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

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

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

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

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

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

which agrees with my own result. Planck arrives at equation (3) by the help of his assumption of "näturliche 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

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

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

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

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

LETTERS TO THE EDITOR.

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The Problem of the Random Walk.

THIS problem, proposed by Prof. Karl Pearson in the current number of NATURE, is the same as that of the composition of n iso-periodic vibrations of unit amplitude and of phases distributed at random, considered in *Phil. Mag.*, x., p. 73, 1880; xlvii., p. 246, 1899; ("Scientific Papers," i., p. 491, iv., p. 370). If n be very great, the probability sought is

$$\frac{2}{n}e^{-r^2/n}rdr.$$

Probably methods similar to those employed in the papers referred to would avail for the development of an approximate expression applicable when n is only moderately great. RAYLEIGH.

Terling Place, July 29.

The Causation of Variations.

IT is sometimes said that natural selection has ceased as regards civilised man; but very clearly this is an error. All civilised and most savage races are very stringently selected by various forms of zymotic disease. Thus in England practically everyone is brought into contact with the organisms which give rise to tuberculosis, measles, and whooping-cough; those individuals who are the most resistant to the organisms repel infection (*i.e.* do not fall ill), the less resistant suffer illness but survive, the least resistant perish. Abroad, malaria, dysentery, and many other complaints play a similar *rôle*. Probably no one is absolutely immune to any disease; but since illness only follows invasion of the tissues by a sufficient number of the microbes (the sufficiency of the number varying with the individual attacked), and since the microbes are more abundant in some localities than in others, the stringency of selection as regards any disease is greater in some places than elsewhere. For example, selection by tuberculosis is more stringent in the slums of cities than in the country. It should be noted, also, that resisting power against any one disease does not imply resisting power against any other; thus an individual innately strong against measles is not necessarily strong against tuberculosis. The result of all this elimination by diseases demonstrates the action of natural selection very beautifully. Every race is resistant to every disease strictly in proportion to its past experience of it. Thus Englishmen who have suffered much from tuberculosis are more resistant to it than West African Negroes who have suffered less, and much more resistant than Polynesians who have had no previous experience of it; that is, as a rule, Englishmen, under given conditions, contract the disease less readily, or if infected recover more frequently, or if they perish do so after a more prolonged resistance than Negroes and Polynesians. Negroes, on the other hand, as South American plantation experience proves, are more resistant to malaria than Asiatic coolies, who in turn are more resistant than Englishmen and Polynesians.

Against some diseases (*e.g.* tuberculosis) no immunity can be acquired, that is, experience of the disease confers no increase of resisting power, the disease pursuing a course of indefinite length. Against other diseases (*e.g.* measles) immunity may be acquired, that is, experience of the disease, if not fatal, confers after a definite time a more or less permanent immunity on the sufferer. In the former case the survivors are mainly those who have an inborn power of *resisting* infection; in the latter they are those who have an inborn power of *recovering* from infection. Evolution has proceeded on these lines. Thus Englishmen are less readily infected with tuberculosis than Polynesians, but nearly all Englishmen, like Polynesians, readily take measles, though a much greater proportion of them survive and acquire immunity. Lastly, in relation to such very "mild" diseases as chicken-pox, which render the individual very ill while they last, but cause hardly any elimination, no race appears to have undergone any change; for instance, no race, apparently, is more resistant to chicken-pox than any other race.

