myself about the year 1854, and in which I found a velocity comparable with the velocity of light. We did not then know the relation between electro-static and electro-magnetic units. If we work that out for the case of air instead of gutta-percha we get simply v (that is, the number of electro-static units in the electro-magnetic unit of quantity) for the velocity of propagation of the impulse. That is a very different case from this very rapidly varying electrification I have ideally put before you, and I have waited in vain to see how we can get any justification of the way of putting the idea of electric and magnetic waves in the so-called electro-magnetic theory of light.

"I may refer to a little article of mine in which I gave a sort of mechanical representation of electric, magnetic, and galvanic forces—galvanic force I called it then, a very badly-chosen name. It is published in the first volume of the reprint of my papers. It is shown in that paper that the static displacement of an elastic solid follows exactly the laws of the electro-static force, and that rotatory displacement of the medium follows exactly the laws of magnetic force. It seems to me that an incorporation of the theory of the propagation of electric and magnetic disturbances with the wave theory of light is most probably to be arrived at by trying to see clearly the view that I am now indicating. In the wave theory of light, however, we shall simply suppose the resistance to compression of the luminiferous ether, and the velocity of propagation of the condensational wave in it, to be infinite. We shall sometimes use the words 'practically infinite' to guard against supposing these quantities to be absolutely infinite."

The second extract which I give is from p. 143 of the Papyrograph edition of the "Baltimore Lectures"—a portion not yet reprinted.

"The want of indication of any such actions is sufficient to prove that if there are any in nature, they must be exceedingly small. But that there are such waves, I believe, and I believe that the velocity of propagation of electro-static force is the unknown condensational velocity that we are speaking of.

"I say 'believe' here in a somewhat modified manner. I do not mean that I believe this as a matter of religious faith, but rather as a matter of strong scientific probability."

J. T. BOTTOMLEY.

13 University Gardens, Glasgow, January 16.

The Astronomical Theory of the Ice Age.

MAY I first acknowledge the gentle kindness with which my early teacher and friend, Sir Robert Ball, has pointed out my error in quoting from the old edition of his work. I much regret that I did not make further inquiries, but I was satisfied when the library clerks at Trinity College, Dublin, told me that if there had been any alteration in the text, they would have received a copy of the second edition. It appeared from Sir H. Howorth's letter that the mistake originated with the publishers, who erroneously informed the library agent that the second edition was a mere reprint, and therefore refused to supply a copy.

Both Sir Robert Ball and Dr. Wallace, in their letters in *NATURE* of January 9, have misunderstood the way in which I present my argument. If Dr. Wallace would read my papers again, I think he will see that, so far as I am concerned, the whole of his letter is founded on a complete misapprehension; and Sir Robert Ball will, I hope, also agree that he has somewhat altered the form in which I have stated my conclusions, and that I have fully recognised the difference which he thinks I have ignored. But as the matter really at issue is the present position of the astronomical theory, I may be excused from discussing this misunderstanding further, for even if every word of their criticisms on my conclusions were valid, the astronomical theory, as it issued from the labours of Croll and Ball, would be in no better position than before. Whether I am right or wrong in my belief that the astronomical factor cannot have been the principal one, I venture to think there can be no doubt that the existing exposition of that theory must be given up.

The foundation of the astronomical theory is the fall in temperature *directly* due to diminished sun-heat. Croll and Ball accordingly give calculations which indicate a large fall. Croll gets $45^{\circ}3'$ F. for the lowering of mid-winter temperature in Great Britain during the long excentric winter, and Ball's modification of Croll's method gives about 25° F. as the lowering of the winter temperature. The first five pages of my article in the *Phil. Mag.* for December 1894 are devoted to showing that

there is no justification for the principle on which this calculation is made, and that the fall must be a mere fraction of that postulated in either exposition of the astronomical theory. The chief flaw in the calculation is, curiously enough, that which Sir Robert and Dr. Wallace erroneously attribute to me, viz. that of considering that changes in terrestrial temperature are directly proportional to the changes in sun-heat, and ignoring the important element of storage and transference by ocean and air currents. How unsafe this is may be judged from the fact that if the method used to calculate the temperature in the Glacial Age from that in the present day were applied to find the summer temperature from the winter temperature, we should find for the British Isles a summer temperature of above 300° F. if we take Ball's hypothesis, and some thousands of degrees Fahrenheit if we take Croll's. If we calculate the winter temperature from the summer one, we should get -125° F. for our winter temperature. A method which gives results in such striking contrast to the truth can hardly be accepted as a basis for a scientific theory.

If, therefore, this first portion of my criticism be correct (and hitherto no attempt has been made to refute it) the astronomical hypothesis is in just the position it would occupy if neither Croll's nor Ball's book had been written. So far, *the hypothesis itself* may be true or false; it is only the *reasoning* which has been put forward in its support that has to be abandoned or modified. The theory is, as all will admit, a tempting one, and accordingly I sought for some other means of establishing it. After several fruitless efforts to hit on a fairly satisfactory method of estimating the *direct* effect of an altered distribution of sun-heat on terrestrial temperatures, the method which Prof. Darwin has described occurred to me, and from it, *combined with a discussion on the transference of heat by the Gulf Stream* (see *Phil. Mag.* December 1894, p. 548 and p. 551), I was led to infer that for the British Isles at least the glaciation could not with any degree of probability be attributed to the long winter of great excentricity.

