is also the case with the N_2 line from solid nitrogen. This fact, which has also been confirmed by McLennan, was taken by him to be an argument against my theory. We see, however, that the spectrogram obtained for the second green line, on the contrary, in this respect has confirmed my view with regard to the origin of the auroral spectrum.

On account of the small dispersion and the broad slit we cannot at present find the wave-length of the

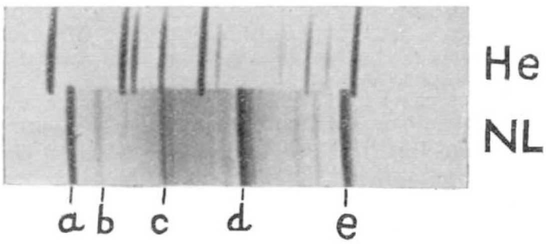

FIG. 1.

various components of the second green line; we have only been able to measure the wave-length of the maximum and limits of the band obtained by our plate. The limits are λλ5220-5269, and for the maximum we find λ5238. On account of the small dispersion, errors of a few angstroms are not excluded.

Comparing this result with those obtained for the luminescence from solid nitrogen, it is of interest to notice that when small quantities of solid nitrogen are condensed in a solid system of inert gases, we find one of the components of N_2, which in certain cases is dominant, to have wave-lengths of λλ5236-5239, *which within the limits of experimental errors correspond to the wave-length found for the maximum of the second green auroral line.*

L. VEGARD.

Physical Institute, Oslo, Feb. 11.

Biological Fact and Theory.

PROF. JOHNSTONE'S letter in NATURE of Feb. 26 suggests an analogy. I happen for my sins to be gifted with very poor mathematical powers. Like him, also, on this account I "feel that I may be missing something that will help in an understanding" of the scientific problems with which mathematics are concerned. But I do *not* therefore attempt to belittle mathematical physics as Prof. Johnstone attempts to belittle the results of Mendelism.

He asks "what are the fundamentals of genetics?" The fundamentals of genetics to date are, I take it, the laws of segregation, independent assortment, and linkage; the proof that the chromosomes carry the genes, and that the genes are arranged in linear order; the genetical results of heteroploidy and chromosome aberrations; the individuality of the chromosomes (as, of course, complicated by crossing-over); the normal chromosomal determination of sex; the theory of genic balance; the facts concerning multiple allelomorphs, and multiple, modifying, and lethal factors; the new insight provided by neo-Mendelian methods into species-crosses and into the effects of inbreeding and cross-breeding; the origin of certain variations by point-mutation, chromosome-mutation, genome-change, deficiency, duplication, balanced lethals, and abnormal crossing-over; the demonstration that Mendelism and biometrics are not opposed; the fact that no development is possible at all in the absence of at least one haploid set of chromosomes; the demonstration that genes often determine the *rate* of definite developmental processes. There are doubtless other points which I have forgotten in this hasty survey; but it is absurd to imply that this is not a very considerable achievement and an "ample foundation" for future work.

Prof. Johnstone and Prof. Walker both seem to think that the sum of the genes *cannot* be responsible for the development of the "organism as a whole" or large characters such as the head. But has Liverpool never heard of Boveri's experiments on disperm sea-urchin eggs, published exactly twenty years ago? It may be at present impossible to understand how the sum of the genes is responsible for the development of the organism as a whole, but Boveri made it reasonably certain that it is actually the case.

However, the work of the experimental embryologists, of Child, and of Goldschmidt, is at last beginning to give us an insight into the *how* of this problem —but only by building on the Mendelian foundation which Prof. Johnstone scorns. I would refer critics to Goldschmidt's new book ("Physiologische Theorie der Vererbung") and to a brief critical summary of my own (NATURE, Feb. 23, 1924) as showing how the obvious difficulties of the situation may perhaps be surmounted. I hope to summarise some of the recent work on the relations of hereditary constitution to developmental physiology in an article in NATURE in the near future. Meanwhile I would merely ask Prof. Johnstone whether he, like Prof. Noël Paton, wants to leave on one side all the results of Mendelian work in our attack upon the problem of heredity and its relation to the development of the organism as a whole? That is the only meaning I can attach to his concluding sentences; and it appears to me to be a counsel of despair.

Prof. Walker says (NATURE, Jan. 29, p. 161) that Dobell 'proved' that hereditary characters could not be controlled by chromosomes in certain Protozoa. The main reason advanced by Dobell concerned sex, and was that the Protozoa in question were haploid during all their sexually differentiated phase. If Prof. Walker had been better acquainted with genetical literature he would have remembered that almost simultaneously with Dobell's 'proof,' Wettstein was demonstrating experimentally, and conclusively, the control of sex by chromosomes in another group of organisms in which sex is displayed in the haploid phase—the mosses. Dobell's *a priori* arguments were never even theoretically valid, and long since fell to the ground on confrontation with actual fact.

As to Tornier:—If Prof. MacBride chooses to believe that experiments on developmental physiology, unaccompanied by breeding, have any direct bearing on heredity, I fear I cannot argue with him; to my mind, Tornier's work has just as much (or as little) bearing on the origin of mutations as has that of Driesch or Jenkinson or Child. For the information of readers of NATURE, however, it should be recorded that Berndt (*Zts. Ind. Abst. Vererb.*, 36, 1925) has repeated Tornier's goldfish work, and has also bred goldfish, on a large scale, and fails to verify either Tornier's facts or conclusions save in a few negligible details.

JULIAN S. HUXLEY.

King's College,
Strand, London, W.C.2, Feb. 22.

The Radcliffe Science Library, Oxford.

THE two paragraphs on pages 247-8 of NATURE of Feb. 12, 1927, in reference to the proposed transfer of the Radcliffe Science Library at Oxford to the University of Oxford, have been framed in such a way as to convey a false impression of the facts of the proposed arrangement. The Radcliffe Trustees would therefore be obliged if the following corrections could be inserted: