them hitherto. *P. N. Zealandiæ* is not hermaphrodite. I examined several males, which differ in no essential points in their structure from those of *P. capensis*. Like those of *P. capensis*, they are less numerous than the females, and Capt. Hutton has been unlucky enough not to meet with any amongst the twenty specimens examined by him. The jaws of *P. N. Zealandiæ* are further, I believe, developed just as are those of *P. capensis*. At least I saw that the earliest stages corresponded, and recognised the first pair of members of the embryo in *P. N. Zealandiæ* in the stage in which they are not yet turned inwards to become foot jaws. I have prepared a more extended answer to Capt. Hutton's paper with an account of my own observations on *P. N. Zealandiæ* for the *Ann. and Mag. of Nat. Hist.*, but as this cannot probably be published immediately, I should be much obliged if you would insert this reply in NATURE.

H. N. MOSELEY,
Naturalist to H.M.S. *Challenger*

The Age of the Rocks of Charnwood Forest

IT is no doubt to be regretted that Mr. Woodward, misled by insufficient authority, should have introduced, in his excellent work on the geology of England and Wales, still further confusion into the maltreated old rocks of Charnwood Forest, but I doubt whether their age is quite so certain as Prof. Hull seems to think. I fully agree with him that there is not a particle of evidence for their Laurentian age, and that their syenites and hornblendic granites cannot be correlated with the hornblendic gneiss of the Malverns, but I must demur to his grouping them with the Cambrian rocks of the Longmynds or of Llanberis. The authority of Prof. Sedgwick is great, but it must be remembered that the term Cambrian with him included far more than in the nomenclature of the Geological Survey, and I am not aware that he ever committed himself to the Charnwood rocks being equivalent to his *Lower* Cambrians. Except a slight lithological resemblance of some Charnwood rocks to those of Harlech and Llanberis, and a still slighter to Longmynd rocks, there is really nothing in favour of this special correlation. One point, however, there is which may give some clue to their age, which does not seem to have been much noticed hitherto, probably because the facts have been strangely overlooked in the Geological Survey description of the district. It is that beds of coarse volcanic agglomerate and ash abound among the Charnwood series. Further, the resemblance of the rocks as a whole (when not unusually metamorphosed) is very close to the "green slate and porphyry series" (or Borrowdale rocks) of the Lake District. Compared with the Welsh rocks, they are far more like those of Cader-Idris than of Llanberis. With these there is scarce any lithological resemblance, but if I mixed my Charnwood collection with those from the other two localities, especially the former, I should have great difficulty in separating many specimens. It seems then to me far more likely that this great volcanic activity in the Charnwood district should have corresponded in time with that in the Lake District or with some part of that in Wales, than that it should have happened in the age of the Harlech, Llanberis, and Longmynd groups, where we have no evidence of any volcanic disturbance. The argument may be summed up thus, as it seems to me :—The Charnwood rocks are old, so are both the competing groups; they are unfossiliferous, so are both; they are cleaved, so are both; they contain evidence of great volcanic action, so do the Borrowdale series, and not the Welsh Lower Cambrians. One point for the former. The general correspondence of their strike with that of the Borrowdale series under Ingleborough may also perhaps count for something.

T. G. BONNEY

St. John's College, Cambridge, November 25

THOUGH the discussion of the age of the rocks of Charnwood Forest is not likely in the present state of our knowledge to lead to any useful result, there are still a few points in Prof. Hull's letter on the subject which seem to call for remark. In the first place the late Prof. Jukes was by no means so strongly in favour of the Cambrian age of these rocks as Prof. Hull states. Prof. Jukes' words, in Potter's (not Porter's) "History of Charnwood Forest" are as follows :—"It is therefore uncertain whether they (the rocks of Charnwood) belong to the Devonian, Silurian, or Cambrian systems, the probability only being in favour of the latter." Secondly, the Cambrian of Sedgwick includes a great deal more than the Cambrian of the Geological Survey, and therefore there is not the perfect unanimity between these two authorities that Prof. Hull's remarks would lead us to believe. Thirdly, if lithological resemblance is to go for anything, it may be used directly against the Cambrian age of the rocks. On the western side of the forest we find sheets of crystalline rock and beds of highly altered conglomerates and breccias, which have a suggestive likeness to the lava flows and ash beds of the green slate and porphyry series of the Lake District. I don't say the resemblance proves anything, but it is worth quite as much as the similarity between the slates on the east side of the forest and the slates of Llanberis. Mr. Bonney has also called attention to the fact that the strike of the Charnwood Forest rocks is the same as that of the Volcanic Series in the Lake Country, when that group is last seen. Again, it is far from certain that the rocks of Charnwood Forest are all of the same age. I recollect seeing many years ago some sections (of which I am afraid I have kept no record) that seemed to show that some of the bosses of Dioritic rock near Markfield were older than the slates that surrounded them. If this be so, perhaps these crystalline hills may be the projecting points of a nucleus of similar rock that underlies the whole area, and which may be Laurentian in age. The rocks are not gneiss, but I know of no reason why the equivalents of the rocks of the Hebrides must be gneiss all the world over; they are, however, rich in hornblende, and so are the Hebridian rocks. With all these possibilities before us, I am afraid it will be hard to arrive at that enviable state of security which Prof. Hull seems to have been in when he penned his letter.

