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AN ANNUAL ADDRESS (CHIEFLY ON THE SUBJECT OF THE RÖNTGEN RAYS).

My Lords, Ladies and Gentlemen,

I had intended, in opening my address, to say a few words as to the objects of the Victoria Institute; but I think what you have heard from Professor Hull in great measure relieves me of that duty, and it will therefore leave me more time to speak on a particular scientific subject, which is one that excites a great deal of interest at the present day. The objects of the Institute are contained, shortly, in the Report which the Hon. Secretary has just read. In furtherance of those objects it has struck me that it would be a useful thing, from time to time, to "take stock," if I may so express myself, as to what is known in this or that branch of science; because sometimes the general public take for granted the accuracy of what has been asserted in the name of science, although it may be by persons who themselves have no very strong claim to be regarded as scientific men. The public accordingly are liable to be misled as to what it is that is well established in science and what it is that is mere conjecture.

Now although truth is but one and cannot contradict itself, yet what is supposed to be truth may be in contradiction to something else which, on totally different grounds, is supposed to be true. In such a case, it behoves us to examine the evidence of that which is supposed to be true on the one hand or the other.

One of the chief objects of the Victoria Institute is to examine, from the point of view of science, such questions as may have arisen from an apparent conflict between scientific results and religious truths; to enquire whether the scientific results are or are not well founded. The utility of the Institute depends, in my opinion, in a very great measure, on the perfect impartiality with which that endeavour is carried out.

It may be that the result of the enquiry is to show that what was supposed to be established on scientific grounds is really nothing more than a more or less plausible conjecture which this or that scientific man may have thrown out. It may be, on the other hand, that the point which has been supposed to be in conflict with religion is really something which is well established on scientific grounds. In such a case it behoves us to re-examine, from the theological or religious side, the arguments on the strength of which the opposition was supposed to have arisen. That latter task the Victoria Institute has not itself undertaken, but leaves it to each individual, avoiding as a society to engage in theological discussions.

In furtherance of the general objects of the Society, I have, as I said, thought it might be useful to "take stock" of what is known in the various departments of science.

On two former occasions I have delivered addresses before the Institute with such an end in view, having relation to certain branches of science, to which I have myself paid more particular attention. In one case I brought before you some of the conclusions to which we have been led regarding the nature of light, and of that mysterious medium for its propagation which we call the luminiferous ether. Those conclusions, so far as I have brought them before you, I think we may regard as thoroughly well established on scientific grounds.

In another address I brought the same subject before you in connection with the perception of light, and the vast utility of the sense of vision to us; and there we have to deal with a very mysterious subject, that of connecting external nature with our own sensations. Here I had to enter on ground which was, more or less, uncertain, and which gives rise to considerable liberty of holding different opinions. To-day, I have chosen as a subject to bring before you one of remarkable interest, which has excited the scientific world, and, I may say, mankind in general, more especially as regards its practical applications to the medical and surgical professions.

Who would have dreamt at the last annual meeting of the Victoria Institute, that before a year was out, we should be able to see on a screen, to receive on a photographic plate, which is afterwards developed, the skeleton, or a portion of the skeleton of a living man, or at least a living child? And as the modes of exciting these rays improve, we shall probably go on, step by step—indeed already, I believe, the whole body of a full grown man has been penetrated by these rays, the discovery of which we owe to Dr. Röntgen. [Applause.]

I feel some diffidence in bringing this subject before you, because I have never, myself, made experiments with the Röntgen rays. Nevertheless I have read a good deal about them, following what others have done, more especially where it connected itself with the subject of light, to which I have paid a good deal of attention. So I cannot but have a tolerably definite idea in my own mind as to the nature of these Röntgen rays which has been a matter in dispute and, I may say, is still in dispute, although I think opinions are generally coming round to that which I will bring before you in the end.

Now before I go to the Röntgen rays direct, I must touch on previous work which gradually led up to them.

For a very long time it has been known that an electric discharge passes more readily through tolerably rarefied air, than through air of greater density, and so with other gases. If we have a longish closed tube, provided with electrodes at the ends by means of platinum wires passing through the glass, if the air be tolerably exhausted from it, an electric discharge passes, comparatively speaking, freely through it forming a beautiful skein of light, if I may so speak, and under certain circumstances that skein of light is divided into strata in a very remarkable manner. These strata fill the greater part of the tube from the positive electrode, or anode, as it is called, till we get nearly, but not quite, to the negative electrode, or cathode. There is a dark space separating the end of the positive discharge which, as I said under suitable conditions and sufficiently high exhaustion, shows stratification, from a blue glow enveloping the negative electrode or part of it. The luminosity about the cathode is somewhat indefinitely bounded on the side of the stratification.

