to the untechnically instructed. Its natural meaning implies echo or reverberation, and has a definite relation to sound. Now, although a sort of reverberation or repetition is part of the effect intended to be denoted by the phrase resonance, yet the most essential feature of that phenomenon, and the one most to be emphasized in the recent extensions of the term, viz. the accord of frequency or similar tuning between two vibrators, is not connoted at all. Hence, even in acoustics the term is hardly satisfactory, while its extension to other departments of physics may be misleading.

It was suggested, however, by Dr. Arthur Myers, that the existing word *σύντονος* has almost exactly the right connotation, and has no special limitation to sound; while the derivatives *syntony*, *syntonic*, and *syntonise* may readily become English without exciting repulsion.

The adjective "symphonic," suggested by the reporter of the Physical Society, does not strike me as so good, because it specially refers to sound again, and because the word "symphony" has already another definite meaning.

July 10. OLIVER J. LODGE.

Force and Determinism.

I DO not think there are many non-physicists who will attempt to gainsay the fact that, under physical constraint, the direction of motion may be determined without affecting the quantity of the energy concerned, and without expenditure of energy. This is seen when the earth and sun revolve around their common centre of gravity, or when I twirl my stick around my finger and thumb; the earth and sun in the one case, and the ferrule and knob of my stick in the other case, being bound into one system physically. But I do think that an able and clear-headed physicist like Dr. Oliver Lodge would be doing a great service to non-physicists if he would, in your widely-circulated columns, explain and solve, shortly and in non-technical language, the difficulties which trouble some of them; aiding them, for example, to comprehend the exact force of the words expenditure of energy, and helping them to see that in all known cases of change of direction of motion such change is effected under physical constraint. It is when they are told by a certain class of metaphysicians, who quote, or misquote, physics in support of their assumptions, that physical motion is controlled by will-power or volition, always acting at right angles to direction of motion, and therefore leaving the amount of energy unchanged; it is *then*, I say, that they begin to grow restive, and to demand definite and verifiable evidence that such metaphysical constraint is (*pace* Sir John Herschel) a necessary or philosophical conception, and that it is impossible to explain the phenomena without having recourse to it. If Dr. Lodge would consent to help non-physicists in this way, and would indicate what are the "important psychological consequences" to which he alludes, he would be doing some of us a good turn. C. LLOYD MORGAN.

University College, Bristol.

As Prof. Lodge says he is glad to see that his statement, "although expenditure of energy is needed to increase the speed of matter none is required to alter its direction," called in question, and as he has so kindly answered one letter on the subject, may I ask him to criticize the following remarks?

The theory of kinematics is based on certain geometrical concepts, which may be summed up in the term space, and on the concept of time. The laws of motion, together with the assertion that mass is not a function of space or time, may logically be regarded as implicitly defining mass and force. Energy may similarly be defined, in terms of these kinematic concepts, as $\Sigma\frac{1}{2}mv$. For I think the progress of science is tending to show that the term "potential energy" is only a cloak to cover our ignorance of the kinetic energies which for the moment have escaped our ken. But in any case the statement quoted is logically only a truism, deduced from the definitions of its terms, and is therefore indisputable in all mechanical theorems. But if it is to be applied outside the sphere of pure mechanics, the moral will lie in the application of it—that is, it will be necessary to examine, before applying it to any new subject-matter, whether the definitions from which it was deduced apply to that subject-matter or not.

For example, by the third law of motion, mechanical force only acts *between two masses*, the momenta generated in them being equal and opposite. If, therefore, psychic force is to come under the definition of mechanical force, it can only act *between* two particles. And, therefore, if psychic force is to do no work, by reason of its always acting in a direction normal to the path of a particle, it can only act between two particles whose paths happen to have a common normal—an occurrence which must be infinitely rare. EDWARD T. DIXON.

12 Barkston Mansions, South Kensington, July 4.

Magnetic Anomalies.

THE discovery of very strong magnetic anomalies between Charkov and Kursk in Russia, to which A. de Tillo has lately referred in the *Comptes rendus* and in NATURE, raises the question whether the values there observed are strictly local, or extend over a relatively wide area. Thus, it would be of great interest to know if, on moving, say, some metres away from a station, the declination and inclination hold the same value. If not, there is clearly some cause which acts at a short distance; but if constancy is observed, a great step would be taken towards the settlement of the question as to the existence of strong variations common to a wide area.