A. H. GREEN

I AM pleased to find in NATURE, vol. xv., p. 78, a letter from Prof. Hull, with reference to the age of our Charnwood Forest rocks. He writes against their assignment by Mr. H. B. Woodward, in his "Geology of England and Wales," to the Laurentian period (see p. 24).

But, in fact, as Prof. Hull himself points out, we also find on p. 30 a statement that part of the series may belong to the Cambrian epoch.

It would appear that as Mr. Woodward is not personally acquainted with the region, he has endeavoured to give the views of the various authors whom he knows to have written on the subject, and as these are conflicting, and based upon little personal work, it is no wonder that he has been led astray.

I do not think sufficient importance has been attached to the study of this isolated outcrop of old rocks. We can trace its continuation to the south and south-east for a considerable distance, and I would venture to suggest the possibility of a flexure or spur in this direction connecting with the old palæozoic ridge for which we have lately been fishing in the Wealden. In my "Geology of Leicestershire and Rutland," which will shortly be published, there will be found some fine photographs of the principal quarries and natural outcrops of the Charnwood rocks; and I have there given the reasons which induce me on the whole to refer the main mass of the rocks to neither Laurentian nor Cambrian, but to the *Silurian* period. The evidence is but scanty however, but a balance of probabilities at the best. As to Sedgwick's determination of the region, we must remember that much that he then called Cambrian has since been assigned to Lower Silurian.

WM. JEROME HARRISON

Town Museum, Leicester, November 24

Minimum Thermometers

SOME time ago a correspondence appeared in NATURE (vol. vi. pp. 122, 142, 221) on the subject of moisture deposited in minimum thermometers exposed on the grass. As I was at the time much annoyed with this myself I took up every hint I could get in the matter, though I must confess with indifferent success. I tried for a long time india-rubber packing, with sealing-wax, &c., of varying coats, as advised, but still moisture insinuated itself.

At last I bethought myself of a *cork* packing. I cut a piece of cork so as to fit tightly round the neck of the thermometer tube, then inserted the tube and packing into the glass case—the cork packing being about a quarter of an inch long. The exposed end of the cork I covered with two or three coats of asphalte, as used on microscopic slides. At first a slight bubbling was seen through the asphalte, but soon disappeared, and a fine uniform surface at last set in. The thermometer has now been in use for several months, and not the least trace of moisture has ever been seen within the cases, although moisture has been abundant, especially for the last three months. The process is simple enough, and I venture to send it to you, hoping that it

may answer the purpose for others as it has now for some time answered mine. THOMAS FAWCETT

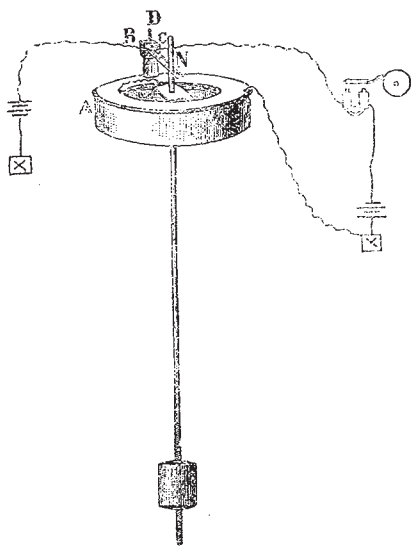
Blencowe School, Cumberland, November 6

Electric Motor Pendulum

THE following very simple apparatus may I think be of use in any laboratory or other place where at times it is necessary to have a pendulum beating seconds in order to give the time for any experiments needing it.