The pathogenetic organisms of all prevalent human diseases are more or less entirely parasitic on man. Most of them, therefore, flourish best in crowded populations, where they can pass readily from one susceptible individual to another. Thus tuberculosis is most prevalent in the slums of great cities. An important exception is malaria, the parasites of which require special conditions, and which, therefore, is more prevalent in the open country than in towns. The inhabitants of the eastern hemisphere have been afflicted by a multitude of zymotic diseases for thousands of years. Of old, with the increase of population, the conditions slowly became worse, the stringency of selection became greater, and the human races underwent continual evolution. But before the voyage of Columbus zymotic disease, with the exception of malaria, appears to have been almost, if not quite, unknown in the New World. We have fairly definite accounts of the first introduction of most Old World diseases to this and that aboriginal race, and of the frightful destruction of life that followed, the principal agent of elimination being tuberculosis. With their diseases the European immigrants introduced modern civilised conditions of life, especially churches, schools, and other enclosed spaces in which the natives, crowded together, conveyed infection to one another, and clothes, which acted as a deterrent to cleanliness, and which, besides, harboured the microbes of disease better than the naked skin. As a consequence, except when protected by malaria in extensive forests or when dwelling remote in unsettled regions, the natives rapidly perished. It is a significant fact that, whereas in Asia and Africa every town inhabited by Europeans has its native quarter, no European town in the temperate parts of the western hemisphere (*i.e.* where tuberculosis is most rife) has its native quarter. Published health statistics demonstrate quite definitely that the abnormally high mortality of the natives is caused by introduced diseases. Since civilisation implies a dense and settled population, it follows that no race can now achieve civilisation that has not undergone evolution against tuberculosis and kindred diseases. The case of the Negroes is interesting. In Africa they had undergone some evolution against tuberculosis. In America, when they were first taken to it, the disease prevailed to a comparatively slight extent, especially amongst the agricultural population; but the conditions slowly became worse, and the descendants of the early slaves underwent concurrent evolution. To-day they are able to persist in the northern cities, though their death-rate there is still abnormally high. But though a constant stream of Negro slaves and soldiers (*e.g.* in Ceylon) was poured for centuries into parts of Europe and Africa, they have left no trace on the population. All perished in a few generations, the elimination being so stringent as to cause extinction, not evolution. It is tolerably certain that a fresh immigration of African Negroes to America would end as disastrously.

These facts appear to establish conclusively two truths, first that evolution is due solely to natural selection, and second that variations, except, perhaps, in rare instances, are not due to the direct action of the environment on the germ-plasm, but are "spontaneous." The Lamarckian doctrine is quite out of court. If ever acquirements are transmitted, it should be in the case of the profound and lasting changes affecting the whole body which result from disease; but in no instance is the effect produced by any disease on the race similar to that produced by it on the individual. Thus tuberculosis injures the individual but confers resisting power on the race; measles confers immunity on the individual, but none on the race. Were the Lamarckian doctrine true, man could not persist on the earth. Presumably this is true of all other species, since probably all organisms are subjected to causes of slow deterioration similar to disease. If ever external agencies acting directly on the germ-plasm alter its composition and so cause variations (of any sort) in offspring, while Mr. Fletcher, paymaster, and Dr. Simpson are collecting the insects and land plants. I may say at once that the latter are of the type which one would expect to find on purely oceanic islands, but their distribution from island to island is interesting, as well as their preferences for sand or rock, drought or moisture, &c., most of the islands having definite zones with their peculiar plants.

"It is really as yet too early to say anything about the reefs here, as there are one or two places which I have not yet been able to visit. What strikes one, however, very forcibly is the comparative absence of life on them. Of course there are in places plenty of corals, but the number of species is quite limited. There is a fair number of the usual Alcyonaria, but Sponges, Hydroids, and Tunicates are very few in species and in quantity. Turbellaria are very rare, while Molluscs, Echinoderms, and Crustacea are few in species and, except certain common forms, not numerous. Ptychodera we have obtained, as well as a few Sipunculids, but Amphioxus and Thalassema we have not found. At Minikoi in two tides I have brought to the camp as great a variety of animals as Cooper and I have obtained here working ten tides up to the present. Indeed, life here is strictly limited in variety, and, when the marine collections have been fully worked up, one is inclined to anticipate, even so early, that some definite light will be thrown on the distance to which the larvæ of marine animals can cross the open ocean, on the distribution, in fact, of marine animals. The same, too, is true as well of the marine plants, nullipores alone being common.

"I am now endeavouring to work up the physical conditions of the atoll so as to find, if possible, whether there is any physical cause for the comparative paucity of free-living animals. I am sending Cooper in the ship to-morrow to Diego Garcia, where he will have four or five days while she is coaling to examine the land and reefs. I remain here, but I hope by the time of his return, in about twelve days, to have finished my work and to move on to Peros Banhos, while the *Sealark* is sounding between the banks and round the Chagos Archipelago."

The Problem of the Random Walk.

