Letters to the Editor.

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Stellar Structure.

IT has frequently been stated, by myself amongst others, that it is necessary to assume in the inside of stars a temperature of the order of mc^2/k in order to explain the generation of energy by the annihilation of matter, m being the mass destroyed in each process, c the velocity of light, and k Boltzmann's constant. This letter is written in order to make clear that this assumption is not necessary. It is perfectly true that the equilibrium constant of a process, subject to the laws of thermodynamics, is of the order $e^{-\epsilon/kT}$, ϵ the energy of the process in this case, of course, being equal to mc^2. The equilibrium constant does not, however, determine the generation of energy. What one is concerned with in the case of a star is the rate at which energy is produced; in other words, if one presupposes the simplest process of annihilation, the rate at which protons and electrons disappear in the form of radiation. This is analogous to the rate of chemical reaction, not to the equilibrium constant of a reversible reaction.

In most chemical processes the rate of reaction is governed by the number of molecules activated per second, which again depends upon the number of particles the energy of which exceeds a certain value, say, ϵ_1. This number is proportional to $e^{-\epsilon_1/kT}$, and one therefore finds, roughly speaking, that the rates of reaction are negligible unless the temperature is such that the mean energy of the molecule is comparable with the energy of excitation. Since this activation energy is usually of the same order as the energy of the reaction, the conclusion is often extended without great inaccuracy to the total energy of the process.

In the case under consideration, the annihilation of protons and electrons, it seems difficult to imagine any form of excitation, and the rate at which it proceeds can therefore scarcely depend upon a function of this type. Presumably, in such collisions as are effective, certain circumstances, which occur but rarely, have to be fulfilled. When these are fulfilled, and they may not be such as require any high velocities, matter is converted into radiation; in the vast majority of cases, a collision has no such result. If this view is correct, the rate of annihilation, and therefore the rate of generation of energy, will depend in the first instance on the number of collisions per second, which of course varies with the density and with something like the square root of the temperature; and in the second instance, upon the special circumstances which render a collision effective, and which may, or may not, depend upon the temperature. In either event the simple exponential expression is not applicable, and the conclusion that matter can only be annihilated and energy produced in stars where interiors are at temperatures of the order mc^2/k, that is, $1{\cdot}1 \times 10^{13}$ degrees, is valid.

It would be true if the matter-radiation equilibrium has been attained and any further production required a change in the equilibrium constant. It is incorrect if the system has not reached equilibrium, for in this case thermodynamical reasoning is insufficient to determine the rate at which equilibrium will be approached.

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REFERRING to Sir James Jeans's letter in NATURE of Jan. 17, p. 89, I may say that I fully acknowledged in my paper of November 1929 (*Mon. Not. Roy. Ast. Soc.*, **90**, p. 20) that Sir James was the first to recognise the principle that the mass M and luminosity L of a star are independent variables as regards steady state considerations. On p. 53 of that same paper (a page of which Jeans himself quotes in another connexion) I made a general reference of obligation to his work. In my last paper (*Mon. Not. Roy. Ast. Soc.*, **91**) I build on Jeans's permanent contributions to science in three places, mentioning him by name (pp. 4, 9, 51). I could not, however, adduce any of the specific results of his theory of stellar equilibrium in support of my conclusions, for they are totally different; and I could not contrast his results with mine without venturing to discuss his mathematics.

I cannot assent to Jeans's mathematics, because his theory of stellar equilibrium is in formal contradiction with his own (L, M) independence principle. It is an immediate consequence of this principle that for a given mass M in equilibrium the ratio λ of gas pressure to radiation pressure may have any value whatever between zero and infinity, depending on the arbitrarily assigned L. This is fundamental in my analysis. According to Jeans ("Astronomy and Cosmogony", pp. 88, 89) λ is small for large masses and large for small masses, and is calculable in terms of M (p. 97). Jeans may claim the principle, but his theory is not consistent with it.

The point of my analysis is the construction of configurations which satisfy the (L, M) independence principle, even for models for which $\kappa\eta$ is constant. The special properties attributed to these models by both Jeans and Eddington then disappear, and the new general properties which emerge (explaining as they do why some stars are very dense and others not) are shared by other models, since they depend only upon the occurrence of the central singularity $r=0$ in a certain system of differential equations. Jeans uses throughout Emden's solutions, which possess no singularities.

As regards the branching-out of solutions near the boundary of a star, Jeans is considering a variety of models. *For any one model*, the solution is unique up to the boundary The work of Mr. Fowler, Mr. Fairclough, and myself published in *Mon. Not. Roy. Ast. Soc.*, **91** (November 1930), discusses the family of such solutions arising from Emden's equation; with any definite configuration, of arbitrarily assigned mass, luminosity, and opacity, there is associated *one* member of the family of solutions, selected by a boundary condition which ensures that the boundary layers, of the prescribed opacity, enclose M and radiate L.

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Generalisations and Modern Cosmogonies.

PROF. R. A. MILLIKAN in his retiring presidential address of the American Association for the Advancement of Science (NATURE, Jan. 31, p. 167), refers to the assumption "that the radiation laws which seem to us to hold here cannot possibly have any exception anywhere" as "precisely the sort of sweeping generalisation that has led us physicists into error half a dozen times during the past century". This emboldens me to ask again whether there is any evidence whatever for the uniform propagation of radiation in all directions in space from a sun or a star. I asked it (NATURE, Nov. 29, 1913, p. 339) at the time of Millikan's "fifth significant discovery", when radioactivity was indicating the necessity of extending