

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 Collegen Quincke, bevor er seine Hypothese der Plasmabewegungen veröffentlichte, mehrfach meine Ansicht über die wahrscheinliche Structur dieser Substanz gesprochen mitgeteilt 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

establishment, in 1889, the club migrated to Limmer's Hotel, where it now meets.

As the club grew older, the price of its dinners grew with it, from "one shilling and sixpence, for eating," in 1743, to ten shillings in 1843, at which latter price it has remained ever since. The time of dinner has also changed first from 1 o'clock to 2, and then successively to 3, 4, 4½, 5, 5½, 6, and 6½, the time of serving now.

The bill of fare for the commemorative dinner last Thursday was copied, spelling and all, from the earliest *menu* preserved, that of March 28, 1748, and the price to the members was 1s. 6d., the same as in the earliest days of the club.

The bill of fare was as follows:—

Two dishes Fresh Salmon, Lobster Sauce.
Cod's Head.
Pidgeon Pye.
Calve's Head.
Bacon and Greens.
Fillet of Veal.
Chine of Pork.
Plumb Pudding.
Apple Custard.
Butter and Cheese.

The members are indebted to the managers of Limmer's Hotel for the readiness with which they entered into the project of reproducing a dinner on the ancient model.

As the month was November, salmon was not to be had, so that other fish was substituted. An important addition was made to the *menu*, for a haunch of venison was presented to the club by one of its members.

In early days whole bucks, haunches of venison, turtles, and barons of beef were not unfrequently presented, the donors being in each case elected honorary members for the then current session.

These contributions became rather inconvenient, and on July 29, 1779, it was "resolved that no person in future be admitted a member of this Society in consequence of any present he shall make to it."

The club consists of fifty ordinary members, and this number is increased by *ex officio* members (present or past office-bearers in the Royal Society) and by a few honorary (octogenarian) and supernumerary members, until the total in 1893 has reached sixty-one. Of these forty-four were present on the 16th, with twenty-three guests, making a total of sixty-seven.

From the earliest times each member of the club has had the privilege of bringing one guest with him, the President for the day being not limited to one. This practice of bringing guests has been generally carried out, and a study of the list of visitors given in Admiral Smyth's "History" shows that many of the leaders of European science have at various times entered their names in the club records. Berzelius, Cuvier, Gay-Lussac, Linnæus, and Volta were guests of which any club may justly be proud.

We may also fairly assert, in conclusion, that since the middle of the last century, there are but few names really prominent in British science which do not appear in the list of ordinary members of the Royal Society Club at some time of its existence.

THE DE MORGAN MEDAL.¹

THE duty has this year devolved upon the Council of making the fourth triennial award of the medal which was instituted in memory of our first President, the distinguished logician and mathematician, Augustus De Morgan. In making their award, the Council are not restricted in their choice to mathematicians of this country, or to the recognition of excellence in any

¹ Address to the London Mathematical Society, on the occasion of the presentation of the De Morgan Medal, November 10, 1893, by the President, A. B. Kempe, F.R.S.

particular branch of mathematical science. It will scarcely, however, be imputed to them that they have been influenced by feelings of patriotism rather than by scientific impartiality in having selected as the first three recipients of the medal, Prof. Cayley, Prof. Sylvester, and Lord Rayleigh. The position of those eminent mathematicians suffers no depreciation, if our survey is extended beyond the borders of our own country. On the other hand we shall, I think, be equally exempt from adverse criticism in the choice we have this year made of Felix Klein, Professor of Mathematics in the University of Göttingen, as the next recipient of the honour which we are privileged to confer.

Prof. Klein, who has for many years been enrolled in our books as an honorary member of our Society, has attained the highest distinction as a mathematician. In estimating the value of his work, a mere consideration of the advance due to him in our knowledge of the details of special subjects would be sufficient to place him in the first rank; the wide influence of his work must be apparent to anyone who studies the memoirs of writers, of whatever country, on those subjects to which he has set his hand. Let me in particular refer to his contributions to the geometry of complexes, and to non-Euclidean geometry, to his memoirs on the theory of equations, on the transformation of elliptic functions, on the general theory of functions, especially in exposition and development of Riemann's theory, to his discussion of Riemann's surfaces, and, in more recent times, his researches on Abelian and Hyperelliptic functions, to his treatment of the polyhedral functions, automorphic functions, and of the elliptic Modular functions, the last of which is expounded in the treatise by Fricke on the subject. One must not forget to record the fact that his important memoir on the transformation of elliptic functions in the *Mathematische Annalen*, vol. xiv., was preceded by a communication made to our Society in 1878; Prof. Klein thus doing us the honour of indicating in advance the principal results he had obtained (*Proceedings*, vol ix. p. 23).

But, in the necessarily brief remarks to which I must limit myself this evening, to indicate Prof. Klein's claims to distinction by dwelling upon individual subjects which he has treated, would, I think, be wanting in perspective and proportion. Great as is the reputation which he has acquired in connection with particular branches of mathematical research, that which would seem to be his especial merit is the comprehensiveness of his view, and the uniformity of his treatment. For him the study of one of his special subjects is the study of all; the binding influence being the theory of discrete groups, a theory he has made his own. With this unity of conception he combines a great power of simple, elegant, and interesting expression. The expositions of his method contained in his early "Comparative Review of Recent Researches in Geometry," and his more recent "Lectures on the Icosahedron," in which the formal identity of investigations apparently the most diverse is made apparent, belong to the romance of mathematics. The important influence which his mode of investigation has had and is destined to have on the progress of the higher mathematics, the encouragement of largeness of view, rather than the elaboration of minutiae, and the stimulating influence he exercises upon pupils who now hold positions of eminence in Germany, must take a foremost place among the grounds upon which we honour Prof. Felix Klein to-day by the award to him of the De Morgan Medal.

NOTES.

THE agricultural exhibit of Sir John Lawes and Sir Henry Gilbert at Chicago appears to have been much appreciated by our American cousins. The Association of American Agri-