When magnetic anomalies are observed, the first thing to be done is to ascertain whether the values found in a given locality have a definite meaning—that is, whether they do not change for slight displacements; otherwise, the determination of the magnetic elements has no meaning, as it is impossible to refer them to geographical co-ordinates.

The overlooking of this precaution has often led to serious mistakes. ALFONSO SELLA.

Biella, July 4.

Physical Religion.

AS a constant reader of NATURE from its commencement, and the possessor of its forty-three and a half volumes, I venture (after reading the review of "Physical Religion" in this week's number) to ask if it is intended to develop it into a theological journal. Because, however smart it may be to abolish Abraham without "even taking the trouble to discuss" him, or to dispose of *Lux Mundi* in a contemptuous sentence, it is hardly in accordance with scientific methods.

It is curious that many "Agnostics," though by their own showing (if they would talk Latin instead of Greek) they are *Ignoramuses* at best, should be so certainly sure of everything, when a little reflection and modesty might satisfy them that as "*Know-nothings*" (in plain English) they have no more right to *deny* than to *assert*.

The standing motto of your title might be improved by the addition of "Ne supra crepidam sutor."

Hampstead Heath, July 11. B. WOODD SMITH.

SOME APPLICATIONS OF PHOTOGRAPHY.[1]

ONE of the subjects to which I propose to invite your attention this evening is the application of instantaneous photography to the illustration of certain mechanical phenomena which pass so quickly as to elude ordinary means of observation. The expression "instantaneous photography" is perhaps not quite a defensible one, because no photography can be really instantaneous—some time must always be occupied. One of the simplest and most commonly used methods of obtaining very short exposures is by the use of movable shutters, for which purpose many ingenious mechanical devices have been invented. About two years ago we had a lecture from Prof. Muybridge, in which he showed us the application of this method—and a remarkably interesting application it was—to the examination of the various positions assumed by a horse in his several gaits. Other means, however, may be employed to the same end, and one of them depends upon the production of an instantaneous light. It will obviously come to the same thing whether the light to which we expose the plates be instantaneous, or whether by a mechanical device we allow the plate to be submitted to a continuous light for

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[1] Friday Evening Discourse, delivered at the Royal Institution of Great Britain, on February 6, 1891, by Lord Rayleigh, F.R.S., Professor of Natural Philosophy, R.I.

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only a very short time. A good deal of use has been made in this way of what is known as the magnesium flash light. A cloud of magnesium powder is ignited, and blazes up quickly with a bright light of very short duration. Now I want to compare that mode of illumination with another, in order to be able to judge of the relative degree of instantaneity, if I may use such an expression. We will illumine for a short time a revolving disk, composed of black and white sectors; and the result will depend upon how quick the motion is as compared with the duration of the light. If the light could be truly instantaneous, it would of necessity show the disk apparently stationary. I believe that the duration of this light is variously estimated at from one-tenth to one-fiftieth of a second; and as the arrangement that I have here is one of the slowest, we may assume that the time occupied will be about a tenth of a second. I will say the words one, two, three, and at the word three Mr. Gordon will project the powder into the flame of a spirit-lamp, and the flash will be produced. Please give your attention to the disk, for the question is whether the present uniform grey will be displaced by a perception of the individual black and white sectors. [Experiment.] You see the flash was *not* instantaneous enough to resolve the grey into its components.

I want now to contrast with that mode of illumination one obtained by means of an electric spark. We have here an arrangement by which we can charge Leyden jars from a Wimshurst machine. When the charge is sufficient, a spark will pass inside a lantern, and the light proceeding from it will be condensed and thrown upon the same revolving disk as before. The test will be very much more severe; but, severe as it is, I think we shall find that the electric flash will bear it. The teeth on the outside of the disk are very numerous, and we will make them revolve as fast as we can, but we shall find that under the electric light they will appear to be absolutely stationary. [Experiment.] You will agree that the outlines of the black and white sectors are seen perfectly sharp.

