Letters to the Editor.

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Liquid Stars and Atomic Volume.

THE problem of the general state of matter at high temperature is so fundamental in stellar physics that I venture to pursue the discussion with Dr. Jeans (NATURE, Feb. 25, p. 278). I would first thank him for his reply to my letter, which, though I differ from it on a number of points, deals fairly with the questions raised. I still believe I was not exaggerating in saying that Jeans's theory requires that the ions (in giant *M* stars) should be so large that they jam at densities $\frac{1}{100}$ that of air. Jeans wishes to amend this to $80 \times$ air. But my figure represents the order of magnitude of the central density of Betelgeuse according to the usual gas model, and he has himself said that the star contracts as a gas until there are substantial deviations from the gas laws in the central regions. To form his liquid core of higher density it would seem that Betelgeuse must have contracted as a gas to $\frac{1}{20}$ its present radius, and then for unexplained reasons thrown out a gaseous envelope filling the present volume. Or starting from his liquid core of density $80 \times$ air, we have to face the problem of balancing on this a gaseous atmosphere containing much less mass and subject initially to 8000 times the gravitation of the ordinary model. I think that to achieve this Dr. Jeans will have to depart much more widely from current theory than he intended.

Again, Dr. Jeans favours the hypothesis that long-period variables are pulsating stars. Like others, he finds this supported by the relation of period to density. It is well known that the periods and densities fit approximately if the usual densities are accepted; but the agreement will not be maintained if the dynamical behaviour is determined by a liquid core of density 8000 times greater. The high density and comparative incompressibility would make the period much too short.

Accordingly, I do not give up my point that Dr. Jeans requires the ions to have diameters 50 times greater than the ordinary neutral atoms. For the rest of this letter, I am content to accept his own estimate of diameters 50-80 times the diameter of the electron system in the Bohr model, which he agrees is "a bit perplexing," but will not admit as "certainly wrong." May I protest that the words "certainly wrong" were not used in this connexion in my letter? They were applied to one of his defences (or suggestions) beginning with "we might, in any case, expect," referring therefore to current knowledge and not to unforeseen developments of atomic theory. The expectation was one which would seem very plausible to most readers, who naturally think that the classical electrostatic repulsions between the ions, by tending to prevent unduly close approaches, have effect equivalent to atomic volume. When, however, the attractions between ions and electrons are included and the whole correction to the pressure is calculated according to the general methods (set forth clearly in Jeans's "Dynamical Theory of Gases"), it is at once found that the electrostatic forces actually give the reverse effect to atomic volume. The material accordingly remains as compressible as (or more compressible than) a perfect gas until the density is so great that non-classical reactions become important. There we come to more doubtful ground, but the general cause of the atomic volume effect in ordinary gas seems fairly clear. It is not the ions, but the bound electrons which set a limit to the packing. One quantum orbit or one unit cell of phase-space is required for each electron. With increasing density the cells become filled, and ultimately we should be unable to proceed further without squeezing electrons out of the material. Alternatively, if the electrons are endowed with high energy extra cells of phase-space corresponding to high velocities become available, and the congestion is relieved. In this way high temperature obviates the congestion that gives rise to atomic volume effects.

With ions distant 50-80 times their own diameter and any number of intervening quantum orbits lying vacant, there is no approach to the congested condition. I think that a quantum physicist confronted with such a problem would not hesitate to treat it by classical perturbations in accordance with the Correspondence Principle. Dr. Jeans seems to be demanding something which goes against not the details, but the broadest principles of the quantum theory. Even the broadest principles may of course need amendment, and it would be rather pleasing if astronomical results were definite enough to dictate amendment; but I have devoted part of my former letter to challenging Jeans's argument that considerations of stability make the liquid star theory *compulsory*. To my mind the main interest in the theory of the constitution of the stars lies in connecting the laws and conclusions reached by physicists with those discovered in astronomy; if once we begin modifying the former, the investigation loses definite aim and takes on a speculative character. If a new astronomical theory provides its own rules for atomic volume, it may equally well provide its own rules for the absorption coefficient, etc.

With regard to the significant features of the Russell diagram, which Jeans believes to be explained by his theory, I admit his counter charge that I am waiting for something to turn up. It is possible in cosmical theories to be too precipitate.

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I CAN only adhere to my original views as to atomic diameters. Prof. Eddington tries to challenge my position by arguments based on "the usual gas model." By this I think he means his own gas model, which is of a very special and restricted type, and by no means characteristic of the general gaseous configurations of a star. I believe his difficulties arise solely from the defects of his own model.

Eddington's model is developed from the assumption that the generation of energy per unit mass G is connected with the coefficient of opacity k by the relation

$$G=\frac{1}{\rho r^2}\frac{d}{dr}\left[\frac{1}{k}\int_0^r \rho r^2 dr\right]\times \text{constant}.$$

This seems to me an extraordinarily artificial relation. As Russell has repeatedly pointed out, it makes G violently negative in the outer layers of the star, so that these re-destroy the energy created in the inner layers, only a small balance escaping from the star as radiation. I think I am right that the reason why Eddington adopted this surprising value for G was merely that it makes the equations integrate out with amazing ease—in fact, just like a Tripos problem.

When this special value for G is discarded, the problem of gaseous stars becomes far more complex. I have given partial solutions in the *Monthly Notices of the Royal Astronomical Society* (1925 and 1926), and a fuller solution appears in a book, "Astronomy and