Correcting for the difference in the critical potentials by means of Rosseland's formula we obtain

$$L_{\alpha_1} : L_{\alpha_2} : L_{\beta_1} = 100 : 10 : 43$$

as the intensity ratio to be expected for an applied voltage very large compared with the difference of the critical potentials.

It is of interest to compare this result with the intensity relations of optical spectra. For the lines *np-md* of the alkalies, which have a formal analogy with the above X-ray lines, one would expect $9 : 1 : 5$ [1] (100 : 11 : 55). Here the measurements in Utrecht have given results agreeing with the theory. Although there is a close resemblance, it seems that our values differ from this ratio by more than the experimental error which we estimate at 5 per cent. Such a deviation is, however, not unexpected, since the theoretical result is only strictly valid so long as the frequency difference of the lines is very small compared with their frequencies. While this condition is fulfilled in the case of the doublets *np-md* of the alkalies, we meet in our case with a frequency difference of 15 per cent. which can scarcely be considered to be small.

We hope later to publish an account of the experimental details and of further measurements.

Y. NISHINA.
B. B. RAY.
Universitetets Institut for teoretisk Fysik,
Copenhagen.

The Convection of Light by Moving Matter.

IN the paragraph under the above title, in NATURE of December 26, p. 948, a new question is put forward quite distinct from the discussion of Einstein's theory, dealt with in my unpublished letter of November 5, to which it refers.

The writer of the note, having noticed that I do not belong to that multitude who blindly follow Einstein, seems to conclude that I am of the Einstein-antagonist party, all of whom aim more or less to return to older ideas. Even a scientist of well-deserved universal repute, such as Prof. Lenard in Berlin, in his polemic and anti-Einstein pamphlets, which have been reprinted many times, only proposes older theories in a somewhat modified form. But is that generally existing conformity a valid excuse to attribute to me what as a matter of fact is only the impression of the writer of the note ?

It obliges me to state emphatically that there is not the least essential connexion between the writings of all those Einstein-antagonists and my papers. I challenge the writer to point out in my papers (*C.R.*, vol. 175, 1922, p. 574, and *Phil. Mag.*, Ser. 6, vol. 49, 1925, p. 579) a single word which may be interpreted as my advocating a return to older electromagnetic theory.

According to the paragraph, my " deduction is not entirely free from ambiguity, in so far as equation (11) in his earlier and more fundamental paper leads at once to the expression $\mu'_w = \mu + A(n' - n)$. . . . For A, which is a constant of integration, M. Menges puts $(\mu - 1)/n + d\mu/dn'$."

As to this, I can only say that neither the reasoning nor the formulæ is to be found in my papers.

My equation is :

$$\frac{d\mu'_w}{dn'} = \frac{\mu'_w - \mu}{n' - n}.$$

This is not given in NATURE. It is immediately obvious that the expression presented in NATURE as the solution of my equation is incorrect. For it may be written :

$$A = \frac{\mu'_w - \mu}{n' - n},$$

which, as a solution of my differential equation, is absurd.

Within the scope of a letter, it is quite impossible to explain my new formulæ fully. I must refer the reader to my books " Nouvelles vues Faraday-Maxwelliennes " and its " Supplément " (Gauthier-Villars and Co., Paris, 1924). There he may see that my mathematical deduction is quite free from ambiguity and undoubtedly correct, and that it leads to the true solution of the question. He will find also that it is by no means by returning to, but, on the contrary, by radical departing from older electromagnetic theory, that my new results are obtained. My new formulæ and new insight in electromagnetism, confirmed by experiment, then leads to the result that Einstein's theory is inadmissible.

CHARLES L. R. E. MENGES.
The Hague,
December 30.

M. MENGES seems to have misunderstood the drift of my notice, which was intended as a *critical review*, not an *abstract*, of his two published papers, without any direct reference to his unpublished covering letter at all. I am not conscious of having attributed any views respecting Einstein's theories, either for or against, to M. Menges, whilst the references to Newtonian principles and the older electromagnetic theory followed naturally from the *Phil. Mag.* paper and its references to the books of Jeans and H. A. Lorentz. I added the reference to v. Laue's paper for the sake of completeness, for it shows that the experiments of Fizeau and Zeeman do not lead to the result that Einstein's theory is inadmissible, as M. Menges states in his letter.

The paragraph of which M. Menges complains is a criticism of his solution (12), not a reproduction of his method of deriving it from his equation (11), for he gives no details in his papers. If we write $n' - n = x$, $\mu'_w - \mu = y$, n and μ being constant parameters, (11) reduces to $dy/dx = y/x$, which is the familiar differential equation of a plane pencil of straight lines through the origin. Its *general* integral is $y = Ax$, where A is the constant of integration ; I fail to see the absurdity of this well-known solution, for its verification is immediate. The point of my criticism is that M. Menges' solution (12) is only a *particular* integral, obtainable of course by choosing a particular value of A ; in the papers specifically referred to, M. Menges gives no sufficient reason why this particular integral should be selected rather than any other. It may well be that good reasons are given in his book, but I have not had access to it. My wording no doubt is liable to misconstruction : it would have been better to write : " We can obtain M. Menges' solution (12) by putting for A the particular value $(\mu - 1)/n + d\mu/dn'$."

THE WRITER OF THE NOTE.

Mullet as an Enemy of the Oyster.

DURING the past summer large numbers of oysters carrying larvæ were examined in the Plymouth laboratory, and in order not to waste the larvæ, batches were thrown as food into an aquarium tank containing anemones, sea-cucumbers, small fishes, and other smaller marine animals. After throwing the larvæ into the tank, the animals being fed were

[1] D. Coster and S. Goudsmit, *Naturwiss.*, 13, p. 11, 1925; A. Sommerfeld, *Ann. d. Phys.*, 76, p. 284, 1925.