LETTERS TO THE EDITOR.

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Prof. Meldola and Mr. Herbert Spencer as Critics.

THREE letters have lately appeared in these columns commenting on my book lately published under the title of "Organic Evolution Cross-examined." Two of these are by Prof. Meldola, and one by Mr. Herbert Spencer. By the first I ought to feel much honoured, because the Professor lays down the law that nobody outside the class to which he himself belongs, namely professional experts, should be allowed to write or be listened to on such subjects as biology. One feels the atmosphere of condescension throughout his ostensible criticism, and he thinks it necessary to excuse himself for taking so much trouble as to criticise it at all. Perhaps he will allow me to explain what my doctrine and practice about experts has always been. It has been to take them frankly for all that they are worth, and that is much. First, I always accept everything they can tell us on matters of fact. Secondly, I always examine closely the language or phrases under which those facts are expressed, to see how far silent assumptions, or artificial conceptions, are imported into the interpretations of the facts of nature. Thirdly, I watch to see how far they set up an artificial vocabulary of their own, having for its object to wipe out of all natural phenomena the highest intellectual conceptions to which they are related. I am sorry to say that I do not know enough of Prof. Meldola's writings to be able to say how they would be found to stand this weapon of analysis. But I am struck by the fact that he seems to attribute to me the application of the word "plan" to organic structures: thus showing, on the one hand, great antipathy to the word; and, on the other hand, complete ignorance or forgetfulness that the word, in that sense, is not mine, but the word systematically used by Prof. Huxley in all his most typical works. This makes me suspect that Prof. Meldola may yield to the very common temptation to manipulate language so as to keep out of sight suggestions of thought which are instinctive but which are dangerous to his own theories and philosophy. I note, also, that in his condescending criticism of my book he deals a good deal in chaff; and when he encounters a fact or an argument not easily dealt with, he rides off in some flippant joke, as in the case of the electric organs of the torpedo and other fishes. Considering that Dr. Romanes thought my argument on those organs so strong, that if there were many other cases in nature of the same kind, he would be obliged to give up the Darwinian theory, it is surely worth the trouble even of so great an expert as Prof. Meldola to give some serious reply.

But I pass from Prof. Meldola to a criticism which concerns me a great deal more—because it comes from Mr. Herbert Spencer, for whose intellectual integrity I have the highest admiration and respect.

In my book I have dwelt at some length on the sad fate which has befallen both the celebrated phrases in which the Darwinian theory has been clothed. I have represented both of them as having come to grief, and as having been exposed to a most destructive criticism at the hands of no less an authority than Mr. Herbert Spencer, himself the author of one of these phrases, and one of the earliest patrons of the other. In this Mr. Spencer thinks I have been unfair to him. Let us hear, then, what he says in explanation.

As regards the phrase "natural selection," Mr. Spencer says that he pointed out that "its metaphorical character is apt to mislead." Exactly so! But how? It is all metaphor together. "Selection" is the idea on which it turns, and in the Darwinian theory there is no selector. But its whole popularity, and the whole possibility of the phrase representing the facts of biological science depended on the analogy of a breeder; and when this element of meaning was abandoned and denounced, nothing remained behind. Men may choose to go on using it, if they like, but as the expression of a systematic theory it is gone.

Well, now, what has Mr. Spencer done with the alternative phrase invented by himself? He says that "kindred objections may be urged against 'survival of the fittest,'" for just as selection suggested a human selector, so did the word "fittest" suggest some fitter. Therefore both phrases were alike metaphors, and both were therefore equally deceptive. There is no fitting, he says, in nature like that of a glove to a hand, or of a key

to a lock. I deny this absolutely. But assuming it to be so, then the word should be given up as applied to the marvellous adaptations of structure to functions in organic life. The whole virtue of the phrase is gone. I have not meant to allege that Mr. Spencer himself sees the full effect of his destructive criticism, or that he may not continue to hold by the child of his earlier years for some purposes of literary convenience. But we—the public and the scientific world—have nothing to do with that. What we have a right to deal with is a phrase which has enjoyed a wide popularity, and which has purported to express and to explain correctly the course of development in organic life. We find the author of this phrase admitting that it has in it elements lending themselves to deception, and that it suggests correlations of a kind which have no place in nature. This is to me an abandonment of the phrase, whether the original author of it thinks it so or not. The phrase is no longer his exclusive property. It belongs to the history of philosophy, and the criticisms of its own originator are among the most valuable helps we have in estimating any value it ever had. Mr. Spencer has now invented another phrase—"the theory of indirect equilibration"—which he represents as equivalent, and with reference to which I venture to predict that it will speedily share the fate of both its predecessors, as only one more attempt to hide out of sight, under the cover of a new and grotesque vocabulary, some of the most salient facts of biological science. ARGYLL.

