ethics, are well brought out by this author. His book may be heartily recommended to students of the period described.

A Text-book of Physics, Heat. By Prof. J. H. Poynting, Sc.D., F.R.S., and Prof. J. J. Thomson, M.A., F.R.S. Pp. xvi+354. (London: C. Griffin and Co., Ltd., 1904.) Price 15*s*.

THE third volume of this well known text-book more than sustains the standard set by its predecessors. The volumes on sound and properties of matter have already appeared. The volumes on light and on electricity and magnetism we hope may follow at a somewhat shorter interval than has intervened between the first three volumes of the series. It is hardly necessary to say that the work is well up to date, and extremely clear and exact throughout, and that it is as complete as it would be possible to make such a text-book within the limits which the authors have laid down for the scope of their work. Among the more original features which should be valuable to the student as filling gaps which are noticeable in similar text-books, we observe that a useful chapter is included on the subject of circulation and convection, with illustrations from meteorology and ventilation. The treatment of the important subject of radiation, especially in relation to temperature and thermodynamics, is unusually complete and clear, and presents in a simple, connected form a number of most important results which the student would have difficulty in finding elsewhere. The experimental spirit is maintained throughout the work in such a manner that the student will feel that he is learning from a practical master of the subject, and will unconsciously imbibe something of the attitude of mind of the original investigator. H. L. C.

The Oxford Atlas of the British Colonies. Part i. British Africa. Seventeen maps. (Oxford Geographical Institute: William Stanford and Co., Ltd., n.d.) Price 2*s*. 6*d*. net.

THE first thirteen plates consist of coloured maps, and the remaining four are outlines intended for use as "test" maps or for other class purposes. The first map shows a hemisphere in which Cape Colony occupies the centre, and it is possible from it to see at once the relation of South Africa to the other continents. Map ii. is a political map of the world drawn in accordance with Mollweides's equal area projection, and the student will notice at a glance the apparent distortion in shape, though the relative sizes of land areas in different parts of the map are correctly shown. In addition to meteorological charts, the atlas includes physical and political maps of Africa, and maps of Cape Colony, Natal and Zululand, the Transvaal and Orange River Colony, Rhodesia, and of West, East, and Central Africa.

High Temperature Measurements. By H. Le Chatelier and O. Boudouard. Authorised translation and additions by Dr. G. K. Burgess. Second edition. Pp. xv+341. (New York: John Wiley and Sons; London: Chapman and Hall, Ltd. 1904.) Price 12*s*. 6*d*. net.

IN preparing the present edition it was found necessary to make a large number of additions, and the book now gives a useful summary of what is known about pyrometry. The advances in optical pyrometry during the last few years are recognised by the authors, and a useful chapter on the laws of radiation has been inserted. A number of pyrometers are described, but the discussion of the principles involved is in general more adequate than the description of instruments. No mention is made of some of the best of these in use in this country.

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.*]

A Comparison between Two Theories of Radiation.

ON two occasions (NATURE, May 18 and July 13) Lord Rayleigh has asked for a critical comparison of two theories of radiation, the one developed by Prof. Planck (*Drude's Annalen*, i. p. 69, and iv. p. 553) and the other by myself, following the dynamical principles laid down by Maxwell and Lord Rayleigh. It is with the greatest hesitation that I venture to express my disagreement with some points in the work of so distinguished a physicist as Prof. Planck, but Lord Rayleigh's second demand for a comparison of the two methods leads me to offer the following remarks, which would not otherwise have been published, on the theory of Prof. Planck.

Early in his second paper, Planck introduces the conception of the "entropy of a single resonator" S. There are supposed to be N resonators having a total entropy $S_N = NS$, and S_N is supposed to be given by $S_N = k \log W + \text{constant}$, where W is the "probability" that the N resonators shall be as they are. Without discussing the legitimacy of assigning entropy to a single resonator, we may at present suppose S defined by $S = k/N \log W + \text{cons.}$

The function W, as at present defined, seems to me to have no meaning. Planck (in common, I know, with many other physicists) speaks of the "probability" of an event, without specifying the basis according to which the probability is measured. This conception of probability seems to me an inexact conception, and as such to have no place in mathematical analysis. For instance, a mathematician has no right, *quâ* mathematician, to speak of the probability that a tree shall be between six and seven feet in height unless he at the same time specifies from what trees the tree in question is to be selected, and how. If this is not so, may I ask, "What *is* the probability that a tree shall be between six and seven feet high?"

