

—one from whom the mathematicians of the future may derive valuable and fertile methods.

For the advance of the exact sciences depends upon the discovery and development of appropriate and exact ideas, by means of which we may form a mental representation of the facts, sufficiently general, on the one hand, to stand for any particular case, and sufficiently exact, on the other, to warrant the deductions we may draw from them by the application of mathematical reasoning.

From the straight line of Euclid to the lines of force of Faraday this has been the character of the ideas by which science has been advanced, and by the free use of dynamical as well as geometrical ideas we may hope for a further advance. The use of mathematical calculations is to compare the results of the application of these ideas with our measurements of the quantities concerned in our experiments. Electrical science is now in the stage in which such measurements and calculations are of the greatest importance.

We are probably ignorant even of the name of the science which will be developed out of the materials we are now collecting, when the great philosopher next after Faraday makes his appearance.

#### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

Tyndall and Tait

I HAVE hitherto refrained from intruding upon your space with reference to this deplorable Forbes' controversy, but now that the occasion has come when a brief deliverance on my part seems called for, I trust to your courtesy, if not to your justice, to allow me room for it.

In the first place I would ask permission to inform such of your readers as may feel an interest in the subject, that if they wish to form a correct opinion of the tone and logic of my rejoinder to Principal Forbes and his biographers, they will consult the rejoinder itself, as published by Longmans, and not the extracts and inferences of Professor Tait.

They will thus learn, among other things, that what Professor Tait calls "plausible," is simply unanswerable.

With regard to the taking up of the various points in Principal Forbes's reply, item by item, that may be done some day should I deem it a worthy occupation. In my rejoinder I converged attention on the two points which Principal Forbes himself considered the really serious ones, and having broken the neck of the argument in both these cases I cared little about prolonging the controversy. Nevertheless if circumstances show it to be necessary it may be prolonged.

Professor Tait invariably writes on the hypothesis that what is not contradicted cannot be contradicted, and must therefore be accepted as true—a natural, if not inevitable, assumption on his part. For example, Forbes's argument regarding the crevasses of Rendu was left unanswered by me, hence the conclusion that it was unanswerable. That argument, however, is now in shreds, as it might have been, had I so willed, any time during the last dozen years. Again, Principal Forbes makes an assertion regarding his tutelage of Agassiz; the assertion is left uncontradicted; it must therefore be accepted as true, and I am unjust because I do not so accept it. Thirteen years ago, however, I was in possession of a diametrically opposite assertion from M. Agassiz. Quite as distinctly, though not so specifically, he writes thus within the present year. "When Forbes came to visit me upon the glacier of the Aar, he knew not only everything that I had done, but also my plans for the future. When he left he positively declined to express any opinion concerning glacier phenomena, under the plea that he only came to gratify his curiosity, and had no intention of following up the subject, as he had no desire to be involved in the controversy then raging

regarding the former extension of glaciers.\* When he showed his hand I did not enter into a protracted discussion, but simply made a statement of facts and let the matter rest. . . . When I look," adds M. Agassiz, "on the whole transaction it seems incredible. There is in it no vestige either of the gentleman or the honest investigator."

With statements of this character confronting the assertions of Principal Forbes, the proper course for me was to ignore assertions on both sides, and to confine myself to demonstrable facts. This I accordingly did.

With regard to Mr. Tait's criticism of my "popular" writings it has, of course, nothing to do with his defence of Forbes, but is the product of mere ignoble spite. He asks me to reply to him not according to the letter, but according to the spirit of his attack. If I might use the expression I would say, "God forbid!" for how could I do so without lowering myself to some extent to his level. The antecedents of Mr. Tait with reference to me are pretty well known. When I sought to raise from the dust a meritorious man whose name is now a household word in science, who has been elected by acclamation a member of the French Academy, and who has received the crowning honour of the Royal Society—when I sought to place Dr. Mayer in the position which he now holds, and from which no detraction can remove him, it was Mr. Tait who, in *Good Words*, charged me with misleading the public; who followed up his attack in the "Philosophical Magazine," and who when publicly hoisted by his own petard, retired to void his venom against me in the anonymous pages of the "North British Review." It is this man whose blunders and whose injustice have been so often reduced to nakedness, without ever once showing that he possessed the manhood to acknowledge a committed wrong, who now puts himself forward as the corrector of my errors and the definer of my scientific position. That position is happily not dependent upon him, and his opinion regarding it, is to me, as it will be to most others, a trifle light as air. But graver considerations than mere personal ones here arise. Might I venture, Mr. Editor, to express a doubt as to the wisdom of permitting discussions of this kind to appear in your invaluable journal. Having opened your columns to attack you are, of course, in duty bound to open them to reply, but if I might venture a suggestion, you would wisely use your undoubted editorial rights, and consult the interests of science, by putting a stop to proceedings which dishonour it. An illustrious person writes to me thus:—"I have just read Professor Tait's letters in NATURE, and feel a recurrence of that pain which similar communications once inflicted on myself—pain felt, not on my own account, for I knew that the attacks would no more sully me in the opinion of those whom I loved and respected, than they did in my own opinion; but pain for the wounded honour of science and the outraged dignity of scientific controversy."

JOHN TYNDALL

Athenæum Club, Sept. 16

[We deeply sympathise with Professor Tyndall's remarks on the injury done to scientific controversy by the introduction into it of personalities, and we should have made his own letter square with his canon if his reference to our duty in this matter, and his insinuation of injustice did not take the matter out of our hands. Prof. Tyndall forgets (1) that Prof. Tait's letter is an answer to a pamphlet by Dr. Tyndall, and that space was asked for it as such; and not an *attack* in the sense in which Prof. Tyndall uses the word; (2), that if the Editor were to assume the power and responsibility that Prof. Tyndall suggests, NATURE might easily fall from the position of absolute justice and impartiality in all scientific matters which it now occupies and become the mere mouthpiece of a clique.

What the Editor can do and has endeavoured to do in this case, is to guard the reputations of men of Science against the attacks of men of straw, and to see that no personalities are used; and it is under strong protest that he allows to pass in Prof. Tyndall's letter, for the reasons already stated, personalities, the equivalents of which, the Editor, in the exercise of his "undoubted editorial rights," struck out of Prof. Tait's communication.—ED. NATURE.]

\* This tallies with Forbes's own account (*Travels*, page 33). "Far from being ready to admit, as my sanguine companions wished me to do in 1841, that the theory of glaciers was complete, and the cause of their motion certain, after patiently hearing all they had to say, and reserving my opinion, &c." This reservation of opinion is probably the reticence referred to by Agassiz.