LETTERS TO THE EDITOR.

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On Röntgen's Rays.

PROF. RÖNTGEN'S remarkable discovery will materially affect our views concerning the relation between the ether and matter; but further experimental evidence is required before any opinion can be expressed as to the character of the rays, which behave in so straightforward a manner that they seem to upset all one's notions of the laws of nature. Prof. Röntgen, on the strength of his carefully-conducted experiments, has arrived at a conclusion adverse to the idea that the rays only differ from light rays by the smallness of wave-length. Perhaps the following considerations may show that the evidence is not conclusive in this respect.

Röntgen's rays are not kathode rays—there can be no doubt on that point—but they are generated at the point of impact between the kathode ray and solid substances.

The discoverer has not been able to obtain any interference effects, possibly, as he says, owing to the weakness of the radiation. An absence of interference would not, however, be sufficient to show that the radiation is not of the nature of ordinary light, but only that it does not possess sufficient regularity, or, in other words, that the disturbance is not sufficiently homogeneous. That this is the case is not at all improbable, for the radiation is produced by an impact, which in the first instance may be an impulsive motion propagated outwards, and after passing through the screen, would only possess such regularity as is impressed on it by the absorption of the longer waves.

The great argument against the supposition of waves of very small length lies in the absence of refraction; but is this conclusive?

When we speak of the size of the atoms, we mean their distance in the solid and liquid state. The properties of the ether may remain unaltered within the greater part of the sphere of action of a molecule. The number of molecules lying within a wave-length of ordinary light is not greater than the number of motes which lie within a sound-wave, but, as far as I know, the velocity of sound is not materially affected by the presence of dust in the air. Hence there seems nothing impossible in the supposition that light-waves, smaller than those we know of, may traverse solids with the same velocity as a vacuum. We know that absorption bands greatly affect the refractive index in neighbouring regions; and as probably the whole question of refraction resolves itself into one of resonance effects, the rate of propagation of waves of very small lengths does not seem to me to be pre-judged by our present knowledge. If Röntgen's rays contain waves of very small length, the vibrations in the molecule which respond to them would seem to be of a different order of magnitude from those so far known. Possibly we have here the vibration of the electron within the molecule, instead of that of the molecule carrying with it that of the electron.

I should like, further, to express a certain sense of satisfaction that Röntgen's rays are not deflected in a magnetic field. They are thus clearly separated from kathode rays. The idea that kathode rays are due to vibrations has become fashionable; yet the fact that the magnet deflects them just as it would an electrified molecule, has always seemed to me to be conclusive against this view. No one has, so far, given any plausible reason why a ray of *invisible* light should be able to run round in a spiral, while a ray of *visible* light goes straight; and, so far, Röntgen's rays behave as we should expect well-conducted vibrations to do.

It is not my intention to argue in favour of any particular theory, or against Röntgen's suggestion that we have at last found the formerly missed longitudinal wave. I only desire to put those points forward which at first sight seem to go against the supposition of ordinary light vibrations, and to raise the question whether they constitute an insuperable difficulty.

ARTHUR SCHUSTER.

IN connection with the wonderful discovery by Prof. Röntgen of photographic rays, apparently hitherto unknown, and in connection with the speculation which concludes Prof. Röntgen's most interesting paper, that these rays may perhaps be longitudinal vibrations of the luminiferous ether, the following extracts will probably be found of interest to the readers of NATURE. They are taken, by permission of Lord Kelvin, from his Baltimore Lectures, delivered at the Johns Hopkins University in 1884.

The first extract is from the reprint (now in progress) of Lecture IV. Referring to mathematical work immediately preceding, Lord Kelvin says:—". . . We can do that [obtain certain forms of solutions of equations] for the purpose of illustrating different problems in sound, and in order to familiarise you with the wave that may exist along with the wave of distortion in any true elastic solid which is not incompressible. We ignore this condensational wave in the theory of light. We are sure that its energy, at all events, if it is not null, is very small in comparison with the energy of the luminiferous vibrations we are dealing with. But to say that it is absolutely null, would be an assumption that we have no right to make. When we look through the little universe that we know, and think of the transmission of electrical force, and of the transmission of magnetic force and of the transmission of light, we have no right to assume that there may not be something else that our philosophy does not dream of. We have no right to assume that there may not be condensational waves in the luminiferous ether. We only do know that any vibrations of this kind, which are excited by the reflection and refraction of light, are certainly of very small energy compared with the energy of the light from which they proceed. The fact of the case as regards reflection and refraction is this, that unless the luminiferous ether is absolutely incompressible, the reflection and refraction of light must generally give rise to waves of condensation. Waves of distortion may exist without waves of condensation, but waves of distortion cannot be reflected at the bounding surface between two mediums without exciting in each medium a wave of condensation. When we come to the subject of reflection and refraction, we shall see how to deal with these condensational waves, and find how easy it is to get quit of them by supposing the medium to be incompressible. But it is always to be kept in mind as to be examined into, are there or are there not very small amounts of condensational waves generated in reflection and refraction, and may, after all, the propagation of electric force be by these waves of condensation?

