

THE circumstances spoken of by Prof. Young, as alluded to in the accompanying letter, tell of special solar activity at the time of magnetic disturbance, observed solar paroxysms occurring apparently in correspondence with magnetic movements; but the question whether definite connection exists, is the really critical point, as in the Carrington observation of 1859. Prof. Young himself says ("The Sun," p. 159):—"So far as appears, the magnetic action of the sun was instantaneous. After making allowance for longitude, the magnetic disturbance in England was strictly simultaneous, so far as can be judged, with the spectroscopic disturbance seen on the Rocky Mountains." (The italics are mine.) Without being over-critical, it may be remarked that the terms "instantaneous" and "strictly simultaneous" are somewhat strong, in the circumstances of the case.

Feeling that too much importance had been by various writers attached to the Carrington observation, I may have been led to the expression of a too pronounced opinion thereon. Rather it might be said that direct connection is not proved. It is to be remembered that the cases of recorded occurrence together of solar and magnetic phenomena are few, whilst solar change (such as is sometimes actually observed, or as is remarked in the changed solar appearance from day to day) without magnetic action, and very frequently magnetic action without recorded solar change, both occur in greater degree than, on the supposition of direct connection between the two classes of phenomena, would be expected. Prof. Young, indeed, further says:—"No two or three coincidences such as have been adduced are sufficient to establish the doctrine of the sun's immediate magnetic action upon the earth, but they make it so far probable as to warrant a careful investigation of the matter—an investigation, however, which is not easy, since it implies a practically continuous watch of the solar surface." One main difficulty is here pointed out. Continuous magnetic registration is easily maintained, but how far the observation of solar change is adequate (in spite of the numbers of observers) for the purposes of such an inquiry is possibly somewhat doubtful. The problem of a sufficiently comprehensive and satisfactory comparison of the irregularities in solar and magnetic changes is evidently one of very considerable difficulty.

Greenwich, November 14.

WILLIAM ELLIS.

#### Artificial Amœbæ and Protoplasm.

I REVIEWED in NATURE, No. 1251, Prof. Bütschli's recently published work "Mikroskopische Schäume und das Protoplasma." The book is distinctly polemical, and on pages 5 and 6 the author refers to his own, and his colleague Prof. Quincke's work, and states his indebtedness to the latter's investigation upon physical emulsions, but accuses him of having adopted his own view as to the structure of protoplasm, and that without acknowledgment.

"Ich habe Herrn Kollegen Quincke, bevor er seine Hypothese der Plasmabewegungen veröffentlichte, mehrfach meine Ansicht über die wahrscheinliche Structur dieser Substanz gesprächsweise mitgetheilt und betont, dass gewisse Eigenschaften des Plasmas wohl mit dieser Bau direct zusammenhängen dürften. Quincke hat in seiner Mittheilung von 1888 das Plasma noch als einfache Flüssigkeit behandelt, von einer Schaumstructur desselben nirgends gesprochen; wenn er später (1889), nach Veröffentlichung meines ersten Berichtes (1889) die Schaumstructur betont, so kann ich darin nur den Einfluss meiner Erfahrungen erkennen, auch wenn er derselben in dieser Publication, welche über das Plasma und seine Bewegungsercheinungen handelt, nirgends gedenkt."

(Trans.)—In the course of conversation, and before he published his hypothesis of protoplasmic movement, I frequently mentioned my view as to the probable structure of this substance to my colleague Quincke, and I emphasised the probability of a direct relation between certain properties of the plasma and this structure. In his note of 1888 Quincke still treated the plasma as a simple fluid, and nowhere made mention of the foam-like structure. When, later on, in 1889, after the publication of my first report, he emphasises the foam structure, I cannot but recognise the influence of my own experiences, though he makes no mention of them in this publication, which treats of the plasma and of the phenomena of its movement.

In NATURE, No. 1253, a letter appeared from Prof. Quincke, stating that he "was the first to point to the foamy nature of protoplasm, which was later on further investigated by Prof. Bütschli."

Prof. Quincke is evidently annoyed that his prior claim to the discovery, if discovery it be, was not made clear by me in the review. But my duty as a reviewer was with Prof. Bütschli, whose views as to the foamy nature of protoplasm I sketched to the best of my ability, and I ventured to criticise them adversely. If Prof. Bütschli was not the first to describe the foamy nature of protoplasm, and if he was anticipated by Prof. Quincke, then it is the latter's duty, not mine, to make this clear. I could not possibly be expected to deal with such a controversy in a review, for such an extended historical inquiry as this would imply, would hardly have found acceptance.

As Prof. Bütschli distinctly states that before 1889 Prof. Quincke looked upon protoplasm as a simple fluid, the latter, in order to establish his position, has only to send definite quotations from one of his publications prior to this date, in which it is clear that the foamy nature of protoplasm was described by him.

I scarcely think that Prof. Quincke can himself have read my review, for had he done so he would hardly have accused me of slighting his well-known and valued scientific work. Prof. Quincke charges me with calling "his investigations" "toys for the physicist." I never referred to him at all in this connection, but spoke definitely of the preparations of foam as manufactured by Prof. Bütschli. I moreover would point out to Prof. Quincke that we cannot compare an "investigation" with a "toy," for one is an *action*, the other a *thing*.

I regret exceedingly that the "Q" in Prof. Quincke's name appeared as "N," and take to myself the sole responsibility. I write the capital "Q" not unlike an "N," and omitted to notice the mistake in the proofs.

JOHN BERRY HAYCRAFT.  
Physiological Laboratory, University College, Cardiff.

#### THE ROYAL SOCIETY CLUB.

THERE are not many social institutions which can point to an antiquity of a century and a half, and this is what the Royal Society Club was able to celebrate on Thursday, the 16th instant.

The club is almost, if not quite, the oldest club in existence. The Dilettanti Society, which was founded a year earlier, in 1742, is not a club, and has, from the first, imposed a fine on any of its members who should apply that designation to it.

The Royal Society Club was formally inaugurated on October 27, 1743, but its very act of inauguration recognises the existence of a still earlier body. This "Memorandum of Association" is headed as follows: "Rules and Orders to be Observed by the Thursday's Club, called the Royal Philosophers."

We hear of the Virtuoso's Club, meeting on Thursdays, among the clubs of London in 1709, and in the year 1742 the club was described by Hutton as "Dr. Halley's Club." It is possible that the inaugural meeting of October 27, 1743, may have been the reorganisation of the club after Dr. Halley's death in the previous year.

The title of "Royal Philosophers" lasted till 1786, when the dinner bills were charged to "the Royals." The full title Royal Society Club was adopted later.

The history of the club was drawn up in 1860 by Admiral W. H. Smyth, and privately printed, under the title of the "Rise and Progress of the Royal Society Club." Many interesting particulars may be gathered from this compilation.

At the very first, Fellowship of the Society was not a necessary condition of membership of the club, as it now is. Mr. Colebrooke, who was treasurer of the club in 1743, was not elected into the Royal Society till 1755.

The meetings were at first held at the Mitre Tavern in Fleet Street, for forty years from 1743. The club then moved to the "Crown and Anchor" in the Strand, where it remained until 1848, when it went to the Freemasons' Tavern. On the removal of the Society to Burlington House in 1857, the club followed it westwards to the Thatched House Tavern, and subsequently to Willis's Rooms. On the final closing of the last-named