the measurements were numerous. The proper course is to determine it by the method of least squares.

Writing f for $f(u)$ and omitting $1 + \frac{S}{F}$, (3) may by the aid of (7) be written in the form—

$$\frac{1}{f} = \frac{H}{2m}(r^3 - P_0 r).$$

This is exactly analogous to the equations used by Maxwell in the determination of the quantity A_2, which in his notation and method of development corresponds to P_0 ("Electricity and Magnetism," second edition, vol. ii. p. 100). It is unnecessary to occupy the pages of NATURE with a reproduction *mutatis mutandis* of his formulæ. We can get, as he does, a general expression for P_0 when we have n equations at our disposal, and when $n = 2$ this reduces (in the notation of Prof. Harkness) to—

$$(\alpha) \qquad P_0 = (A - A_1)/(A/r^2 - A_1/r_1^2).$$

If then in a magnetic survey observations are made at two distances at a number of stations, we should take as the final value of P_0 the mean of the most probable values found at each station. As this would be unduly laborious, we approximate. By an obvious transformation (α) becomes—

$$\log\left(1 - \frac{P_0}{r_1^2}\right) - \log\left(1 - \frac{P_0}{r^2}\right) = \log A - \log A_1$$

$$\therefore P_0(r^{-2} - r_1^{-2}) + \frac{P_0^2}{2}(r^{-4} - r_1^{-4}) + \&c. = \frac{\log A - \log A_1}{M}.$$

Thus to a first approximation—

$$(\beta) \qquad P_0 = \frac{r^2 r_1^2}{r_1^2 - r^2}\,\frac{\log A - \log A_1}{M}.$$

And if we substitute this value in the small term—

$$(\gamma) \qquad P_0 = \frac{r^2 r_1^2}{r_1^2 - r^2}\,\frac{\log A - \log A_1}{M} - \frac{r_1^2 r^2}{2}\,\frac{r_1^2 + r^2}{(r_1^2 - r^2)^2}\left(\frac{\log A - \log A_1}{M}^2\right).$$

This is the expression I gave. The effect of the small term in (γ) is, as I pointed out, less than the error of experiment, but it diminishes the difference between the rigorous and approximate values of P_0 given in (α) and (β), and it is useful in indicating the magnitude of the difference between them.

Fortunately all methods lead to (β) as a first approximation which we are agreed is close enough for practical purposes. If, however, we regard the observations as fallible, (α) gives a better value of P_0 than (14), and equation (γ) gives a closer approximation to it than (β) does.

ARTHUR W. RÜCKER.

Science Schools, South Kensington, November 24.

P.S.—It may be well to add that, although the formula for A_2 is correctly given by Maxwell in line 17, p. 101, the value of A_2 deduced below is incorrect, being really that of $2MA_2/H$. There is another misprint immediately below, δD being substituted for δQ in the second edition.

Instability of Freshly Magnetized Needles.

I SHOULD like to be permitted to support Prof. Rücker in his reply to Prof. Nipher (NATURE, vol. xxxvii. p. 77), with a few remarks on the subject of observations of magnetic dip.

The question of the degree of accuracy of dip observations is one that has been repeatedly raised and discussed. In 1864 in his report to the Board of Visitors, the Astronomer-Royal, Sir G. B. Airy, referred to the matter, and a correspondance between him and the Chairman of the Kew Committee (Mr. L. P. Gassiot) ensued, which is printed *in extenso* in the Report of the British Association for 1864, pp. xxxiv.-xlvii.

In reply to an inquiry by Mr. Gassiot as to whether the paragraph in the Report was intended to apply to dip observations made at the Kew Observatory, Sir G. B. Airy quoted the following statement by Sir E. Sabine :—"The probable error of a *single observation of the dip* with reliable instruments of easy procurement is known to be ± 1'·5. It has been shown to be so by a series of 282 observations made at Kew, employing twelve circles and twenty-four needles, all of the pattern which has been in use at Kew for several years past. The observations were made by seven different observers; the results are published in the Proceedings of the Royal Society, March 1861, vol. xi. p. 156, from entries in the Kew Observatory books, not a single observation having been omitted. The probable error ± 1'·5 may be regarded as including *constant* errors, considering the number of different circles and needles which were employed, as well as the peculiarities of different observers, of whom there were seven" (the italics are General Sabine's). The Astronomer-Royal then concluded by stating "these are the probable errors which I cannot accept as accurate."

As a result of the correspondence, a series of observations was made at both the Greenwich and Kew Observatories by the observers of both institutions, with the same Kew pattern instruments, and then Sir G. Airy wrote, in a letter dated November 15, as follows: "As regards the results of observations, those made with the Kew instruments are consistent to a degree which I never saw before; and the results for dip obtainable with the Kew dip instruments are undoubtedly more consistent and more certain than I had supposed them to be."

A similar inquiry was set on foot by Dr. H. Wild, of St. Petersburg, and in 1886 we made a large number of observations with different needles for him, the resulting error of an observation being in this case ± 1'·3. The most severe test, so far as we are aware, which has been applied to dip observation, is that recently described by M. E. Leyst, of St. Petersburg, in a quarto volume of 133 pages, published in the *Repertorium für Meteorologie*, entitled "Untersuchung über Nadel Inclinatorien."

The author discusses some 6576 observations of dip made with different instruments and needles, and determines their probable errors, which he always find small, so much so that he deduces the corrections to hundredths of a minute of arc. To quote particular cases, he determines from thirty series of comparisons between observations and the simultaneous readings of the magnetographs and the induction inclinometer, that the difference amounts to only 1'·06; and again, by comparing at Pawlowsk the fifteen needles of the three dip instruments of the Pawlowsk, Irkutsk, and Ekaterinburg Observatories (all of English make, obtained through this Observatory), he finds their mean correction to be nil.

Judging from the experience gained at Kew by the examination of probably 150 circles and 500 needles by various makers and different observers, I can thoroughly indorse Prof. Rücker's opinion that Prof. Nipher's instruments are scarcely capable of satisfying modern requirements as to accuracy, and are such that were they submitted to us for examination they would be promptly returned to their makers for adjustment.

G. M. WHIPPLE.

Kew Observatory, November 26.

Gore's Railway.

As I have had several letters concerning my use of Dr. Gore's arrangement, depicted on p. 107 of your last week's issue, perhaps I may as well say that I am aware it is commonly regarded as a Trevelyan rocker, and that I doubt not its function in that connection. This point of view is so familiar to every one, through Tyndall's "Heat," that I thought it unnecessary to mention it. But I have occasionally heard the motion of the ball attributed to the electro-magnetic action of the current on itself—which is impossible—and I thought it useful to point out that it could nevertheless be used as an illustration of electromagnetic force, provided a vertical magnetic field is applied as well as a current. I should imagine the earth not too weak to have an effect under favourable conditions; but of course such an effect would be strictly definite in direction, and reversible.

OLIVER J. LODGE.

The Highclere Bagshots.

THE notice in NATURE for December 1 (p. 104), by my friend Mr. R. S. Herries, of casts of shells in the Bagshot Beds at Highclere tends strongly to confirm the results of my own work in that district. On the strength of physical and stratigraphical evidence, I have shown the development in that neighbourhood of *all the three stages* of the Bagshot formation