

LETTERS TO THE EDITOR.

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"The Primrose and Darwinism."

I DESIRE to make a short reply in answer to two or three of your reviewer's criticisms on "The Primrose and Darwinism," and on its author, which appeared in your issue of August 28. "We do not propose," to adopt the words of your reviewer, "to go through the whole review, but to discuss one or two points and to leave your readers to judge of the remainder."

My first and chiefest point is in reference to the charge which the reviewer makes in the following statement (p. 411):—"The only point which is worthy of notice" (relative to the cleistogamic flowers) "is a quotation (Prim. and Dar., p. 191) from Darwin's 'Form of Flowers,' which has several copyist's mistakes, and, moreover, contains interpolated words which do not occur in the original, the whole being within inverted commas. It is this sort of treatment of Darwin's text that makes it almost impossible to read the 'Field Naturalist.'"

I give here an exact copy of Darwin's paragraph from "Form of Flowers," p. 323, and an exact copy both of words and inverted commas of my own comments on Darwin's statement. It will be evident to every reader that Darwin's own observations are always marked off by inverted commas, and that my own comments are not included within the commas. Your reviewer seems to have read my comment with exceeding carelessness.

Darwin's Text.

"The most singular fact about the present species is that long-styled cleistogamic flowers are produced by the long-styled plants, and mid-styled as well as short-styled cleistogamic flowers by the other two forms; so that there are three kinds of cleistogamic and three kinds of perfect flowers produced by this one species! Most of the hetero-styled species of *Oxalis* are more or less sterile, many absolutely so, if illegitimately fertilised with their own form pollen. It is therefore probable that the pollen of the cleistogamic flowers has been modified in power, so as to act on their own stigmas, for they yield an abundance of seeds" (p. 323 of last edition, 1892).

My own comment.

But in *Oxalis Sensitiva* "the long-styled cleistogamic flowers are produced by long-styled plants; the mid-styled as well as the short-styled cleistogamic flowers are produced respectively by the other two forms; so that there are three kinds of cleistogamic and three kinds of perfect flowers produced by this one species" (F. Fl., p. 323). Now, as Darwin, from his *net experiments*, concluded that "most of the hetero-styled species of *Oxalis* are more or less sterile, many absolutely so, if illegitimately fertilised with their own form pollen" (F. Fl., p. 323), he had in some way to account for this extreme contradiction in results between the naturally abundant fertility of these cleistogamic flowers, and his own results, which we have given above, of *Lythrum Salicaria*, under the unnatural method of experimenting with his net. Under this difficulty, Darwin suggests, "it is probable that the pollen of the cleistogamic flowers has been modified in power, so as to act on their stigmas, for they yield an abundance of seed" (F. Fl., p. 323. The italics are ours). (Prim. and Dar., p. 191.)

Again the reviewer states that the "Field Naturalist's" sentence (p. 11):—"To attribute the capacity for fertilisation in the unprotected flowers to the bees is perfectly gratuitous, as the flowers under the net (when bees were excluded) 'when they touched the net and the wind blew' produced seeds without any cross-fertilisation"—contains, in the words 'when they touched the net and the wind blew,' an "incorrect quotation" (p. 409).

Darwin's words are:—

"*Salvia tenori*. Quite sterile; but two or three flowers on the summits of three of the spikes, which touched the net when the wind blew, produced a few seeds" (Cr. and S.F., p. 362).

My quotation.

Salvia tenori under the net, Darwin tells us, "was quite sterile; but two or three flowers on the summit of the spikes, *which touched the net when the wind blew*, produced a few seeds" (Cr. and S.F., p. 362. The italics are ours). (Prim. and Dar., p. 11.)

The quotation is word for word from Darwin in the italicised words; yet the reviewer takes no notice of this, but produces a merely shortened form a few lines below, and which though shortened conveys exactly the same sense, and calls it "an incorrect quotation"!

