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LXXV. On dielectric strain along the lines of force

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We append a sample curve drawn with the help of these tables.

The case of $\epsilon = \frac{R_2}{R_1 + R_2}$ gives $\tanh \omega = 0$ at the negative plate, while the value at the positive is determined by a transcendental equation.

LXXXV. *On Dielectric Strain along the Lines of Force.* By LOUIS T. MORE, Ph.D., Professor of Physics in the University of Cincinnati*.

ALTHOUGH I have reviewed in former papers the work accomplished in electrostriction, yet, as the stress-problem is undoubtedly the fundamental one in the theory of electricity, I may be permitted to summarize briefly what has been done in this branch of the stress-problem before describing any new experiments.

By electrostriction is meant the strain in dielectrics and their consequent mechanical deformation, produced by the action of electricity in them. The effect is supposed to occur in solid, liquid, and gaseous non-conductors when they are electrified. The first observations date back to the beginning of the last century. In its early stages, electrostriction was looked upon as an isolated property of electricity, and although laws were formulated, it was not until Faraday and Maxwell adduced the effect as one of the fundamental phenomena on which to build their new theory of ætherial stresses, that any

* Communicated by the Author. Read in part before the International Electrical Congress, St. Louis, 1904; and in part before the American Physical Society, March 1905.

considerable theoretical importance was attached to this property of electricity. That they did so regard it as one of the corner-stones of their theory is clearly shown by their own statements. Thus Faraday says (Par. 1297):—"The direct inductive force, which may be conceived to be exerted in lines between the two limiting and charged conducting surfaces, is accompanied by a lateral or transverse force equivalent to a dilatation or a repulsion of these representative lines;" and (Par. 1224):—"The attractive force which exists amongst the particles of the dielectric in the direction of the induction is accompanied by a repulsion or a diverging force in the transverse direction." Maxwell ('Electricity and Magnetism,' vol. i. p. 165) writes even more explicitly:—"The hypothesis that a state of stress of this kind exists in a fluid dielectric, such as air or turpentine, may at first sight appear at variance with the established principle that at any point in a fluid the pressures are equal. But in the deduction of this principle from a consideration of the mobility and the equilibrium of the parts of the fluid, it is taken for granted that no action such as that which we here suppose to take place along the lines of force exists in the fluid. The state of stress which we have been studying is perfectly consistent with mobility and equilibrium of the fluid, for we have seen that, if any portion of the fluid is devoid of electric charge, it experiences no resultant force from the stresses on its surface, however intense these may be. It is only when a portion of the fluid becomes charged that its equilibrium is disturbed by the stresses on its surface, and we know that in this case it actually tends to move."

As is well known, he measures this stress by the famous formula $p = \frac{KV^2}{8\pi d^2}$, where p is the numerical value of the tension, K the specific inductive capacity of the dielectric, and V/d the fall of potential per unit length in the dielectric.

In support of this theory and equation a number of investigators have obtained an expansion both in the volume of an electrified glass thermometer and an elongation of a glass tube electrified radially, and have apparently found these deformations agree with the theoretical formula. When the mechanical is substituted for the electrical pressure, by the

formula $p = F/S = \mu \frac{\delta l}{l}$, we obtain $\frac{\delta l}{l} \cdot \frac{d^2}{V^2} = \frac{K}{8\pi\mu}$. The left-

hand member is the expansion per unit length of the tube under unit electrical conditions at right angles to the lines of force. By taking the average of Cantone's results we get

the value $\frac{\delta l}{l} \cdot \frac{d^2}{V^2} = 6.5 \times 10^{-13}$; when the best value for

Young's modulus, μ , and the dielectric constant for the glass are substituted in the right-hand member, there results a value of the same order and about one-half as large. Quincke's average results are in somewhat closer agreement. This discrepancy they assumed to be due to the inadequacy of Maxwell's formula, which should contain another term, signifying the relation between mechanical and electrical pressures.