When, however, the exhaustion is carried still further, at the same time the strata become wider apart, and the luminosity recedes from the cathode and expands, forming a sort of glowing halo much more sharply defined on the inside than the outside; in that respect resembling the ordinary luminous halo—not the corona—occasionally seen round the moon. We have here then, these two dark spaces, one outside the halo, where the luminosity gradually fades off, and another dark space on the inside, where the luminosity is more sharply defined, and which reaches to the negative electrode.

Now it is the phenomena in connection with this second dark space that I have more particularly to bring before you. As the exhaustion is rendered higher and higher, the inner dark space gets wider and wider, until at a sufficiently high exhaustion it fills the whole tube or bulb. Mr. Crookes has worked more especially at this subject, and, indeed, the tubes which are now used for the production of the Röntgen rays are generally called "Crookes tubes." I have seen in some of the foreign periodicals the word "Crookes" used to signify one of these tubes. Mr. Crookes's researches in very high vacua led him up to that most remarkable instrument, the Radiometer, the nature of which led us to form clearer conceptions, than we had hitherto done, of the nature of the motion of molecules in gas, or rather, when the theory of the Radiometer was made out, presented us as I may say with a visible exhibition of the thing in actual working.

Now these researches, which led Mr. Crookes to improve his vacuum, naturally led him to examine the electrical phenomena produced by excessively high vacua.

I have said that it was with the second or inner dark space that I had chiefly to do. When the exhaustion is sufficient, that fills the whole tube.

Now what takes place in this dark space? Suppose we interpose a screen, such as a plate of mica with a hole in it. A portion of the discharge from the negative electrode goes through that hole and continues onwards in a straight course until it reaches the wall of the tube. When it reaches the wall of the tube (I will suppose the tube, as it is called, to be made of German glass) it produces a greenish-yellow fluorescence, or phosphorescence, of very brief duration. I need hardly say that if you do not limit what comes from the negative electrode by the screen with a hole in it, you get a broader beam which affects the glass wall over a larger space.

Now what is it that proceeds from the negative electrode towards the glass, and, when it gets there, produces this phosphorescence? Is it light, or is it matter?

One remarkable circumstance connected with this *something* is, that you can deflect it in its course by a magnet. If you present a magnet to a ray of light it does not deflect it at all; but this *something* is easily deflected by a magnet, even by a tolerably weak magnet. Mr. Crookes found, that in addition to that property, if this discharge of a something fell upon one side of a very light fan, formed of thin, split mica, and delicately mounted so as to enable it to spin readily, it sent it spinning round; and he believed that the nature of that which we have here to do with is, that it is a stream of molecules. Nobody, I suppose, denies that there is matter propelled; but there has been a considerable difference of opinion as to whether the matter propelled is of the essence of the phenomenon, or whether it is something merely accidental. Mr. Crookes held that it was of the essence of the phenomenon, and that we had here, really, a stream of molecules, and I must say, for my own part, I believe he was right. But some foreign men of science hold that the projection of matter is altogether a secondary phenomenon, and that what comes through this small hole is really only a process which goes on in the ether—something so far of the nature of light, but yet differing from ordinary light most markedly in the property of being deflected by a magnet. To illustrate what I mean by saying something secondary, Professor Wiedemann, who holds the opinion that it is of the nature of light, or a process going on in the ether, imagines that the projection of matter has no more to do with the phenomenon than the path of a cannon ball has to do with your hearing the sound of the cannon. I think, myself, that it has a great deal more to do with it than that. However, I will leave that matter for the present, to pass on to some researches which led up to the remarkable discovery by Dr. Röntgen.

In Germany, Professor Lenard made a very remarkable series of experiments in what the Germans call, and what we may call, the cathodic rays, and which he believed to be actual rays, and not streams of molecules sent from the cathode. In order to produce these rays, as I will call them, you want a very high vacuum. If, however, you make your vacuum too high and too nearly perfect, you cannot get the electric discharge to pass through it. A perfect vacuum appears to be a non-conductor, and if you attempted to make the electric discharge pass through it, it would go, by preference, on the outside from one electrode to the other, so that you cannot work directly with anything too nearly approaching to a perfect vacuum. But it is a very remarkable thing, though Lenard, I believe, was not the first to discover it, but Hittorff, that these cathodic rays pass or appear to pass through a plate of aluminium which is perfectly impervious to light, or even to the ultra-violet rays, which we know by their effects, though we do not see them directly; so that you may have these cathodic rays at one side and something of the same kind at the other.