I HAVE to thank several correspondents for assistance in this matter. Mr. G. J. Bennett finds that my case of $n=3$ can really be solved by elliptic integrals, and, of course, Lord Rayleigh's solution for n very large is most valuable, and may very probably suffice for the purposes I have immediately in view. I ought to have known it, but my reading of late years has drifted into other channels, and one does not expect to find the first stage in a biometric problem provided in a memoir on sound. From the purely mathematical standpoint, it would still be very interesting to have a solution for n comparatively small. The sections through the axis of Lord Rayleigh's frequency surface for n large are simply the "cocked hat" or normal curve of errors type; for $n=2$ or 3 they do not resemble this form at all. For $n=2$, for example, the sections are of the form of a double U, thus **UU**, the whole being symmetrical about the centre vertical corresponding to $r=0$, but each U itself being asymmetrical. The system has three vertical asymptotes. It would be interesting to see how the multiplicity of types for n small passes over into the normal curve of errors when n is made large.

The lesson of Lord Rayleigh's solution is that in open country the most probable place to find a drunken man who is at all capable of keeping on his feet is somewhere near his starting point!

KARL PEARSON.

Proposed Magnetic and Allied Observations during the Total Solar Eclipse on August 30.

IN response to my appeal for simultaneous magnetic and allied observations during the coming total solar eclipse, cooperative work will be conducted at stations distributed practically along the entire belt of totality and also at outside stations, nearly every civilised nation participating.

These observations will afford a splendid opportunity for further testing the results already obtained. All those who are able to cooperate are invited to participate in this important work.

The scheme of work proposed embraces the following:—

(1) Simultaneous magnetic observations of any or all of the elements according to instruments at the observer's disposal, every minute from August 29, 22h., to August 30, 4h., Greenwich mean astronomical time.

[To ensure the highest degree of accuracy attainable, the observer should begin work early enough to have everything in complete readiness in proper time. See precautions taken in previous eclipse work as explained in the journal *Terrestrial Magnetism* (vol. v., p. 146, and vol. vii., p. 16). It is essential, as shown by past experience, that the same observer make the readings throughout the entire interval.]

(2) At magnetic observatories, all necessary precautions should be taken so that the self-recording instruments will be in good operation, not only during the proposed interval, but also for some time before and after, and eye readings should be taken in addition wherever it be convenient.

[It is recommended that, in general, the magnetographs be run on the usual speed throughout the interval, and that, if a change in the recording speed be made, every precaution possible be taken to guard against instrumental changes likely to affect the continuity of the base lines.]

(3) Atmospheric electricity observations should be made to the extent possible by the observer's equipment and personnel at his disposal.

(4) Meteorological observations in accordance with the observer's equipment should be made at convenient periods (as short as possible) throughout the interval. It is suggested that, at least, temperatures be read every fifth minute (directly after the magnetic reading for that minute).

(5) Observers in the belt of totality are requested to take the magnetic reading every fifteen seconds during the time of totality, and to read temperatures as frequently as possible.

(6) At those stations where the normal diurnal variation cannot be obtained from self-recording instruments, it is desirable to make the necessary observations for this purpose on as many days as possible before and after the day of the eclipse, and to extend the interval of observations given above if conditions permit. In general, those who will have self-recording instruments have decided to run them for at least eight days before and after the day of the eclipse.

It is hoped that observers will send full reports of their work to me as soon as possible for incorporation in the complete monograph on this subject to be published by the Carnegie Institution of Washington.

L. A. BAUER.

Department Terrestrial Magnetism, Carnegie Institution, Washington, D.C., July 15.

British Fruit Growing.

IN your remarks on p. 297 (July 27) on the above subject, you mention "the diversity of yield from farms in the same neighbourhood . . . due presumably to differences of shelter and aspect." It is a remarkable thing that, so far as I know, nothing has ever been done to find out and publish the most suitable localities, as regards soil and climate, for orchard planting. It is a question of very great complexity, and can only be dealt with properly by officials appointed for that purpose; but its importance in fruit culture is so obvious that a considerable expenditure would be well repaid. Few people have any idea of the great climatic differences in localities within even a few hundreds of yards!

This house is on the south slope of the long range of Lower Greensand hills which runs parallel with the Chalk range the whole length of Kent from west to east. At this point the slope rises steeply from 200 feet above sea-level to 500 feet, my house being about 350 feet. I have carefully observed the effects of frost, &c., for the last six years, and it appears to me that the variations in temperature in the vertical limits mentioned are much greater than would be expected. Up to the 400-feet contour line the climate is singularly equable, which is proved not only by daily thermometrical observations, but by the