Passing over the cruder work of early writers, the best direct results were obtained by Quincke* and Cantone†. In previous papers‡ I attempted to show, by their own results, that both neglected to consider extraneous effects which were of the same magnitude as their recorded values and which, if introduced, would have accounted for the entire deformations observed. These errors are due chiefly to heating, lateral displacements, distortions caused by using thin tubes more or less irregular in diameter and thickness, or by the lack of sphericity in any blown bulb of glass. Quite recently Wüllner and Wien§ attacked the problem in an indirect manner, attempting to show that the elasticity of glass is greater when obtained from electrical stress than when found in the usual manner by mechanical or acoustical methods. It is interesting to note the contradictions of these different observers. Wüllner and Wien confirm Quincke's conclusions, though not his values, and condemn Cantone's and mine; while Sacerdote and Cantone consider Quincke's to have little value. In the first place, the measurement of the rise or fall of an electrified capillary water column, such as Wüllner and Wien use, is in my opinion a faulty method, and their agreement with Quincke, who often employed the same method, is natural. I may add that this method is condemned by other investigators. Wüllner and Wien themselves have to introduce elaborate corrections to obviate the irregularities in the motion of the water column. Secondly, their results show great variations in the value of the elasticity, measured mechanically and electrically, for different kinds of glass. For some glass the latter is not more than one-half of one per cent., but for others it is as much as one hundred per cent.

* Quincke, *Wied. Ann.* Bd. x. pp. 161, 374, 513; Bd. xix. pp. 545, 705; Bd. xxviii. p. 529; Bd. xxxii. p. 503.

† Cantone, *Rend. d. R. Accad. Linc.* t. iv. pp. 341, 471; *Rend. d. R. Ist. Lomb.* [2] t. xxxiii. : *Nuovo Cim.* [4] t. xii. p. 160.

‡ More, *Phil. Mag.* [5] vol. i. p. 198, [6] vol. ii. p. 527, [6] vol. vi. p. 1; *Elect. World and Engineer*, vol. xliii. p. 127.

§ Wüllner and Wien, *Drude's Ann.* [4] Bd. ix. p. 1217.

greater. A variation of such magnitude due solely to differences in the composition of the glass is out of all proportion, and certainly if electrifying glass may more than double its ordinary elasticity, such an elaborate method of experimentation is unnecessary. Lastly, a variation in elasticity is directly contrary to theory. Lippmann, and his formulæ are shown by Sacerdote to be essentially in agreement with the other writers on the theory of this subject, states:—"That the dilatation produced electrically must be due to a direct action of electricity *and cannot be caused by a variation of the coefficient of elasticity*, or, *the coefficient of elasticity is independent of the electrification*; on the other hand, the dielectric constant varies with the electrification."

Some years ago I published a paper in the Philosophical Magazine containing experiments which did not confirm the conclusions just cited, and in fact made me doubt the existence of electrostriction. It was criticised by Sacerdote and Cantone on the ground that my method was faulty; they claimed I had employed a mechanical instead of an optical device for magnifying the elongations, and that it was not sufficiently delicate to observe this minute effect. The criticism was easy to meet, and in a letter to the Philosophical Magazine I demonstrated that my apparatus was an optical system, as sensitive or even a little more sensitive than interference methods, and that their criticism of its lack of sensibility and accuracy was founded on a misinterpretation. Later work has confirmed my opinions.

Maxwell's formula has been discussed by Korteweg *, Lorberg †, Kirchhoff ‡, Lippmann §, Sacerdote || and others. Their methods may be divided into two categories: first, the determination of the attractions and repulsions of electrified conducting particles immersed in a non-conducting medium, or in other words the results of a polarization of the dielectric; and secondly the application of the thermodynamic equation and the interdependence of the laws of conservation of energy of electricity and matter. Sacerdote has made a careful synopsis of all the articles published on this subject, with a critical discussion of methods and results. He finds, barring small differences, that both general methods lead to the same conclusion, which may be expressed by the formula

$$p = (K_1 + a) \frac{KV^2}{8\pi d^2},$$

* Korteweg, Wied. Ann. Bd. ix. p. 48.

† Lorberg, Wied. Ann. Bd. xxi. p. 300.

‡ Kirchhoff, Wied. Ann. Bd. xxiv. p. 52.

§ Lippmann, *Annal. de Chim. et de Phys.* [5] t. xxiv. p. 159.

|| Sacerdote, *Thèse*.

where a is the inverse of Young's modulus and K_1 the variation of the dielectric constant with mechanical pressure, represented by $\delta K/\delta f$. It is then Maxwell's equation with the addition of a second constant. This variation of the dielectric constant must be an exceedingly small quantity, for of the few who have investigated it, some have obtained a positive value, some a negative, and others a null effect. So that the pressure p may be increased, diminished, or unaffected by this quantity.