Lenard constructed an apparatus commencing with a Crookes tube, in which there was very high, though not too high, exhaustion, with a cathode which was either flat or cup-shaped at one end, and opposite to that, in the part where the cathodic rays would strike the glass if it were there, instead of glass it was closed by a thin plate of aluminium foil, so thin that it would not support the atmospheric pressure although it was impervious to air. But as a continuation of that tube he had another tube, which was also capable of exhaustion. The two tubes had glass tubes leading from them to the same air-pump. There was communication with the air-pump and communication between the two tubes, and you could exhaust them together, and the pressure would be so far reduced that the aluminium plate was strong enough to sustain the reduced pressure. They were both exhausted together until a suitable exhaustion was produced for the production of the cathodic rays in the first tube, and then the connection between the two tubes was intercepted, and the exhaustion of the second tube, which was kept connected with the air-pump, was continued for several days, until, as near as he could get it, there was nothing at all, in the way of gas, left in it. What was the result? In the first tube the cathodic rays were produced by the electric discharge. They fell on the aluminium foil at the end, and then there was a continuation of cathodic rays in the highly exhausted tube—the vacuum tube I will call it—and these went on as if they had been rays of light. They were deflected by the magnet just like the original cathodic rays.

Now at first sight that looks very much as if you had to deal with actual rays, which passed through the aluminium foil, just as rays of light would pass through a plate of glass. But I think the real explanation of it is altogether different. I believe it to be of this nature. First I will use rather a gross illustration, in order that you may the better apprehend the nature of the other explanation that I am about to bring before you. Suppose that I have a row of ivory balls in contact, such as billiard balls, and that another similar ball strikes the first of these. The result is that the last of the balls is sent off, and the striking ball and the intermediate balls remain approximately at rest. Now it is conceivable that something analogous to that may take place as regards these so-called cathodic rays, supposing they are not rays at all, but streams of molecules. It is conceivable that the molecules proceeding from the cathode or negative electrode of the first tube, be they of residual gas, or aluminium, or platinum, might fall upon the thin aluminium plate which forms a wall between the two tubes, separating the one from the other, and that that would give rise to molecular discharge in the second space, although the actual moving molecules never passed through the wall. As I say, that is a rough illustration—rather a gross and material illustration—to enable you to understand more clearly the view I have to bring before you.

I have said that the so-called cathodic rays are easily deflected by a magnet. Now we know from other experiments that if a body sufficiently charged with electricity is in rapid motion, and that motion takes place in a magnetic field, the body tends to be deflected. This looks, therefore, very much as if these cathodic rays are actually streams of molecules, which being highly charged electrically, and of almost inconceivable minuteness, would be deflected by a slight magnetic force. Now, if these highly-charged molecules come to strike on the aluminium wall which separates the two tubes (which are end to end) from one another, it may be that an electrical action goes on which resembles very much what electrolysis is supposed to be according to the views of Grotthuss. I shall not have time to enter into an explanation of that now, for it would lead me too far from the subject; but several present will no doubt understand what I mean when I refer to the views of Grotthuss. The molecules then impinge on the wall, and give rise to a projection of molecules from the second side of the wall, but the latter are not the same molecules which impinged on the first side of it. Whether the molecules projected in the second tube come from a very minute quantity of residual gas, or whether they are derived from the aluminium wall itself, from which they are torn, as it were, does not signify for my purpose. We have here, you see, a conceivable mode of emitting these so-called rays in this way, simulating the transmission of a ray of light through a plate of glass, though it is no ray at all that we are dealing with. I confess I think that that is the true view of the action which takes place. But Lenard himself believed that the cathodic rays were, as he said, processes in the ether. By means of the first tube used alone, as was done in the first instance, but closed with a "window" of somewhat thicker aluminium foil, so as to sustain the atmospheric pressure, he was able to receive the cathodic rays which came from the second surface of the aluminium foil in air, where he could examine them at pleasure, using for their detection sometimes a phosphorescent or fluorescent screen, sometimes a photographic plate. He found that under these conditions they were quickly deflected from their original direction and dispersed, so that they could not be traced far, just like rays of light in a turbid medium, such as water to which a little milk has been added; whereas in a subsequent series of experiments, to which reference has already been made, in which the cathodic rays were received into a second tube, the dispersion became less and less as the exhaustion proceeded, until at the highest attainable approach to a perfect vacuum the dispersion almost disappeared, and the rays were traced straight onwards for a metre and more, and that, without being enlarged by diffraction, as would be the case with rays of light.

