morphological characters, are unmistakably identical with the hyphæ found in specimens of the non-choleraic intestine, lung, skin, kidney, mesenteric glands, preserved, as stated above, over the summer.

In conclusion I wish to say that I shall be most happy to place at the disposal of Messrs. Roy, Brown, and Sherrington, as also of Messrs. Vines and Gardiner, the materials or sections, mounted and stained, of the various non-choleraic tissues in which are present the mycelial threads of common mould identical with the Cambridge cholera fungus. E. KLEIN

94, Philbeach Gardens, Earl's Court, December 18

The Longitude of Rio

MAY I ask for a few lines in which to correct an erroneous impression naturally made by a sentence in my recent paper on "Ten Years' Progress in Astronomy," which you have honoured me by reprinting in NATURE. The sentence relates to the longitude of Rio; and although it does not really assert that the error in this longitude was first detected and corrected by our American naval officers, yet I must frankly admit that the connection and form of expression are such that this would be the natural, though incorrect, inference. The fact is that Admiral Mouchez and his coadjutors in the French Navy had already, by their chronometric and other work, brought the uncertainty to very narrow limits (say ± 2s.) before the telegraphic campaign of the Americans. The history of the case is peculiar, but too long to be given here: it affords an excellent example of the uncertainty of longitudes based on lunar observations.

The misleading form of the sentence is due to a little carelessness on my part in cutting down the much more extended statement I had made in the first draft of the paper. The available limits of time and space compelled me to compress my material to the utmost.

I cheerfully make this correction in justice to Admiral Mouchez, who has called my attention to the matter.

Princeton, N.J., December 9 C. A. YOUNG.

An Error in Maxwell's "Electricity and Magnetism"

MANY of your readers will be aware that Maxwell (ii. § 544) deduces the equations of induction of currents from the laws of electro-dynamics with the aid of the principle of energy, using a proof taken from Helmholtz. I find that this proof is erroneous; and, as a point of considerable physical interest is involved, I wish to call attention to the error in your columns.

We suppose two circuits carrying currents to be moving relatively to each other. Let R_1, R_2 be the resistances, I_1, I_2 the currents, A_1, A_2 the electromotive forces of the batteries, and $-I_1I_2\frac{dV}{dt}$ the rate at which work is done by the external forces which are moving the circuits. Then $A_1I_1 + A_2I_2$ is the rate at which the batteries are doing work, and $R_1I_1^2 + R_2I_2^2$ is the rate at which energy is being changed into heat in the wires. So Maxwell says we have—

$$A_1I_1 + A_2I_2 - I_1I_2\frac{dV}{dt} = R_1I_1^2 + R_2I_2^2 \quad . \quad . \quad (1)$$

and it is this equation that is wrong. He has omitted to take into account the change in the electro-kinetic energy which is taking place. If, for instance, the two batteries were suddenly thrown out of the circuits, the quantity of heat that would afterwards appear, either in the wires or in the form of sparks, would depend on the relative position of the circuits. And the energy that would then appear as heat previously exists in the form of electro-kinetic energy.

Let M be the coefficient of mutual induction. Then, if we neglect the rate of change of the currents, the rate of increase of the electro-kinetic energy is $I_1I_2\frac{dM}{dt}$. So, instead of (1), we should write—

$$A_1I_1 + A_2I_2 - I_1I_2\frac{dV}{dt} = R_1I_1^2 + R_2I_2^2 + I_1I_2\frac{dM}{dt} \quad . \quad (2)$$

If we assume the accepted equations of induction of currents, viz.—

$$\left.\begin{aligned} A_1 &= R_1I_1 + I_2\frac{dM}{dt} \\ A_2 &= R_2I_2 + I_1\frac{dM}{dt} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (3)$$

neglecting, as before, the rate of change of the currents, we see that $\frac{dM}{dt} = \frac{dV}{dt}$.

And therefore the decrease of electro-kinetic energy is equal to the work done by outside mechanical forces on the system. This result was long ago obtained by Sir William Thomson, as is indeed noticed by Maxwell in this very article.

Notwithstanding the use of the incorrect equation (1), Maxwell obtains a correct result. In fact, he falls into a second error which exactly compensates for the first. He supposes I_2 to be very small compared with I_1, and says that we may then with sufficient accuracy put $A_1 = R_1I_1$ in (1). But by (3) we see that the term thus neglected is $I_1I_2\frac{dM}{dt}$, which is not negligible.

As I have not had access to Helmholtz' original memoir, I cannot say whether Maxwell has correctly transcribed his proof.

JAMES C. MCCONNEL

St. Moritz, Engadine, Switzerland

Seismometry

IN reply to my letter answering Prof. Milne's assertions (NATURE, Nov. 25, p. 75), Mr. T. Gray (his associate in seismometric work) says nothing in support of these, but attacks me on two distinct and quite irrelevant issues. The tone of Mr. Gray's letter (Dec. 9, p. 126) is unusual: as to that no answer is necessary; but the two questions of fact raised by him require reply.

(1) Mr. Gray writes:—"He [Prof. Ewing] says, or leads one to infer, that he introduced horizontal pendulums in seismology." On the contrary, what I *have* said (in my memoir on "Earthquake Measurement," Tokio, 1883, p. 21) is this:—

"It appears that the earliest attempt to apply the horizontal pendulum to the measurement of earthquake-motions was made by Prof. W. S. Chaplin, of the University of Tokio, about 1878. His apparatus consisted of a wooden rod, free to turn about a vertical axis, and carrying at its end a rigidly attached block. It was intended that the motion of the earth should be recorded by a tracing-point fixed to the block, writing on a smooth surface fixed to the earth below it. There was no multiplication of the motion, and either for this reason, or because friction was not sufficiently avoided at the joints and pointer, no results were ever obtained, and the apparatus was abandoned."

The passage Mr. Gray alludes to as having been "read in my presence" was a casual reference by Prof. Milne to these unsuccessful experiments. Prof. Chaplin, their author, has himself written to me:—

"I certainly think you were the first to use successfully a seismograph depending on the principle of the horizontal pendulum. I believe the records obtained by you with this seismograph were the first obtained in Japan (and probably in the world) which showed the motion of the earth during an earthquake from beginning to end of the shock. I cannot better mark the effect which the first record produced than by relating my own experience. I was, up to that time, working on an instrument for determining the velocity and direction of an earthquake; and my design was founded on the idea that an earthquake began with a sudden and violent *shock*. Your records showed (I believe for the first time) that an earthquake often began with an almost imperceptible motion, which increased in amplitude and might have many maxima; hence my machine would have been useless had I completed it."

What I do claim in this matter is that I succeeded in constructing the earliest successful seismograph capable of making absolute measurements of the horizontal motion throughout an earthquake, in conjunction with the time, and giving records from which the amount, direction, velocity, and acceleration of the successive movements could be, and *were*, for the first time determined. The earliest records, referred to by Prof. Chaplin, were obtained in November 1880, and are described in the *Transactions* of the Asiatic Society of