If one is willing to grant the fundamental assumption made for these theories, there is little doubt that the conclusions are correct. It is easy to see that the assumption made for each of them is essentially the same; namely, that a stress in the æther is communicated to matter immersed in it, or, in other words, the granting of the vital point of the discussion. Those who develop the polarization theory suppose material particles to be immersed in a fluid æther, as may be illustrated by fragments of silk thread in pure turpentine. If charged electrodes are placed in the turpentine, the silk threads are observed to form chains extending approximately along the Faraday lines of force. We may readily imagine the ends of each fibre to be polarized by induction which by their mutual attractions and repulsions form lines of stress tending to shorten along the lines of force and to repel each other perpendicularly to them. In the first place, it is rather doubtful to consider this phenomenon as due to a static system of forces which the Faraday lines demand. But the real error lies deeper,—to assume that particles of matter in the æther act like particles of silk in turpentine is to grant the whole question. We know that a stress in one kind of matter is transmitted undiminished to any other kind of matter, as in this case from turpentine to silk. But we not only have no confirmation, but in fact many reasons for believing that static stresses in the æther are not communicated to matter. Lorentz indeed goes so far as to say that it is absurd to imagine a strain in the æther itself and so impossible to conceive of a strain imparted to matter. One of the chief reasons for the abandonment of the elastic solid theory of light in favour of the electro-magnetic theory is to obviate the absurdities introduced when the æther was supposed to be an elastic solid. As to the theories viewed from a thermodynamic standpoint, the assumption is just as glaring as it is in fact the same. Sacerdote and the other writers on electrostriction from this standpoint claim that the connecting link between the æther and matter is expressed by the coefficient $K_1 = \delta K/\delta f$, that is by the variation of the inductive capacity

with mechanical pressure. In the first place, no such variation has been found experimentally, as the results so far are discordant and subject to serious criticism. The question still remains, whether if it were found that K changes when a body is distorted mechanically, electrostriction necessarily exists. We know already that as a general rule this constant is least for gases, greater for liquids and greatest for solids, or it varies approximately as the density of the matter. We may then readily imagine that an increase in density caused by pressure will change the dielectric constant slightly because of a variation in the elasticity; but the theorists maintain that electrostriction is due to a direct action of electricity and not to a variation of the coefficient of elasticity, as shown by my former quotation from Lippmann. Wüllner and Wien, who alone regard the effect as being due to such a variation in elasticity, have certainly mistaken the nature of the problem. Their results, as stated before, show large variations, equal at times to the value of the whole coefficient of elasticity, in the elasticity of electrified and unelectrified glass. And though I believe that this difference must have been exaggerated by extraneous effects, still such a variation may exist in a less degree, but it has no bearing on the subject of electrostriction. The question may be put in this form. Does a mechanical pressure (a stretching force which produces a decrease in density should theoretically increase the dielectric power) cause a variation in the mutual relations of the æther and matter? This is the same question discussed with respect to the polarization theory.

There seems to be a considerable degree of confusion prevalent in regard to the significance of electrostriction. It does not involve the mechanical pressure produced by the attraction of two charged bodies, but is supposed to be a property of the electric flux in a non-conductor. For example, two charged bodies attract each other, and if they touch a dielectric, glass for example, placed between them, the glass must be deformed mechanically whatever theory is adopted. But those who believe in electrostriction maintain that if the charged plates be separated from the glass, and if the intervening spaces be filled with a fluid dielectric of the same dielectric constant as the glass and filled in such a manner that the mechanical force of attraction of the electrified armatures cannot be communicated by the fluid to the glass, still the latter will contract in the direction of the lines of force and expand perpendicularly to them. This mechanical deformation, called electrostriction, is due then entirely to the electrical stresses in an æther and communicated by it to

the matter immersed in it. As an illustration, we can make use of the system of glass and liquid dielectrics just described. The lines of electric flux in the æther stretch from one charged armature to the other and may experience, for all we know, the tensions and pressures generally ascribed to them. Since both the glass and the liquid have the same dielectric constant, there is no discontinuity of the lines at the surface of the glass, yet it is supposed to contract in their direction. The only mechanical arrangement I can think of, is to assume that each line does not extend from armature to armature but is like a chain linking particle to particle of the glass. Let us, to be more precise, consider the action of a tension in one of these chain lines on three particles,—one in the surface of the liquid adjoining the glass, a second in the surface of the glass, and the third just within the surface of the glass. Since the dielectric constant is the same in the two substances, the pull on the middle particle is the same in the two directions, and there is no tendency to displacement of the surface of the glass. This is also true for any three particles *in* the glass, and a contraction does not occur. Of course, where there is a discontinuity of the lines, either in passing from one dielectric to another of a different inductive capacity or from a dielectric to a contiguous conductor, an unbalanced force exists, but that is merely the measure of the attraction of the two charged bodies. If the lines of flux are supposed to pass through the glass without being attached to its particles, it is even more difficult to see how the stress in the æther effects the glass.