"Suppose that we have at any place in air, or in luminiferous ether (I cannot distinguish now between the two ideas) a body that, through some action we need not describe, but which is conceivable, is alternatively positively and negatively electrified; may it not be that this will give rise to condensational waves? Suppose, for example, that we have two spherical conductors united by a fine wire, and that an alternating electromotive force is produced in that fine wire, for instance by an 'alternate current' dynamo-electric machine; and suppose that sort of thing goes on away from all other disturbance—at a great distance up in the air, for example. The result of the action of the dynamo-electric machine will be that one conductor will be alternately positively and negatively electrified, and the other conductor negatively and positively electrified. It is perfectly certain, if we turn the machine slowly, that in the air in the neighbourhood of the conductors we shall have alternately positively and negatively directed electric force with reversals of, for example, two or three hundred per second of time with a gradual transition from negative through zero to positive, and so on; and the same thing all through space; and we can tell exactly what the potential and what the electric force is at each instant at any point. Now, does any one believe that, if that revolution were made fast enough, the electro-static law of force, pure and simple, would apply to the air at different distances from each globe? Every one believes that if that process be conducted fast enough, several million times, or millions of million times per second, we should have large deviations from the electro-static law in the distribution of electric force through the air in the neighbourhood. It seems absolutely certain that such an action as that going on would give rise to electrical waves. Now it does seem to me probable that those electrical waves are condensational waves in luminiferous ether; and probably it would be that the propagation of these waves would be enormously faster than the propagation of ordinary light waves.

"I am quite conscious, when speaking of this, of what has been done in the so-called electro-magnetic theory of light. I know the propagation of electric impulse along an insulated wire surrounded by gutta-percha, which I worked out 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 kindliness 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 45·3° 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

VOTE OF CONVOCATION ON THE COWPER COMMISSION SCHEME.

ANOTHER step has been taken in the long controversy with respect to the equipment of the University of London with teaching functions. While the other bodies represented on the recent deputation to the Duke of Devonshire had passed resolutions asking the Government to introduce a Bill similar to Lord Playfair's "London University Commission Bill, 1895," but with an added clause giving a right of appeal to the Privy Council (NATURE, December 5, 1895), Convocation had not expressed any opinion either on the Bill or on the proposed appeal, owing to Lord Playfair's Bill being introduced into the House of Lords too late to allow of a resolution approving its terms to be moved at the last meeting in May. On Tuesday last, the Annual Committee recommended Convocation to adopt the following resolution: "That this House desires the early introduction into Parliament of a Bill for the reconstitution of the University similar to that introduced last year by Lord Playfair, but with an inserted clause securing to the Senate, to Convocation, and to other bodies affected, the right of appeal to the Privy Council on any of the provisions which may hereafter be settled by the Statutory Commission." This resolution was carried by 470 votes against 244, and thus for the third time Convocation, in the only legal way, has pronounced decisively in favour of the Cowper Commission scheme. The progressive rise in the majorities is not the least satisfactory feature of the struggle in Convocation—a majority of 24 in a house of 290 in January of last year rose to 122 in a house of 354 in May, and has now become 226 in a house of 714. The next step rests with the Government, but in view of the remarkable unanimity existing among the bodies affected by the scheme, and the universally favourable attitude of the metropolitan press towards it, we can be in no doubt as to what the final settlement must be.

ON A NEW KIND OF RAYS.[1]

1) A DISCHARGE from a large induction coil is passed through a Hittorf's vacuum tube, or through a well-exhausted Crookes' or Lenard's tube. The tube is surrounded by a fairly close-fitting shield of black paper; it is then possible to see, in a completely darkened room, that paper covered on one side with barium platinocyanide lights up with brilliant fluorescence when brought into the neighbourhood of the tube, whether the painted side or the other be turned towards the tube. The fluorescence is still visible at two metres distance. It is easy to show that the origin of the fluorescence lies within the vacuum tube.

(2) It is seen, therefore, that some agent is capable of penetrating black cardboard which is quite opaque to ultra-violet light, sunlight, or arc-light. It is therefore of interest to investigate how far other bodies can be penetrated by the same agent. It is readily shown that all bodies possess this same transparency, but in very varying degrees. For example, paper is very transparent; the fluorescent screen will light up when placed behind a book of a thousand pages; printer's ink offers no marked resistance. Similarly the fluorescence shows behind two packs of cards; a single card does not visibly diminish the brilliancy of the light. So, again, a single thickness of tinfoil hardly casts a shadow on the screen; several have to be superposed to produce a marked effect. Thick blocks of wood are still transparent. Boards of pine two or three centimetres thick absorb only very little. A piece of sheet aluminium, 15 mm. thick, still allowed the X-rays (as I will call the rays, for the sake of brevity) to pass, but greatly reduced the fluorescence. Glass plates of similar thickness behave similarly; lead glass is, however, much more opaque than glass free from lead. Ebonite several centimetres thick is transparent. If the hand be held before the fluorescent screen, the shadow shows the bones darkly, with only faint outlines of the surrounding tissues.

Water and several other fluids are very transparent. Hydrogen is not markedly more permeable than air. Plates of copper, silver, lead, gold, and platinum also allow the rays to pass, but only when the metal is thin. Platinum ·2 mm. thick allows some rays to pass; silver and copper are more transparent. Lead 1·5 mm. thick is practically opaque. If a square rod of wood 20 mm. in the side be painted on one face with white lead, it casts little shadow when it is so turned that the painted face is parallel to the X-rays, but a strong shadow if the rays have to pass through the painted side. The salts of the metals, either solid or in solution, behave generally as the metals themselves.

(3) The preceding experiments lead to the conclusion that the density of the bodies is the property whose variation mainly affects their permeability. At least no other property seems so marked in this connection. But that the density alone does not determine the transparency is shown by an experiment wherein plates of similar thickness of Iceland spar, glass, aluminium, and quartz were employed as screens. Then the Iceland spar showed itself much less transparent than the other bodies, though of approximately the same density. I have not remarked any strong fluorescence of Iceland spar compared with glass (see below, No. 4).

(4) Increasing thickness increases the hindrance offered to the rays by all bodies. A picture has been impressed on a photographic plate of a number of superposed layers of tinfoil, like steps, presenting thus a regularly increasing thickness. This is to be submitted to photometric processes when a suitable instrument is available.

