views on this question was given at the Versammlung Deutscher Naturforscher und Ärzte at Innsbruck in August and published in the *Physikalische Zeitschrift* (25, No. 22, p. 588, 1924).

We must await the results of further detailed experiments to see how far such observations of scattering throw definite light on the problem of the mechanism of a disintegrating collision. It seems clear, however, that a large amount of careful quantitative work as well as a great number of photographs of α ray tracks will be required before we can hope to obtain detailed evidence of the mechanism of such collisions and of the fate of the bombarding α particle for all the " active " elements.

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The Source of Stellar Energy.

I AM glad to have drawn so interesting a letter from Prof. Eddington as appears in NATURE of March 21. I cannot, however, agree with him that the present position is one of " an almost hopeless deadlock," and neither do I agree with his criticisms of my scheme (NATURE, Feb. 28), by which a star consists of a mixture of different types of destructible matter which spontaneously dissolve into radiation at different rates, the rate in each case being unaffected by physical conditions of temperature and pressure. The process imagined by me was in fact analogous to radioactive decay except that the end product is radiation instead of other forms of matter.

Prof. Eddington considers that under this scheme the stars would be unstable. A star which in some way increased its rate of generation of energy would expand and this expansion would, he says, lessen the rate at which it was able to radiate its energy away. But why ? The expanded configuration is not one of equilibrium and I cannot see that we have any knowledge as to the corresponding rate of radiation. *A priori* we would certainly expect that the star's first move, when it found too much radiant energy accumulating in its interior, would be in the direction of getting rid of more radiation, not less, as Prof. Eddington asserts. If so, Prof. Eddington's argument collapses entirely. It may be remarked that if the argument were sound and instability were proved, we could only restore stability by supposing that a decrease in a star's density and temperature *decreased* its spontaneous generation of energy, whereas in actual fact it is the stars of lowest density and of lowest internal temperature which radiate the most energetically.

My suggestion that when a star breaks up its ingredients are not fairly distributed between its constituent parts is criticised in the light of some conclusions Prof. Eddington has drawn from a certain recently published mathematical theorem. May I here merely state that in my opinion this theorem is entirely fallacious ? I hope to justify this statement in print very shortly.

My scheme certainly requires " that the rate of emission of radiation by the star shall be very largely dependent on its previous history." In actual fact considerable ranges of luminosity are shown by stars of identical mass. These I should attribute to differences of birth and previous history, and it may be possible to infer something as to the past histories of the stars from these ranges. Prof. Eddington considers that the observed range is too small, but does he know enough of the past history of different stars to say how large a range ought to be expected ? He discusses two hypothetical stars born originally with masses 12 and 3, but is there any reason for thinking that stars can be born with so great a disparity of mass ? I have, of course, to admit that in time a real difficulty may appear here, but at present its existence is not proven. To my mind the present difficulty lies in precisely the opposite direction ; it is to account for the tendency towards equality of mass which appears in the two components of a binary as its evolution proceeds.

I find it difficult to understand the advantages of the hypothesis which Prof. Eddington offers as an alternative to mine. He supposes certain destructible types of matter to be formed at a rate which depends on temperature and density. Their rate of spontaneous dissolution does not depend on the temperature and density at the instant, so that the rate of generation of radiation depends only on the total amount of destructible matter present in the star, which in turn depends on all the temperatures and densities of the past. The radiation, in fact, represents a sort of integral of the past temperatures and densities. As regards stability his stars are in the same position as those of my scheme, while as regards dependence on past history they seem to be worse off.

Any variation, either of creation or destruction of matter, with temperature and density ought almost certainly to be in the direction of higher activity accompanying an *increase* of density and temperature, whereas in actual fact the (internally) hot dense stars radiate little and vice versa. If Prof. Eddington insists on any sort of dependence on density and temperature, he must not only, as he says, " admit exhaustion-effects also," but must actually admit more exhaustion-effects than are required by my own hypothesis—unless indeed he can prove that high temperature and density inhibit radiation.

J. H. JEANS.

March 23.

Relation of Light to Bird Migration and Developmental Changes.

THAT light is a factor of prime importance in the inauguration or stimulation of bird migration, has been suggested by many authors from the days of Seebohm onwards. While many of the suggestions will not bear close investigation, at least one very attractive view has been put forward by Sir E. Sharpey-Schafer. In an address delivered some years ago to the Scottish Natural History Society [1] he makes the following comments, " . . . the regularity with which migration occurs, indicates that the exciting cause must be regular. There is no yearly change, outside the equatorial zone, that occurs so regularly in point of time as the change in the duration of daylight. On this ground this may well be considered a determining factor in migration, and it has the advantage over other suggested factors that it applies to the northerly as well as to the southerly movement." He says further " That it [migration] is a result of developmental changes in the sexual organs is improbable."

Evidently inspired by the work of the botanists Garner and Allard on what they have termed " photoperiodism," an American author [2] has lately revived this theory and has, apparently independently, come to the same conclusion as Sir Edward with regard to the absence of relation between developmental changes in the reproductive organs and migration.

On purely theoretical grounds it has always seemed to me that if the waxing and the waning of the days really in any way affect the migratory impulse, they must produce their effect through the gonads. This

[1] " On the Incidence of Daylight as a determining Factor in Bird Migration," E. A. Schäfer, NATURE, vol. 77, pp. 159-163 (December 19, 1907).

[2] " Is Photoperiodism a Factor in the Migration of Birds ? " G. Eifrig, *Auk*, vol. 41, pp. 439-444.