regard to Mr. Smith's alternative suggestion that the use, on one hand, of the near ultra-violet rays has been avoided in vision because of want of contrast, and of the near infra-red rays, on the other, because of the blackness of the shadows: if there were such difficulties in the use of these rays, one would expect ordinary non-colour-sensitive photography to be adversely affected on one hand, and for these black shadows to become visible when the eye is made sensitive to rays between 6800 and 7100 A. on the other.

Yet in my experience such is not the case. Is it not conceivable that the blackness of the shadows was due in R. W. Wood's photographs to slight under-exposure, seeing that only recently have fast infra-red sensitive plates been available. To my mind, it is more likely that the exclusion of ultra-violet rays was due to the difficulty in producing living media transparent to these rays, and that the non-utilisation of the near infra-red rays (which do in point of fact reach the retina) is due to the absence of a suitable retinal pigment which will absorb and will undergo photo-chemical change as a result of the incidence of these rays. Now I will refer to the resolving power of the eye. Mr. Smith suggests that there are ocular powers of discrimination that are more refined than the coarser features of retinal structure—the rods and cones—would lead us to expect. This is a question to which I have given a great deal of thought, and the conclusion to which I have come is that if we are prepared to grant that the cones in some way are able to register increases or decreases of intensity of, roughly, 10 per cent. (which we should not find difficult, since we can prove by experiment that moderate sized areas of retina can register increases or decreases of intensity of less than 1 per cent.), then we can adequately account for all the finer ocular powers. We can explain on this basis how, for example, the eye that can just recognise the 'twoness' of two point sources when they subtend at the eye an angle of not much less than 50″, can see a black line when its width subtends at the eye an angle of only 3·1″; with cones, moreover, the diameters of which subtend at the nodal point of the eye an angle of about 44″.

Lastly, with regard to Mr. Smith's letter in NATURE of Feb. 25 on "An Optical Paradox," would not any method of measurement depending on comparisons break down if put to a similar test? I would propose that the word 'optical' be omitted lest it suggest to the uninitiated the idea that the paradox only applies to visual measurements.

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The Density necessary to Produce the Nebular Spectrum.

IN a recent letter to NATURE (Jan. 7, p. 12) C. T. Elvey attempts to calculate the density, ρ, of the expanding gaseous shell of a nova at the moment when the nebular lines first appear. It is easily shown that $\rho = \rho_0 r_0^2/v^2t^2$ where ρ_0 is the original density of the shell when coincident with the stellar atmosphere, r_0 its original radius, v its velocity of expansion, and t the time elapsing between the outburst and the appearance of the nebular lines. The above equation involves the additional and somewhat questionable assumption that the thickness of the expanding shell does not change. Elvey takes v from velocity displacements on nova spectrograms. For Nova Aquilæ 3 this is about 1700 km./sec. and $t = 19$ days. Hence he finds,

$$\rho = 12{\cdot}7 \times 10^{-20} \rho_0 r_0^2 \text{ gm./c.c.}$$

(r_0 in km.). Since figures for eight additional novæ give coefficients for $\rho_0 r_0^2$ of the same order of magnitude, Elvey concludes that "the novæ originate from stars of similar physical conditions and that there is a limiting density above which the conditions are unfavourable for the production of the nebular spectrum."

It is my opinion that the experimental evidence is far too meagre to draw any further conclusions than those just quoted. Up to this point the assumption of constant thickness for the gaseous shell has been unnecessary. It would have been sufficient merely to suppose that it varies at the same rate for all novæ.

I think that Elvey's attempt to calculate an absolute value for ρ is not justified. Taking $\rho_0 = 10^{-9}$ gm./c.c. (corresponding to a pressure of some 10^{-4} atmosphere) and $r_0 = 6 \times 10^5$ km., he finds for the mean density, critical to the nebular lines, $1{\cdot}8 \times 10^{-17}$ gm./c.c. Elvey's use of $\rho_0 = 10^{-9}$ gm./c.c. is equivalent to the assumption that the phenomenon of the nova originates in that particular layer of the star's atmosphere. There is no *a priori* reason why a value of ρ_0 of 10^4 or more times the above should not have been employed in the calculation; the expanding shell would still be essentially atmospheric. It is true that the value, 10^{-4} atmosphere, is the pressure at which the stellar atmosphere is becoming opaque to visible radiation and hence marks a more or less definite layer, but this fact does not demand that all atmospheric phenomena originate there.

Bowen (*Publ. A. S. P.*, **39,** 295; 1927) has identified the nebular lines with metastable transitions in the atoms of singly, doubly, and triply ionised oxygen. These transitions, at one time spoken of as 'forbidden,' are presumably allowed in the very tenuous nebulæ, where the time between atomic collisions is extremely long. Ordinarily the time spent by an electron in an excited level is only about 10^{-8} sec. The time it would remain in a metastable state is much greater—so long, in fact, that before the natural atomic transition can occur, a collision with another atom (impact of the second kind) will knock the electron from its metastable orbit. Hence the 'forbidden' electron jumps should occur only when the time between collisions is longer than the excited time, that is, in gas of low density. A determination of the critical density, at which the metastable transitions begin to occur, would furnish valuable information regarding their life-time, but Elvey's value for it—a hundred seconds—is almost valueless for the reasons stated above.

The only fact that recommends Elvey's computed densities appears to be the close correspondence between them and the calculated values[1] for the planetary nebulæ (5×10^{-18} gm./c.c.). If the latter be assumed approximately true for the novæ, the more correct interpretation of Elvey's work is that the phenomenon of a nova originates in the star's atmosphere at a layer where the pressure is 10^{-4} atmosphere or less. When more data regarding the life histories of novæ have been obtained, and when the physicists have determined the metastable life of the atoms under consideration, these facts may be used to solve the nova problem. One should not neglect, however, to take into account photo-electric ionisation of the gas by the intense stellar radiation. Recent work by Bowen (*Astrophys. Jour.*, **67,** 1; 1928) shows that conclusions derived without considering this influence may be devoid of physical meaning.

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Mar. 14.

[1] Russell, Dugan, and Stewart, "Astrophysics and Stellar Astronomy," p. 835.