(5) Pieces of platinum, lead, zinc, and aluminium foil were so arranged as to produce the same weakening of the effect. The annexed table shows the relative thickness and density of the equivalent sheets of metal.

	Thickness.	Relative thickness.	Density
Platinum	·018 mm. ...	1 ...	21·5
Lead	·050 ,, ...	3 ...	11·3
Zinc	·100 ,, ...	6 ...	7·1
Aluminium......	3·500 ,, ...	200 ...	2·6

From these values it is clear that in no case can we obtain the transparency of a body from the product of its density and thickness. The transparency increases much more rapidly than the product decreases.

(6) The fluorescence of barium platinocyanide is not the only noticeable action of the X-rays. It is to be observed that other bodies exhibit fluorescence, *e.g.* calcium sulphide, uranium glass, Iceland spar, rock-salt, &c.

Of special interest in this connection is the fact that photographic dry plates are sensitive to the X-rays. It is thus possible to exhibit the phenomena so as to exclude the danger of error. I have thus confirmed many observations originally made by eye observation with the fluorescent screen. Here the power of the X-rays to pass through wood or cardboard becomes useful. The photographic plate can be exposed to the action without removal of the shutter of the dark slide or other protecting case, so that the experiment need not be conducted in darkness. Manifestly, unexposed plates must not be left in their box near the vacuum tube.

It seems now questionable whether the impression on the plate is a direct effect of the X-rays, or a secondary result induced by the fluorescence of the material of the plate. Films can receive the impression as well as ordinary dry plates.

[1] By W. C. Röntgen. Translated by Arthur Stanton from the *Sitzungsberichte der Würzburger Physik-medic. Gesellschaft*, 1895.

I have not been able to show experimentally that the X-rays give rise to any calorific effects. These, however, may be assumed, for the phenomena of fluorescence show that the X-rays are capable of transformation. It is also certain that all the X-rays falling on a body do not leave it as such.

The retina of the eye is quite insensitive to these rays: the eye placed close to the apparatus sees nothing. It is clear from the experiments that this is not due to want of permeability on the part of the structures of the eye.

(7) After my experiments on the transparency of increasing thicknesses of different media, I proceeded to investigate whether the X-rays could be deflected by a prism. Investigations with water and carbon bisulphide in mica prisms of 30° showed no deviation either on the photographic or the fluorescent plate. For comparison, light rays were allowed to fall on the prism as the apparatus was set up for the experiment. They were deviated 10 mm. and 20 mm. respectively in the case of the two prisms.

With prisms of ebonite and aluminium, I have obtained images on the photographic plate, which point to a possible deviation. It is, however, uncertain, and at most would point to a refractive index 1·05. No deviation can be observed by means of the fluorescent screen. Investigations with the heavier metals have not as yet led to any result, because of their small transparency and the consequent enfeebling of the transmitted rays.

On account of the importance of the question it is desirable to try in other ways whether the X-rays are susceptible of refraction. Finely powdered bodies allow in thick layers but little of the incident light to pass through, in consequence of refraction and reflection. In the case of the X-rays, however, such layers of powder are for equal masses of substance equally transparent with the coherent solid itself. Hence we cannot conclude any regular reflection or refraction of the X-rays. The research was conducted by the aid of finely-powdered rock-salt, fine electrolytic silver powder, and zinc dust already many times employed in chemical work. In all these cases the result, whether by the fluorescent screen or the photographic method, indicated no difference in transparency between the powder and the coherent solid.

It is, hence, obvious that lenses cannot be looked upon as capable of concentrating the X-rays; in effect, both an ebonite and a glass lens of large size prove to be without action. The shadow photograph of a round rod is darker in the middle than at the edge; the image of a cylinder filled with a body more transparent than its walls exhibits the middle brighter than the edge.

(8) The preceding experiments, and others which I pass over, point to the rays being incapable of regular reflection. It is, however, well to detail an observation which at first sight seemed to lead to an opposite conclusion.

I exposed a plate, protected by a black paper sheath, to the X-rays. so that the glass side lay next to the vacuum tube. The sensitive film was partly covered with star-shaped pieces of platinum, lead, zinc, and aluminium. On the developed negative the star-shaped impression showed dark under platinum, lead, and, more markedly, under zinc; the aluminium gave no image. It seems, therefore, that these three metals can reflect the X-rays; as, however, another explanation is possible, I repeated the experiment with this only difference, that a film of thin aluminium foil was interposed between the sensitive film and the metal stars. Such an aluminium plate is opaque to ultra-violet rays, but transparent to X-rays. In the result the images appeared as before, this pointing still to the existence of reflection at metal surfaces.

If one considers this observation in connection with others, namely, on the transparency of powders, and on the state of the surface not being effective in altering the passage of the X-rays through a body, it leads to the probable conclusion that regular reflection does not exist, but that bodies behave to the X-rays as turbid media to light.

Since I have obtained no evidence of refraction at the surface of different media, it seems probable that the X-rays move with the same velocity in all bodies, and in a medium which penetrates everything, and in which the molecules of bodies are embedded. The molecules obstruct the X-rays, the more effectively as the density of the body concerned is greater.

(9) It seemed possible that the geometrical arrangement of the molecules might affect the action of a body upon the X-rays, so that, for example, Iceland spar might exhibit different phenomena according to the relation of the surface of the plate to the axis of the crystal. Experiments with quartz and Iceland spar on this point lead to a negative result.

(10) It is known that Lenard, in his investigations on kathode rays, has shown that they belong to the ether, and can pass through all bodies. Concerning the X-rays the same may be said.

