temperature of the air with which it is in contact. It is hard to suggest a better arrangement for getting some notion of the low temperatures which the upper air appears to delight in producing and maintaining in the most trying circumstances; nevertheless, it is easy to show that the temperature of the instrument will differ from that of the air by a variable interval depending on the speed of the balloon, the density of the air, and the intensity of the radiation from earth, clouds, balloon, sun, air, and vapour.

Suppose I wish to take the temperature of the feed air below the fire-bars of a furnace. If the current of air were sufficient, and the screening of the thermometer almost perfect, I might get a close approximation, but if the draught relaxed in speed or density, or if the screening became less perfect, the thermometer would respond to the radiation by which it was surrounded and rise above the temperature of the air. A *ballon sonde* is such an apparatus. It is in a warm situation, but is surrounded by an intensely cold medium. It is a speck extremely close to a great warm planet, and bathed in his radiations and reflections—to say nothing of sun-rays, which sometimes complicate the problem. The screen is open to the earth below and to the balloon above, and the instrument, though screened itself, follows in the wake of the unscreened balloon and is fanned by the air that has passed over its heated surface.

In the ascent the thermometer reading falls briskly, and soon reaches a figure which may be 100° or more below what it would stand at if it were screened *from air currents* for a few minutes; but this gap between the temperature of the instrument and the temperature proper to its position cannot be extended indefinitely. The up-rush of the balloon attains a maximum velocity and declines, and the density of the air also rapidly diminishes. When the receipt by radiation equals the loss by conduction, the thermometer has reached its minimum, and enters the so-called "isothermal layer," the regularity of the occurrence of which on the traces is due to the similarity of pattern of balloon and outfit and of the other circumstances. I know that Mr. Dines contends that speed upwards or downwards can have no effect on the thermometer, but he takes no account of the circumstance that heat is constantly entering the instrument, and that it is solely the current of air that keeps down its temperature by removing the intruding heat.

It seems a pity that the following question stands barred:—Why *is* the material *Air* so cold where the material balloon and other instruments *would be* so warm?

R. F. HUGHES.

16 Westmoreland Street, Marylebone, W., January 9.

IF all balloon ascents had been made by day, I confess that I should be inclined to agree with Mr. Hughes and think that the recorded temperatures were due to radiation, but that idea is disposed of, to my mind, by the fact that the isothermal column of air shows just as plainly in ascents made after sunset as in those made in the day. At night the thermograph must receive some heat by radiation from the earth, and lose some by radiation into space, but both amounts must be infinitesimal in comparison with that which would be given to it by the sun. If, then, exposure to the sun does not seriously alter the temperature, and it does not do so even at the greatest height provided there is a moderate amount of vertical motion, the effect of the radiation after sunset must be utterly insignificant. That solar radiation in the ordinary conditions is not important is proved by the fact that if the balloon bursts, and therefore does not float, it is not possible to say from the trace alone if the ascent was by night or by day. There have been cases in which the balloon did not burst, and the temperature at the top reached the freezing point of water. If I asserted that the rate of ascent does not matter, I must plead guilty to a mistake, but I think I said "apart from radiation," and I still believe that radiation at night to and from the bright metal of the thermograph is so trifling that the rate of ascent is of no consequence. There is also the fact that the up-trace, where the motion is comparatively slow, is identical with the down-trace where the motion is rapid.

W. H. DINES.

An Electromagnetic Problem.

SINCE the solution of the problem put forward by Prof. Comstock in NATURE of November 19, 1908, is apparently not obvious to everyone, will you permit me to point out that, so far as I can see, the difficulty arises, not from any peculiarity of the laws of electromagnetism, but from a simple misconception of the meaning of dynamical terms?

Prof. Comstock says that when an electrified sphere expands it loses electrostatic energy, but does not gain either kinetic energy (for the sphere has no mass) or magnetic energy (for the resultant field due to the motion of all parts of the sphere is everywhere zero). Now the energy of a conservative system, such as is considered, is measured by the amount of work which it can do on some external system in passing from its original to some defined final state; the amount of the work which can be done, and therefore the amount of the energy, will vary according to the external system which is chosen, and the principle of the conservation of energy will be true only if the same external system is taken in measuring the amount of work that can be done at various times during the change.

When he states that the magnetic energy of the expanding sphere is zero, Prof. Comstock is taking as his external system, on which work is to be done, a system unconnected with and independent of the expanding sphere; but the electrostatic energy of the sphere with respect to such a system is quite unaltered by the expansion, if the system is either wholly within or wholly without the sphere throughout the expansion, and the change in the electrostatic energy which ensues, if any part of the system passes through the surface of the sphere during the expansion, is independent of the discreteness or continuity of the electrification on the sphere, and perfectly consistent with the conservation of energy. Adopting such an independent system as that on which work is to be done, there is no relevant change in either the electrostatic or the magnetic energy.

On the other hand, when he says that the sphere in expanding loses electrostatic energy, Prof. Comstock is taking as the system on which work is to be done part of the expanding sphere or some system connected rigidly therewith; but then it is clear that in estimating the magnetic energy no account must be taken of the magnetic field due to the motion of this part. Leaving out of account the magnetic field due to this part of the sphere, a simple calculation shows that the magnetic field due to the motion of the rest of the sphere relatively to this part is *not* zero everywhere, but that the value of $\mu H^2/8\pi$, integrated throughout the entire field, is equal to the value of the electrostatic energy with reference to this part lost in expansion.

NORMAN R. CAMPBELL.

Trinity College, Cambridge, January 16.

Radium in the Earth.

IN the discourse entitled "Radio-active Changes in the Earth," delivered at the Royal Institution by the Hon. R. J. Strutt, and printed in NATURE of December 17, 1908, the lecturer advanced the opinion that the mineral beryl contained an hitherto unknown element from which the comparatively large quantity of helium present is generated.

This interesting and remarkable conclusion has induced me to direct attention to a statement which occurs in a paper entitled "The Heat of Formation of Glucinum Chloride," by J. H. Pollok (Chem. Soc. Trans., 1904, p. 603). Mr. Pollok prepared a large quantity of basic glucinum carbonate from 2000 grams of beryl, and during the preparation of this compound he detected the presence of another substance, the nature of which he was not able to ascertain. His statement is as follows:—"This precipitate consisted chiefly of iron, zinc, and nickel sulphides, but another substance appeared to be present; its amount was, however, too minute to admit of any satisfactory conclusion being drawn regarding it. This sulphide has also been observed by Kruss and Moraht."

PERCY EDGERTON.

The Laboratories, 61 Cornhill, London, E.C., December 31, 1908.