When Prof. Planck calculates the probability function W, he in effect assumes that *a priori* equal small ranges of energy are equally probable. Thus he tacitly introduces as the basis of his probability calculations an ensemble of systems of resonators such that the number of systems in which the energy of any given resonator lies between E and $E + dE$ is proportional simply to dE. This, of course, he has a right to do, only he must continue to measure probability according to this same basis.

The systems of resonators are in motion, their motion being governed by the laws of dynamics. Will they, as the motion progresses, retain the statistical property which has been the cause of their introduction, namely, that the number of systems in which the energy of any given resonator lies between E and $E + dE$ is proportional simply to dE? It is easily found, by the method explained in my "Dynamical Theory of Gases" (§ 211), that in general they will not; the probability function W is not simply a function of the coordinates of the system. Prof. Planck's position is as though he had attempted to calculate the probability that a tree should be between six and seven feet high, taking as his basis of calculation an enclosure of growing trees, and assuming the probability to be a function only of the quantities six and seven feet. His ensemble of systems has not yet reached a statistical "steady state."

Prof. Planck supposes his function S to possess the property of the entropy function, so that $1/T = dS/dU$, where T is the temperature. Combining this with Planck's calculation of S, we find

$$1/T = k/\epsilon \log (1 + \epsilon/U) \quad . \quad . \quad . \quad . \quad (1)$$

Here ϵ is a small quantity, a sort of indivisible atom of energy, introduced to simplify the calculations. We may legitimately remove this artificial quantity by passing to the limit in which $\epsilon = 0$. In this way we obtain

$$1/T = k/U \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Thus the mean energy of each resonator, according to this equation, is the same multiple of the temperature, no matter how many degrees of freedom the resonator possesses, or what the form of its potential energy. Indeed, according to this argument, equation (2) is proved for any dynamical system, *e.g.* the molecules of a gas.

It is, however, known that equation (2), with Planck's meaning of k, is true if, and only if, the energy of each dynamical system is expressible as the sum of two squares. It can, indeed, be shown directly that this latter condition is exactly the condition that Prof. Planck's assumed basis of probability calculations shall be a legitimate basis, *i.e.* shall be independent of the time. Happily, this condition of the energy being a sum of two squares may be supposed to be satisfied by Planck's resonators, so that we may regard equation (1) as true for such resonators. The equation has, however, no physical meaning, owing to the presence of the arbitrary small quantity ϵ, and can acquire a physical meaning only by putting $\epsilon=0$. It then leads merely to equation (2), which can be obtained much more readily from the theorem of equipartition.

Taking $\mathfrak{u}d\nu$ to be the law of radiation, where ν is the reciprocal of the period of vibration, Planck introduces from his first paper the equation

$$\mathfrak{u}=(8\pi\nu^2/c^3)\mathrm{U} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which in combination with equation (2) would lead to the law of radiation,

$$(8\pi k/c^3)\mathrm{T}\nu^2 d\nu \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

and this, on replacing ν by c/λ, becomes

$$8\pi k\mathrm{T}\lambda^{-4}d\lambda \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which agrees with my own result. Planck arrives at equation (3) by the help of his assumption of "nätürliche Strahlung," but I believe it will be found that this "assumption" is capable of immediate proof by the methods of statistical mechanics. Except for this, and the other differences already stated, the way in which expression (5) has been reached in the present letter is identical, as regards underlying physical conceptions, with the way in which it has been obtained by Lord Rayleigh and myself.

Planck does not reach expression (5) at all, as he does not pass from equation (1) to equation (2). Instead of putting $\epsilon=0$, he puts $\epsilon=h\nu$, where h is a constant, and this leads at once to his well known law of radiation. It will now be clear why Planck's formula reduces to my own when $\lambda=\infty$. For taking $\lambda=\infty$ is the same thing as taking $\nu=0$, or $\epsilon=0$.