Now, by means of this arrangement we might investigate a limit to the duration of the spark, because with a little care we could determine how fast the teeth are travelling—what space they pass through in a second of time. For this purpose it would not be safe to calculate from the multiplying gear on the assumption of no slip. A better way would be to direct a current of air upon the teeth themselves, and make them give rise to a musical note, as in the so-called siren. From the appearance of the disk under the spark we might safely say, I think, that the duration of the light is less than a tenth of the time occupied by a single tooth in passing. But the spark is in reality much more instantaneous than can be proved by the means at present at our command. In order to determine its duration, it would be necessary to have recourse to that powerful weapon the revolving mirror; and I do not, therefore, propose to go further into the matter to-night.

Experiments of this kind were made some twenty years ago by Prof. Rood, of New York, both on the duration of the discharge of a Leyden jar, and also on that of lightning. Prof. Rood found that the result depended somewhat upon the circumstances of the case, the discharge of a small jar being generally more instantaneous than that of a larger one. He proved that in certain cases the duration of the principal part of the light was as low as one twenty-five-millionth part of a second of time. That is a statement which probably conveys very little of its real meaning. A million seconds is about twelve days and nights. Twenty-five million seconds is nearly a year. So that the time occupied by the spark in Prof. Rood's experiment is about the same fraction of one second that one second is of a year. In many other cases the duration was somewhat greater; but in all his experiments it was well under the one-millionth part of a second. In certain cases you may have multiple sparks. I do not refer to the oscillating discharges of which Prof. Lodge gave us so interesting an account last year; Prof. Rood's multiple discharge was not of that character. It consisted of several detached overflows of his Leyden jar when charged by the Rhumkorff coil. One number mentioned for the total duration was one six-thousandth part of a second; but the individual discharges had the degree of instantaneity of which I have spoken.

It is not a difficult matter to adapt the electrical spark to instantaneous photography. We will put the lantern into its proper position, excite the electric sparks within it, causing them to be condensed by the condenser of the lantern on to the photographic lens. We will then put the object in front of the lantern-condenser, remove the cap from the lens, expose the plate to the spark when it comes, and thus obtain an instantaneous view of whatever may be going on. I propose to go through the operation of taking such a photograph presently. I will not attempt any of the more difficult things of which I shall speak, but will take a comparatively easy subject—a stream of bubbles of gas passing up through a liquid. In order that you may see what this looks like when observed in the ordinary way, we have arranged it here for projection upon the screen. [Experiment.] The gas issues from the nozzle, and comes up in a stream, but so fast that you cannot fairly see the bubbles. If, however, we take an instantaneous picture, we shall find that the stream is decomposed into its constituent parts. We arrange the trough of liquid in front of the lantern which contains the spark-making apparatus—[Experiment]—and we will expose a plate, though I hardly expect a good result in a lecture. A photographer's lamp provides some yellow light to enable us to see when other light is excluded. There goes the spark; the plate is exposed, and the thing is done. We will develop the plate, and see what it is good for; and if it turns out fit to show, we will have it on the screen within the hour.

In the meantime, we will project on the screen some slides taken in the same way and with the same subject. [Photograph shown.] That is an instantaneous photograph of a stream of bubbles. You see that the bubbles form at the nozzle from the very first moment, contrasting in that respect with the behaviour of jets of water projected into air (Fig. 1).

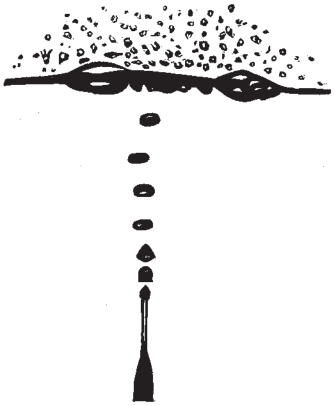

FIG. 1.

The latter is our next subject. This is the reservoir from which the water is supplied. It issues from a nozzle of drawn-out glass, and at the moment of issue it consists of a cylindrical body of water. The cylindrical form is unstable, however, and the water rapidly breaks up into drops, which succeed one another so rapidly that they can hardly be detected by ordinary vision. But by

