

by your distinguished correspondent was familiar to Mr. Darwin, and that it was dealt with by him in the sixth chapter of the "Origin," in what seemed to me to be the only way which was then, or is now, possible. We should learn to understand it, he said, by observing "by what graduated steps" [electrical organs] "have been developed in each separate group of fishes." By this I understand him to have meant that what we require to know is, under what conditions the development of electrical organs has actually taken place.

On morphological grounds, we know that a striped muscular fibre taken together with its nerve, and the electrical disk of the organ of the skate taken together with its nerve, are homologous structures—that is, that they are made up of corresponding parts, and have corresponding places in the normal order of development; so that they are in collateral, not in sequential, relation to each other. In other words, both spring from the same origin, not one from the other; and the development of one is quite as *normal* as of the other. An electrical organ is no more an abnormal muscle, than a muscle a misdeveloped electrical organ.

In accordance with Mr. Darwin's teaching, external conditions, whether antecedent or collateral, influence development only in accordance with morphological laws—that is, with the normal order of development. In the present instance we have some knowledge of the order, but the conditions are unknown; and what we have to do is to ascertain what conditions of existence have given predominance to one order rather than to the other, so as, in certain cases, to determine the development of apparatus for producing electrical discharges in place of apparatus for doing mechanical work.

This is the problem, and it will take a long time to investigate it. We know a great deal more now than Mr. Darwin did twenty-five years ago about the structure, development, and mode of working of the electrical organ, but scarcely more than he did about the "why" of its existence in such animals as the skate. Nor shall we be able to give any better account of it until time and opportunity have been afforded for the examination and comparison of a much larger number of instances than are at present accessible to us.

I need only add a word as to his Grace's suggestion that the electrical organ of the skate may be regarded as a "prophetic germ." I would observe that, although in some species of skate the organ is imperfect, it shows no sign of incompleteness in others, and therefore cannot be properly designated a germ. As to the organ being prophetic, I am not sure that I understand what the word means. If the prophecy is such as might encourage the present race of skates to hope to be provided at some future period with more efficient apparatus, I am afraid that any such expectation on their part would be illusory.

Oxford, August 15.

J. BURDON-SANDERSON.

ON the part of, I believe, a very large class of unprofessional students of science and theology, I should like to express the profound dissatisfaction, not unmingled with irritation, with which we have read the Duke of Argyll's recent contributions to the subject of evolution. The complete collapse of the grave charges made against the advocates of evolution in the article entitled "A Great Lesson" in the September (1887) number of the *Nineteenth Century*, is too well known to need comment.

The letter on "Functionless Organs" affords another instance of the illogical and dogmatic style with which we are too familiar. Passing over any notice of the absolute inconceivability of any *cause* for the development of "prophetic structures," the Duke of Argyll once more repeats the exploded notion that "the element of fortuity is inseparable from the idea of natural selection," whereas, as has been proved over and over again, the ideas of fortuity and of evolution, of which process natural selection is so integral a part, are absolutely incompatible. But perhaps the climax is reached in the following quotation: "Hitherto I have never yet met with a case in which an expert interprets functionless organs as structures on the way to use." Having at last found a solitary case which, it is thought, by one expert, may be interpreted against the Darwinian conception of evolution, he immediately jumps to the conclusion that "everywhere, in reasoning and observation, it is breaking down."

*Propos* of Mr. J. G. Hurst's pertinent queries on p. 364 of your last issue, it may be well to recall the Duke of Argyll's

dictum given in the "Reign of Law," *i.e.* that in man's structure "there is no aborted member. Every part is put to its highest use."

SAMUEL F. WILSON.

Warsop, August 18.

#### Lamarckism *versus* Darwinism.

IT is to be regretted that Dr. Romanes has not written anything which can be considered as a reply to my letter. Although Prof. Weismann's essays, to which I referred, are certainly "two of the most notorious essays in the recent literature of Darwinism," it is nevertheless equally certain that a large and important part of their contents is devoted to the consideration of the causes of variation. This being the case, I may safely leave the evidence in support of the statement in my first letter to anyone who will take the trouble to read p. 841 of the June number of the *Contemporary Review*. As it is probable that many people have already read the article in question, and that others may be induced to do so as a result of this correspondence, I think that on this account it may be worth while for Dr. Romanes to notice the criticism, and if possible to show that his remark about Prof. Weismann is intended to bear some other than its obvious meaning.

I need hardly make any further reference to the second and third paragraphs of Dr. Romanes's letter, for I have already explained my position in my first letter. I need only reassert that I was in no way influenced by Dr. Romanes's remarks or opinions about myself; nor am I concerned to allude to the personal references contained in his letter, except to express regret if anything in the form as apart from the substance of my first letter should have caused the annoyance which Dr. Romanes takes no pains to conceal.

In conclusion, it may be worth while to draw attention to the curious coincidence which brings into the same number of NATURE a letter from Prof. E. Ray Lankester, containing an expression of opinion diametrically opposed to that of Dr. Romanes upon the interesting question of Lamarck *versus* Darwin.

EDWARD B. POULTON.

Oxford, August 17.

WITH reference to the recent revival of what may be considered as "pure" Lamarckism, it appears to me of importance that those who have followed the course of biological work and thought in this direction should at the present juncture declare their views with respect to the interpretation of such results as those obtained by Mr. Poulton, and referred to by Dr. Romanes in his letter of August 9 (p. 364). I am glad of the present opportunity of discussing this matter, because Mr. Poulton's work is to a large extent an expansion and experimental confirmation of views to which I gave expression in a paper published in 1873 (*Proc. Zool. Soc.*, p. 159). I have no desire to enter into the personal question as to whether Dr. Romanes has or has not made himself acquainted with Weismann's essays, but I must express my disappointment that he has not given us a more explicit statement concerning the precise manner in which he interprets the experiments in the Lamarckian sense. For my own part I may add that I have had opportunities of witnessing Mr. Poulton's experiments at intervals during their progress, and of discussing their bearings with him, and I must confess that I am at present completely at a loss to see how they can by any means be interpreted in the manner Dr. Romanes suggests.

The conclusions at which I arrived in the paper referred to may be very briefly summarized. We find in many species of insects, &c., a variability in colour which is distinctly of an *adaptive* character, enabling the insect to become adapted to a variable environment, and thus being obviously advantageous to the possessors of such a faculty. From this it seemed but a natural conclusion that such a power of adaptability should have been conferred by the usual operation of the law of the survival of the fittest. This conclusion I ventured to draw in 1873, after carefully considering all the cases which I could collect. But in thus grouping what I called at the time "variable protective colouring" among the biological phenomena capable of being regarded as the result of the action of natural selection, I was careful to point out that the precise mechanism of the process by which this adaptability was brought about remained to be investigated for each case. This is the work which has been so admirably carried out by Mr. Poulton for certain Lepidopterous larvæ, pupæ, and cocoons, and the results which he has obtained go far to show that this adaptability in colour is possessed by a