It consists of a Siemens' galvanoscope, A, to which is attached the pendulum; the needle N, preferably with platinum contacts, works between two platinum wires, B and C, with a small amount of play; these platinum wires are insulated from one another by being fastened into a piece of ebonite, which works on a pivot D. The needle is connected by its support to one end of the coil of the galvanoscope, the other end being to earth. To the wires, B and C, are connected the opposite poles of a small battery, the centre of the battery being to earth.

The action of the instrument is as follows:—On slightly oscillating the pendulum the needle N makes contact between B say, and the coil, the magnet being so arranged that the needle then deflects towards B, thus carrying with it the movable contact wires until the pendulum reaches its limit of oscillation, when it



falls, breaks contact with B and makes contact with C, which thus tends to pull the needle over to C, and so on; in this way the pendulum receives at each oscillation the impulse necessary to overcome the forces tending to stop it; and thus will keep oscillating as long as the battery supplies the motive power. For small arcs the beat is *not affected by variation in battery power.*

In the circuit of the battery we can introduce an electromagnet which at each contact of the pendulum on one side will make a stroke on a bell, or indeed by a detent will move by a small train of wheels the hands of a clock. If the pendulum is made to beat half seconds, then the contact being made alternately on each side, the bell stroke would beat seconds. We could of course introduce any number of arrangements of this sort at any intervals along the circuit, and so move any number of clocks at different positions in a large establishment, only one pendulum being requisite to control the whole set.

P. HIGGS

PROF. YOUNG ON THE SOLAR SPECTRUM

THE paper of Prof. C. A. Young read at the last meeting of the American Association for the Advancement of Science describing his recent measures of the displacement of the D and other absorption lines at the receding and approaching limbs of the sun, has a double interest.

By careful measures to which all the necessary corrections have been rigorously applied, obtained by using a diffraction grating in combination with a prism, Prof. Young deduces from observation of the D lines a value of 1·42 ± 0·035 miles per second for the surface velocity of rotation at the sun's equator Direct observation of the motion of spots gives 1·25 miles per second, and the author thinks that the difference of these two values being so many times larger than the probable error of the spectroscopic method, the result of which agrees so well with Vogel's result, indicates that a portion of the displacement observed is produced by the difference in the angular velocity of rotation of the solar atmosphere which causes the absorption lines and the underlying luminous surface, and the sign of the difference would indicate that the atmosphere is swept forward with the greater velocity of the two.

This conclusion is itself one of great interest, but for many persons the fact that it is based on the acceptance of Doppler's theory will be a source of satisfaction as indicating that the recent disputes as to its soundness are beginning to be considered settled and in its favour, as at any rate a near approximation to the truth.

One of its first assailants, on mathematical grounds, was Prof. Petzvall. But, as was pointed out by Mach in a "Contribution to Doppler'sTheory," published at Prague, in 1874, his main argument fell beside the mark, while the only one which touched it went to prove that for comparatively small velocities of translation in the source of sound or light, compared to the velocity of wave transmission, Doppler's theory was a correct approximation.

More recently Van der Willigen's mathematical objections have been apparently fairly disposed of by Mr. Christie, while the discrepancy that Father Secchi has lately pointed out between the measures of displacement of spectral lines in the case of certain stars as observed by Mr. Huggins on the one hand and at Greenwich on the other, does not really affect Doppler's theory at all, but only the degree of certainty with which it can be applied to the determination of stellar motion. But the facts are not as Father Secchi represents them. He points out, in a list of thirteen stars, that the displacement in the case of some five stars as observed by Huggins is in the opposite direction to that observed at Greenwich. But the Greenwich observations that he takes are some early tentative observations. We have taken the trouble to refer to the most recent Greenwich measures, and find that of the five disagreements insisted on only one holds.

INDIAN GEOLOGY

THERE seems to be a very pretty quarrel just now—and one urged with the usual absence of acrimony in scientific controversies—as to the age or ages of an important group of rocks in Her Majesty's Indian empire.

For years it has been known that while a large mass of the rocks forming the Peninsula of India are unfossiliferous, there is also in that country an extensive series of beds the predominant, and frequently the only, fossils of which are vegetable remains. These beds were often spoken of as the Plant-beds of India. Among the flora certain forms which used to be called *Palæosamiæ*, now *Ptilophyllum*, were pretty generally distributed, while the genera *Schizoneura* and *Glossopteris* were found in lower portions of the series.

On the evidence of the first-named fossils and several others, a Jurassic age was assigned to the containing beds, while the identity of the *Glossopteris* with Australian forms involved these Indian beds in the dispute as to whether the coal-rocks of that country were likewise Jurassic or really carboniferous.

One portion of the Indian plant beds contained a limited terrestrial fauna which on high authority (Hux-