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means of instantaneous photography the individual drops can be made evident. I will first project the jet itself on the screen, in order that you may appreciate the subject which we shall see presently represented by photography. [Experiment.] Along the first part of its length the jet of water is continuous. After a certain point it breaks into drops, but you cannot see them because of their rapidity. If we act on the jet with a vibrating body, such as a tuning-fork, the breaking into drops occurs still earlier, the drops are more regular, and assume a curious periodic appearance, investigated by Savart. I have some photographs of jets of that nature. Taken as described, they do not differ much in appearance from those obtained by Chichester Bell, and by Mr. Boys. We get what we may regard as simply shadows of the jet obtained by instantaneous illumination ; so that these photographs show little more than the outlines of the subject. They show a little more, on account of the lens-like action of the cylinder and of the drops. Here we have an instantaneous view of a jet similar to the one we were looking at just now (Fig. 2). This is the continuous part ; it gradually ripples itself as it comes along ; the ripples increase ; then the contraction becomes a kind of ligament connecting consecutive drops ; the ligament next gives way, and we have the individual drops completely formed. The small points of light are the result of the lens-like action of the drops. [Other instantaneous views also shown.]

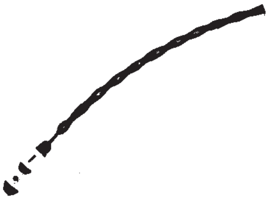
FIG. 2.

The pictures can usually be improved by diffusing somewhat the light of the spark with which they are taken. In front of the ordinary condensing lens of the magic lantern we slide in a piece of ground glass, slightly oiled, and we then get better pictures showing more shading. [Photograph shown.] Here is one done in that way ; you would hardly believe it to be water resolved into drops under the action of a tremor. It looks more like mercury. You will notice the long ligament trying to break up into drops on its own account, but not succeeding (Fig. 3).

There is another, with the ligament extremely prolonged. In this case it sometimes gathers itself into two drops (Fig. 4).

[A number of photographs showing slight variations were exhibited.]

The mechanical cause of this breaking into drops is, I need hardly remind you, the surface tension or capillary force of the liquid surface. The elongated cylinder is an unstable form, and tends to become alternately swollen and contracted. In speaking on this subject I have often been embarrassed for want of an appropriate word to describe the condition in question. But a few days ago, during a biological discussion, I found that there is a recognized, if not a very pleasant, word. The cylindrical jet may be said to become *varicose*, and the varicosity goes on increasing with time, until eventually it leads to absolute disruption.

There is another class of unstable jets presenting many points of analogy with the capillary ones, and yet in many respects quite distinct from them. I refer to the phenomena of sensitive flames. The flame, however, is not the essential part of the matter, but rather an indicator of what has happened. Any jet of fluid playing into a stationary environment is sensitive, and the most convenient form for our present purpose is a jet of coloured in uncoloured water. In this case we shall use a solution of permanganate of potash playing into an atmosphere of other water containing acid and sulphate of iron, which exercises a decolourising effect on the permanganate, and so retards the general clouding up of the whole mass by accumulation of colour. [Experiment.] Mr. Gordon will release the clip, and we shall get a jet of permanganate playing into the liquid. If everything were pefectly steady, we might see a line of purple liquid extending to the bottom of the trough ; but in this theatre it is almost impossible to get anything steady. The instability to which the jet is subject now manifests itself, and we get a breaking away into clouds something like smoke from chimneys. A heavy tuning-fork vibrating at ten to the second acts upon it with great advantage, and regularizes the disruption. A little more pressure will increase the instability, and the jet goes suddenly into confusion, although at first, near the nozzle, it is pretty regular.

It may now be asked "What is the jet doing?" That is just the question which the instantaneous method enables us to answer. For this purpose the permanganate which we have used to make the jet visible is not of much service. It is too transparent to the photographic rays, and so it was replaced by bichromate of potash. Here the opposite difficulty arises ; for the bichromate is invisible by the yellow light in which the adjustments have to be made. I was eventually reduced to mixing the two materials together, the one serving to render the jet visible to the eye and the other to the photographic plate. Here is an instantaneous picture of such a jet as was before you a moment ago, only under the action of a regular vibrator. It is *sinuous*, turning first in one direction and then in the other. The original cylinder, which is the natural form of the jet as it issues from the nozzle, curves itself gently as it passes along through the water. It thus becomes sinuous, and the amount of the sinuosity increases, until in some cases the consecutive folds come into collision with one another. [Several photographs of sinuous jets were shown, two of which are reproduced in Figs. 5 and 6.]


FIG. 3.


FIG. 4.

