I AM very glad to have seen Prof. Eddington's critical comments on the theory of liquid stars.

The difficulty as to atomic diameters, which he places in the forefront, and also in the tail, of his letter, seems to me also to be the most serious difficulty in the way of the theory. But Prof. Eddington overestimates its amount, though arguing as though I maintained that the stars were liquid throughout, from centre to surface. If a star such as Betelgeuse breaks up by fission, it probably forms a binary system with the dimensions of V Puppis. If so, considerations of angular momentum show that before fission the greater part of its mass must have resided within about a twentieth part of its radius from the centre. For this reason I imagine Betelgeuse to consist of a liquid core having a radius perhaps only five per cent. of that of the star, while the other 95 per cent. of the radius is occupied by very tenuous gas. The small core determines the dynamical behaviour of the star, because it contains most of the mass; the rest is a mere obscuring veil. In more ordinary stars the liquid core may extend over perhaps a third or a fifth of the radius.

This consideration reduces the diameters which Eddington assigns to my ions by a factor of from 3 to 20, and the ionic volumes by a factor of from 27 to 8000; for example, the concluding words of his letter should not be "densities $\frac{1}{100}$ that of air," but "densities 80 times that of air," which makes a difference.

All the same, the hypothesis admittedly requires effective diameters many times larger than the orbital diameters of the Bohr atom. What Eddington describes as my "defence" of this was only meant as a suggestion. It may be wrong, but I am surprised at Eddington describing it as "certainly wrong"; it had never occurred to me that modern quantum-dynamics was quite so sure of itself as this, especially in dealing with states of matter of which we have no experience. Meanwhile the atom, like the stars, is dissolving into radiation, and the wave-mechanics may throw new light on the matter before long. But I frankly admit the difficulty as a bit perplexing, although not in the least as fatal or insuperable.

I cannot follow either Eddington's arguments or his statements about instability, and I have not yet studied Vogt's paper in detail. I ought, however, to say that my own mathematical analysis did not confirm Eddington's conjecture as to the efficacy of a time-lag in promoting stability. With a long enough time-lag all matter is obviously reduced to the purely radioactive condition in which the liberation of energy is uninfluenced by changes of temperature and density, and I think Eddington agrees with me that gaseous stars of this type are unstable all along the line and in every conceivable configuration in which the gas laws are obeyed. I would also remark that, even if I were to concede all of Eddington's statements and arguments, the validity of the theory of liquid stars would remain absolutely untouched; his arguments are not directed against the tenability or accuracy of the theory, but only against its inevitability.

On this question, may I point out that there are only two possibilities open—in the central regions of stars, either the gas laws are obeyed or they are not. The former is the hypothesis of gaseous stars, and the latter of liquid stars. I still consider that stability considerations rule out the former, and so make the latter inevitable. But, apart from this, the theories admit of almost direct observational test, by comparing their predictions with the observed Russell diagram, which is observationally indisputable.

For the configurations possible for stars of given mass, the theory of gaseous stars predicts a system of parallel, slant, approximately straight lines. The theory of liquid stars predicts the wavy curves I showed in my article in NATURE. Seares (*Astrophys. Jour.*, **55**, p. 195; 1922) has drawn the lines indicated by observation and gives a set of curves which are very wavy indeed, and show the same general characteristics as the curves requisite for liquid stars; they show no resemblance at all to the straight lines of gaseous theory.

The two opposing theories can also be tested in terms of the areas of the Russell diagram which are tenanted by stars. The theory of liquid stars predicts a diagram shaped like a hand with white dwarfs lying along the thumb. Observation shows a diagram shaped like a hand, with white dwarfs lying along the thumb, the only complication being that observation cannot reach down to where the thumb joins the hand. The theory of gaseous stars predicts merely a flat, featureless diagram, into which features can only be introduced by extraneous *ad hoc* assumptions. Yet the observed features of the Russell diagram represent the outstanding facts of physical astronomy. Consider, for example, the almost sensational fact that no star of solar mass is known with a density intermediate between 1·4 (the sun) and 50,000 (Sirius B). The atomic nuclei are 15 times as widely spaced in one star as the other, and no intermediate spacing is known to astronomy. What does it mean? Apart from liquid stars, I know of only one suggested explanation, and this is purely *ad hoc*. All stability considerations being thrown to the winds, the stars are supposed to radiate by the same mechanism as an explosive at its flash-point. The flash-points of the sun and Sirius B are supposed to be so different that one is reached at a density of 1·4, and the other only at a density of 50,000; and it is assumed that no type of stellar matter exists with a flash-point intermediate between these two extremes. Does Prof. Eddington really prefer to accept this medley of *ad hoc* assumptions rather than concede the effective diameters demanded by the theory of liquid stars? Of course, he may say he prefers neither, thereby laying himself open to the charge he brings against me of waiting for something unknown to present-day physics to turn up.

I obviously cannot occupy more space, but I think all the other points raised by Prof. Eddington are dealt with in my papers in the *Monthly Notices*.

J. H. JEANS.

The Nature and Function of Golgi Bodies.

As he attributes to me the fallacy "that things which look alike are necessarily the same," it would appear that Dr. Ludford had forgotten parts of my letter to NATURE, Jan. 21, and corresponding parts of my paper (*Proc. Roy. Soc.*, B, vol. 101, 1927), before he wrote (NATURE, Feb. 4). These parts are very important, and show that, whether right or wrong, my arguments are not based upon the fallacy that Dr. Ludford sets up, and then proceeds to knock down.

All cells contain lipins. If acetic acid is used in fixing fresh material, the appearances known as 'Golgi bodies' are absent. If no acetic acid is used in the fixative, they appear after suitable treatment. My mixtures containing lipins behave in exactly the same way. The Golgi bodies appear or are absent, under the same conditions as they appear or are absent, in fixed cells.

If the Golgi bodies are really cell structures and not the products of the treatment to which the cells are subjected, then there should be two sets of Golgi bodies in each cell, for lipins are present in the cells