Sir Robert Ball's views, as presented in his letter, seems to involve a return to Croll's point of view, at least to the extent that the purely astronomical reason requires to be supplemented by a discussion of the oceanic and atmospheric currents. This view appears to me a true one; the only hope for the astronomical theory would be to show that the adjustment of terrestrial temperatures by the interaction of ocean and air currents with direct sun-heat is such that a very slight alteration of sun-heat produces a very great alteration of temperature; so that if the sun-heat which falls on Cornwall in winter were to be reduced to that which falls on Yorkshire, with corresponding changes for the temperate latitudes, and somewhat greater ones for the tropical belt, the ultimate result would be an Ice Age. But how can we hope to establish such a theory when we remember what a comparatively small change of temperature is due to the far greater changes of sun-heat from equator to pole as summer gives way to winter? EDWD. P. CULVERWELL.

Trinity College, Dublin, January 14.

Changes of Length in Bars and Wires of Magnetic Material due to Magnetisation.

THE appearance of a paper, by Dr. L. T. More, on the changes in length produced in iron wires by magnetisation (*Phil. Mag.*, October 1895, p. 345, and *Physical Review*, vol. iii. p. 210), has drawn my attention to a curious divergence of opinion on a fundamental point in magnetism. Dr. More has attempted to analyse the change of length accompanying magnetisation into a "*direct action*," which "may possibly be due to the orienting of the magnetised particles," and to "*indirect actions*." He adds (p. 224): "These indirect actions are the mechanical stresses created in the rod by the magnetism. The first of these . . . is the tractive force of the magnet and is measured by $B^2/8\pi$. That this force exists, tending always to *contract* the rod (italics mine), is seen from the fact that if the magnet is cut in two, the ends are held together. . . . This effect for high intensities of magnetisation is a large one, and becomes one of the most important factors in the observed changes in length." The stress referred to by Dr. More is that usually associated with the name of Maxwell ("Electricity and Magnetism," vol. ii. Arts. 641 *et seq.* Cf. Ewing, "Magnetic Induction in Iron and other Metals," § 147).

The first to propound the view adopted by Dr. More—that the mechanical force tends to *shorten* the rod—would seem to be

Mr. Shelford Bidwell (*Phil. Trans.*, 1888, pp. 216-7). He has been followed by Prof. Ewing, on pp. 242-3 of his "Magnetic Induction."

On the other hand, Maxwell, *l.c.*, speaks of a *tension* along the lines of force, and even more explicit statements to the same effect are due to Kirchhoff (*Wied. Ann.*, Bd. 24, p. 52, and Bd. 25, p. 601) and Prof. J. J. Thomson ("Applications of Dynamics to Physics and Chemistry," p. 52). The latter says: "... there are the strains arising from Maxwell's distribution of stress. Kirchhoff has investigated the effect of this on a small soft iron sphere placed in a uniform magnetic field and has shown that it would produce an *elongation* . . . along the lines of force and a contraction at right angles to them. We may therefore assume that in general this distribution of stress causes an *expansion* . . . in the direction of the lines of force . . ." (italics mine).

Prof. Thomson's view was accepted and restated by myself in discussing the phenomena in cobalt (*Phil. Trans.*, 1890, p. 339). The fact that my views escaped Dr. More's notice is not surprising, but it is strange that he overlooked Prof. Thomson's statement, and that I overlooked Mr. Shelford Bidwell's. My belief now, as before, is that the view of Kirchhoff and J. J. Thomson is the correct one.

Let us take the case of a long iron bar magnet suspended vertically. Suppose it cut in two transversely at a section A; then, supposing the magnetism sufficiently intense, the lower portion will, we know, adhere to the upper until a considerable weight, W, is hung from the lower end, B. If *w* be the weight of AB, the magnetic force over A must balance *W* + *w*, acting downwards, and so must itself act upwards, or away from AB. In other words, the magnetic force across A acts precisely like the elastic stress across a section of a rope fastened at one end and pulled at the other. The rope, as every one will realise, is under tension not pressure, and so clearly is the iron bar.

A more general way of looking at the matter is as follows:

$$\begin{array}{c|c|c|c} A' & A & B & B' \\ \hline -m & +m & -m & +m \end{array}$$

Take imaginary sections A and B of a long uniformly magnetised straight or bent bar, and regard coterminous surfaces AA' and BB' as separated by distances small compared to AB. Taking the usual elementary theory, we see the force acting on the distribution +*m* over A is *F* - *F'* + *F''* to the left, where *F* arises from -*m* over A', -*F'* from -*m* over B, and *F''* from +*m* over B'. Now clearly *F'* and *F''* are approximately equal, and each is small compared to *F*, so that A is obviously pulled to the left. Similarly B is pulled to the right, or AB is under tension.

