit; but on an atomic theory a limit might be reached when the subdivisions no longer included more than a single molecule. The energy, originally mechanical, would then have become entirely thermal.

Can we escape from the difficulties, into which we have been led, by appealing to the slowness with which equipartition may establish itself? According to this view, the energy of radiation within an enclosure at given temperature would, indeed, increase without limit, but the rate of increase after a short time would be very slow. If a small aperture is suddenly made, the escaping radiation depends at first upon how long the enclosure has been complete. In this case we lose the advantage formerly available of dividing the modes into two sharply separated groups. Here, on the contrary, we have always to consider vibrations of such wave-lengths as to bear an intermediate character. The kind of radiation escaping from a small perforation must depend upon the size of the perforation.

Again, 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 not itself shine in the dark?

It seems to me that we must admit the failure of the law of equipartition in these extreme cases. If this is so, it is obviously of great importance to ascertain the reason. I have on a former occasion (*Phil. Mag.*, vol. xlix. p. 118, 1900) expressed my dissatisfaction with the way in which great potential energy is dealt with in the general theory leading to the law of equipartition.

RAYLEIGH.

May 6.

The Cleavage of Slates.

In his critique of Dr. Becker's theory of slaty cleavage in NATURE of May 4, "A. H." says that it is substantially the same as mine, and rightly objects that, "if the cleavage plane were a plane of shearing it would correspond with a circular section of the ellipsoid" of distortion. It is true that I made that suggestion in the body of my first paper on cleavage in the *Geological Magazine*, 1884, but in a postscript to that paper I stated that a conversation with Mr. Harker had led me to the conclusion that the cleavage surfaces are determined by the position of the principal axes of the ellipsoids of distortion produced by a shearing movement, and to this view I have ever since adhered.

"A. H." says that "there are many slates in which the strain ellipsoid is actually presented in deformed spherical concretions or colour-spots." Is this certain? Is it not probable that these discolorations took place after the rock became a slate? In that case the chemical influence emanating from the foreign particle, usually obvious in the centre of the spot, found the greatest conductivity in the direction of the longest axis of the ellipsoid, the next greatest along the mean axis, and very little along the least. It is from this property of little conductivity across the cleavage that slates are eminently suited for roofing. I have a piece of a school slate with two sharply defined oval patches, of each of which the two diameters are 25 mm. and 16 mm. The thickness of the slate is less than 4 mm., and yet the discoloration does not pass through to the other side. If these spots are sections of ellipsoids formed out of spheres by compression, the resulting condensation must have been incredibly enormous. The spots in Borrowdale slates are of a different character from spots of dis-