The comparison of the two classes of jets is of great interest. There is an analogy as regards the instability, the vibrations caused by disturbance gradually increasing as the distance from the nozzle increases ; but there is a

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great difference as to the nature of the deviation from the equilibrium condition, and as to the kind of force best adapted to bring it about. The one gives way by becoming varicose; the other by becoming sinuous. The only forces capable of producing varicosity are symmetrical forces, which act alike all round. To produce sinuosity, we want exactly the reverse—a force which acts upon the jet transversely and unsymmetrically.

I will now pass on to another subject for instantaneous photography—namely, the soap film. Everybody knows that if you blow a soap bubble it will break—generally before you wish. The process of breaking is exceedingly rapid, and difficult to trace by the unaided eye. If we can get a soap film on this ring, we will project it upon the screen and then break it before your eyes, so as to enable you to form your own impressions as to the rapidity of the operation. For some time it has been my ambition to photograph a soap bubble in the act of breaking. I was prepared for difficulty, believing that the time occupied was less than the twentieth of a second. But it turns out to be a good deal less even than that. Accordingly the subject is far more difficult to deal with than are those jets of water or coloured liquids which one can photograph at any moment that the spark happens to come.

Fig. 5.

Fig. 6.

There is the film, seen by reflected light. One of the first difficulties we have to contend with is that it is not easy to break the film exactly when we wish. We will drop a shot through it. The shot has gone through, as you see, but it has not broken the film; and when the film is a thick one, you may drop a shot through almost any number of times from a moderate height without producing any effect. You would suppose that the shot in going through would necessarily make a hole, and end the life of the film. The shot goes through, however, without making a hole. The operation can be traced, not very well with a shot, but with a ball of cork stuck on the end of a pin, and pushed through. A dry shot does not readily break the film; and as it was necessary for our purpose to effect the rupture in a well-defined manner, here was a difficulty which we had to overcome. We found, after a few trials, that we could get over it by wetting the shot with alcohol.

We will try again with dry shot. Three shots have gone through and nothing has happened. Now we will try one wetted with alcohol, and I expect it will break the film at once. There! it has gone!

The apparatus for executing the photography of a breaking soap film will of necessity be more complicated than before, because we have to time the spark exactly with the breaking of the film. The device I have used is to drop two balls simultaneously, so that one should determine the spark and the other rupture the film. The most obvious plan was to hang iron balls to two electro-magnets, and cause them to drop by breaking the circuit, so that both were let go at the same moment. The method was not quite a success, however, because there was apt to be a little hesitation in letting go the balls. So we adopted another plan. The balls were not held by electro-magnetism, but by springs (Fig. 8) pressing laterally, and these were pulled off by electro-magnets. The proper moment for putting down the key and so liberating the balls, is indicated by the tap of the beam of an attracted disk electrometer as it strikes against the upper stop. One falling ball determines the spark, by filling up most of the interval between two fixed ones submitted to the necessary electric pressure. Another ball, or rather shot, wetted with alcohol, is let go at the same moment, and breaks the film on its passage through it. By varying the distances dropped through, the occurrence of one event may be adjusted relatively to the other. The spark which passes to the falling ball is, however, not the one which illuminates the photographic plate. The latter occurs within the lantern, and forms part of a circuit in connection with the *outer* coatings of the Leyden jars,[1] the whole arrangement being similar to that adopted by Prof. Lodge in his experiments upon alternative paths of discharge. Fig. 8 will give a general idea of the disposition of the apparatus. [Several photographs of breaking films were shown upon the screen; one of these is reproduced in Fig. 7.][2]

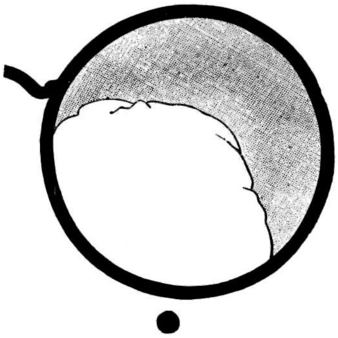

Fig. 7.

This work proved more difficult than I had expected; and the evidence of our photographs supplies the explanation—namely, that the rupture of the film is an extraordinarily rapid operation. It was found that the whole difference between being too early and too late was represented by a displacement of the falling ball through less than a diameter, viz. $\frac{1}{4}$ inch nearly. The drop which we gave was about a foot. The speed of the ball would thus be about 100 inches per second; therefore the whole difference between being too soon and too late is represented by $\frac{1}{300}$ second. Success is impossible, unless the spark can be got to occur within the limits of this short interval.