Of course if the bar, when supported horizontally on a frictionless table, were actually cut through at A, foreign material—a finger, for instance—inserted between the naturally coterminous surfaces A and A' would experience compression. I cannot avoid the conclusion that Mr. Shelford Bidwell and Dr. More have fallen into confusion through regarding the problem from this point of view, forgetting that the surfaces supposed to attract one another have nothing between them to squeeze.

That the question is of vital importance to a true understanding of the phenomena in iron is unquestionable if Dr. More's experiments—which seem carefully designed—gave anything like the true results. A reversal of the sign in his correction $B^2/8\pi$ in his tables I. to IV.—necessitated by the view I advocate—would completely alter the character of the physical deductions to be made. The occasion for the correction, I may add, is as clearly indicated by Mr. Shelford Bidwell as by Dr. More, but the experimental data obtained by the former led him to attach less importance to it.

It is not my intention to discuss the actual magnitude of the magnetic stress or of the consequent alteration in length. An interesting experimental paper on the former subject by Mr. E. Taylor Jones, with references to recent conflicting authorities, appeared in the *Phil. Mag.*, March 1895, pp. 254-267. I may say, however, that the theoretical importance of investigations such as those of Prof. J. J. Thomson into the mutual relationships of magnetism and elasticity renders it all the more desirable that any doubt as to the true nature of the experimental results should be removed. To be altogether satisfactory, these investigations require trustworthy magnetic and elastic data for one and the same specimen.

C. CHREE.

January 11.

The Metric System.

MR. BROOK-FOX does not state accurately, in his letter to NATURE of January 9, what passed in relation to legislation on the subject of Weights and Measures in British India in the years 1870-1871.

It is true that an Act was passed in April 1870, enabling the Governor-General in Council to require the use of the Metrical Weights and Measures by Government departments, railway or other companies, registered under the law relating to such companies, and persons exercising specified occupations or trades; but only after certain preliminary steps had been gone through.

This Act was disallowed by the then Secretary of State for India, the Duke of Argyll, who had at first authorised legislation on the subject, in the general direction given to this Act, but considered that the actual form in which the measure was passed went beyond his original instructions, as it might have been extended to the dealings of persons engaged in business or trade, and was otherwise considered to be too drastic. Objection was also taken to the inclusion of the metre as the standard of length, the original authority to legislate having been limited to the adoption of the kilogramme as the unit of weight.

Immediately on the receipt of the Secretary of State's disallowance of the Act of 1870, a new permissive Bill was prepared, extending only to the standard of weight, which was again stated to be the kilogramme, and of measures of capacity which are subsidiary to those of weight; and limited, as to the power of applying it, to the case of Government departments, municipal bodies, and railway companies.

This measure became law as Act xxxi. of 1871, and is still in force. But it has never been put into operation. This, however, was not the result of the Duke of Argyll's action, but of the change of policy that followed on Lord Mayo's most unfortunate murder. Had Lord Mayo lived, he would certainly have given effect to the Act, the importance of which he thoroughly recognised. His successor was of a different opinion, and the combination of circumstances that for the moment appeared to have rendered possible the introduction of a rational system of weights and measures into our Indian Empire, passed away, and has not yet returned.

As having had charge of the Bill of 1870, in the Council of the Governor-General, I may be permitted, in conclusion, to quote the words that I wrote twenty-five years ago with reference to the fears expressed by the Secretary of State as to the danger of precipitate action in such a matter as this.

"The history of this very Act regarding weights and measures, which has been under consideration in one form or other for not less than fourteen years, which in its last stage has taken seven or eight years in coming to maturity, and to carry out the provisions of which no measures have yet been taken, or can be taken in all probability for another year at least, seems to me to teach that the one thing perfectly certain in the future is that the progress made in giving effect to any change in the present case will be slow in a degree most painful to all who are concerned in it; and that what with active opposition, and what with indifference, this Act is far more likely to remain a dead letter on the statute book for an indefinite period, than to be brought into operation with improper haste."

January 13.

RICHARD STRACHEY.

Marsupial with an Allantoic Placenta.

I HAVE just received from my friend and former pupil Mr. J. P. Hill, Demonstrator of Biology in the Sydney University, an advanced copy of the *Abstract of Proceedings* of the Linnean Society of New South Wales for November 27, in which he announces the discovery of a true allantoic and highly vascular placenta, of a discoidal and most probably deciduous type, in the Bandicoot (*Perameles obesula*).

In the second part of his magnificent "Forschungsreisen im Australien und dem malayischen Archipel," Prof. R. Semon has substantiated the discovery of apposition between the allantois and sub-zonal membrane in *Phascolarctus*, and shown that a fusion is effected. He points out that the foetal appendages of that animal are of a type involving certain Marsupialia in a position intermediate between the Placentalia on one hand, and the Monotremata and Sauropsida on the other, and regards the earlier recognised Marsupial condition, in which the allantois is remote from the serous membrane, as secondary and associated with the loss of a respiratory function by that organ.