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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*

"A Conspiracy of Silence."

MAY I ask your correspondents who have been good enough to read my article on "Darwin's Theory of Coral Islands," published in the September number of the *Nineteenth Century*, to begin addressing themselves to the merits of the scientific question there dealt with, and to cease wasting their own time and your space upon scolding me for a few words—perhaps exaggerated—respecting the wide-spread reluctance to question any theory advanced by Charles Darwin? I have already explained in your columns the sense in which I spoke, and, subject to that explanation, I have nothing to retract. I observe in Prof. Tait's notice of Dr. Balfour Stewart, published in your latest issue, a passage which shows that this very eminent man of science speaks in a tone very similar of certain "advanced" geologists who "ignore" views which "tend to dethrone" their own "pet theories." Moreover, since I last addressed you in explanation, I have observed the remarkable passage ("Darwin's Life," vol. ii. p. 186) in which my censor, Prof. Huxley, positively blasphemes against no less a distinguished body of scientific men than the French Institute for their conduct towards evolutionism. He speaks of the "ill-will of powerful members of that body producing for a long time the effect of a conspiracy of silence." This is the very same expression which I used, but without the offensive aggravations added by Prof. Huxley.

Inveraray, December 30, 1887. ARGYLL.

Mr. Seebohm on Physiological Selection.

FROM a footnote to page 23 of Mr. Seebohm's recently published and magnificent monograph on the Charadriidæ I learn that I owe him an apology for having inadvertently misrepresented his views upon a point of considerable importance in the philosophy of evolution. In his British Association paper (which he now re-publishes) he went even further than I had gone in recognizing the "swamping effects of intercrossing" upon incipient varieties, with the consequent importance of isolation in the differentiation of species. I therefore supposed that he likewise agreed with me in holding it improbable that new species arise as a result of many beneficial variations of the same kind arising at the same time and in the same place. I now find, however, that he is a strong advocate of the opposite opinion—apparently going further than Asa Gray, Nägeli, Mivart, the Duke of Argyll, or indeed any other evolutionist, in support of the doctrine of teleological variation in determinate lines. I therefore write to withdraw my previous misrepresentation of his views upon this matter, and to apologize for my inadvertency in making it.

At the same time, I may observe, it does not seem to me quite intelligible how Mr. Seebohm can reconcile his doctrine of teleological variation with his doctrine of the paramount importance of geographical isolation. For it is evident that, in whatever measure geographical isolation is found to be of importance as a condition to the origin of species (*i.e.* by preventing free intercrossing), in that measure is the doctrine of teleological variation invalidated. Indeed, Mr. Seebohm himself puts Mr. Wallace on the horns of a dilemma with regard to a precisely parallel case. In order to meet me where I draw attention to the difficulty which free intercrossing imposes upon the theory of natural selection, Mr. Wallace argued in favour of collective variation, *i.e.* of the doctrine that a considerable percentage of identical and beneficial variations may arise simultaneously in the same community. Now, Mr. Seebohm very pertinently observes (p. 13):—"It seems to me that, by the admission of this fact, Mr. Wallace has dethroned his theory of natural selection from its proud position as the main factor in the origin of species." With this, of course, I fully agree; but does it not equally follow that by *his* admission of this same "fact" Mr. Seebohm is no less effectually dethroning his own theory of the paramount importance of isolation as one of the main factors in the origin of species?

In conclusion, I cannot understand why Mr. Seebohm should have ignored my answer to the criticisms which he now republishes. For, as I have pointed out in these columns before, the whole brunt of his criticism (like that of Mr. Wallace) was directed against a theory which never so much as occurred to me. Both my critics took it for granted that I supposed my "physiological complements" to arise only in pairs; and therefore they both had an easy case in showing how improbable it was that the two complements should chance to come together. But even in my original paper there were passages to show that I supposed these physiological variations to occur in large numbers, or "collectively," leading to what botanists now call "prepotency," and thus explaining why hybridization is so rare in Nature. Possibly in that paper I was not sufficiently explicit in guarding against a misconception which it never occurred to me could arise. But certainly in my reply to this misconception, no further doubt as to my meaning could possibly remain. I confess, therefore, to being not a little surprised at this re-appearance of Mr. Seebohm's criticism, without allusion to my full repudiation of it a year ago. I should much like to learn his views upon the theory which I have published, but must protest against this absurd substitution being still attributed to me, after I have disclaimed it with all the emphasis of which the English language is capable. GEORGE J. ROMANES.

An Incorrect Footnote and its Consequences.

IN all the five editions of Baltzer's "Theorie und Anwendung der Determinanten" there stands at the foot of the first page an historical note, in which reference is made to a work entitled, "Demonstratio eliminationis Cramerianæ," by Mollweide (Leipzig, 1811). About a year ago it became necessary to examine this demonstration for the purpose of having it reported upon in an historical work. The University Libraries in Scotland were applied to in succession, but no copy could be heard of. Inquiries made at the more important libraries in Cambridge by friends resident there, or by letter, ended in the same unsatisfactory way. Letters, followed by an actual visit, to several libraries in London, brought no better result; and after every possible biographical scrap about Mollweide had been ferreted out in the British Museum, the suspicion began to form itself that some curious error had crept into Baltzer's footnote. In order to get to the bottom of the matter, the excellent mathematical library of Göttingen University was next applied to, and the library of Giessen University, where Baltzer was Professor; but in both cases in vain. A last effort was then made about a month ago in a letter to the University Library of Leipzig, where the reputed author Mollweide had taught, and where the "Demonstratio" (or *Demon*, as it had for more than one reason come to be called) had been published. Even here, at first, there was failure. But Prof. Virchl, who most kindly interested himself in the matter, was soon successful in his quest. What he found, however, was not a "Demonstratio" by Mollweide; the title was simply as follows: "Ad memoriam Kregelio-Sternbachianam in auditorio philosophorum die xviii. Julii, MDCCCXI. h. ix. celebrandam invitant ordinum Academiæ Lips. Decani seniores cæterique adsessores——'Demonstratio eliminationis Cramerianæ.'" Either, therefore, no author should have been mentioned by Baltzer, or an indication should have been given that Mollweide's name was an interpolation in the title. One or other of these courses would likewise have been less hurtful to Baltzer's reputation for accuracy; for, after all, Mollweide was not the author. In the Leipzig Library Catalogue the work is entered under the name of De Prasse, and Prof. Virchl had no doubt whatever, for perfectly conclusive reasons which he gave, that De Prasse was the author. The work extends to only 15 pages quarto, and is considered by the same authority to be very rare.

The point which we have now reached in the story might seem a not unfitting one to stop at; but the end is not yet. De Prasse's modesty requires explanation, and so likewise does the intrusion of Mollweide's name. Both are partly cleared up by the following facts supplied by Prof. Virchl. (1) The Kregel-Sternbach dissertation (which the "Demonstratio" was) falls to be delivered by the Dean of the Philosophical Faculty for the time being: the author's name was thus not an absolute necessity on the invitation title-page. (2) Mollweide was De Prasse's suc-