Prof. Dewar has directed my attention to the fact that Dupré, a good many years ago, calculated the speed of rupture of a film. We know that the energy of the film is in proportion to its area. When a film is partially broken, some of the area is gone, and the corresponding potential energy is expended in generating the velocity of

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[1] In practice there were two sets of three jars each.

[2] The appearance of the breaking bubble, as *seen* under instantaneous illumination, was first described by Marangoni and Stephanelli, *Nuovo Cimento*, 1873.

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the thickened edge, which bounds the still unbroken portion. The speed, then, at which the edge will go depends upon the thickness of the film. Dupré took a rather extreme case, and calculated a velocity of 32 metres per second. Here, with a greater thickness, our velocity was, perhaps, 16 yards a second, agreeing fairly well with Dupré's theory.

I now pass on to another subject with which I have lately been engaged—namely, the connection between aperture and the definition of optical images. It has long been known to astronomers and to those who study optics that the definition of an optical instrument is proportional to the aperture employed; but I do not think that the theory is as widely appreciated as it should be. I do not know whether, in the presence of my colleague, I may venture to say that I fear the spectroscopists are among the worst sinners in this respect. They constantly speak of the dispersion of their instruments as if that by itself could give any idea of the power employed. You may have a spectroscope of any degree of dispersion, and yet of resolving power insufficient to separate even the D lines. What is the reason of this? Why is it that we cannot get as high a definition as we please with a limited aperture? Some people say that the reason why large telescopes are necessary is, because it is only by their means that we can get enough light. That may be in some cases a sufficient reason, but that it is inadequate in others will be apparent, if we consider the case of the sun. Here we do not want more light, but rather are anxious to get rid of a light already excessive. The principal *raison d'être* of large telescopes is, that without a large aperture definition is bad, however perfect the lenses may be. In accordance with the historical development of the science of optics, the student is told that the lens collects the rays from one point to a focus at another; but when he has made further advance in the science he finds that this is not so. The truth is that we are in the habit of regarding this subject in a distorted manner. The difficulty is, not to explain why optical images are imperfect, no matter how good the lens employed, but rather how it is that they manage to be as good as they are. In reality the optical image of even a mathematical point has a considerable extension; light coming from one point cannot be concentrated into another point by any arrangement. There must be diffusion, and the reason is not hard to see in a general way. Consider what happens at the mathematical focus, where, if anywhere, the light should all be concentrated. At that point all the rays coming from the original radiant point arrive in the same phase. The different paths of the rays are all rendered optically equal, the greater actual distance that some of them have to travel being compensated for in the case of those which come through the centre by an optical retardation due to the substitution of glass for air; so that all the rays arrive at the same time.[1] If we take a point not quite at the mathematical focus but near it, it is obvious that there must be a good deal of light there also. The only reason for any diminution at the second point lies in the discrepancies of phase which now occur; and these can only enter by degrees. Once grant that the image of a mathematical point is a diffused patch of light, and it follows that there must be a limit to definition. The images of the com-

