with the Continental ones. Where single orders for thousands were received by Continental firms from all sources, British firms received orders for tens. Anyone who is familiar with industry knows how difficult it is to compete with large firms under such conditions.

During the war, however, the British optical firms have received large and continuous orders, and as a result the increase of output has been very great. To have rushed a small industry in war-time and in so short a period up to its present size is a marvellous performance, probably unsurpassed in any other industry demanding exceptional skill.

But the important points to observe are that, so far as their knowledge was concerned, the principal British firms were competent to undertake the work, and that, having the orders, they were able to erect new premises, install plant, provide special jigs and tools, train unskilled labour, find suitable materials, and in these extraordinary circumstances to produce instruments that satisfy the requirements of the Services.

If in peace-time military orders of reasonable size had been placed in this country, the optical industry would have dealt with them as it has during the war. It is largely a question of orders of reasonable size and, above all, continuity of orders.

Having pilloried the optical manufacturer, Sir Joseph Larmor proceeds to praise the British optical writers. It is suggested that their works contain information that the manufacturers lack.

In Germany and in Britain it is not the type of book cited by Sir Joseph Larmor that is used by the optical manufacturers. Not one of the books cited deals with the method of optical computation actually adopted in the German workshops, and, indeed, there are extremely few books in Germany that do divulge the whole system. The British books are, no doubt, well adapted to enable students to pass examinations on general optics. For example, one of the best of those cited has a large index, which includes the rainbow and the principle of relativity—questions no doubt that will find a place of honour in examination papers —but which does not refer to so vital a question as coma, to which alone a whole book should be devoted.

There is a great similarity in the present optical books. They all contain the same stereotyped material. Some deal with it in a non-mathematical way, while others attack the propositions with heavy algebraic artillery. Generally the sign convention changes without warning from page to page, for the simple reason that the matter is mostly copied from previous writers who used no standard system.

No doubt these books are the unfortunate result of circumstances. A book devoted to, say, coma would have a very limited market, whereas a book on optics, if made sufficiently general, can be made to appeal to students and school teachers and thus find a profitable market.

In the early days of the optical industry in this country our pure mathematicians were also real craftsmen. They were not content with the evolution of general equations. To day "the science of the best optical instrument makers is far ahead of the science of the text-books." That is the opinion of the late Prof. Silvanus P. Thompson, who also said: "But the teaching of the colleges and the university teaching at Cambridge—well, what is it in optics? They call it optics, but it is really purely mathematical gymnastics applied to the optical problems of a hundred years ago. I do not think there is really what one can truly call optical work going on at Cambridge. . . . Optical teaching, I am sorry to say, is very largely at its lowest conceivable ebb."

If our present-day mathematicians wish to help the industry (and their help is desired), they must enter the workshops first as learners, not teachers. They may find the work laborious and monotonous from the point of view of the mathematician to whom a pretty solution is an object of importance, but once they have experienced the pleasure of testing a system that accords with their calculations, they will never again be satisfied with the publication of untried formulæ.

James Weir French.

Anniesland, Glasgow, March 28.

Floating Earths.

In reference to the inquiry of Dr. Walter Leaf in Nature of March 15 as to the interpretation of a passage of Strabo, the fact may possibly be of some interest that in the island of Mors, in Denmark, bricks are made from a local sandy clay which, after burning, float in water. These bricks are used, I understand, both as a refractory material and for ordinary building purposes, their lightness and porosity giving them certain advantages for the latter purpose. Their mechanical strength is said to be considerable. The porosity is not obtained by the addition of combustible or volatile matter during moulding.

If the expression πηγνυμενας, used by Posidonius, be consistent with a process of burning the clay into bricks, and if clays of somewhat similar physical character to that of Mors, although of different geological origin, occur in Asia Minor and Spain, an explanation of the passage might perhaps be found in this direction.

Cecil H. Desch.

Metallurgical Laboratory,
University of Glasgow,
March 24.

Gravitation and Thermodynamics.

If Dr. P. E. Shaw's contention (Nature, March 29) for a perpetual motion consequence of gravitational heat were justified, it would be an argument against the supposed effect on which such a conclusion could be based; but it does not seem to me that the contention is justified. For the line joining maximum to minimum temperature is vertical, and, unless the rate of heating differs from the rate of cooling, every horizontal chord will be an isothermal; so there is nothing to keep a vertical disc rotating.

Oliver Lodge.

The suggestion in Nature of March 1 that thermodynamics might throw light on the question of the temperature variation of gravitation has not been unkindly received. The criticisms have not been directed so much against this suggested application of thermodynamics as against the expression deduced for the attraction between two bodies.

It has been pointed out to me that dQ is not a perfect differential, and therefore it is not valid to equate

$$\frac{\partial^2 Q}{\partial r . \partial \theta} = \frac{\partial^2 Q}{\partial \theta . \partial r}.$$

The correct expression for the attraction, assuming that the specific heat is independent of r, is

$$F = m . \theta . f(r) + \psi(r),$$

where m is the mass of the body the temperature of which is θ. This expression has none of the objections which the previous incorrect expression had, for at the absolute zero the temperature coefficient vanishes and $\psi(r)$ is probably $GMmr^{-2}$.

The assumption that $\partial s/\partial r = 0$ is, of course, only a special case, for s may depend on r or on the gravitational field in which the mass m is situated. Since