celestial globe. It is intended primarily for the determination of bearings by observations of stars, for use in night marching. Briefly, it is a planisphere in which the principal stars which are not too far from the equator are represented in a cylindrical projection; the star-chart is adjustable for different dates, and there is a movable celluloid protractor on which are marked the projection of the horizon and the projections of vertical circles at intervals of 10°. Following the simple instructions given, the magnetic bearing of a star, even if its name be unknown, can readily be ascertained. The operations are entirely mechanical, and anyone of ordinary intelligence should be able to determine directions with considerable accuracy. The form of projection adopted, however, has the defect of failing to give bearings of stars towards the north, and it is not very clear why the results are expressed in magnetic instead of in true bearings. A protractor adapted for southern Scotland and northern England is obtainable alternatively or separately.

LETTERS TO THE EDITOR.

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The Temperature Coefficient of Gravity.

IN *Phil. Trans.*, May 17, 1916, Dr. P. E. Shaw published an account of a research from which he deduced that the gravitational constant is increased by one part in eighty thousand when the temperature of the larger mass is raised one degree. According to Dr. Shaw's interpretation of the experiments, it is the mean temperature of the system which affects the coefficient of attraction, so that in the case of extremely unequal masses it is the temperature of the larger mass only which counts. The evidence does not seem strong enough to support so revolutionary a conclusion in view of the almost insuperable theoretical objections.

One or two of the more obvious difficulties may be formulated in a few words.

Take as an example the earth and a mass of 1 kg. Divide up the earth (ideally) into "terrestrial particles" of, say, 1 mgrm. each. When the temperature of the kilogram mass alone is raised one degree its attraction for each "terrestrial particle" should be increased proportionally by $1{\cdot}2 \times 10^{-5}$. But by the same reasoning the attraction between the kilogram mass and the earth as a whole should remain sensibly unaltered. In like manner, if we keep the kilogram mass at constant temperature and alter the temperature of the earth, the attraction between the kilogram and each "terrestrial particle" will be sensibly unaltered, while the attraction between the kilogram and the earth as a whole will have changed. This seems so essentially paradoxical that it is difficult to conceive of any supplementary hypothesis elastic enough to reconcile the contradiction involved.

The only way of avoiding this inconsistency is to admit that it is the product of the two values of a temperature-function which counts—*i.e.* that the temperature of the smaller mass is just as important as the temperature of the larger mass. Once this is admitted the experiments of Poynting and Phillips prove that no variation exists greater than 10^{-9} per degree Centigrade.

It may be argued that the temperature of the *attracting* body determines the attraction—*i.e.* that action is not necessarily equal to reaction. In addition to violating the principle of momentum, this involves the possibility of constructing a *perpetuum mobile.* An elongated body, kept hotter at one end than at the other by means of ideal thermal insulation, would experience a resultant force in the direction of its length, and could be made to do work indefinitely by harnessing it like a horse to a mill. Is anybody prepared to believe this on any but the most conclusive experimental evidence?

Again, it has been suggested by Prof. Barton that the temperature of the intervening radiation may determine the attraction. But the temperature of radiation is independent of the intensity, so that indefinitely feeble radiation would produce a finite effect.

If the intensity of the radiation is substituted as the determining factor, it implies that the attraction of two bodies is increased if a beam of light passes between them. If energy is to be conserved, this would imply that two bodies moving relatively to one another could increase or diminish the energy of a beam of light passing between them, and such a result would certainly be rather startling. Still more extraordinary would it be to find that a variation of 0·01 of a stellar magnitude on the part of the sun would change the length of the year by several minutes; yet this is what would be implied. There is no record of an appreciable change in the earth's orbit caused by sun-spots.

When one comes to examine the evidence out of which all these paradoxes arise, it can scarcely be said to be sufficient. Thus, for instance, as "indirect experimental evidence," Dr. Shaw cites Cornu, who found 5·50 for the earth's mean density from winter work, and 5·56 from summer work, a difference of 1·1 per cent. To reconcile the sign of this variation with his own temperature coefficient, Dr. Shaw suggests that the apparatus in a laboratory may have a higher temperature in winter than in summer. He can scarcely have noticed that the excess of temperature in winter would have to be some 900 degrees. Again, from Prof. Boys's work on the gravitation constant, Dr. Shaw deduces a temperature coefficient of 10^{-3}, of which, according to his own results, 98·7 per cent. must be ascribed to error. Can we have much confidence in the remaining 1·3 per cent.?

While we must all admire the experimental skill which enabled Dr. Shaw to observe a change of 0·2 mm. at either end in a range of 200 mm., using a telescope and scale (especially when we know the difficulties he had to contend with), we can scarcely be expected to make these radical changes in our theories on the strength of such a very small effect. Though his reasons for rejecting experiments which gave a negative value for the temperature coefficient were no doubt excellent, the fact that such readings occurred is a little disquieting. Again, the readings vary amongst themselves by as much as the whole effect, and one knows how misleading a mean value of, say, 176·2, 175·9, 175·75 may be when the whole residual effect is only 0·4 mm.

In conclusion we should like to express our admiration for Dr. Shaw's experimental work. We feel that as the result of such an elaborate research a null result is quite as important as, if less sensational than, a positive one. To have reduced the apparent temperature coefficient of gravity from the 10^{-3} deduced from Prof. Boys's measurements to one-eightieth of that value is certainly no mean achievement.

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South Farnborough, Hants, December 4.

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