LETTERS TO THE EDITOR.

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Velocity of Propagation of Electrostatic Force.

DR. BOTTOMLEY'S note published in NATURE of January 23, quotes an extract from my Baltimore Lectures of October 1884, in which this subject is spoken of, with an illustration consisting of two metal spheres at a great distance asunder, having periodically varying opposite electrifications maintained in them by a wire connecting them through an alternate current dynamo.

For an illustration absolutely freed from connecting wire and all complications, consider four metal spheres, A, B, c, d, with their centres all in one straight line;—their relative magnitudes and positions being such as shown in the accompanying diagram. Let each of the four be initially electrified, A and c, positively, B and d, negatively. Let the charges on c and d be so strong that a spark is only just prevented from passing between them by the influence of B and A. Let A be gradually brought nearer to B till a spark passes between them. Will the consequent spark between c and d take place at the same instant or a little later? It is not easy to see how this question could be answered experimentally; but remembering the wonderful ingenuity shown by Hertz in finding how to answer questions related to it, we need not perhaps despair to see it also answered by experiment.

The elastic solid theory restricted to the supposition of incompressibility (which is expressed by Maxwell's formulas) makes the difference of times between the two sparks infinitely small.

The unrestricted elastic solid theory gives for the difference of times the amount calculated according to the velocity of the condensational-rarefactional wave.

But I feel that it is an abuse of words to speak of the "elastic solid theory of electricity and magnetism" when no one hitherto has shown how to find in an elastic solid anything analogous to the attraction between rubbed sealing-wax and a little fragment of paper; or between a loadstone or steel magnet and a piece of iron; or between two wires conveying electric currents. Elastic solid, however, we must have, or a definite mechanical analogue of it, for the undulatory theory of light and of magnetic waves and of electric waves. And consideration of the definite knowledge we have of the properties of a real elastic solid, which we have learned from observation and experiment, aided by mathematics, is exceedingly valuable in suggesting and guiding ideas towards a general theory which shall include light (Old and New), old and new knowledge of electricity, and the whole of electro-magnetism.

KELVIN.

The New Actinic Rays.

MAY I point out that an unnecessary amount of energy is being expended on Röntgen's photographs—I mean electrical energy.

I have succeeded in obtaining perfectly sharp and fully-exposed negatives from an action of four minutes' duration, even when a thin aluminium plate is placed in front of the sensitive film, and the rays are excited in a Crookes' bulb connected direct (*i.e.* with no Leydens inserted) with the secondary terminals of an Appa's induction coil, which gives (in its present condition) a three-inch spark in air when worked, as in the present experiments, by three small accumulator cells. This is much smaller, however, than that used in the published experiments of others who have been doing similar work.

University College, London.

ALFRED W. PORTER.

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THE accompanying photograph may perhaps interest those who are engaged in the photography of invisible objects. It was taken in the following way. Upon a piece of board I placed a sensitive plate, on this a penny-piece with the obverse side downwards, and on the top of the penny-piece a $\frac{1}{16}$ -inch cedar board. The whole was exposed to the light emitted by the burning of sixteen inches of magnesium ribbon at a distance of six inches. I developed with pyro-ammonia. An inspection of the photograph will show a distinct image of the Queen's head.

On repeating the experiment with fifteen inches of magnesium ribbon at six inches, but without the use of the cedar board, the part of the plate surrounding the coin was solarised, whilst the part underneath was over-exposed, so that no trace of the image was visible.

I then repeated the last experiment, using a slow lantern-plate, and burning four inches of ribbon at nine inches distance; on development a faint image of the Queen's head was visible. Hence it is only a matter of exposure and development to produce a much better result than the one presented.

The phenomenon does not appear to be due to the varying thickness of the coin, since the impression of the reverse side has not modified the result, but rather to the different *directions* in which the penetrating rays are refracted from the irregular refracting surface on to the sensitive plate. W. SAUNDERS.

[A FAINT image of the Queen's head is quite visible upon the print received from Mr. Saunders, but it will not bear reproduction.—ED. NATURE.]

A STORY was current at Cambridge some forty years ago that an aspirant to mathematical honours replied to the question,

"Construct a prism through which no ray can pass," in the terms following:—

"Take a prism of wood: then if no ray passes through, what was required is done. But if a ray does pass through, paint it."

Surely a marvellous anticipation of Röntgen's X-rays!

R. B. H.

The Stress in Magnetised Iron.