The relation $\epsilon=h\nu$ is assumed by Planck in order that the law ultimately obtained may satisfy Wien's "displacement law," *i.e.* may be of the form

$$\nu^3/c^3 f(\mathrm{T}/\nu)d\nu \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This law is obtained by Wien from thermodynamical considerations on the supposition that the energy of the ether is in statistical equilibrium with that of matter at a uniform temperature. The method of statistical mechanics, however, enables us to go further and determine the form of the function $f(\mathrm{T}/\nu)$; it is found to be $8\pi k(\mathrm{T}/\nu)$, so that Wien's law (6) reduces to the law given by expression (4). In other words, Wien's law directs us to take $\epsilon=h\nu$, but leaves h indeterminate, whereas statistical mechanics gives us the further information that the true value of h is $h=0$. Indeed, this is sufficiently obvious from general principles. The only way of eliminating the arbitrary quantity ϵ is by taking $\epsilon=0$, and this is the same as $h=0$.

Thus it comes about that in Planck's final law

$$\frac{8\pi ch}{\lambda^5}\frac{1}{e^{ch/k\lambda\mathrm{T}}-1}d\lambda \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

the value of h is left indeterminate; on putting $h=0$, the value assigned to it by statistical mechanics, we arrive at once at the law (5).

The similarities and differences of Planck's method and my own may perhaps be best summed up by saying that the methods of both are in effect the methods of statistical mechanics and of the theorem of equipartition of energy, but that I carry the method further than Planck, since Planck stops short of the step of putting $h=0$. I venture to express the opinion that it is not legitimate to stop short at this point, as the hypotheses upon which Planck has worked lead to the relation $h=0$ as a necessary consequence.

Of course, I am aware that Planck's law is in good agreement with experiment if h is given a value different from zero, while my own law, obtained by putting $h=0$, cannot possibly agree with experiment. This does not alter my belief that the value $h=0$ is the only value which it is possible to take, my view being that the supposition that the energy of the ether is in equilibrium with that of matter is utterly erroneous in the case of ether vibrations of short wave-length under experimental conditions.

J. H. JEANS.

On the Spontaneous Action of Radium on Gelatin Media.

SINCE my communication to NATURE on the subject of the experiments in which I have been for some time past engaged, my attention has been directed to the fact that M. B. Dubois, in a speech at Lyons last November, stated that he had obtained some microscopic bodies by the action of radium salts on gelatin bouillon which had been rendered "aseptic," but in what manner it is not stated.

I write to direct attention to the fact, as also to add that M. Dubois's experiments were quite unknown to me.

Moreover, the theory that some elementary form of life, far simpler than any hitherto observed, might exist and perhaps be brought about artificially by "molecular and atomic groupings and the groupings of electrons"—in virtue of some inherent property of the atoms of such substances as radium—was pointed out in my article on the "Radio-activity of Matter" in the *Monthly Review*, November, 1903, whilst the experiments which I have been carrying out to verify this view have been for a long time known in Cambridge.

Although I did not make a speech on the subject, I demonstrated the growths to many people at the Cavendish and Pathological laboratories early in the Michaelmas Term last year.

So momentous a result as it seemed required careful confirmation, and much delay was also caused in taking the opinions of various men of science before I ventured to write to you upon the subject.

That M. Dubois's experiments have been made quite independently I do not entertain the slightest doubt.

Some critics have suggested that these forms I have observed may be identified with the curious bodies obtained by Quincke, Lehmann, Schenck, Leduc and others in recent times, and by Rainey and Crosse more than half a century ago; but I do not think, at least so far as I can at present judge, that there is sufficient reason for so classifying them together. They seem to me to have little in common except, perhaps, the scale of being to which as microscopic forms they happen to belong.

JOHN BUTLER BURKE.

The Problem of the Random Walk.

CAN any of your readers refer me to a work wherein I should find a solution of the following problem, or failing the knowledge of any existing solution provide me with an original one? I should be extremely grateful for aid in the matter.

A man starts from a point O and walks l yards in a straight line; he then turns through any angle whatever and walks another l yards in a second straight line. He repeats this process n times. I require the probability that after these n stretches he is at a distance between r and $r+\delta r$ from his starting point, O.

The problem is one of considerable interest, but I have only succeeded in obtaining an integrated solution for *two* stretches. I think, however, that a solution ought to be found, if only in the form of a series in powers of $1/n$, when n is large.

KARL PEARSON.

The Gables, East Ilsley, Berks.

British Archæology and Philistinism.

AT the end of the second week in July two contracted skeletons were found in a nurseryman's grounds near the famous British camp at Leagrave, Luton. Both were greatly contracted; one, on its right side, had both arms straight down, one under the body the other above; the other skeleton lay upon its left side, with the left hand