Lenard mentioned incidentally that these cathodic rays, as he supposed they were, were able to pass through the hand even. He missed the discovery of the "X" rays because he had, I may say, the cathodic rays too much in his head, and attributed the whole effect on either side of the wall to the cathodic rays. Really the effect is due in part to the cathodic rays, and in part to the Röntgen rays, the existence of which he was not aware of. They cannot be distinguished merely by their effect on a fluorescent screen or on a photographic plate, since both these recipients are affected by the rays of both kinds.

Such was the state of things when Röntgen made his remarkable discovery. According to an account which I saw in one of the newspapers (we cannot vouch for the truth of everything we see in the newspapers), the discovery was made in the first instance accidentally. I cannot give you more authentic information than that, but he had been working with a Crookes tube and he observed that a photographic plate, enclosed in the usual case in which these plates are enclosed when you want to protect them from light, showed on development certain markings on it; so he put the whole apparatus as it had been, with a photographic plate in its case in the same position as before, and the thing was repeated. That is according to the account in the newspapers. A very remarkable discovery was the result. He found that rays were capable of coming out of some part of a Crookes tube which had the remarkable property of passing through substances that are opaque to ordinary light, and opaque even to the ultra violet with which we were previously acquainted. They pass freely through black paper, through cork, wood, or even through the flesh of the hand, though less freely through the bones, so that by simply laying his hand upon the case containing the photographic plate, he actually got a photograph of the bones of his hand.

Well, what is the nature of these rays, and from whence do they come? As Röntgen said in his original paper, a slight examination shows that they have their origin in the part of the Crookes tube opposite to the cathode, and which is rendered phosphorescent by the discharge from the cathode.

The rays, however, which come from this part of the tube, and which appear to have their origin there, differ utterly in some respects from the so-called cathodic rays. If you isolate a portion of them, you find that a magnet has no action upon them; unlike the cathodic rays, they proceed onwards without deflection, just as if the magnet were not there. Like light they proceed in a straight course, but these rays are able to pass through a variety of substances that are opaque to ordinary light, while on the other hand they are stopped by other substances which let light freely through. That, however, does not prove that they are not of the nature of light. You may have, suppose, a red glass which is opaque to green rays, but lets red rays through very freely, so that as regards merely the fact of the "X" rays being stopped by substances transparent to light, while they pass more or less freely through other substances which are quite opaque to ordinary light, that establishes no greater distinction than exists between green and red light. Are they then of the same nature as light?

The "X" rays have some very remarkable properties by which they appear at first sight to differ *in toto* from ordinary light. They pass with either no refraction, or excessively small refraction, through prism-shaped bodies, which we know rays of light do not. They suffer hardly any, if any, regular reflection, unless perhaps at a grazing incidence.

Röntgen himself, in his original paper, dwelt on these peculiarities of the new rays. He formed a prism of aluminium, with which he attempted to obtain deviation of the new rays, but the experiment showed that if there were any deviation at all, at any rate the refractive index could not exceed 1·05. He speaks of the rays not being apparently capable of regular reflection, but he brought forward experiments which show that in a certain sense they appear to be capable of reflection.

A photographic plate with the sensitive surface downwards was placed in its case under a Crookes tube, and immediately under the plate, and inside the case, were placed portions of different kinds of metal, which would be capable of reflecting back the rays on to the sensitive surface, if they admitted of reflection; and it was found that the plate was much more darkened over certain of those metals than where the metal did not exist. There was very little darkening over aluminium and a great deal of darkening comparatively over platinum. This indicated that some effect was produced, though the greater part of it is not one of regular reflection. He conceived the effect to be one of reflection such as you might have from a turbid medium.

There is, however, another mode of explanation which seems worth considering, viz.: that the Röntgen rays, falling upon the metal throw the molecules into a state of vibration, which they communicate to the ether, by a sort of phosphorescence or fluorescence of X light; so that the rays which come from the molecules, though perhaps not of exactly the same nature as the X rays that fell upon them, still have enough of the "X" quality about them, whatever that is, to enable them to get through objects which are opaque to ordinary light.