I AM glad that Dr. Chree, in his letter published in NATURE of January 23, has raised a discussion of this matter, regarding which, as he says, the most contradictory statements are to be found. For some time I have been aware that the passage referred to in my book on "Magnetic Induction in Iron" requires correction. The magnetic stress, $B^2/8\pi$, in a long rod or ring uniformly magnetised, is there spoken of as if it were of the same nature as a simple longitudinal stress of compression, producing a contraction of the length in consequence of the elasticity of the metal. Dr. Chree, if I understand him rightly, would treat it as of the same nature as a simple longitudinal stress of tension, producing an elongation of the iron.

But it now seems clear to me that both of these views are equally wrong. There is no proper comparison, in the general state of magnetised iron, with the stress in a loaded pillar or the stress in a stretched rope.

Take the case of a uniformly magnetised ring, where we have no complications due to end effects. Imagine a plane of section, and call the halves of the ring A and B. According to the first view, A is, as a consequence of the magnetisation of the ring, pushing against B, and B against A. According to the other view, A is pulling B, and B is pulling A. But if A is either pushing or pulling B, the equilibrium of B demands that some other force must act on it to balance this push or pull. No such

force exists. We cannot conceive the action at any section to be simply a stress of push or a stress of pull.

What we know from experiment is that, if the ring be actually cut in two, and a piece of, say, paper be put between the halves, the paper will be squeezed with a stress equal to $B^2/8\pi$. Also, that a pull of that amount would be required to separate the two halves of the ring.

This means that when they are separated by paper the half-ring A is pulling the half-ring B towards it, and the paper is pushing the half-ring B in the opposite direction with the same force, namely $B^2/8\pi$ per square centimetre of the section.

Of course we may, if we please, say that when there is no paper interposed, each half-ring is both pulling and pushing the other. If a mechanical analogy is wanted, it might be found by imagining a stiff tubular ring with a stretched india-rubber band inside it. Suppose, further, that when such a ring is cut through at any section the india-rubber band is not cut, but only its stiff envelope. Then if we try to separate the halves, we have to apply a force equal to the pull in the rubber band. And when the halves are allowed to come together with a piece of paper between them, they will squeeze it with the same force.

Mr. Shelford Bidwell and Dr. More have done what is equivalent to asking whether the change of length which a ring undergoes when it is magnetised can be accounted for by what I have here called the pull of the rubber band acting to shorten the stiff tube in which it is stretched, the tube being treated as having the same section and the same modulus of elasticity as the real iron ring has.

But I see no ground for treating this purely hypothetical strain as a "correction" to be applied, either one way or the other, to the observed changes of length.

The case illustrated by Dr. Chree (on p. 270) is a special one. He there considers the middle piece of a long magnetised bar, separated by actual gaps from the end-pieces from which it has been cut. To preserve the gaps, the end-pieces must be held fixed. He shows that under these conditions the middle-piece is in a state of tensile stress. So it is, but only because of the pull which the other pieces apply to it across the gaps. Make the iron continuous by closing up the gaps, and the tensile stress disappears.

To discuss the sign of the magnetic stress at all in the case of a closed ring, seems much like discussing whether a man sitting in a clothes-basket exerts a pull or a push when he tries to lift it by the handles.

J. A. EWING.

Engineering Laboratory, Cambridge, January 28.

DR. CHREE'S letter in NATURE of January 23 corrects an error which it is curious has prevailed so long, and in part forestalls a communication Mr. H. Nagaoka and I had intended to make on the subject of magnetic stress. It might, however, be added that the expression $B^2/8\pi$ used by Dr. More (*Phil. Mag.*, October 1895), and originally given by Mr. S. Bidwell, for the magnetic stress causing changes of length, is incorrect also on another ground, viz. that this quantity is on Maxwell's theory the magnetic stress *in air* (where, according to the ordinary convention as to dimensions, $B = H$) and not in iron, where the expression is necessarily of a different form.

In conjunction with Mr. Nagaoka, I hope before long to discuss this subject more fully.

E. TAYLOR JONES.

University College of North Wales, Bangor, January 25.

The Astronomical Theory of a Glacial Period.

MR. CULVERWELL has pointed out to me that I am in error when I include him among those writers who think that the problem of glacial periods is to be solved by considering only the varying amounts of sun-heat at different epochs. On referring to his paper, which I had not at hand when I wrote, I find that this is the case, and that he is careful to limit his calculations as giving only the variations of temperature due to *direct* sun-heat. He also discusses, though very briefly and inadequately, the effects due to *transference* of heat from one area to another. Although willingly making this correction at his request, I am still, after another perusal of his paper, quite unable to see that it

finally disposes of Croll's theory, much less of that modification of it which I have myself set forth.