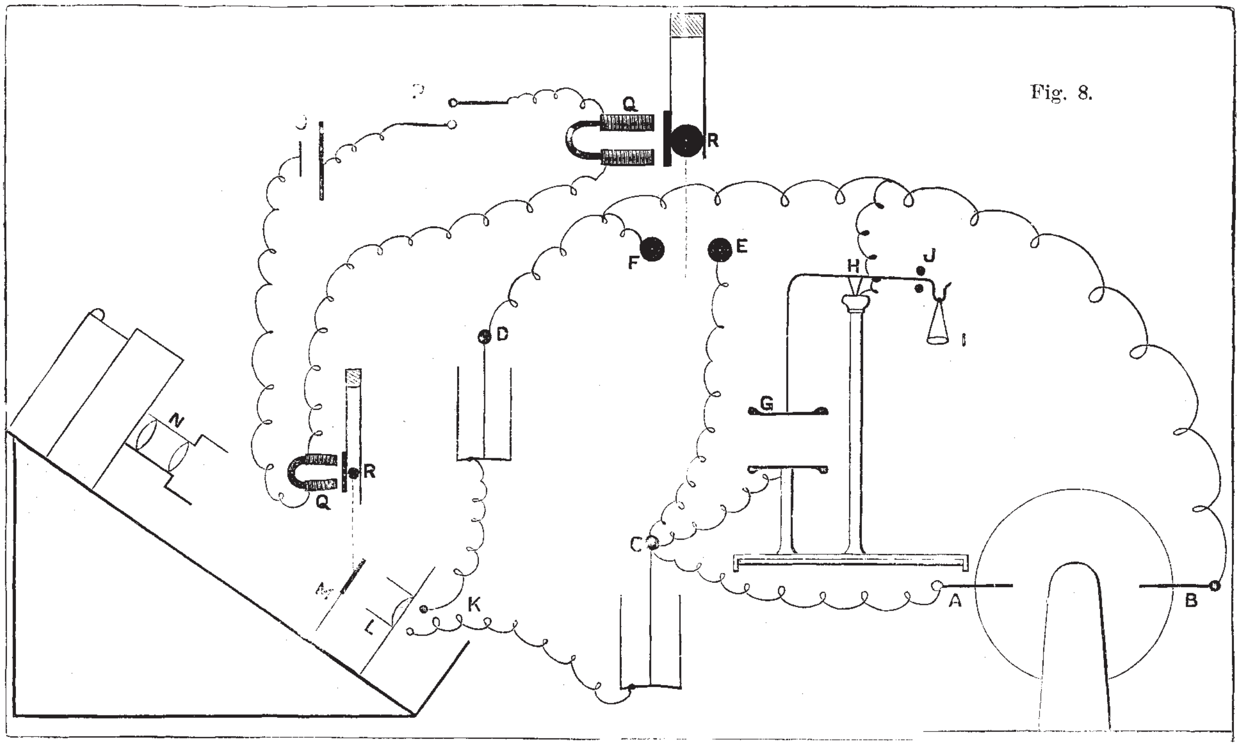

Fig. 8.

A, B, Electrodes of Wimshurst machine.
C, D, Terminals of interior coatings of Leyden jars.
E, F, Balls on insulating supports between which the discharge is taken.
G, Attracted disk of electrometer.
H, Knife edge. I, Scale pan.
J, Stops limiting movement of beam.
K, Sparking balls in connection with exterior coatings of jars. [These exterior coatings are to be joined by an imperfect conductor, such as a table.]
L, Lantern condenser. M, Soap film.
N, Photographic camera. O, Daniell cell.
P, Key. Q, Electro-magnets. R, Balls.

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[1] On this principle we may readily calculate the focal lengths of lenses without use of the law of sines (see *Phil. Mag.*, December 1879).

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ponents of a close double point will overlap; and if the distance between the centres do not exceed the diameter of the representative patches of light, there can be no distinct resolution. Now their diameter varies inversely as the aperture; and thus the resolving power is directly as the aperture.

My object to-night is to show you by actual examples that this is so. I have prepared a series of photographs of a grating consisting of parallel copper wires separated by intervals equal to their own diameter, and such that the distance from centre to centre is $\frac{1}{10}$ inch. The grating was backed by a paraffin lamp and large condensing lens; and the photographs were taken in the usual way, except that the lens employed was a telescopic object-glass, and was stopped by a screen perforated with a narrow adjustable slit, parallel to the wires.[1] In each case the exposure was inversely as the aperture employed. The first [thrown upon the screen] is a picture done by an aperture of eight hundredths of an inch, and the definition is tolerably good. The next, with six hundredths, is rather worse. In the third case, I think that everyone can see that the definition is deteriorating; that was done by an aperture of four hundredths of an inch. The next is one done by an aperture of three hundredths of an inch, and you can see that the lines are getting washed out. In focussing the plate for this photograph I saw that the lines had entirely disappeared, and I was surprised, on developing the plate, to find them still visible. That was in virtue of the shorter wave-length of the light operative in photography as compared with vision. In the last example, the aperture was only two-and-a-half hundredths of an inch, and the effect of the contraction has been to wash away the image altogether, although, so far as ordinary optical imperfections are concerned, the lens was acting more favourably with the smaller aperture than with the larger ones.