IGNORING all personalities, expressed or implied, in the above communication, there are a few points which call for rejoinder. In the first place, the statement that I have laid down the law that none but professional experts should be allowed to write on biological subjects is a complete misrepresentation of my views. I have on more than one occasion made statements in precisely the opposite sense. If I may be permitted to quote from an address to the Entomological Society of London in 1896 (reprinted in these columns), I will invite the Duke of Argyll's attention to the following passage:—"As far as my reading extends, I am inclined to believe that even in the case of the purely literary treatment of biological problems by writers who are not experts, the danger of over-weighting the science with hypothesis is much exaggerated. Writers of this class are often capable of taking a wider and more philosophic grasp of a problem than a pure specialist, and ideas of lasting value have sometimes emanated from such sources. I imagine that nobody will dispute that Mr. Herbert Spencer's writings have largely influenced the public mind—whether we agree with the details of his doctrines or not—in accepting the broad principle of Evolution, although this profound thinker lays no claim to an expert knowledge of any branch of natural history. But every working naturalist can ascertain for himself the credentials of any particular writer: my remarks are simply offered with the object of claiming more consideration for such writers, as a class, on the part of practical workers. The philosophic faculty is quite as powerful an agent in the advancement of science as the gift of acquiring new knowledge by observation and experiment. It is not often that the faculties are combined in one individual."

In the next place, it is a misrepresentation to credit me with an antipathy to the word "plan." I have not the least objection to the word or to the idea which it conveys, but I do protest most emphatically against its being introduced by way of an explanation into any branch of science, biological or otherwise. The attempt to make Huxley responsible for the use of the term in this sense is, as I have already pointed out, a misrepresentation of that writer's views.

The paternal insight into the affairs of nature which leads a non-expert writer to put himself into the position of a judge of the value of experts must be a source of immense admiration to the working body of naturalists. I have nowhere laid claim to the distinction of being classed among that body, as my working days in that field are, I am afraid, closed. Nevertheless, I feel duly honoured at the Duke of Argyll's classification of myself with the "experts." Any obscurity under which I may be suffering in the scientific world through the neglect of his Grace to put my writings through his analytical process is, however, relieved—at any rate temporarily—by the invitation of the editor of NATURE to review the book which has given rise to this correspondence. In performing that duty in what I conceive to be the best interest of science, I have preferred to encounter the Duke's views in the open, rather

than under the cover of anonymity. His Grace will, I trust, extend to me at least a small measure of "intellectual integrity" on that score. If the "experts" are to be weighed and measured by some standard of the Duke's creation, then it is obvious that this body of gleaners might reasonably express some kind of opinion as to the value of the treatment to which their facts are being submitted. I have already endeavoured to put forward a plea on behalf of the non-expert philosophical writer, and I have expressly said that naturalists will ascertain for themselves the credentials of each writer. The Duke's contributions have for many years been of a purely destructive order; how far the credentials of a writer who has contributed so little constructively to the edifice of biological science will carry weight with the body of working naturalists is for them to declare.

A few words in conclusion as regards the electric organs of the torpedo. I am the last person to ignore the difficulties in the way of the theory of natural selection. Perhaps I have more faith than my late lamented friend Dr. Romanes in the power of a theory which explains so much being able, when we know more about them, to meet such cases as these; but this is a purely personal matter. The particular difficulty in question, like most others of weight, was long ago suggested to Darwin himself, and was discussed in the "Origin of Species" (6th ed., p. 150). It was discussed also in a letter to Lyell in 1860 ("Life and Letters," vol. ii. p. 352). But if the whole theory of natural selection were to break down on such a difficulty, the doctrine of a "preconceived plan" would not help us in the least. It would not be a philosophical explanation, but, with the very greatest deference to my noble antagonist, a pseudo-philosophical explanation, and, as such, I have felt, and always shall feel, it my bounden duty to science to warn the public from attaching any serious importance to it. R. MELDOLA.

The British Museum Catalogue of Birds.

MY attention has been drawn to some inaccuracy (no doubt unintentional) in the historical account of the production of the Catalogue of Birds given in a recent number of NATURE. Will you, therefore, kindly allow me to correct that account?

At the time of Mr. Bowdler Sharpe's appointment (September 1872) to an assistantship in the Zoological Department of the British Museum, the Keeper of the Department, Dr. Gray, was in so feeble a state of health (consequent on a paralytic stroke) that the administration of the Department had devolved upon Dr. Günther holding the post of assistant-keeper. During the preceding period of his assistant-keepership, Dr. Günther had gone through the whole collection of birds, and formed the opinion that a descriptive catalogue on the lines of his own Catalogue of Fishes ought to be prepared for publication. With this object in view Dr. Günther recommended to the Trustees the appointment of Mr. Sharpe, who, on account of his enthusiasm, energy and general ornithological knowledge, seemed to be specially qualified to undertake the catalogue. Dr. Gray gave his ready consent to the preparation of the catalogue, and the Trustees sanctioned the publication when the MS. of the first volume was laid before them in 1874. Thus, although it is the fact that the preface to the first volume was signed by Dr. J. E. Gray, yet the plan of the work was initiated and elaborated by Dr. Günther, and the work was, during its progress, kept under his constant supervision.