ALFRED R. WALLACE.

The Fall of the Altels Glacier, September 11, 1895.

JE vous adresse aujourd'hui un travail sur l'Avalanche du Glacier de l'Altels, que vient de publier la Commission des Glaciers de la Société helvétique des Sciences naturelles et qui complète l'intéressant article de Miss Maria M. Ogilvie: "The Gemmi Disaster" (NATURE, vol. lii. p. 573).

Ce travail rédigé par Mr. Heim n'est pas tout à fait définitif en ce que plusieurs points touchant à l'histoire antérieure du glacier de l'Altels n'ont pu être résolus encore. Il serait important de tirer au clair ces points pour pouvoir déterminer avec exactitude les causes de l'avalanche du 11 sept. 1895; mais, pour cela, il nous faut des photographies du glacier de l'Altels prises avant l'avalanche et remontant jusqu'à quelques années en arrière si possible. Après avoir fait depuis plusieurs mois des recherches peu fructueuses à cet égard je prends la liberté de m'adresser à vous, Messieurs, pour donner quelque publicité à ces lignes. Persuadé que beaucoup d'amateurs ont photographié l'Altels de l'W., ou du N.W., je les prie de bien vouloir me communiquer leurs épreuves, en indiquant la date (au moins le mois et l'année) à laquelle la photographie a été prise.

En vous remerciant d'avance de votre obligeance, je vous prie, Messieurs, d'agréer l'assurance de ma considération distinguée.

LÉON DU PASQUIER.

Secrétaire de la Commission des Glaciers.

Neuchâtel, le 21 janvier.

Remarkable Sounds.

IN Major Head's "Forest Scenes" (London, 1829, p. 205), I have found the passage already quoted by Mr. C. Tomlinson (p. 78, *ante*), subjoined with this phrase: "It being, in real fact and without metaphor, the voice of winds imprisoned on the bosom of the deep." In a similar manner, Olaus Magnus describes the similar sounds thus: "Mais es lacs Septentrionaux gelés, on oit sous la glace une tempête aussi horrible, à raison des vens enfermés sous la glace, qu'on fait d'un tonnerre provenant de la grâde épaisseur des nués." ("Histoire des pays Septentrionaux," Paris, 1561, fol. 21, b).

Sebastian Münster, in his article on Iceland, says:—"Car la glace divisée par loppins et brisée en plusieurs parties tourne à l'entour de ceste isle l'espace de huyt moys, et se froisse de si grande impetuosité contre le rivage, qu'elle rend un son horrible et espouventable, et semble advis que ce soit le gemissement ou brayement d'une voix humaine. Cela fait que les plus idiotz croyent que les âmes des hommes sous la tormentes de froid." ("La Cosmographie universelle," Basle, 1552, p. 1051.) Against this error Angrimus Ionas writes, but at the same time he admits that "this ice at sometimes by shuffling together maketh monstrous soundings and cracklings, and againe at sometimes with the beating of the water sendeth forth an hoarse kind of murmuring." (Hakluyt, "Principal Navigation," 1599, vol. i. p. 563.)

If it be taken into consideration that so often in the volcanic craters and thermal springs¹ man found the types of the perpetual Abode of Fire, a suggestion would seem quite reasonable that the so-called "Cold Hells" of the Buddhists² and the Tauists³ had been the outcome more or less of such dreary, icy sounds.⁴

KUMAGUSU MINAKATA.

January 31.

The Antiquity of the Finger-Print Method.

IN my letter on this subject that appeared in NATURE (vol. li. p. 199, December 27, 1894), I have suggested that the ancient Japanese usage on divorce-papers of the finger-marks was probably adopted from the Chinese "Laws of Yung-Hwui"

¹ Cf. Hardy, "Manual of Buddhism," second edition, p. 27. I remember a note in NATURE about the Indian confusion of thermal springs with the hell, but at the present moment cannot refer to the number and page.

² See Beal, "A Catena of Buddhist Scriptures from the Chinese," 1871, p. 36.

³ See "Twan Ching-Shih, Yu-yang Tsah-tsu," Japanese edition, tom. ii. fol. 3, b.

⁴ Indeed, according to Münster, the Icelanders of old believed that their hells were in both the Hecla and the ice.