

## LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

## The Duke of Argyll and Mr. Herbert Spencer.

HAD I read Mr. Spencer's reply to the Duke of Argyll in 1888, I should have been even more astonished than the writer of the "Counter Criticism," that the Duke should have sanctioned the publication of his essays in their present form without a word of warning to his readers, that Mr. Spencer had not only not sanctioned but had explicitly denied the interpretation which the Duke had forced upon his analysis of the term "survival of the fittest." Any person would conclude from the first essay that Mr. Spencer had altogether abandoned this term, and (by implication) the factor of organic evolution expressed by it. I am sure that biologists will be generally glad to have it again authoritatively from Mr. Spencer himself that he is still so far Darwinian. He will also bear with me, I hope, when I point out that the mass of literature which the working man of science has to digest at the present time is so great that very few have time to seek light in the pages of the current magazines. Certainly we do not turn to these publications as a rule for information on scientific questions, and, I am bound to add, that the principles which determine the selection of writers on scientific subjects for such magazines have always appeared to me to be a profound mystery. It is not mere flattery when I state that we are in the habit of regarding Mr. Spencer's magazine contributions in the light of "preliminary notices," and that we always look forward to having them in a collected form at some later period.

With respect to the apparent change of attitude on the question of the relative importance of direct and indirect equilibration, I can, of course, only accept Mr. Spencer's explanation that the great prominence into which he has of late years brought the first of these factors, has led biologists in this country to suppose that he attaches more weight to it than he did formerly. It may be also that since the admissibility of this factor has been seriously questioned by those who accept the views of Prichard, Galton and Weismann, the attitude of each party has become unconsciously stiffened towards the other. In the passages from his "Principles of Biology," referred to by Mr. Spencer in his letter (which passages I had by no means forgotten), it is made perfectly clear that even at the time of writing that work he went beyond Darwin in the part assigned to direct equilibration. In his "Factors of Organic Evolution," published in 1886 in the *Nineteenth Century*, and collectively in 1887, Mr. Spencer certainly produces the impression that he is inclined to go still further in this direction:—

"Was the share in organic evolution which Mr. Darwin latterly assigned to the transmission of modifications caused by use and disuse, its due share? Consideration of the groups of evidences given above will, I think, lead us to believe that its share has been much larger than he supposed even in his later days" (p. 33).

"But the fact we have to note is that while Mr. Darwin thus took account of special effects due to special amounts and combinations of agencies in the environment, he did not take account of the far more important effects due to the general and constant operation of these agencies" (p. 46).

"But gradually with that increase of activity which we see on ascending to successively higher grades of animals, and especially with that increased complexity of life which we also see, there came more and more into play as a factor, the inheritance of those modifications of structure caused by modifications of function. Eventually, among creatures of high organisation, this factor became an important one; and I think there is reason to conclude that, in the case of the highest of creatures, civilised men, among whom the kinds of variation which affect survival are too multitudinous to permit easy selection of any one, and among whom survival of the fittest is greatly interfered with, it has become the chief factor: such aid as survival of the fittest gives, being usually limited to the preservation of those in whom the totality of the faculties has been most favourably moulded by functional changes" (p. 74).

I have not the least desire to raise once again the whole ques-

NO. 1525, VOL. 59]

tion as to whether "direct equilibration" plays any part at all in the development of species, but such passages as those above quoted, and generally the whole tendency to exalt this factor in the essays from which they are quoted, has produced a very widespread notion that Mr. Spencer has diverged more widely from Darwin now than he did in 1864. Personally I can only express satisfaction that Mr. Spencer has himself disilluminated us.

January 13.

R. MELDOLA.

## The late Prof. George James Allman, as a Botanist.

IN the notice of my distinguished namesake and friend—the late George James Allman—which appeared in NATURE of December 29, 1898, it is stated:

"Allman's first paper was a botanical one, 'On the Mathematical Relations of Forms of Cells of Plants,' and it is worthy of note that in this he in a sense anticipated one of the most recent among our biological departures."

This is not so. I send you herewith a copy of an "Abstract of a Memoir on the Mathematical Connection between the Parts of Vegetables," by William Allman, M.D., who was Professor of Botany in the University of Dublin, 1809–1844, and the predecessor of the late George James Allman in the chair. The memoir is plainly the paper referred to above, and was read before the Royal Society in the year 1811.

GEORGE J. ALLMAN.

St. Mary's, Galway, January 2.

THE paragraph in my obituary notice of the late George James Allman, cited by Prof. George Johnston Allman, was intended to refer to a paper read before the British Association in 1835, entitled "On the Mathematical Relations of the Forms of the Cells of Plants," which heads the list of works ascribed in the Royal Society's Catalogue of Scientific Papers to George James Allman, and not to that by William Allman mentioned in the accompanying letter by his son, of which at the time of writing I was ignorant. While collecting data for my necrology of George James Allman, my suspicions were aroused by the fact that in the original form the paper alluded to by me is attributed but to a "Dr. Allman"; assuming, however, that the Royal Society's Catalogue must have had authority for definitely associating it with George James Allman, I did not inquire further. In consideration of the point now raised, the matter becomes further complicated by the fact that the President of the Linnean Society, in making the award of the Society's Gold Medal to the late George James Allman in 1896, was, at my instigation, led to refer (*Proc. Linn. Soc.*, 1895–1896, p. 30) to the same paper in terms apposite to those of my obituary notice now under discussion. The memoir by William Allman, referred to by Prof. George Johnston Allman, is preserved in the Department of Botany, British Museum, together with a copy of an abstract of the same printed privately in 1844, as has been pointed out by my colleague at the Linnean Society, Mr. B. Daydon Jackson, in his article "William Allman" in the Dictionary of National Biography, on Prof. Allman's own authority, and by Prof. Percival Wright in his "Notes from the Botanical School in Trinity College, Dublin" (No. 1, p. 3); (*cf.* also Messrs. Britten and Boulger's "Index of British and Irish Botanists," p. 3). And on inspection, I find them accompanied by a letter to Robert Brown, dated 1844, which seems to show that the abstract was printed at his suggestion, *apropos* of an application by W. Allman for an appointment for which testimonials were being sought. MS. and abstract, and the paper to which I alluded, however, though cognate, are unquestionably distinct; and, on making further inquiry since the receipt of Prof. Allman's letter, I have been interested to find in the British Association's Index for the years 1831–1860 yet another of a similar character, recorded (but in title only) under the name of George James Allman. Mr. Griffith, the Secretary of the British Association, has very generously aided me by looking up the original records in his possession, and other reports and publications likely to bear on the question; and he informs me that he has no doubt whatever that the series of papers under discussion were by William Allman, pointing out that the paper regarding which I was misled by the Royal Society's Catalogue and British Association's Report is rightly attributed to him, on authority, in Poggenorff's "Handwörterbuch." Further consideration of the dates of events in the lives of the two Allmans fully bears this conclusion out. The series of papers were clearly expressive of successive phases in a long-cherished idea revolving in its author's mind for a period of nearly forty