

the subject either tedious or involved. It is, on the contrary, an eminently readable and attractive volume. It is divided into chapters describing the preparation and reactions of the diazo-compounds, their derivatives, and their constitution. In an appendix a short account is given of the author's new theory of their structure. This is not the place to enter upon a discussion of the subject, but a strong case is made out for the new view, which should stimulate fresh experimental work of an interesting character.

J. B. C.

Handbuch der Physik. By Dr. A. Winkelmann. Second edition. Fifth volume, second part: Elektrizität und Magnetismus, II. Pp. xiv+971; illustrated. (Leipzig: J. A. Barth, 1908.) Price 16 marks.

THE present portion of this encyclopædic treatise consists of electrodynamics and induction, by K. Waitz; absolute measurements of magnetic and electric quantities, by A. Oberbeck; technical applications of induction, by Th. des Coudres; telephony, by L. Reilstab; and the theory of electric phenomena, by L. Graetz. Important though every one of these sections is, it is doubtless to the last that the reader will turn first on account of the great developments of theory during the last decade; and especially will he turn to the chapters on electrons and on the electromagnetic equations for bodies in motion. We have stated in reviewing the previous parts that the treatise is not intended for continuous reading. It is essentially an encyclopædia, a book of reference. But it is the treatise *per excellence* to which reference should be made by all those who wish to know what has been done and what theories have been enunciated in the domain of physics.

LETTERS TO THE EDITOR.

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The Condensation of Helium.

IN NATURE of March 12 I have found a note referring to my experiments on the expansion of helium, made in consequence of my determinations on the isothermals of helium, at -252° C. and -259° C., which yielded nearly -5° K. for the critical temperature of helium.

The prosecution of the experiments has shown that what I observed in expanding the gas was not the evaporation of solid helium, but solution phenomena of solid hydrogen in gaseous helium. I have communicated to the Amsterdam Academy a note on my experiments, which at the moment leave the condensation of helium a yet undecided question.

Of course, I have written the details to Sir James Dewar, and I hoped to do so to you to-day, but by pressing duties I cannot do it before to-night, and you will probably go to press before that letter reaches you. So I beg to be allowed to send you first this short notice.

Leyden, April 14.

H. KAMERLINGH ONNES.

Mendelian Characters among Shorthorns.

PROF. WILSON is welcome to any satisfaction he can obtain out of the Mendelian interpretation he gives to our statistics of coat-colour in Shorthorns. As a matter of fact, some readers may consider that the same interpretation is given with greater numerical accuracy on pp. 440-4 of our original memoir (*Biometrika*, vol. iv.). For example, we give 656 crosses of roan and *whole* red alone, resulting in 243 whole reds, eighty-five red and whites, and *four whites*. The remainder consists of 324 roans. Of this we say "the close approximation to the Mendelian number of the roans is noteworthy, but the appearance of 4(WW) is again impossible unless some of

the reds are to be treated as heterozygous." Why does Prof. Wilson reduce our *total* red roan crosses to 456, and leave out the inconvenient four whites? Why does he give only three whites crossed by white as giving three whites, while we dealt with ninety-one such crosses giving eighty-six whites, *four roans*, and *one red*? Why, further, does he leave out the whole of our Table I. on p. 441? We followed up the white cattle pedigrees, writing to the breeders about special cases, and finding in the great bulk of instances the crosses and colours stated in the Herd-book confirmed. If it be asserted that the colours given in the Herd-book are incorrect, or, still more vitally, that the confirmation of those facts given to us by reputable breeders are misstatements, then the only conclusion is that Mendelism cannot be discussed on the basis of the Shorthorn data. That is a logical position; it is not, however, logical to use the data, and escape inconvenient facts by the statement that they are due to errors or to deception, or to omission to enter calves (which we found on inquiry among English large breeders to be not so frequent as has been asserted).

The facts stated by us on p. 442 of our paper, which cannot at present be made fully public, show that there are probably latent colour determinants in white cattle which can be made patent if two individuals of pure white coat, but one of mixed race, be crossed. Recent experiments seem to show that the actual amount of pigmentation in the coat is an inherited character in mammals; no explanation, Mendelian or other, which overlooks the difference between whole and parti-coloured animals can in the present state of our knowledge be considered satisfactory. As it is, the parti-coloured cattle are being bred out, and the possibility of this shows that red and parti-colour are *not* interchangeable. This point is illustrated again by the fact that in whole red crossings about 3 per cent. of roans appear, but in parti-colour crossings about 8 per cent. of roans occur. There is at the bottom of this, I believe, a physiological fact, and I am not prepared to overlook it by saying, with Prof. Wilson, that 438 red by red matings gave twenty-five roans, which are to be put down as due to errors and misstatements because they do not fit his view of the case.

Within broad lines Shorthorns do show segregation in the results of the crossings; this is really the great idea embodied in the Mendelian view. It may be possible on a determinantal theory to offer a reasonable account of the Shorthorn data; such a theory would certainly follow recent Mendelian work in discriminating between whole and parti-colour coats. On the other hand, it is a possible attitude to discard the data as untrustworthy; it is not logical, I hold, to discard just as much as you please of the data and no more in order to make it fit the simpler Mendelian ratios.

KARL PEARSON.

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THE validity of Prof. Pearson's criticism of the view that Shorthorn cattle are Mendelians turns upon the accuracy of my statements (a) that in the Herd-book roans are sometimes registered as reds and reds as roans; (b) that many white calves are not registered at all; and (c) that coloured calves are sometimes substituted for white ones. Unfortunately, these statements are all true, although the last one only need cause very serious regret. The following may make the position clearer.

A short time ago a very distinguished breeder was regretting the substitution of coloured calves and the difficulty of proving cases of substitution to be such. This breeder persistently uses white bulls in order to get roan calves from his red cows, and, in proof that red calves entered in the Herd-book as the progeny of reds and whites are probably substitutions, he mentioned that in all his experience he had got only one red calf from his white bulls and his red cows.

That red calf—a bull—came to Ireland, and is still alive. To the great disappointment of his owner, he has bred several white calves from roan cows.

First, by being the son of a white bull and a red cow, and, next, by breeding white calves from roan cows, this red bull disproves the theory that Shorthorns are Mendelians; but I had the privilege of seeing him this afternoon, and he is not red, *he is roan*. He is, however,