If we assume Maxwell's formula for electric stress in the æther, and I know no other adequate, we must not lose sight of a most important fact, that theories involving the æther must always remain purely hypothetical, as we have no possible method of experimentation upon it. And only when matter is immersed in the æther does experimentation become possible. Now all our observation goes to show, that for static and even for kinetic phenomena, at least where the velocity is less than that of light, the æther is unmodified by the presence of matter and does not affect the properties of it: witness the absence of friction between the earth and æther in its motion about the sun; the Kerr effect of the polarization of transparent matter when electrified; Brace's experiments on the action of a magnetic field on transparent media showing that the Faraday stresses affect the velocity of light by an amount less than 2.0×10^{-14} for a c.g.s. unit of intensity per centimetre; and many others all supporting the doctrine of the independence of æther and

matter. My claim is, not that Faraday stresses may not exist in the æther, but that these stresses are not transferred in any appreciable degree to matter as *unbalanced* mechanical forces. The fact is, a dynamic system in a hypothetical medium may be either affirmed or denied, but it cannot be proved as the question is a metaphysical one. The absence of the electrostrictive effect does not preclude the existence of ætherial stresses, but it does prevent it from serving as a fundamental experimental verification of the stress-problem.

The recent discussion of Maxwell's stress theory by Bjerknes* is opportune to this subject, and his conclusion should be quoted:—"In the meanwhile it will therefore be safest to consider the Maxwell stresses as only *fictitious* stresses that might have produced the required forces, and not as the real stresses which do produce them. Other authors have also termed them fictitious stresses, especially Lorentz, who also considers the stress formulæ only as useful analytical formulæ, but not as representing any physical reality. The reason, however, why he has come to this opinion is quite different from the reason brought forward here. His view is that the stress-problem in itself has no physical meaning. For, according to the doctrine of the immobility of the æther, it is an absurdity to speak of forces acting upon or stresses existing in the æther."

Dielectric Strain along the Lines of Force.

Maxwell's theory, which supposes an expansion perpendicularly to the lines of force, also demands an equal contraction in their direction. A test of this latter effect has never, so far as I know, been attempted: it is obviously a much more difficult piece of work, but it also should prove to be of greater importance. In the former method, the effect sought and the extraneous effects are all in the same direction and are difficult to separate. But the effect along the lines of force should be a contraction, and so opposite in effect to the heating and other sources of error. Furthermore, it does not introduce a correction for the little-known value for Poisson's ratio. It is besides possible to reduce the magnitude of the disturbances themselves.

It is evident that a sheet of dielectric placed between two metal electrodes will be compressed by the attraction of these electrodes when they are charged. We must neutralize this attractive force by making a system of balanced electric forces. This can be best done by building up a pile of alternate metallic and dielectric plates, each held horizontal and

* V. Bjerknes, *Phil. Mag.* vol. ix. p. 491 (1905).

accurately equi-distant. Alternate metal plates are then connected by wires into two sets: one set to be charged positively and the other negatively. The number of metal plates must be odd, so that the top and bottom plates may be charged with electricity of the like sign and connected together rigidly by metal rods. Each plate in this system is thus attracted equally up and down, or is electrically in equilibrium. Whereas, if the dielectric itself expands or contracts along the lines of force, the whole pile will tend to shorten or lengthen according to the formula $\mu \frac{\delta d}{d} = \frac{nKV^2}{8\pi d^2}$, where d is the thickness of each dielectric plate, K the dielectric constant, V the potential, n the number of dielectric plates in the pile, and μ is Young's modulus.

To accomplish this object, it was necessary to cast a large number of plates, which should be exactly parallel, equi-distant and close together, in a dielectric, to form a solid block. A great deal of difficulty was found in getting a dielectric which was a good insulator for high potentials, and at the same time rigid and not viscous, and which would not contract when solidifying. The last was very important and debarred paraffin, as this and similar substances contract sufficiently to fissure in the thin places between the electrodes. The substance finally adopted was a mixture of four parts by weight of the best shellac, one of resin and two of Venice turpentine. This compound gives a clean and smooth surface when broken, and is highly elastic (Young's modulus about 10^{10}). It melts at 110° and when solidified shows little contraction,—a two-litre flask, half full of the hot liquid, presents an almost level surface when it solidifies.

