more time to physiological chemistry, important as the subject is, than his brother in this country.

The book labours from the disadvantage under which all books which see many editions labour; no one is more acutely conscious of this than the present reviewer; it is so easy to add, so heart-breaking to excise. At the same time, Prof. Hawk has made a praiseworthy attempt to cut down the multiplicity of methods which assail him. For example, the only methods given for urea estimation are those based on the use of urease, and Van Slyke's procedure is the only one described for the determination of acetone bodies. The same ruthless use of the pruning-knife in relation to other materials (*e.g.* sugar) would add to the practical usefulness of a most admirable book.

It would be easy to criticise details; for example, the book starts with a study of the most difficult of all chemical problems, namely, enzymes, so that it is scarcely one to recommend to the beginner; then, too, it is not always up to date; for instance, we are told that English physiologists speak of metaproteins as infraproteins, a term they dropped many years ago; the account of muscle physiology does not appraise the work of Hopkins and Fletcher on lactic acid (probably the key to the whole situation) at its full value. But where so much is good, picking holes is neither profitable nor kind.

W. D. H.

Joseph Priestley. By D. H. Peacock. (Pioneers of Progress. Men of Science.) Pp. 63. (London: Society for Promoting Christian Knowledge; New York: The Macmillan Co., 1919.) Price 2*s.* net.

THE story of Priestley's life has been told and retold; but to the man of science it is always an attractive story, and to the general reader its appeal is perhaps scarcely less strong. To the chemist there is a never-failing interest in reading how this village minister, theological controversialist, and political reformer, who had no special scientific training and no particular facilities for experimentation, nevertheless was drawn to chemical studies, and acquired a just and lasting fame by his brilliant discoveries.

Priestley's mind was one of rare alertness, and if he missed many things through the weakness of his theoretical deductions, a remark of his biographer helps us to understand pretty clearly why this was so. "Chemistry was really little more than a hobby to him; theology was his life work. . . . Priestley was Priestley, not Cavendish."

Of this notable "pioneer" we get a good picture in Mr. Peacock's pages. There are only about sixty of these, but they suffice to tell pleasantly, even if briefly, of Priestley's early struggles, his prolific pugnacity in pamphleteering, his delight in experiments, his serenity under adversity, his pathetic exile, and his peaceful passing.

C. SIMMONDS.

LETTERS TO THE EDITOR.

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A Darwinian Statement of the Mendelian Theory.

So far as the present writer knows, no public notice has yet been given to a series of statements by Darwin in his "Animals and Plants under Domestication" that constitute virtually a statement of the Mendelian theory of the distribution and recombination of factors in hybrid offspring. Darwin's idea of dissociation is, of course, founded on Naudin's conception of disjunction; but the remainder of his theory is as original as Mendel's, except that it is purely speculative instead of being derived directly from experimental data. It is worked out, as a matter of fact, by means of his theory of pangenesis.

Darwin begins as follows:—"Another form of reversion is far commoner, indeed is almost universal with the offspring from a cross, namely, to the characters proper to either pure parent-form. As a general rule, crossed offspring in the first generation are nearly intermediate between their parents, but the grandchildren and succeeding generations continually revert, in a greater or lesser degree, to one or both of their progenitors" (vol. ii., p. 22).

He then quotes Naudin's view that "a hybrid is a living mosaic-work, in which the eye cannot distinguish the discordant elements, so completely are they intermingled. We can hardly doubt that, in a certain sense, this is true, as when we behold in a hybrid the elements of both species segregating themselves into segments in the same flower or fruit by a process of self-attraction or self-affinity, this segregation taking place either by seminal or bud-propagation" (p. 23).

Darwin goes on to comment on Naudin's view that the segregation of the male and female elements would be most likely to occur in the reproductive cells, since in this way their reunion through the fusion of pollen-grains and ovules would explain the phenomenon of reversion.

He then says:—

"If . . . *pollen which included the elements of one species happened to unite with ovules including the elements of the other species, the intermediate or hybrid state would still be retained, and there would be no reversion*" (p. 23).

Here is a statement of a theory of heterozygosis which, although not complete in exactly Mendelian form, is, so far as the writer knows, the first before the appearance of Mendel's paper. Darwin's more elaborate explanation comes later. He continues:—

"But it would, I suspect, be more correct to say that the elements of both parent-species exist in every hybrid in a double state, namely, blended together and completely separate" (p. 23).

Finally, in his chapter on pangenesis, Darwin approaches the theory of hybrids in thorough-going fashion, driving his pangenesis theory to its legitimate conclusions. By this theory, as is well known, it was assumed that the character-units existed in the somatic cells in the form of physical entities, however small, known as "gemmules." These, passing into the reproductive cells, conveyed thither the sum-total of the inheritance.

Darwin then approaches the subject of the theory of hybrids as follows:—

"The tendency to reversion is often induced by a change of conditions, and in the plainest manner by crossing. Crossed forms of the first generation are

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