In his latest work, Lenard has investigated the absorption coefficients of various bodies for the kathode rays, including air at atmospheric pressure, which gives 4·10, 3·40, 3·10 for 1 cm., according to the degree of exhaustion of the gas in discharge tube. To judge from the nature of the discharge, I have worked at about the same pressure, but occasionally at greater or smaller pressures. I find, using a Weber's photometer, that the intensity of the fluorescent light varies nearly as the inverse square of the distance between screen and discharge tube. This result is obtained from three very consistent sets of observations at distances of 100 and 200 mm. Hence air absorbs the X-rays much less than the kathode rays. This result is in complete agreement with the previously described result, that the fluorescence of the screen can be still observed at 2 metres from the vacuum tube. In general, other bodies behave like air; they are more transparent for the X-rays than for the kathode rays.

(11) A further distinction, and a noteworthy one, results from the action of a magnet. I have not succeeded in observing any deviation of the X-rays even in very strong magnetic fields.

The deviation of kathode rays by the magnet is one of their peculiar characteristics; it has been observed by Hertz and Lenard, that several kinds of kathode rays exist, which differ by their power of exciting phosphorescence, their susceptibility of absorption, and their deviation by the magnet; but a notable deviation has been observed in all cases which have yet been investigated, and I think that such deviation affords a characteristic not to be set aside lightly.

(12) As the result of many researches, it appears that the place of most brilliant phosphorescence of the walls of the discharge-tube is the chief seat whence the X-rays originate and spread in all directions; that is, the X-rays proceed from the front where the kathode rays strike the glass. If one deviates the kathode rays within the tube by means of a magnet, it is seen that the X-rays proceed from a new point, *i.e.* again from the end of the kathode rays.

Also for this reason the X-rays, which are not deflected by a magnet, cannot be regarded as kathode rays which have passed through the glass, for that passage cannot, according to Lenard, be the cause of the different deflection of the rays. Hence I conclude that the X-rays are not identical with the kathode rays, but are produced from the kathode rays at the glass surface of the tube.

(13) The rays are generated not only in glass. I have obtained them in an apparatus closed by an aluminium plate 2 mm. thick. I purpose later to investigate the behaviour of other substances.

(14) The justification of the term "rays," applied to the phenomena, lies partly in the regular shadow pictures produced by the interposition of a more or less permeable

body between the source and a photographic plate or fluorescent screen.

I have observed and photographed many such shadow pictures. Thus, I have an outline of part of a door covered with lead paint; the image was produced by placing the discharge-tube on one side of the door, and the sensitive plate on the other. I have also a shadow of the bones of the hand (Fig. 1), of a wire wound upon a bobbin, of a set of weights in a box, of a compass card and needle completely enclosed in a metal case (Fig. 2), of a piece of metal where the X-rays show the want of homogeneity, and of other things.

FIG. 1.—Photograph of the bones in the fingers of a living human hand. The third finger has a ring upon it.

For the rectilinear propagation of the rays, I have a pin-hole photograph of the discharge apparatus covered with black paper. It is faint but unmistakable.

(15) I have sought for interference effects of the X-rays, but possibly, in consequence of their small intensity, without result.

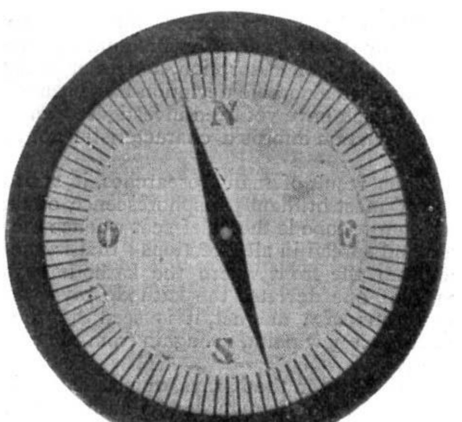

FIG. 2.—Photograph of a compass card and needle completely enclosed in a metal case.

(16) Researches to investigate whether electrostatic forces act on the X-rays are begun but not yet concluded.

(17) If one asks, what then are these X-rays; since they are not kathode rays, one might suppose, from their power of exciting fluorescence and chemical action, them to be due to ultra-violet light. In opposition to this view a weighty set of considerations presents itself. If X-rays be indeed ultra-violet light, then that light must possess the following properties.

(*a*) It is not refracted in passing from air into water, carbon bisulphide, aluminium, rock-salt, glass or zinc.

(*b*) It is incapable of regular reflection at the surfaces of the above bodies.

(*c*) It cannot be polarised by any ordinary polarising media.

(*d*) The absorption by various bodies must depend chiefly on their density.

That is to say, these ultra-violet rays must behave quite differently from the visible, infra-red, and hitherto known ultra-violet rays.

These things appear so unlikely that I have sought for another hypothesis.

A kind of relationship between the new rays and light rays appears to exist; at least the formation of shadows, fluorescence, and the production of chemical action point in this direction. Now it has been known for a long time, that besides the transverse vibrations which account for the phenomena of light, it is possible that longitudinal vibrations should exist in the ether, and, according to the view of some physicists, must exist. It is granted that their existence has not yet been made clear, and their properties are not experimentally demonstrated. Should not the new rays be ascribed to longitudinal waves in the ether?

I must confess that I have in the course of this research made myself more and more familiar with this thought, and venture to put the opinion forward, while I am quite conscious that the hypothesis advanced still requires a more solid foundation.

PROFESSOR RÖNTGEN'S DISCOVERY.

THE newspaper reports of Prof. Röntgen's experiments have, during the past few days, excited considerable interest. The discovery does not appear, however, to be entirely novel, as it was noted by Hertz that metallic films are transparent to the kathode rays from a Crookes or Hittorf tube, and in Lenard's researches, published about two years ago, it is distinctly pointed out that such rays will produce photographic impressions. Indeed, Lenard, employing a tube with an aluminium window, through which the kathode rays passed out with comparative ease, obtained photographic shadow images almost identical with those of Röntgen, through pieces of cardboard and aluminium interposed between the window and the photographic plate.