One more charge of this kind of your reviewer scarcely needs being noticed. But I notice it in order to avoid any misinterpretation if I passed it over. The charge is one in reference to *Sarothamnus scoparius*. Darwin states concerning it (Cr. and S.F., p. 360):—"Extremely sterile when the flowers are neither

visited by bees, nor disturbed by being beaten by the wind against the surrounding net." The reviewer says:—"The *Field Naturalist* quotes the passage incorrectly, omitting 'when the flowers are neither visited by bees.'" In my chapter headed "The Sterilising Influence of Darwin's Net," where the quotation occurs, the bees in this reference—as they were excluded by the net—had nothing whatever to do with the subject, and so reference to them was omitted; the effect of the net and of the net alone on fertilisation was there being discussed.

Such are the passages which the reviewer cites as misquoted or interpolated. I should have esteemed it a deep dishonour if I had knowingly misquoted any statement of Darwin, or had interpolated any words in quotations from Darwin, and should not lightly have excused myself even had it been done carelessly or unwittingly. To avoid all such charges like those of the reviewer, I distinctly state in the preface:—"We have carefully given the references to all the passages quoted, or referred to, in the following pages." This was done that every reader might find without trouble, if he desired, the original passages and could compare the quotation with them.

At p. 409, the reviewer cites from "The Primrose and Darwinism":—"In calm weather the net would prevent the free access of the wind and would prevent it from shaking, and so from freely disturbing and distributing the pollen" (p. 8), and states "not a particle of evidence is given from his point of view." The evidence in this case is supplied by Darwin himself:—"In all cases the flowers were protected from the wind" (Cr. and S.F., p. 23); and again, as quoted in Prim. and Dar., "The wind does hardly anything in the way of conveying pollen from plant to plant when insects are excluded" (F. of Fl., p. 93).

The reviewer says, "When the author ventures on suggesting a function we are liable to come across such a theory, as the orifice in the carina of *Lotus* is to serve for the ventilation of the pollen stored within the carina." As I spent three and a half to four years of my life in the uninterrupted study of physiology and its sister sciences, there still remains a sufficient residuum of its flavour in the cask that I can venture to assert that if your reviewer will only consult a competent physiologist about a pistil surrounded with packed pollen in a closed carina, like Fig. 13, p. 132 (Sowerby's "English Botany," v. iii.), of the *Lotus*, he will tell the reviewer that such ventilation of a cone, if not absolutely necessary in every season, yet would be absolutely necessary in some seasons, and would be very conducive in all seasons to the healthy fertilisation and fructification of the pod.

Finally, the reviewer states, "the author makes the astonishing statement that Darwin's predecessors are to be commended for strictly subordinating theory to natural facts. They thus happily avoided the error into which Darwin, in this instance at least, most assuredly and most conspicuously fell." The reference here is to the dimorphism of the primrose and to Darwin's statement in reference to such a state—"One form of *Primula* must unite with the other form in order to produce full fertility" ("Form of Flowers," pp. 49, 56). And again, "heterostyled flowers stand in the reciprocal relation of different sexes to each other" ("Form of Flowers," pp. 2, 28, 245).

The late Professor J. S. Henslow was acquainted with the heterostylism of the primrose as stated (and quoted) by me in the preface to the book, but Darwin alone fell into the error that "the two forms stood in the reciprocal relation of different sexes to each other." I will leave to the judgment of botanists who are also acquainted with the long-tongued *Hymenoptera aculeata* and Lepidoptera to decide the question in the spring by observing the flowers from the middle of March to the end of April, whether the short-styled primrose, though fully productive, is cross-fertilised by insects.

In the same way we will leave to all observers or naturalists, by their observing the flowers in the month of May, the question whether the *Arum* is not, with possibly some very accidental exceptions, "a purely self-fertilised flower." We know of no English plant which gives plainer and more easily observable evidence to the fact of self-fertilisation. This is our decided opinion after having examined more than 500 specimens of opened spathes and found in them no evidence to the contrary.