Lord Blythswood, who has worked a great deal with the Röntgen rays, has written a paper, which was communicated to the Royal Society by Lord Kelvin, in which he establishes a minute regular reflection of those rays from speculum metal at an angle of about 45°. Two plane specula were placed side by side so as to receive at that angle the X rays coming from a Crookes tube, and a duly protected photographic plate was placed in such a position as to receive the regularly reflected rays if there should be any. The developed plate appeared to show a slight indication of the junction between the mirrors; and that the appearance was not illusory was shown by Lord Kelvin, who made measurements on the image and compared the results with what they ought to be on the supposition of a regular reflection. The indication was so faint that I could not myself perceive it (I have not seen the negative, but only positive copies), but Lord Blythswood has given me some positive copies of a negative which he subsequently obtained by reflection from a concave speculum at a small angle of incidence, and which show for certain a minute regular reflection of X rays, while at the same time they prove that the quantity of X light returned by regular reflection is extremely small compared with that which comes from the mirror by some different process.

Now there is another remarkable property of these rays, or absence of property if you like so to call it. Rays of light, as we know, admit of diffraction. If you pass light from a luminous point through a very small slit, or a small hole, the riband, or the beam of light at the other side does not follow merely the geometrical projection of the slit or hole as seen from the source of light, but is more or less widened, and certain alternations of illumination are visible, a phenomenon referable to interferences which I have not time to go into. How do these "X" rays behave under such conditions? It is a very remarkable thing that they do not show these enlargements or exhibit any sign of interference.

The last number of the *Comptes Rendus* contains a paper by M. Gouy in continuation of a former paper, but describing experiments carried out in a still more elaborate manner, which proves the truth of this to a very high degree of strictness. He makes out that if these X rays are periodical, the wave length cannot well be more than the one-hundredth part of the wave length of green light, indicating an enormously high degree of frequency.

Now if we assume that the X rays, like rays of light, and unlike the cathodic rays, are a disturbance propagated in the ether, ponderable matter being concerned only in their origination, not in their propagation, the question arises, what is the relation between the direction of vibration and the direction of propagation? Are the vibrations normal or transversal? We know that the vibrations of the air which constitute sound take place in a to and fro direction, or are what is called normal, that is, perpendicular to the waves of sound. We have the fullest evidence that the vibrations of the ether which constitute light take place in directions perpendicular to that of propagation, or are what is called transversal. To which category do the vibrations belong which constitute the X rays?

If we could obtain polarisation or even partial polarisation of the X rays, that would settle the question, and prove that they are due to transversal vibrations. But most of those who have attempted to obtain indications of their polarisation have failed. This, however, does not prove that the vibrations are normal, for the peculiar properties of the X rays shut us out, or at least almost completely shut us out, from the ordinary means of obtaining polarisation. There is, however, one paper in the *Comptes Rendus*, by Prince Galitzine and M. de Karnojitsky, in which the authors profess to have obtained by a special method undoubted indications of polarisation. No reasonable doubt can remain as to the abstract capacity of these rays for polarisation after what has been done by another physicist. I wish I had time to go into the experiments that have been made by M. H. Becquerel in the direction of polarisation; but I have already kept you too long. He had more particularly studied a very remarkable phenomenon, viz.: that certain phosphorescent bodies, such as sulphide of calcium for instance, and salts of uranium, on exposure to ordinary sunlight give out rays of some kind which pass through bodies opaque to light and are able to affect a photographic plate beneath them. So far these agree in their properties with the X rays which are obtained from a Crookes tube, which they far more closely resemble than they do rays of ordinary light; but the rays thus obtained were found by Becquerel to admit of polarisation by means of tourmalines in a manner altogether unmistakable. I think therefore that we may take it as established that the Röntgen rays are due to some kind of transversal disturbance propagated in the ether.

The non-exhibition of the ordinary phenomena of diffraction and interference is explicable on the supposition that the vibrations in the X rays are of an excessively high order of frequency. I am not sure that a different sort of explanation might not, perhaps, be possible which I have in my mind, though I have not matured it; but, save the possibility of that, one is led to regard them as consisting of transverse vibrations of excessively high frequency. This opens out some points of considerable interest in the theory of light; but I am afraid it would keep you too long if I were to attempt to go further into this matter. I will merely remark that taking the way in which these rays are most commonly produced, viz.: as coming from a point where the cathodic discharge in the Crookes tube falls on the opposite wall, we may understand how it is that vibrations of excessively unusual frequency may be produced. These highly charged molecules, charged with electricity, coming suddenly against the wall, may produce vibrations of a degree of frequency which we are not at all prepared for; but I see by the clock that I must not detain you any longer on speculations. [Applause.]