Prof. Röntgen has, however, shown that this aluminium window is unnecessary, as some portion of the kathode radiations that are photographically active will pass through the glass walls of the tube. Further, he has extended the results obtained by Lenard in a manner that has impressed the popular imagination, while, perhaps most important of all, he has discovered the exceedingly curious fact that bone is so much less transparent to these radiations than flesh and muscle, that if a living human hand be interposed between a Crookes tube and a photographic plate, a shadow photograph can be obtained which shows all the outlines and joints of the bones most distinctly.

Working upon the lines indicated in the telegrams from Vienna, recently published in the daily papers, I have, with the assistance of Mr. J. C. M. Stanton, repeated many of Prof. Röntgen's experiments with entire success. According to one of our first experiments, an ordinary gelatinous bromide dry photographic plate was placed in an ordinary camera back. The wooden shutter of the back was kept closed, and upon it were placed miscellaneous articles such as coins, pieces of wood, carbon, ebonite, vulcanised fibre, aluminium, &c., all being quite opaque to ordinary light. Above was supported a

Crookes tube, which was excited for some minutes. On development, shadows of all the articles placed on the slide were clearly visible, some being more opaque than others. Further experiments were tried with thin plates of aluminium or of black vulcanised fibre interposed between the objects to be photographed and the sensitive surface, this thin plate being used in place of the wood of the camera back. In this manner sharper shadow pictures were obtained. While most thick metal sheets appear to be entirely opaque to the radiations, aluminium appears to be relatively transparent. Ebonite, vulcanised fibre, carbon, wood, cardboard, leather and slate are all very transparent, while, on the other hand, glass is exceedingly opaque. Thin metal foils are moderately opaque, but not altogether so.

As tending to the view that the radiations are more akin to ultra-violet than to infra-red light, it may be mentioned that a solution of alum in water is distinctly more transparent to them than a solution of iodine in bisulphide of carbon.

So far as our own experiments go, it appears that, at any rate without very long exposures, a sufficiently active excitation of the Crookes tube is not obtained by direct connection to an ordinary Rhumkorff induction coil, even of a large size. So-called high frequency currents, however, appear to give good results, and our own experiments have been made with the tube excited by current obtained from the secondary circuit of a Tesla oil coil, through the primary of which were continuously discharged twelve half-gallon Leyden jars, charged by an alternating current of about 20,000 volts pressure, produced by a transformer with a spark-gap across its high-pressure terminals.

For obtaining shadow photographs of inanimate objects, and for testing the relative transparency of different substances, the particular form of Crookes tube employed does not appear to greatly signify, though some forms are, we find, better than others. When, however, the human hand is to be photographed, and it is important to obtain sharp shadows of the bones, the particular form of tube used and its position relative to the hand and sensitive plate appear to be of great importance. So far, owing to the frequent destruction of the tubes, due to overheating of the terminals, we have not been able to ascertain exactly the best form and arrangement for this purpose, except that it appears desirable that the electrodes in the tube should consist of flat and not curved plates, and that these plates should be of small dimensions.

The accompanying photograph of a living human hand (Fig. 1) was exposed for twenty minutes through an aluminium sheet ·0075 in thickness, the Crookes tube, which was one of the kind containing some white phosphorescent material (probably sulphide of barium), being held vertically upside down, with its lowest point about two inches above the centre of the hand.

By substituting a thin sheet of black vulcanised fibre for the aluminium plate, we have since been able to reduce the exposure required to four minutes. Indeed with the aluminium plate, the twenty minutes' exposure appears to have been longer than was necessary. Further, having regard to the great opacity of glass, it seems probable that where ordinary Crookes tubes are employed, a large proportion of the active radiations must be absorbed by the glass of the tube itself. If this is so, by the employment of a tube partly constructed of aluminium, as used by Lenard, the necessary length of exposure could be much reduced.

