Letters to the Editor.

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The Problem of Stellar Luminosity.

I ASK your permission to reply to the arguments brought forward by Prof. Milne (NATURE, Mar. 22) against my treatment of the problem of stellar luminosity. The outcome of my investigation is a formula which predicts the luminosity of a star of given mass and radius (or mass and effective temperature). I have followed the common procedure of first employing special assumptions to make the mathematical answer definite, and then removing the assumptions by calculating the effect of the greatest admissible variation from the conditions adopted as standard—thus obtaining what is equivalent to a probable error of the prediction. The calculations of luminosity must, of course, be considered in conjunction with these estimates of probable error. I think that the discussion in "Internal Constitution of the Stars" of all *known* sources of uncertainty is exhaustive; and I conclude that the error is not so great as to impair the practical value of the formula. I have shown that differentially the result agrees excellently with observation, but absolutely it makes all the stars about ten times too bright. In view of this remaining discordance, I have been as eager as my critics in searching for possible loopholes or complications. If Milne's scrutiny brought to light any new possibility I should be grateful; but his criticism is not of this type. He claims that there is a fundamental inadequacy in my method, so that not even a rough value can be computed in this way.

In my deduction I employ those laws and conclusions of pure physics which are generally accepted as trustworthy. I think that most physicists will assent to my including among these the conclusion that matter in the usual stellar conditions of temperature and density behaves as a perfect (or slightly superperfect) gas—notwithstanding the doubts of my friend Sir James Jeans. But there is one result that as yet ought only to be used tentatively, namely, the physical calculation giving the absorption coefficient or opacity of matter in stellar conditions. If we accept it, my formulæ determine the luminosity; if we mistrust it, the application of my formulæ is inverted, and we use the observed luminosity to determine the intrinsic opacity of the material in the interior. In the opening paragraph of his long *Monthly Notices R.A.S.* paper, Milne proclaims his main conclusion that it is not possible to infer from the observed masses, luminosities, and temperatures the value of the absorption coefficient for the stellar interior; and in his letter to NATURE he puts the same challenge in the converse form, denying the dependence of luminosity on internal opacity.

Now the greater part of Milne's letter is occupied with a problem which is only indirectly related to my investigation, namely, the problem of supplying energy (presumably from sub-atomic sources) at a suitable rate to maintain the star in an approximately steady state for vast periods of time. I think I was the first to insist on the difficulty of this regulation of supply (NATURE, Mar. 21, 1925, p, 419; May 1, 1926, Supp.). Many dilemmas similar to those touched on by Milne are collected and surveyed in "Internal Constitution of the Stars", Chap. xi. But these difficulties in reaching a satisfactory theory of evolution and in accounting, for example, for the Hertzsprung-Russell diagram, do not concern my investigation of luminosity, which depends only on the present equilibrium. Milne complains that my scheme of equations does not automatically provide that $L=\int\epsilon dM$ shall be satisfied. Of course, it does not. As well might he demand that the equations by which the flow of water in a main is calculated from the head of pressure should automatically control the machinery of the pumps. (It is not very clear whether in referring to contracting stars Milne takes to mean the actual rate of liberation of sub-atomic energy or the rate from a fictitious distribution of sources which he introduces in a formulation of the cooling problem; with the latter meaning $L=\int\epsilon dM$ is automatically fulfilled without reference to my equations.) My equations refer to ordinary equilibrium and they are indifferent as to whether the slow secular changes proceed with a time-scale of a million or a thousand million years.

Only at one point do considerations of evolution and energy-supply impinge on my theory of stellar luminosity. If fully known, they would enable us to assign an exact value to a certain factor (α in my formulæ, η in Milne's version), which depends on the relative distribution but not on the magnitude of the energy sources. As it is, we leave this factor undetermined within limits; the extreme range is about $2\frac{1}{2}:1$, and this is included in the uncertainty of prediction above-mentioned. Thus the one really vital sentence in Milne's letter is, "Similar considerations show that the luminosity of a star cannot be even roughly independent of the relative distribution of energy-sources". That is to say, α and $\bar{\eta}$ have unrestricted range. This throws over a conclusion on which Milne, Jeans ("Astronomy and Cosmogony", p. 101), and I had all reached agreement. Instead of following up this assertion with the expected justification, he proceeds to argue that the luminosity depends on the radius. (As this is what my formulæ assert, I have no need to dissent.) It is strange that Milne should leave his most essential assertion unsupported both in his letter and in his longer paper. It is strange that he should think it unnecessary to say why he rejects my calculation of the amount of dependence. Strangest of all, he treats with equal contempt his own paper on "Stellar Equilibrium and the Influence of the Distribution of Energy-production" (*Monthly Notices, R.A.S.*, vol. 87, p. 708).

Finally, I can scarcely agree that Prof. Milne has disproved my conclusion that the uranium model is unstable. His objection is that physically a star cannot "contract indefinitely"; sooner or later it must find a (contracted) configuration of equilibrium. By the same argument Humpty Dumpty's position on the wall was not unstable; ultimately he found a new configuration of equilibrium. A. S. EDDINGTON.

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Mar. 17.

The Relation of Fluidity of Liquids to Temperature.

NONE of the formulæ proposed for the effect of temperature upon the viscosity of liquids has secured general acceptance, in the sense of permitting satisfactory comparison of liquids.

In the shearing over unit area defining viscous resistance, we may suppose at any moment an equilibrium between undeformed and deformed molecules, to which the Boltzman distribution function applies. This leads to an expression relating fluidity to temperature of the form

$$\log\phi=-\frac{k}{T}+C,$$