After examining these cases the reviewer will not, I think, "find it hard to tell why this book was written." But lest he should still after that find a difficulty, I will tell him myself. It was, and is, to show that artificial experiments conducted under a close-meshed net was an unnatural and very defective method to discover the operations of Nature in flowers when

exposed to the unlimited influence of sun, wind, dew and other atmospheric agencies; and to show that Nature must be interpreted under the atmospheric conditions which she herself provides, and not under those conditions minimised and in some cases almost absolutely intercepted.

AUTHOR OF "PRIMROSE AND DARWINISM."

September 2.

In my review of "The Primrose and Darwinism," I thought it necessary to call attention to the inaccuracy of the author in the matter of quotation, but I had not the least intention of accusing him of anything more than carelessness. For instance, in the case of *Sarothamnus*, to which he refers in his letter, I was quite ready to believe that the omission of words within inverted commas was an oversight. But in his letter he tells us that they were omitted because "the bees in this reference—as they were excluded by the net—had nothing whatever to do with the subject." He stands self-convicted of knowingly altering what he quotes, but I readily believe that he is guilty of nothing worse than ignorance of the usage of literary work.

The *Field Naturalist* objects to my statement that there are "several copyist's mistakes" as well as "interpolated words" on p. 191 of his book. I therefore give the passage in his book to which I referred, followed by the corrections needed to make it agree with "Forms of Flowers," ed. ii. p. 323.¹

But in *Oxalis sensitiva* "the long-styled cleistogamic flowers are produced by long-styled plants; the mid-styled as well as the short-styled cleistogamic flowers are produced respectively by the other two forms."

The mistakes are:—

For "the long-styled read the "long-styled.

For produced by long-styled read produced by the long-styled.

For the mid-styled read mid-styled.

For the short-styled read short-styled.

Dele, produced respectively.

If the *Field Naturalist* really considers this a justifiable sample of the art of citation I shall be surprised.

With regard to *Salvia tenori*, the *Field Naturalist* complains that I describe (p. 409) the words, "when they touched the net and the wind blew" ("The Primrose," &c., p. 11) as an incorrect quotation. When I read the phrase in question I was so much surprised to find these words attributed to Mr. Darwin that I turned to his book, where I found, "which touched the net when the wind blew." I still think that the *Field Naturalist* is not justified in placing within inverted commas a passage which does not occur in the original; nor can I agree with him that the correct and incorrect versions convey "exactly the same sense." This was the only inaccuracy in regard to *Salvia tenori* to which I called attention in my review; but I now learn, from the parallel passages given in the *Field Naturalist's* letter, that he quotes incorrectly the words "two or three flowers on the summits of three of the spikes," changing them by a not unimportant omission to "two or three flowers on the summits of the spikes."

Lastly, the *Field Naturalist* complains of my saying that he has not a "particle of evidence" for his point of view in regard to the supposed injurious effect of the net in keeping the wind from the experimental plants. He goes on: "The evidence in this case is supplied by Darwin himself. 'In all cases the flowers were protected from the wind.'" What we want is not evidence of protection from wind, but evidence that such protection has any hurtful effect on the reproductive organs of the plants.

The rest of the *Field Naturalist's* remarks do not seem to me to call for reply.

THE WRITER OF THE REVIEW.

A Method of Treating Parallels.

In your issue of July 3, just to hand, Dr. Richardson suggests a method of treating parallels which differs from the orthodox Euclidean method. Improvements of a kind similar to that suggested by him will go far towards rendering the teaching of geometry more effective than it is at present. I differ from him to a slight degree in this particular instance, in that I consider it preferable to take the more general case of equal inclination of parallels to any straight line which cuts them as expressing the clearest and most useful conception of parallelism. By constituting sameness of direction the criterion of parallels—direction being purely relative, this sameness is determined by

¹ The passage is the same in edit. i.