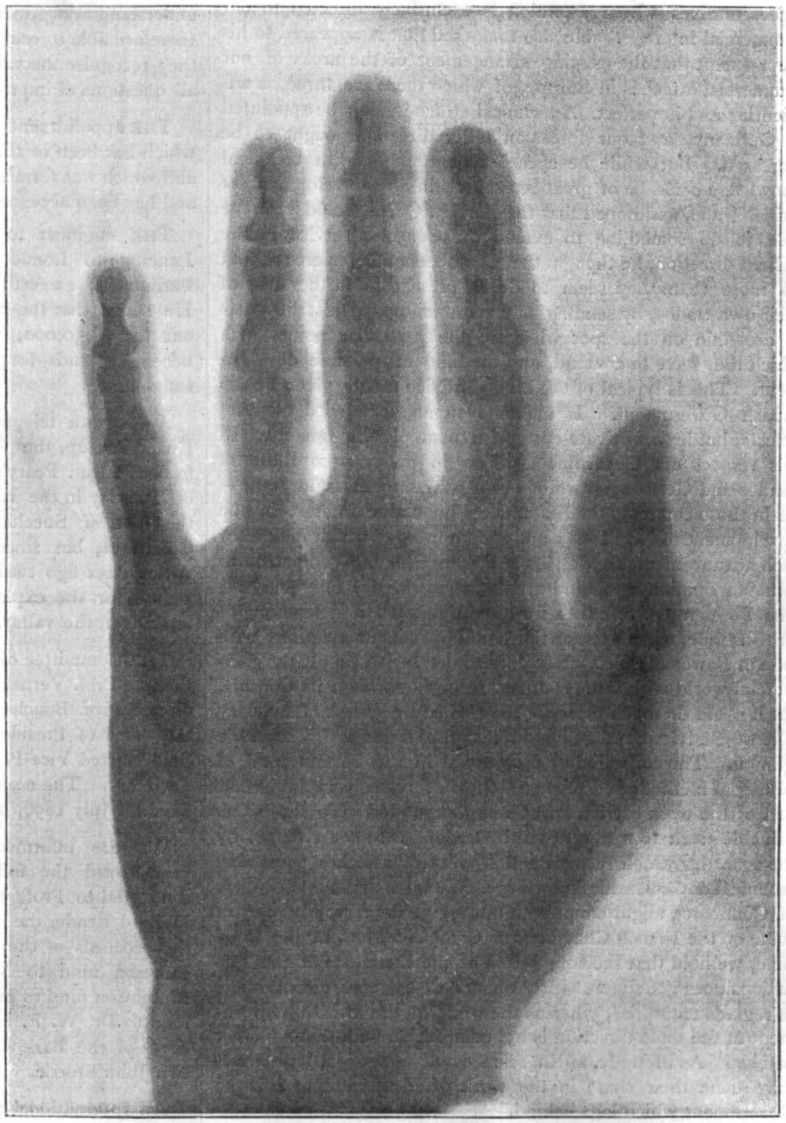

Fig. 1.—Photograph of a living human hand.

A. A. C. Swinton.

NOTES.

At their scientific meeting on March 3, the Zoological Society propose to discuss the much-vexed question of zoological nomenclature. This subject will be introduced by Mr. Sclater, the Secretary of the Society, who will read a paper on the "Rules for naming Animals," lately adopted by the German museums, but since they cannot, the person who has formed a private collection can most successfully manage one for the use of the public, since he better than anyone else is able, in considering the needs of the museum visitor, to keep in mind that saying which is so useful a guide in museum practice—'Put yourself in his place.'

G. BROWN GOODE.

THE X-RAYS.

HELMHOLTZ, Hertz and Kundt, the three greatest physicists of modern Germany, have died within two years, and the friends of German science feared that this loss would be followed by a standstill in physics, or at least by a lack of really important discoveries. But now we have Professor W. Röntgen's investigations in the physical laboratory of the University in Würzburg, the importance of which does not stand behind the famous electrical discoveries of Hertz in Bonn. Röntgen has found a new kind of rays—he calls them the X-rays—which, though invisible to the eye, affect the photographic plate; which produce fluorescent phenomena; which pass through wood, metal and the human body; which are neither broken by prism and lenses nor reflected.

The chief facts about the X-rays are the following: It is well known that the discharges of a large Ruhmkorff induction coil produce in a vacuum tube, such as Crookes' or Hittorf's, colored rays which go in straight lines from the cathode to the glass of the tube. These cathode rays, which have been much studied, are visible to the eye and are well characterized by the fact that the magnet changes their direction; they do not pass thick cardboard, wood, etc. The place where these cathode rays reach the glass of the tube is the centre of Röntgen's X-rays. They are not visible and are not turned aside by a magnet; in short, they are not cathode rays, but are produced by them. If in a dark room we cover the tube by thin, black cardboard, nothing can be seen at all, even if we bring the eye in the direct neighborhood of the tube during the electric discharges. But if we now bring a card covered with barium platinocyanide near it the paper flashes up with every discharge, and this fluorescent effect is visible even if the paper is distant 2 meters from the tube, and it does not matter whether the varnished or the other side of the paper is directed towards the tube. The X-rays thus go through the black cardboard which is opaque to sunlight, and the same effect follows when a bound volume of a thousand printed pages is put between the tube and the fluorescent paper. We can measure the perviousness of the different substances to the new rays by the intensity of the light on the paper, comparing the effect with and without objects between the tube and the fluorescent surface. But there is also an objective way possible to study the perviousness, as the rays produce an effect upon photographic dry plates, which, of course, remains and allows us to control the subjective comparisons. Both methods show that wood is not much less pervious than paper; boards 3 cm. thick absorb very little. Hard rubber disks several centimeters thick do not stop the rays, and even aluminium plates 15 mm. thick do not make the fluorescence entirely disappear. Glass plates vary with the lead in them, those containing lead being less pervious. Platinum is slightly pervious, if the plate is not thicker than 0.2 mm., silver and copper can be a little thicker; lead plates 1.5 mm. thick are no longer pervious. All substances become less pervious with increasing thickness, a fact which is nicely demonstrated by photographs taken through tinfoils of gradually increasing number. The perviousness of substances of equal thickness seems chiefly dependent on the density, but special experiments showed that different metals are not equally pervious if the product of thickness and density is equal; the perviousness of platinum 0.018 mm. thick and a density of 2.15 equals that of lead 0.05 mm. thick, density 11.3 and that of tin 0.1 mm. thick, density 7.1, and that of aluminium 3.5 mm. thick and a density of 2.6. Aluminium may thus be 200 times thicker than platinum, while its density is one-tenth.

The fluorescent effect of the new rays is not confined to barium platinocyanide, but it occurs also on glass, calc-spar, rock-salt, etc. Prisms and lenses do not diffract the rays, nor do prisms of hard rubber or aluminium. With regard to reflection and diffraction the following experiment is interesting. It is well known that pulverized substances do not let pass much light owing to refraction and reflection. Röntgen found with pulverized salt, calc-spar, zinc and other substance that the ray pass through the powder with exactly the same intensity as through the solid substance. Objects with rough surface let it pass exactly like polished ones. The shadow of a round stick is in the middle darker than at the edges; the shadow of a metal tube is in the middle lighter than at the edges.