This experiment may be easily made with very simple apparatus; and I have arranged that each one of my audience may be able to repeat it by means of the piece of gauze and perforated card which have been distributed. The piece of gauze should be placed against the window so as to be backed by the sky, or in front of a lamp provided with a ground-glass or opal globe. You then look at the gauze through the pin-holes. Using the smaller hole, and gradually drawing back from the gauze, you will find that you lose definition and ultimately all sight of the wires. That will happen at a distance of about 4½ feet from the gauze. If, when looking through the smaller hole, you have just lost the wires, you shift the card so as to bring the larger hole into operation, you will see the wires again perfectly.

That is one side of the question. However perfect your lens may be, you cannot get good definition if the aperture is too much restricted. On the other hand, if the aperture is much restricted, then the lens is of no use, and you will get as good an image without it as with it.

I have not time to deal with this matter as I could wish, but I will illustrate it by projecting on the screen the image of a piece of gauze as formed by a narrow aperture parallel to one set of wires. There is no lens whatever between the gauze and the screen. [Experiment.] There is the image—if we can dignify it by such a name—of the gauze as formed by an aperture which is somewhat large. Now, as the aperture is gradually narrowed, we will trace the effect upon the definition of the wires parallel to it. The definition is improving; and now it looks tolerably good. But I will go on, and you will see that the definition will get bad again. Now, the aperture has been further narrowed, and the lines are getting washed out. Again, a little more, and they are gone. Perhaps you may think that the explanation lies in the faintness of the light. We cannot avoid the loss of light which accompanies the contraction of aperture, but to prove that the result is not so to be explained, I will now put in a lens. This will bring the other set of wires into view, and prove that there was plenty of light to enable us to see the first set if the definition had been good enough. Too small an aperture, then, is as bad as one which is too large; and if the aperture is sufficiently small, the image is no worse without a lens than with one.

What, then, is the best size of the aperture? That is the important question in dealing with pin-hole photography. It was first considered by Prof. Petzval, of Vienna, and he arrived at the result indicated by the formula, $2r^2 = f\lambda$, where $2r$ is the diameter of the aperture, λ the wave-length of light, and f the focal length, or rather simply the distance between the aperture and the screen upon which the image is formed.

His reasoning, however, though ingenious, is not sound, regarded as an attempt at an accurate solution of the question. In fact it is only lately that the mathematical problem of the diffraction of light by a circular hole has been sufficiently worked out to enable the question to be solved. The mathematician to whom we owe this achievement is Prof. Lommel. I have adapted his results to the problem of pin-hole photography. [A series of curves (*Philosophical Magazine*, February 1891), were shown, exhibiting to the eye the distribution of illumination in the images obtainable with various apertures.] The general conclusion is that the hole may advantageously be enlarged beyond that given by Petzval's rule. A suitable radius is $r = \sqrt{(f\lambda)}$.

I will not detain you further than just to show you one application of pin-hole photography on a different scale from the usual. The definition improves as the aperture increases; but in the absence of a lens the augmented aperture entails a greatly extended focal length. The limits of an ordinary portable camera are thus soon passed. The original of the transparency now to be thrown upon the screen was taken in an ordinary room, carefully darkened. The aperture (in the shutter) was 0·07 inch, and the distance of the 12 × 10 plate from the aperture was 7 feet. The resulting picture of a group of cedars shows nearly as much detail as could be seen direct from the place in question.

THE SMITHSONIAN ASTRO-PHYSICAL OBSERVATORY.

THE Smithsonian Institution, as we have already announced, has established as one of its departments a Physical Observatory which, with the instruments, has been supplied from the Smithsonian Fund. It occupies at present a temporary structure, though funds have been subscribed for a permanent building when Congress shall provide a suitable site. For the maintenance of the Observatory an appropriation has been made by Congress which became available on July 1. The actual instrumental work of the new Observatory will necessarily devolve largely upon a senior and a junior assistant, who can devote their entire time to research, and it is hoped that with the improved apparatus it will be possible to prosecute advantageously investigations in telluric and astro-physics, and particularly those with the bolometer in radiant energy.

In accepting the position of assistant secretary of the Smithsonian Institution in 1887, Mr. Langley retained the Directorship of the Observatory at Allegheny for the purpose of completing the researches begun there, and after his appointment as Secretary of the Institution, he still continued the titular Directorship, though but a limited amount of time could be spared from his official

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[1] The distance between the grating and the telescope lens was 12 feet 3 inches.

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