Those who wish to read a simple and brief account of the main facts about the sun, planets, asteroids, comets, and stars (normal and abnormal) cannot do better than peruse this book. A general idea of the universe will be easily obtained, and some notion of the vast subject of astronomy will be gained. The illustrations are reproduced from some of the finest photographs ever taken, and they are well distributed and chosen. A brief bibliography of books more or less popular is given at the end; also a chronology of the main events of astronomy from very early times to 1908. With regard to the latter the author seems to think that such a table is unique or "what the compiler has never happened to see before." To take only one instance, he has evidently never seen Miss Clerke's admirable "History of Astronomy during the Nineteenth Century," at the end of which is a chronological table stating the main astronomical advances from 1774 to 1893.

## LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

### Fizeau's Experiment and the Principle of Relativity.

MR. CUNNINGHAM seems determined that the principle of relativity shall assert supremacy as regards the Fizeau effect on the velocity of light, arising from motion of the transmitting medium. This is a first-order effect, while the principle of relativity was invented to account for the second order. Up to the first order the affairs of moving media hang entirely on the ascertained fundamental relations of electrodynamics; and any type of relativity which does not agree with the formula thence deduced must, if substantiated, upset our whole system of ideas.

As regards the arguments adduced: (1) is of course a slip of the pen—he is thinking of steady viscous flow in a pipe, whereas the standard measures for ordinary turbulent flow are in general agreement with the values quoted from the experiment; while as regards (2), it seems certain that the experimenters must have had before them the effect of variation of velocity across the section of the pipe, which would curve the fronts of the waves and thus broaden the interference bands, but not much if only the central part of the pipe, in which the velocity varies but slightly, is employed. The shift at the centre of the band-system would depend on the axial velocity only. JOSEPH LARMOR.

Cambridge, November 2.

THERE was no intention in the letter to which Sir Joseph Larmor refers (NATURE, October 29, p. 226) of suggesting that the principle of relativity and the fundamental relations of electro-dynamics were at variance with respect to this matter. Certainly I have always taken them to be completely at one. But it has been raised in several quarters as an objection to both points of view that the extremely careful experiment of Michelson and Morley was not in complete agreement with theory. They found the convection coefficient for water to be $0{\cdot}434 \pm 0{\cdot}02$, while theory (taking account of dispersion according to Lorentz's formula) gives 0·451, which is very near the limit of possible error.

Sir Joseph Larmor makes it clear that the suggestions which I made as to the possible origin of the discrepancy are untenable, and, I understand, feels that it must lie in the inherent difficulties of the experiment. One of these is the determination of the axial velocity in terms of the mean. The experimental device could not be expected, I think, to give a result correct to one or two per cent., inasmuch as the pressure gradient actually measured is partly the normal pressure gradient of the undisturbed flow, and partly that due to the disturbance produced by the insertion of the gauge-tubes.

But, as a matter of fact, the discrepancy is not so great as has been thought. A recalculation from the figures and formulæ of Michelson and Morley, which Sir Joseph Larmor has now checked for me, gives for the experimental value of the coefficient 0·442, with their estimated possible error of $\pm 0{\cdot}02$. Thus the discrepancy (0·009) with the value given by Lorentz's formula (including dispersion) proves to be less than half the possible error.

E. CUNNINGHAM.

### Wireless Signalling for Shipping in War Time.

THE advantage which wireless telegraphy has been in enabling the whereabouts of vessels at sea to be known from day to day, is now in abeyance, I believe, on account of the war, because of the messages being liable to be read by the enemy.

But if a system of signalling false latitudes and longitudes by a code prearranged by the owners, different for each craft, and variable from day to day, easy and simple to translate by reference to the copy kept, then wireless signalling could go on as freely as before, without danger of the code being captured by the capture of any craft.

To illustrate the method of this, let us suppose a firm of owners have two ships, the *Ariadne* and *Ocean Bird*. A list of dates has been given to each captain with instructions to falsify positions on the respective dates (London time) with additions and subtractions as found in the list in opposition to the dates, thus:—

SHIP "ARIADNE."

| | Lat. | Long. |
|---|---|---|
| Nov. 1 | +14° | −21° |
| 2 | −16° | +15° |
| 3 | +30° | +20° |
| | etc. | |

SHIP "OCEAN BIRD."

| | Lat. | Long. |
|---|---|---|
| Nov. 1 | −18° | +24° |
| 2 | −16° | −21° |
| 3 | +18° | +15° |
| | etc. | |

The owners of these ships could interpret the wireless messages received through the exchange, and the Admiralty be quickly notified of a disappearance with approximate latitude and longitude.

A. BOWMAN.

144 Well Street, Hackney.