With regard to the effect on photographic plates, it must not be forgotten that lenses do not refract the rays and therefore ordinary photography is not possible; the pictures of the objects are only shadows. But these shadow-pictures can be taken in the closed wooden box of the camera in a light room, as the sunlight of course does not pass through the wood while the X-rays do. In this way Röntgen took photographs of a set of metal weights in a wooden box and of a thick wire wound as a spiral around a wooden stick; the wood was pervious, the metal of that thickness not, and so the shadows of the weights and of the wire are seen in the photograph, those of the wood scarcely at all. In the same manner he took the picture of a compass needle in the closed box. The door between two rooms did not hinder the chemical effect.

With regard to the nature of the X-rays it seems too early to say anything definite. Röntgen emphazises the fact that they show no refraction and probably therefore move in all substances with equal velocity and are transmitted by a medium which exists everywhere and in which are the molecules of the substances. That is they are ether rays, but not transverse ether waves like the visible or the ultra red or ultra violet invisible light; Röntgen supposes that they are longitudinal ether waves, the existence of which has for a long time been suspected by physicists. Researches regarding many other qualities of the new rays are in progress, and their results may clear up the theoretical interpretation.

It may be that the practical importance of the discovery is equal to the theoretical. It is well known throughout the world that the physical laboratories of Germany have no windows looking towards the patent office. The hunting for practical inventions is not usually important for theoretical science, but the progress of theory usually has practical applications. One practical result in this case is already clear, as the new rays pass boards but not thick metal plates, so they pass the organic substances of the human body, such as skin, muscles, etc., but not the bones. As the metal weights in the wooden box can be photographed, so can photographs of the human bones be taken. Röntgen has put his hand between the tube and the dry plate in the closed camera; the photograph shows clearly all the bones of the hand without the flesh and skin, and the gold rings seem to hang in the air. The value of such a method for medical diagnosis is clear. Fractures and diseases of bones can be examined by photographic plates and metal pieces in the body, for example, needles, bullets, etc., can be found by this method. It will be a matter of the future to learn whether the rays have psycho-physiological effects.

The newspapers report that the whole thing was discovered by mere chance. Röntgen saw the effects on photographic papers which by chance were near to a covered tube during the discharge. This chance origin is not probable, as Lenard, the assistant of Hertz, has been working in the same direction for a long time, and many preparatory experiments by Röntgen himself cleared slowly the way. But suppose chance helped. There were many galvanic effects in the world before Galvani saw by chance the contraction of a frog's leg on an iron gate. The world is always full of such chances, and only the Galvanis and Röntgens are few.

HUGO MÜNSTERBERG,
Harvard University.

FREIBURG, BADEN, January 15, 1896.

SCIENTIFIC NOTES AND NEWS.

PROFESSOR RÖNTGEN'S DISCOVERY.

THE transmission through wood and other substances of the rays from a Crookes' vacuum tube, discovered by Prof. Röntgen, is reported to have been confirmed by Prof. Klupathy of Pesth, Prof. Domalip of Prague, Prof. Czermak of Gratz, and Mr. A. A. C. Swinton of London. The photographs have been exhibited before several scientific societies and by Prof. Röntgen to the Emperor of Germany, from whom he has received a decoration.

Mr. Swinton writes to the *Standard* that with Mr. J. C. M. Stanton he has obtained distinct proof that the radiations in question do pass easily through various substances that are quite opaque to ordinary light, and do produce strong impressions upon ordinary photographic plates entirely incased in light-proof material. Indeed, all substances that he has so far experimented on in his laboratory appear to be transparent to these radiations, even sheets of ebonite, carbon, vulcanized fibre, copper, aluminium and iron, though there is considerable variation in degree. It is thought that the new method of photography may have important applications, not only in surgery, but also in metallurgy, by revealing flaws, inequalities and fractures in metals.

Hertz discovered that cathode rays pass through metal films not translucent to ordinary light, and that Dr. Lenard and others have published careful experiments on the subject. Attention has been called to Prof. Zeugen's having photographed Mt. Blanc, in 1885, by the cathode rays. Prof. Röntgen, however, states that the rays discovered by him, which he calls X-rays, are not cathode rays, as they are not refrangible nor affected by magnetic influences, but that they are more probably longitudinal waves in the ether.

While Hertz and Lenard hold that the cathode rays are vibrations in the ether or even light of short wave-length, Crookes and J. J. Thomson have urged that the rays are negatively charged matter traveling with great velocity. M. Perrin reported to the Paris Academy, on December 30th, experiments which tend to show that the latter view is correct, and some relation will probably be found between cathode rays and the X-rays.

PHYSICS.

BY constructing what might be termed a reversed level, A. Toepler obtains an instrument which he calls a 'pressure level.' It consists of a tube bent to a slight angle at its middle point; the two ends are equally inclined to the horizontal. A short column of a light liquid fills the central portion of the tube. It will be readily seen that if the two open ends are connected with two receivers of any sort, the liquid will, by its position, give the difference of pressure in them. This method of differentially measuring pressures, Mr. Toepler applies (*Wied. Ann.*, Vol. 56, 1895) to measure the difference in weight of two columns of air at different temperatures but both under the same pressure. A long series of determinations of absolute temperatures bears witness to the efficacy of this method, and theoretical considerations remove some apparent objections and give to it certain advantages over the ordinary form of air thermometer.

rheostat, no condenser, and no special transformer, the ordinary lighting transformer answering the purpose. Being in step with the generator, the regulation of the synchronous motor is perfect under all loads, and the motor will pull out of step and stop running if more than a 50 per cent. overload is applied."