E. RAY LANKESTER.

February 1. Director of the Natural History Departments of the British Museum.

Queries on the Reduction of Andrews' Measurements on Carbonic Acid.

To begin with, let me quote a few passages from Andrews' paper. On pp. 301-302 of his "Scientific Papers" he says: "I have not attempted to deduce the actual pressure from the observed changes in the volume of the air in the air-tube. For this purpose it would be necessary to know with precision the deviations from the law of Mariotte exhibited by atmospheric air within the range of pressure employed in these experiments. . . . It will be easy to apply hereafter the corrections for true pressure when they are ascertained, and for the purposes of this paper they are not required. The general form of the curves representing the changes of volume in carbonic acid will hardly undergo any sensible change from the irregularities in the air-

tube; nor will any of the general conclusions at which I have arrived be affected by them. It must, however, always be understood that, when the pressures are occasionally spoken of, as indicated by the apparent contraction of the air in the air-gauge, the approximate pressures only are meant."

In every one of his papers specially devoted to the subject he was careful to mention the fact that he was unable to give the true pressures which correspond to the indications of the air- or hydrogen-manometer. The question seems to have hung constantly upon the mind of the experimentalist in the course of his investigations. In spite of this, however, the values of pressure given by him have often been treated as if they were the true values, and in the discussions of the characteristic equation of carbonic acid, agreement or disagreement to within 1/10 of an atmosphere is spoken of. But according to Amagat's measurements on air, hydrogen, or nitrogen, the corrections to be applied are quite large.

In a paper in the *Philosophical Magazine*, vol. xxiii., fifth series, Ramsay and Young have tried, on the basis of Andrews' experiments, to show that in the case of carbonic acid there holds good very approximately the constant volume relation that the pressure under this condition varies linearly as the temperature. They state that they have reduced to absolute units, as far as possible, the values of pressure given by Andrews by means of Amagat's experiments on air. These latter experiments were made at 16°, while the temperature of Andrews' manometer varied within a considerable range, from 5° to 15°. Is the influence of temperature on the manometric correction negligible when the pressure is high? I can show in the case of nitrogen, on the basis of Amagat's experiments, that this auxiliary correction due to temperature variation is generally quite large, sometimes of the same order of magnitude as the main correction itself; and it seems very improbable that in this respect air would differ very much from nitrogen.

The same remark applies to the calculations of Margules (*Wien. Sitzbch.*, xcvi. 2a, 1888), and also to my calculations relating to Andrews' measurements on the mixtures of nitrogen and carbonic acid (*Phil. Mag.*, vol. xxxvi. 5th series).

Another point which has for a long time remained a query to me, is the level difference of the mercurial columns in the air and carbonic acid tubes. Let me again quote from Andrews' paper. On p. 303 of his "Scientific Papers" he says: "Having thus ascertained the volumes of the air and of the carbonic acid before compression, at 0° and 760 millims., it was easy to calculate their volumes, under the same pressure of 760 millims., at the temperatures at which the measurements were made when the gases were compressed, and thence to deduce the values of the fractions representing the diminution of volume. But the fractions thus obtained would not give results directly comparable for air and carbonic acid. Although the capillary glass tubes in the apparatus communicated with the same reservoir, the pressure on the contained gases was not quite equal, in consequence of the mercurial columns, which confined the air and carbonic acid, being of different heights. The column always stood higher in the carbonic-acid-tube than in the air-tube, so that the pressure in the latter was a little greater than in the former. The difference in the lengths of the mercurial columns rarely exceeded 200 millims., or about one-fourth of an atmosphere. This correction was always applied, as was also a trifling correction of 7 millims. for a difference of capillary depression in the two tubes." In another place (p. 422) he says: "The pressure in atmospheres, as indicated by the air-manometer, on the gas in the carbonic-acid-tube was given by the equation

$$p = \frac{V_0(1 + \alpha t)}{V_1} \mp \frac{q}{760},$$

in which V_0 is the volume of the air at 0° and 760 millimetres, V_1 the observed volume at the temperature t , and q the difference of level (corrected when necessary for difference of capillary depression) of the surface of the mercury in the manometer and carbonic acid tubes."

Thus what is given as δ or p in Andrews' papers includes in itself the level difference q , and what is really required in calculating the corresponding manometric correction is V_1 ; q is not given in Andrews' papers. The manometric corrections, then, which are based on the given values of p alone, will differ from what they ought to be by something like $\frac{1}{4}$ of an atmosphere.

Thus one will find himself prevented from entering upon a