by a good section on analysis of silicates and some technical products. The book does not attempt to cover all the field of analysis, but what is done will be found really useful by a beginner or a junior student.

W. R. H.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he under-take 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."

THE Duke of Argyll can scarcely be congratulated upon his latest discovery of a new ground of attack upon geologists. In the year 1862 a very eminent physicist, whose loss we all so deeply deplore, made the somewhat rash suggestion that flint implements are found deep down in the drift, owing to their high density as compared with that of the matrix in which they are inclosed. Seeing that the material in which the implements are found is usually a *flint*-gravel, everyone acquainted with the subject saw that the suggestion was, to say the least, a somewhat unfortunate one, and Prof. P. G. Tait, in seeking for an oppor-tunity to sneer at "advanced geologists," was scarcely kind to the memory of a deceased friend in rescuing such a suggestion from oblivion. But to the Duke of Argyll, the finding of a new basis from which to attack geologists seems to have been a chance which he could not afford to let slip.

The Duke of Argyll now asks when we are going to begin to discuss his magazine-article upon coral reefs. I reply that in the article in question there is not a single new fact or fresh argument—nothing which has not been already brought forward by Mr. Murray himself, or by Dr. Archibald Geikie, and met by Prof. Dana in a singularly exhaustive memoir well known to all geologists. The subject has, moreover, been treated at considerable length by Profs. Prestwich, Green, James Geikie, De Lapparent, and others. Surely no exception can be taken either to the eminence of the authorities who have written on the subject, to the length to which their notices have extended, or to the prominence of the journals or treatises in which these dis-cussions have appeared. If it be said that the general scientific public have not had the matter fully laid before them, it is only necessary in reply to call attention to the pages of NATURE, in which a succession of articles dealing with the subject will be found.

The Duke of Argyll says that he has "nothing to retract." Here I regret to have distinctly to join issue with him. He has asserted that scientific men have refrained from discussing a particular theory, and that in taking this course they have been actuated by the worst of motives—a fear of the truth; he has charged the Geological Society with refusing in the spring of 1885, through its then President, to accept a certain paper from the same cause ; and now he adopts and gives fresh currency to an equally offensive charge of a similar kind.

These charges have, each and all of them, been shown to be absolutely destitute of foundation. The Duke of Argyll must judge for himself if the principle of *noblesse oblige* should not lead him, not only to *retract* the charges, but also to *apologize* for having made them. But his Grace may rest assured that, until he does so, the grounds for the deep indignation at his conduct, which is so strongly felt both at home and abroad, will still remain.

JOHN W. JUDD.

On the Constant P in Observations of Terrestrial Magnetism.

I REGRET that Prof. Rücker should have largely misunderstood my last letter. I have not raised the question of fallible obser-vations at all. Referring to the correspondence on pages 127-8 of the present volume of NATURE, my principal contention was and is that the ordinarily accepted formula for P differs by terms

of the second and higher orders from Gauss's theory, and that that difference necessarily persists in any rigorous expansion of the formula. By the ordinarily accepted formula for P I mean Prof. Rücker's formula (a) ; and by Gauss's theory I mean my formulæ (1), (2), and (3). From two observations of $f(u)$, made respec-tively at the distances r and r_1, the L of Gauss's theory might be found by a direct solution of equations (1) and (2) ; but instead of that, it is customary to find L from equations (7) and (8) by substituting in them the value of P_0 computed through equation (a). To render the latter procedure rigorous, P should be used in (7), and P_1 in (8). Equation (11) shows that P and P_1 differ by quantities of the second and higher orders, and as the ordinarily accepted value of P_0 lies between P and P_1, it neces-sarily differs from one or both of these quantities, and there-fore from Gauss's theory, by terms of the second and higher orders.

While freely admitting the justice of Prof. Rücker's criticism upon my arbitrary assumption that $P_0 = \frac{1}{2}(P + P_1)$, I cannot assent to the process by which he has deduced equation (γ). Equations (7) and (8) show that we may have either one L and two P's, or two L's and one P. In the latter case these equations become—

$$\tfrac{1}{2}L' = A(1 - P_0 r^{-2}) \quad \cdots \cdots (15)$$
$$\tfrac{1}{2}L'' = A_1(1 - P_0 r_1^{-2}) \quad \cdots \cdots (16)$$

and P_0 must be determined so as to make L' and L'' as nearly as possible identical with L. To that end we must have $2L = L' + L''$; and then, from the difference between $(7) + (8)$ and $(15) + (16)$

$$P_0 = B(A - A_1)\frac{r_1^2 + r^2}{A r_1^2 + A_1 r^2} \quad \cdots \cdots (17)$$

Expanding to terms of the second order

$$P_0 = B\frac{(A - A_1)}{A}\left\{ 1 + \frac{r^2}{r_1^2 + r^2}\left(\frac{A - A_1}{A}\right)\right\} \quad . (18)$$

Whence, by equation (13)

$$P_0 = \frac{r_1^2 r^2}{r_1^2 - r^2}\left(\frac{\log A - \log A_1}{M}\right) - \frac{r_1^2 r^2}{2(r_1^2 - r^2)}\left(\frac{\log A - \log A_1}{M}\right)^2 \quad . . (19)$$

This result agrees better with equation (14) than with equation (γ).

WM. HARKNESS.

Washington, D.C., December 30, 1887.

I AM afraid that the new method of calculating P_0 adopted by Prof. Harkness is not less arbitrary than that which he previously employed. He says that "P_0 must be determined so as to make L' and L'' as nearly as possible identical with L." If the object is only to deduce a correct value of L by combining equations (15) and (16), this condition is certainly not necessary. For if we substitute from (17) in (15) and (16), and take the mean of the values of L' and L'', we get by a very roundabout process the same value of L as we should have obtained without using P_0 at all. But we should have reached the same final result if we had started with the assumption that

$$(n + m)L = n L' + m L'',$$

where n and m are any numbers whatever. By properly choosing n and m we could deduce the correct value of L with any assigned value of P_0. It appears to me that the equation $2L = L' + L''$ is based upon the tacit assumption that L' and L'' are to be com-bined in accordance with the rules applied to fallible measures, and cannot otherwise be justified if the only object is the correct deduction of L from (15) and (16).

If, however, P_0 is introduced to enable us to calculate another approximate value of L by observing (say) A_2 at some other distance, r_2, the best value to select will depend on circum-stances. If r_2 is nearly $= r$ we shall get the best result by writing $P_0 = P$ and so on, so that the equation $2L = L' + L''$ is again arbitrary.

I am quite in agreement with Prof. Harkness as to the fact that if we start from the basis of equations (1) and (2) a small theoretical error is introduced by substituting P_0 for P and P_1. Indeed I think this step can only be justified by our knowledge that the inaccuracy thus caused is less than the error of experi-