Motorcarts.—By invitation of the vice-presidents and council of the Motorcar Club, several members of the House of Commons, and a number of social and commercial magnates, were shown on Saturday a practical display of horseless vehicles, given in the North Gallery and West Gardens of the Imperial Institute. Among those present were Prince Edward of Saxe-Weimar, Sir William Harcourt, M.P., Dr. Rentoul, M.P., Mr. Atherley-Jones, M.P., Sir Frederick Abel, Sir J. Somers Vine, Mr. Hiram Maxim, Mr. H. H. Cunynghame, Mr. H. J. Lawson, Mr. Bertram Van Praagh, Mr. Frederick R. Simms, and the Hon. Evelyn Ellis. Four cars were exhibited, three of them driven by oil motors and the fourth by electricity. This car was the work of Messrs. Garrard and Bumfield, of Coventry. All the cars were shown in operation, under the superintendence of the Hon. Evelyn Ellis and Mr. F. R. Simms, and subsequently Mr. Bertram Van Praagh, vice-president of the club, delivered an address, in which he ventured to prophesy that motorcars would before long form one of the greatest industries of the world. In reference to this subject, it is stated that the Government Bill dealing with motorcart road traffic has been prepared, and will be introduced shortly. It is based on the lines of Mr. Shaw-Lefevre's Bill of last year, but it deals with the question more broadly. It contains no provision restricting the speed at which the new vehicles may be driven, any abuse being left to be dealt with by the ordinary law against the reckless driving, so that the English law will be rendered more favourable to the new traffic than that of France. There appears at present to be no opposition to the Bill, and the members who have interested themselves in the matter believe that the result of its passing will be the immediate appearance of a number of the new vehicles on English roads. Several road authorities have during the week decided to petition in favour of the Bill. Few things are more remarkable than the complete change that has taken place in public sentiment in reference to the introduction of this kind of vehicle. It is not so very long ago since every horseowner would have been up in arms against it.

The Rontgen Radiations.—A large number of experimenters have been at work on this subject during the last fortnight, but little has been done towards coming to any definite conclusion as to the nature of the new radiations. According to the *Elektrotechnischer Anzeiger*, Mr. Alfred Wehrsen has obtained them in Berlin with the help of an influence machine, while at Renfrew, Lord Blythswood has obtained Röntgen photographs by using the brush discharge of his fine Wimshurst induction machine. It would therefore appear that a high vacuum is not an essential condition for the production of the new radiations. An important discovery of Lord Blythswood's is that the Röntgen rays are liable to something equivalent to colour absorption, for on placing strips of coloured gelatine in front of the photographic plate, the blue was least sensitive, red next, and yellow most. The connection of this fact with the colour of the light in the Crookes tube during the discharge may have an important bearing on the theory of the subject. There are on record several curious experiments in which an effect has been produced by very strong lights on a sensitised plate enclosed in a metal case. Metal dark slides being in common use, these have been hitherto generally discredited or referred to the action of the "heat rays." Several French observers now state that a photographic effect is obtained on a plate enclosed between sheets of metal not only with the electric arc, but even with a petroleum flame if the exposure is long enough. The experiments of Mr. Le Bon in this direction have been accepted by the Paris Académie des Sciences, and the phenomenon must therefore be accepted as a real one. In one case cited the image of an aluminium medal was obtained in this way after an exposure of two hours. The Académie have also received communications from Messrs. Benoist and Hurmuzescu on the action of the new rays in causing a discharge of the electroscope, and from Mr. Chabaud on the transparency of metals to them. Platinum ·01mm. thick was absolutely opaque, as was mercury in a thickness of ·1mm. Other metals were transparent in various degress. The subject was also discussed at last week's meeting of the Royal Society, when a paper by Lord Kelvin on the generation of longitudinal waves in ether was read, in which was described an arrangement for obtaining pressural disturbance through a considerable space of air, accompanied by a very small proportion of ordinary transverse waves. A paper by Prof. J. J. Thomson was also read relating to experiments, from which he concludes that all substances when transmitting the Röntgen rays are conductors of electricity. In the course of the discussion which followed Captain Abney said he ventured to doubt whether the action of the Röntgen rays on a sensitive plate could properly be described as photographic. Several facts, in his opinion, indicated some preference for the view that the Röntgen rays acted by first setting up phosphorescence or action of some unknown kind in the glass at the back of the sensitive film. A large number of experiments were also described by Prof. Dewar, showing that resistance to the passage of Röntgen rays increased with increase of atomic weight. Organic substances were all relatively transparent, following the carbon, oxygen, hydrogen, and nitrogen of which they are composed. Mere complexity of structure made no difference, but substitution products showed increasing opacity in the order of the atomic weights of the combined chlorine, bromine, and iodine. It has also been reported in the non-technical Press that M. Carmichael, of the Faculty of Sciences at Lille, has worked out a way of reflecting the new radiations from steel mirrors. He can thus, he says, use them on photographic plates set between the mirror and a Crookes tube. A clear image is thrown back by the mirror on the plate. A most remarkable report was one that Prof. Salvioni, of Perugia, had succeeded in the conversion of the Röntgen rays into visible light. Startling as this might seem, it was on the longitudinal vibration theory by no means impossible, it being one of the troubles of that theory that under certain circumstances there ought to be a conversion of a part of the vibrations from transverse to longitudinal, or *vice versâ*. A description of Prof. Salvioni's instrument has, however, appeared in a footnote of the *Proceedings* of the Vienna Elektrotechnische Verein, published in the *Zeitschrift für Elektrotechnik*, from which it appears that the essential part of the apparatus is a black paper tympanum, treated with some substance that fluoresces under the action of the Röntgen rays, and mounted in a cylinder. It is therefore merely a fluorescence effect, and not a case of direct conversion. The image is produced on the tympanum by the "shadow" from a Crookes tube at one end of the cylinder and viewed through a lens at the other. Before quitting the subject, it may be remarked that one experimenter has considered that the suggestion that the rays might be used as a germicide was worth following up. There was, of course, a negative result.