find that this is the name now generally given by spongologists to the genus in question, and for very good reasons. I have merely followed precedent, and in any case I fail to see what the nomenclature of this genus has to do with the question at issue.

What Dr. Bidder means by saying that the Suberitidæ have triaxon spicules I do not know. I can only say that Suberites has not. In his reference to the "innumerable 'species' of sponges which infest our books," he seems anxious to belittle the work of "classifiers," but if he would pay more attention to this work he might perhaps come to realise that it has plainly demonstrated that sponge-spicules, like other organic products, have a long evolutionary history behind them, that they can be arranged in evolutionary series, and that in their individual development they may actually recapitulate their ancestral history. It is perfectly useless for any one to attempt to discuss this subject merely on the basis of acquaintance with a few of the simpler spicule forms.

ARTHUR DENDY.

King's College, Strand, London,
March 3.

The Action of Silica on Electrolytes.

THE questions raised in Prof. Mukherjee's letter of January 31 are so important that I should like to reply briefly.

(1) I have not found any reason to depart from the views expressed in our paper in the Transactions of the Chemical Society (T. 1923, 123, 2027) that pure silica does not exhibit any absorbing or reacting power towards acids. I have made experiments with commercial "silica gel" and N/500 hydrochloric acid, and at first obtained an indication of absorption, but soon found that this was due to some impurity, as the specific resistance of distilled water was lowered from 500,000 to 35,000 ohms on addition of the gel. It was therefore purified by washing once with hydrochloric acid and then repeatedly with water until the specific resistance rose to 350,000 ohms, and finally dried and heated to 180°. After this treatment, no absorption of acid could be observed, the P_H of the acid being the same (2·69) before and after the addition of 10 per cent. of the gel. An experiment with oxalic acid, using gel which had been purified with nitric acid, also gave a negative result.

(2) It is not clear how previous treatment with acid can destroy any absorbing properties that silica possesses, and I presume that Prof. Mukherjee's hydrated substance comes in contact with acid at some stage of its preparation. However, to make sure, the gel purified with hydrochloric acid was fused with sodium carbonate and the residue tested with silver nitrate; no chloride could be detected.

(3) I do not quite see Prof. Mukherjee's difficulty in understanding the experiment in which we found that, whilst pure silica reduced the P_H of sodium chloride solution to 3·96, the impure silica (which contained alkali) only did so to 5·55. The concentration of the silica suspension used was 20 grams per litre, and it seems quite reasonable that 20 grams of an impure alkaline material should neutralise 10^{-4} grams of hydrogen-ions.

(4) In this connexion I may point out that the hydrogen-ion concentration of a weak acid solution (*e.g.* N/5000 hydrochloric acid) falls to about half its calculated value when left for a few minutes in an ordinary glass vessel. No change is observed if the vessel is coated internally with paraffin wax or is made of silica. Filter papers also must be used with caution, as all those tried change the specific resistance and P_H of distilled water.

This problem is of great interest to us, and if Prof. Mukherjee could at any time spare a small quantity of his pure hydrated silica, we should very much like to make experiments with it.

A. F. JOSEPH.

Wellcome Tropical Research Laboratories,
Khartoum, February 21.

The Reported Anti-Relativity Experiment.

UNFORTUNATELY, owing to travelling delays, a modified proof of my response to an editorial inquiry (appearing in NATURE of March 21, p. 433, col. 1) did not reach the printers in time. The experiment reported as having been attempted by Prof. Michelson and Dr. Silberstein can scarcely have been conducted in water, though water-pipes were used. It may probably be conveniently regarded as a large-scale reproduction of an experiment by an Italian professor who inverted my whirling disk experiment on ether (Phil. Trans., 1893) by mounting the whole of the apparatus on a turn-table, including source of light and receiving camera, and looking for a shift of interference bands photographically. In the experiment now reported the turn-table was apparently replaced by the earth. I suggested such an experiment on p. 151, vol. 189, of the Phil. Trans. for 1897. If, as is probable, a positive effect can ultimately be securely demonstrated, it will be for relativists to say whether their position is at all affected; or whether the loop-hole—that rotation is exceptional, because in rotation matter is moving oppositely on opposite sides of the axis—is acceptable.

It may be convenient to reproduce the passage from Phii. Trans., 1897, referred to above:

"It is to be observed that since a motion of the disks relatively to the observer and the light causes no effect, the ether being stationary, it follows that a motion of the light and observer would produce an effect, since they would be moving relatively to the ether. Hence if, instead of spinning only the disks, the whole apparatus, lantern, optical frame, telescope, observer and all were mounted on a turn-table and caused to rotate, a reversible shift of the bands should be seen. . . . My present optical apparatus mounted on a turn-table revolving 4 times a minute should show something, viz.: $\frac{1}{100}$th band shift each way. . . . If the ether is stationary near the earth, that is, if it be neither carried round nor along by that body, then a single interference square, 1 kilometre in the side, would show a shift of rather more than one band width, due to the earth's rotation in these latitudes (see p. 772, Phil. Trans., 1893).

"But as the effect depends on the area of the square, a size of frame capable of mechanical inversion is altogether too small; there may, however, be some indirect ingenious way of virtually accomplishing a reversal of rotation—something, for instance, based on an interchange of source and eye—and if so, it would constitute the easiest plan of examining into the question of terrestrial ether drift."

OLIVER LODGE.

The Glow of Phosphorus.

THE process of the slow luminous oxidation of phosphorus presents anomalies which are still incompletely understood. Thus, the non-occurrence of a glow in pure oxygen until the pressure is reduced to about 500 mm. of mercury, or an equivalent dilution with an inert gas is made, is very striking. No less difficult to explain is the ability of traces of certain vapours to inhibit the luminosity. These