Letters to the Editor

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Activities of Life and the Second Law of Thermodynamics

WE regret the necessity of prolonging this discussion, but in spite of the letter of Sir James Jeans[1] we persist in the conviction that it is his reasoning, not ours, which is fallacious. We are quite aware that the change of positional entropy associated with the type of process which he cites involves the factor Nk, where k is the Boltzmann constant and N the number of 'particles' concerned in the process, nor do we dispute the correctness of the well-known formulæ which he quotes. We must, however, point out that he is wrong in assuming that the number of 'particles' *must* coincide with the number of molecules. We might ask, why not the number of atoms, or the number of protons and electrons? The answer is, that for a given process of redistribution the 'particles' are those units whose relationship to one another is altered but whose internal structure remains unaffected. In the process of sorting out trucks each truck is to be reckoned as a 'particle'; in the process of steering the *Mauretania* the ship is a 'particle'.

To revert to the type of case originally considered by Sir James Jeans, let us imagine a large number of equal spheres of glass on a frictionless horizontal plane. If N of these spheres be moved from a place where the (superficial) density of distribution of the spheres is ν to a place of higher density ν^1, then the decrease of positional entropy of the system is equal to kN (log ν^1 − log ν). According to Sir James Jeans, however, the decrease of positional entropy would be kn_1N (log ν^1 − log ν), where n_1 is the number of molecules contained in each sphere. If he reasons in this manner, we would ask him why the decrease of positional entropy should not also be kn_2N (log ν^1 − log ν), where n_2 is the number of atoms, or the number of protons and electrons, contained in each sphere. This paradox clearly reveals the fallacy in his reasoning.

Finally, we would point out that the total entropy of an assembly of N identical systems, each made up of n ultimate particles, may be resolved into the sum of two terms, the first of the order Nk determined by the configuration (and relative motion) of the centres of mass of the N systems, the second of the order $N(n-1)k$ determined by the internal arrangement of the ultimate particles in each system. In any process in which the internal arrangement of the systems remains unchanged, only the *first* term in the entropy is affected. We think it scarcely possible that Sir James Jeans would dispute this statement, although the views expressed in his last letter contradict it.

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Calcium Isotopes and the Problem of Potassium

BY the systematic use of the purest materials, I have succeeded in reducing the effect of potassium in the mass-spectrum of calcium to a negligible quantity. Under these conditions, the line 41 disappears completely and it is quite safe to conclude that the isotope Ca 41 does not exist, at least to 1 part in 1000, in the element. Photometry gives the following provisional constitution for calcium:

Mass numbers ..	40	42	43	44
Abundance ..	97	0·8	0·2	2·3

It will be noted that the abundance of Ca 44 is much greater than that originally reported by Dempster[1] and is in better accord with the chemical atomic weight.

I have been kindly supplied with compounds of calcium extracted from biotite by Prof. G. v. Hevesy and from pegmatites from Rhiconich and Portsoy by Prof. J. Kendall. On the view that the radioactivity of potassium is due to the simple beta ray transformation of K 41 to Ca 41, these samples should be rich in the latter. On analysis their mass-spectra showed no appreciable difference from that of ordinary calcium, so that the abnormal atomic weights reported by Kendall[2] cannot be ascribed to the presence of the hypothetical isotope 41.

Hevesy's beautiful distillation experiments have shown that the radioactivity of potassium is unlikely to be associated with the abundant light isotope 39, so that the failure to detect Ca 41 appears to favour some more complex theory of the disintegration such as that recently suggested by Gamow[3].

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[1] *Phys. Rev.*, **20**, 633; 1922.
[2] NATURE, 131, 688, May 13, 1933.
[3] NATURE, 133, 746, May 19, 1934.

Interaction of Radio Waves

IN our letter published in NATURE of February 10 last, Dr. Martyn and I stated that we had found that Tellegen's observation of an apparent interaction of radio waves could be explained by taking account of the changes, in the mean velocity of agitation of electrons in the ionosphere, produced by a strong electric wave.

Another interesting consequence of this effect due to an electric wave may be pointed out, namely, the production of so-called 'atmospherics' in a radio receiver by modulation of the received carrier wave.

An atmospheric electric pulse, acting on the electrons in a part of the ionosphere through which the carrier wave passes, momentarily increases the absorbing power of that part and so momentarily reduces the amplitude of the received carrier wave. Thus an irregular succession of sounds is produced in the receiver, similar to the effects produced more directly by atmospheric pulses.

Thus it appears possible that observed 'atmospherics' are of two types, one associated with and proportional to the intensity of the carrier wave, and the other completely independent of the carrier wave.

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