Our Fancy Pigeons. By George Ure. Cheap and Enlarged Edition. (London: Elliot Stock, 1890.)

THE title of this book does not give quite an accurate idea of the contents, for in the first part there is a good deal about fishing and things in general, and the third is a collection of "rambling ornithological notes." In the second part, however, the author deals systematically with the pouter and other "high-class breeds," and offers some remarks on minor varieties of fancy pigeons. Mr. Ure is a lively writer, and his facts and opinions are presented as the results of long-continued personal study and experiment.

Alexis and His Flowers. By Beatrix F. Cresswell. (London: T. Fisher Unwin, 1891.)

THIS pretty volume is intended for boys and girls, and, as it is brightly written, ought to be read by them with pleasure. It contains much quaint and interesting "flower-lore."

LETTERS TO THE EDITOR.

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Dr. Romanes on Physiological Selection.

IN his two latest articles dealing with this subject, Dr. Romanes has made certain statements as to my position in regard to it which call for a brief notice on my part.

In his original paper, and in the summary of it published in NATURE, Dr. Romanes adduced variations in regard to fertility and sterility as the fundamental fact in physiological selection. A few quotations will show this. He says: "It becomes almost impossible to doubt that the primary specific distinction (meaning sterility) is, as a general rule, the primordial distinction" (NATURE, vol. xxxiv. p. 339). Again, he enforces this as against Darwin's view that sterility was a consequence or concomitant of other differences, as follows: "My theory, on the other hand, inverts this order, and supposes the primary distinction to be likewise (in most cases) the primordial distinction" (*l.c.*, p. 363). This is very clear, but to show that he limited the term "physiological selection" to the results supposed to arise from this phenomenon, we have his reply to Mr. Galton, who urged a fact also dwelt upon by Darwin—the psychological disinclination to mate between many varieties—as an important factor in the differentiation of species: "Now I have fully recognized this principle as one amongst several others which is accessory to, although independent of, physiological selection" (*l.c.*, p. 407). A little further on he again states his fundamental fact thus: "If my theory is true, it must follow, as Mr. Galton says, that such unions would be more or less sterile, and, as this sterility is itself the only variation which my theory supposes to have arisen *in the first instance, ex hypothesi* we can have no means of observing whether or not the individuals which present this variation 'consort with outsiders,' or with those individuals which do not present it" (*l.c.*, p. 407). As if to leave no possible doubt as to the special point of his new theory, he again enforces it in the following passage: "And forasmuch as the sexual separation arises only by way of a variation locally affecting the reproductive system, when the variation is first sexually separated, it will in all other respects resemble its parent stock, and so be able to compete with it on equal terms" (*l.c.*, p. 408).

Now surely all this makes it absolutely clear that Dr. Romanes's theory of physiological selection, so far as it had any originality, was founded on the supposition of sterility-variation alone, arising in an otherwise undifferentiated species; and he claimed that such variations "cannot escape the preserving agency of physiological selection," and that "physiological selection must be quite as vigilant as natural selection, and it seizes upon the comparatively unuseful variation of sterility with even more certainty than natural selection can seize upon any useful variations" (*l.c.*, p. 364).

These last statements, by the truth of which alone the use of the term "selection" can be justified, I showed by two carefully considered cases to be absolutely unfounded, and the exact opposite of what must really occur (*l.c.*, p. 467; and "Darwinism," p. 182). Having thus proved that "physiological *selection*," in the only form claimed by Dr. Romanes as original, does not exist, and that the only modes by which degrees of sterility between distinct species can arise are those discussed or suggested by Darwin himself, with the addition of the possible action of natural selection in increasing incipient sterility between slightly differentiated forms, will it be believed that I am accused of having appropriated the theory of physiological selection without acknowledgment! In the *Nineteenth Century* (May 1890, p. 831), Dr. Romanes says of me: "He presents an alternative theory to explain the same class of facts. Yet this theory is, purely and simply, without any modification whatsoever, a restatement of the first principles of physiological selection, as these were originally stated by myself." And now, in the October issue of an American magazine, *The Monist*, he has an article entitled "Mr. A. R. Wallace on Physiological Selection," in which the original main point, of sterility-variations alone leading to and constituting "physiological selection," is almost entirely ignored, and the various modes by which isolation is produced between incipient species or in which infertility arises in correlation with other divergent characters, are all claimed as forming part of the theory of physiological selection. He quotes from "Darwinism" my exposition of the effects of partial infertility arising between "two varieties in process of adaptation to somewhat different modes of life within the same area," to show "how unequivocal and complete is Mr. Wallace's adoption of our theory" (*The Monist*, No. 1, p. 11). "Our" refers to Mr. Gulick, who is taken into partnership by Dr. Romanes. And again he speaks of "the peculiar position to which he has eventually gravitated with reference to my views—professing hostility on the one hand, while reproducing them as original on the other" (*l.c.*, p. 19).

I have here confined myself to showing, by Dr. Romanes's own repeated and emphatic statements, what was the essential and original theory to which he gave the name of "physiological selection." The whole of this special doctrine I have argued against as unsound, because, on close examination, it proves to be quite inadequate to produce any such effects as are claimed for it. Whether I was right or wrong in doing so, I did, as a matter of fact, and do still, wholly reject this fundamental and essential part of the theory—the only part which had even a *primâ facie* claim to originality. I also totally reject the two subsidiary doctrines on which Dr. Romanes lays great stress as adjuncts of his theory—that of the inutility of a large proportion of specific characters, and that of the power of isolation alone "without the aid of natural selection" to produce new species; while, so far as I know, the only points in which I agree with him are those in which we both make use of Darwin's facts and adopt Darwin's explanation of them. Yet, notwithstanding this rejection of all that is special in his teachings, Dr. Romanes has the hardihood to assert that I claim them as my own; that I merely restate his theory "purely and simply, without any modification whatsoever"; and that my adoption of his theory "is unequivocal and complete."

I leave it to others to characterize these extraordinary statements in the terms that fitly apply to them.

ALFRED R. WALLACE.

Attractive Characters in Fungi.

I NOTE in your issue of November 6 (p. 9), Mr. Straton mentions the fact of the common mushroom spores being unproductive until they have passed through an animal host, naming horse, sheep, and oxen, but it appears to me it must be rendered equally fertile after passing through the larvæ of beetles, flies, &c., else how could nurserymen supply spawn with mycelium ready for generation? It is possible, therefore, that though larger animals act very often as hosts to mushroom spores, insects are mainly responsible for their reproduction. The soft spongy nature presents but little resistance to the ovipositor, and most mushrooms if examined in a state of decomposition will be found perforated by maggots, the larvæ of Diptera and Coleoptera.

It is possible that a sustained high temperature is necessary to the first stage of development in fungi, which is admirably attained in the living host, but it is probably immaterial whether the mycelium is developed on the excreta of mammal or insect. Heat is evidently a great factor even in the second stage of germination, as the so-called "spawn" will remain dormant for