Postscript.

This "different sort of explanation" is one between which and the supposition of periodic vibrations of excessively high frequency my mind has for a long time oscillated. In the above lecture I gave the preference to the latter; but subsequent reflection leads me strongly to incline to the former. I hope before long to develope fully these views elsewhere; meanwhile suffice it to say that I am disposed to regard the disturbance as non-periodic, though having certain features in common with a periodic disturbance of excessively high frequency.

The Right Honourable Lord Kelvin, G.C.V.O., F.R.S.—Mr. President, my Lord Chancellor, ladies and gentlemen; I must first express, on my own account, great regret that I was unable to arrive in time to hear the whole of the Annual Address. Judging from what I have heard, I am able to appreciate how great has been my loss in not hearing the whole. I do not, however, feel at all unable to ask you to accord a most hearty vote of thanks, for what I have heard is more than enough to justify all I would say in support of the motion which I have to put before you. Sir George Stokes has given a most interesting and important description of one of the greatest discoveries of this century. The subject upon which he has spoken—in fact all that he has touched upon—is full of contentious matter. The scientific world has been greatly agitated in regard to the Röntgen phenomena since the beginning of January. Almost every sentence that the President has put before us is strongly in opposition to a great deal that has been published by many exceedingly able men, both mathematicians and experimenters. I have listened with great pleasure, satisfaction and comfort to Sir George Stokes's declarations. If I had doubts myself, my doubts would have been removed. My faith is strongly fortified by all we have heard from him. In physical science my touchstone as to truth is, "What does Sir George Stokes think of it?" (Hear! hear!) When I hear that he declares for transverse vibrations, and when I hear the strong reasons he has put before us for this conclusion, I myself am very strongly fortified indeed in accepting it. I do not, however, forget that it is put before us not as absolutely demonstrated but as his present opinion. There is no one who is so thoroughly secure as a guide in all scientific matters as our President. He will not put forward anything, and say it is certain, unless he has perfect reason for saying so. He has given us strong reason to believe that the Röntgen phenomena consist of transverse vibrations. He does not tell us that this is *certainly* true; he leaves our minds open to the possibility of other explanations—indeed he has tantalised us very much with the idea of the possibility of another explanation to which he has alluded. I wish the hands of the clock could be set back for half-an-hour, and that the President could be persuaded to give us some idea of the explanation he has referred to; he certainly leaves us with the impression that it is exceedingly probable that the Röntgen rays do consist of transverse vibrations. But how are they related to those of ordinary light? There are, perhaps, a hundredfold more of them in a second of time, so that as there are about 700 million million of vibrations per second in green light, we may have 700 hundred, or 70 thousand, million million vibrations of Röntgen rays in a second of time. Perhaps that does not seem much more marvellous to some than when we talk of 700 million million of vibrations in green light. There is so great a margin that our power of being astonished is at an end, and we are not astonished if we are told that the vibrations of the Röntgen rays are a hundred or a thousand times faster, by mathematicians who have studied the question; but we may feel surprised, indeed, that rays proceeding from the cathode and striking against a target should produce, on their impact, vibrations of a hundred times greater frequency than any of the qualities of light known previously.

I am sure you must all feel that a curtain has been drawn from before your eyes, and that we have been allowed to look into some of the mysteries and wonders of nature. It is only half a year since these X rays, produced by the impact of electrified particles inside a vacuum tube, were known to exist, and it is wonderful how much of the discovery is dealt with in Röntgen's short paper.

We are all deeply indebted to the President for his Address, and I propose a hearty vote of thanks to him for it, and to those who have read papers before the Victoria Institute during this session (Applause).

Sir JOSEPH FAYRER, Bart., F.R.S.—I have much pleasure in seconding the resolution.

[Carried with applause.]

The PRESIDENT (Sir G. G. Stokes, Bart.).—I will only detain you to express the thanks I feel for the very kind manner in which you have received my Address. (Cheers.)

[Dr. GERARD SMITH, M.R.C.S., a Member of Council, very kindly added to the interest of the Meeting by exhibiting some very remarkable specimens of surgical and other subjects taken by means of the Röntgen rays.]

The Meeting was then adjourned.