the House as the political representative of the Royal Society, for two reasons: first, because the Society has, in its corporate capacity, notoriously no political opinions to be represented; and, secondly, because we have not sent him to the House.

ALEX. W. WILLIAMSON.

High Pitfold, Haslemere, November 19.

"The Conspiracy of Silence."

THOUGH I am sorry to have misunderstood the meaning of the Duke of Argyll in his "Great Lesson," when I supposed him to accuse scientific men of virtually conspiring to suppress any unwelcome truth, I think I am not without excuse. Certainly I was not alone in the illusion, and I believe that many would even now say that the Duke of Argyll—in writing some of the passages which I quoted, and in using such phrases as "reluctant to admit such an error in the great idol," "slow and sulky acquiescence," "reluctantly, almost sulkily," "a grudging silence," not to quote any others—has certainly not expressed with felicity the lesson which he intended to inculcate. Further, in regard to the special instance brought forward by the Duke (that of Mr. Murray's paper) it does not appear to me that he has even now established his charge. The Duke states that he has seen a letter, written by the late Sir Wyville Thomson, most strongly urging Mr. Murray to withdraw the paper which he had sent to the Royal Society of Edinburgh. The Duke further tells us candidly that no reason is alleged in the letter. Hence, Sir Wyville Thomson's motive is a matter of inference only. I hope I shall not give offence to my friend Mr. Murray if I suggest that it may have been different from that which the Duke supposes. In 1877, so far as I can ascertain, Mr. Murray had not had much practice in writing papers. There is an art in this, which we have to learn by practice and the kindly criticism of our manuscripts by friends. As the best meat may be spoiled by an inexperienced cook, so the best material may be damaged by an inexperienced author. Sir Wyville Thomson would naturally feel very sensitive about any communications bearing the names of members of the *Challenger* Expedition, for if among its first-fruits had been a paper unsatisfactory either as to style or arrangement, yet controverting the deliberate conclusions of those hardly less well qualified to judge, a spirit of criticism and of distrust as to the thoroughness of the work of the Expedition would have been aroused. Of course this is an hypothesis only, which I trust Mr. Murray will forgive me for making, but I can assure him that I am conscious of my own youthful imperfections (not to mention the mistakes of maturer years), and I submit that it is at least as good as the Duke's, and more charitable to the memory of Sir Wyville Thomson.

In regard to the new case which the Duke of Argyll brings forward, and with which he connects my name, he is not quite accurate in his facts and is wrong in his inference. Mr. Guppy's paper was not "refused" by the Geological Society of London. The President has the power in certain cases, and under certain conditions, to refuse to put down for reading a paper written by a Fellow. I did not exercise that power. The Council, after a paper has been read, can refuse to print it. As Mr. Guppy's paper was never read, obviously this did not happen. Probably the circumstances were as follows,—I say probably, for I have no distinct recollection of them. Mr. Guppy's paper may have been sent, as is often done, for an informal expression of opinion as to whether the paper seemed suitable for the Society's consideration. In such case it would be shown either to one of the secretaries or to the President, and the opinion, favourable or otherwise, communicated to the author, who would then be free to act as he thought best. Now, if Mr. Guppy's paper was identical with that printed in the Proceedings of the Royal Society of Edinburgh (vol. xiii. p. 857) I have no doubt that my answer was to this effect: that it contained so much matter which belonged rather to natural history than to geology that I thought it was likely to suffer much excision before it was printed in our Journal, especially at that time, and was more suited for a Society of a wider scope than our own. I have again referred to the paper, and, without entering upon its merits, of which I am fully sensible, am still of opinion that, while it is in its place in the Proceedings of a Royal Society which includes all branches of science, it would have to be considerably abridged to fit it for those of a Geological Society. Of course that is only my opinion, but after full ten years' experience, eight of them as an officer, on the Council of the Geological Society of London, I may claim some knowledge of the principles on which that body acts. Moreover, at that time the Society was suffering from a falling off in revenue, with no corresponding decline in the number of papers which it was invited to publish. This I knew had compelled the Council to exceptional strictness. The difficulties of the Society were indeed so considerable that I commented on them in my address on quitting office in 1886, expressing at the same time my own view as to how they should be met. But though, as I have said, I have no clear recollection of the circumstances, I can speak positively of one thing, that if in any way I discouraged Mr. Guppy from communicating his paper it was not because I "smelt a heresy." It is something quite new for me to stand accused of being a prompt suppresser of heresies. My orthodoxy has not always been considered unimpeachable among the clergy, and surely my scientific papers are not generally on the side of "established views."

To conclude, the Duke still—and this is our special complaint—treats the matter rather according to ecclesiastical than to scientific methods. He is fully persuaded of the excellence of Mr. Murray's hypothesis, and considers it to be "one of those discoveries in science which are self-luminous," and "must carry conviction to all." Very well, but there are some people, not very few in number, who do not share his opinion. He cannot understand that our doubts can be due to anything else but "prepossession," which has prevented our minds from being "alive to the breadth and sweep of the questions at issue." I humbly reply that this is not the case; that we claim to exercise the right of private judgment, and decline to submit to any pope, from whatever part of the United Kingdom he may issue his Bull.

T. G. BONNEY.

Instability of Freshly-Magnetized Needles.

YOUR reviewer objects to a statement in my "Theory of Magnetic Measurements," to the effect that freshly-magnetized needles give untrustworthy readings for several minutes after magnetization (see NATURE, vol. xxxvi. p. 316). In reply to his statement that this is contrary to experience, I wish to say that it is not contrary to my experience. In working with two 8-inch needles I continually observed this phenomenon for years, and it was so marked that I could not feel satisfied to omit the precaution which the critic condemns. I know of one other observer who has had a similar experience with another needle. My needles were not very hard, and perhaps this may have had to do with the phenomenon.

It is not desirable to make any reply to criticisms, even though they seem not quite fairly taken, but it ought to be suggested that those who are unable to apply general formulæ to a special form of instrument after they have been shown how to apply them to a similar instrument might perhaps meet with more success in some other line of business.

FRANCIS E. NIPHER.

IN the passage to which Prof. Nipher refers I contrasted what seemed to me the excessive precautions prescribed in the directions for obtaining the dip with a rather rough-and-ready method of manipulation elsewhere suggested by him. That the magnetic axis of a piece of steel may shift is possible. My criticism was directed to the question as to whether, as a matter of experience, such a shift is a cause of error of practical importance in the determination of the dip. It would, therefore, be interesting if Prof. Nipher would publish the details of the observations on which his conclusion is based, so that the extent to which a measurement of the inclination may be rendered untrustworthy by not waiting for some minutes after magnetizing the needle may be in evidence. Meanwhile it may be well that I should define my own views on the matter.

On looking through the observations made in the magnetic survey of Missouri, which Prof. Nipher is conducting, I find that the dips obtained with different needles vary widely. Thus, taking the last Report to which I have access, in which the work of the year 1881 is described (Trans. Acad. Sci. St. Louis, vol. iv. No. 3, p. 480), the dip was determined with two needles at fifteen stations. At seven of these the difference between the results obtained by the two needles was equal to, or greater than, 4′. At one station it was 24′·8, and at others 17′·2, 11′·7, 9′·4, and 8′·9 respectively. If these are examples of trustworthy readings (and from their publication we must suppose that they are so), and if the differences obtained when the observations are untrustworthy on account of the shift of the magnetic axis are greater