Several types of moulds for the casting were made and tried before the one was devised which answered. Thirty-one brass plates, 5 cm. in diam. and 2.5 mm. thick, were polished and made true. Three lugs, 12 mm. long and 6 mm. wide, were soldered radially to each plate. A seamless brass tube, 21.5 cm. long and 7.4 cm. inner diameter, was then ruled with thirty-one circular lines, each 5.5 mm. apart, and three holes were bored and tapped, equally distant apart on each line. The plates were then placed each in the plane of one of these circles, with a lug opposite each hole and held in position by three small pointed screws for each plate. Two of the plates are shown mounted in a section of the tube in fig. 1. In the next to the top plate was screwed a brass tooth of the same dimensions as the ebonite tooth shown in fig. 2 (p. 686), and a hole was bored in the top plate so that the tooth passed through it. The bottom plate was tapped with three screw-holes and a large plate, only a little less in

diameter than the mould, was fastened to it by screws. The ends of the moulds were closed by brass screw-caps. After

Fig. 1.

the casting was made, the caps and holding screws were removed, and the perfect casting slipped out of the mould by heating it quickly. The large brass plate at the bottom was removed by heating it gently, and the bottom electrode, thus exposed, was fastened to the base plate of the frame of the apparatus, fig. 2. Then the brass tooth was replaced by the one of ebonite. During this operation the top brass plate was slipped off, this being done so that the upper plate of the frame might replace it as an electrode.

When the casting was out of the mould, the ends of the three lugs on each plate were exposed, forming two sets of three vertical lines each. Two of these lines of lugs of each set were covered with insulating wax to prevent leakage, and the lugs on the third line of each set were connected by wires so that one set might be charged and the other grounded, fig. 2. After the electrical connexion was made and tested, these terminals were also covered with the insulating wax. We thus have sixteen plate-electrodes (counting the upper plate of the frame as one), connected to the ground, of which the top and bottom ones were fastened rigidly together, and

fifteen plate-electrodes connected to the electrical machine. The attraction of these plates when charged is evidently

Fig. 2.

neutralized as they are equally attracted up and down. Between the electrodes are thirty dielectric plates, the whole forming a solid mass except for the discontinuity between the upper dielectric plate and the upper electrode.

The greatest difficulty experienced while making these

condensers is to avoid air-holes and fissures, as the electrodes are only three millimetres apart. I did not accomplish this until I used the arrangement shown in fig. 3, where the

Fig. 3.

whole process takes place in a single bath and with a good vacuum. The constituents, ground and thoroughly mixed, were placed in the two litre-flask, A, in a bath of machine-oil kept at a temperature a little above 110° . From the bottom of the flask a glass and copper tube connects with the bottom of the mould, B, through a stop-cock at C. A second tube from the flask but above the surface of the mixture connected the flask to a water vacuum-pump through driers; and a copper tube from the other end of the mould made connexion with a "Geryk" vacuum-pump. The cock, C, was first closed, and the mixture liquefied under a vacuum made by the water-pump. This heating was continued until the mass formed a homogeneous, smooth fluid. The "Geryk" pump was then started, and when a high vacuum in B was obtained the cock, C, was opened and the fluid gradually forced into the mould by regulating the pressure above the fluid in the flask,—the "Geryk" was worked vigorously until the mould was entirely filled. The pumps were then disconnected and the bath allowed to cool slowly.

Returning now to the completed casting, a stout frame was made of two brass plates, connected by two brass rods (fig. 2). The bottom electrode was fastened, as stated before, to the base plate of this frame and it, in turn, to heavy beams bolted to masonry piers in a basement room. A hole was bored in the top plate of the frame and a brass cylinder screwed into it so that the base of the cylinder was flush with the lower surface of the plate and the upper end on a level with the top of the ebonite tooth. The legs of a tilting mirror were then placed on these upper ends of the cylinder

and tooth. The position of the top plate of the frame was regulated by the set screws at the top of the rods. The dielectric exposed above the uppermost charged electrode was made slightly less thick than that between the other electrodes, and the space between the upper surface of the dielectric and the top plate of the frame was filled with a thin layer of oil of the same dielectric constant as the shellac mixture, and the distance between the two electrodes was adjusted so that this heterogeneous dielectric was electrically equivalent to that between any other two of the electrodes. The ebonite tooth was covered with insulating wax to prevent leakage or sparking from the positively charged electrode.

The method of measuring the deflexions was the same as that employed in my former work, and the reader is referred to those papers for a detailed description. I give here only a brief account of it.

The system of optical levers used to magnify the expansion of the condensers is shown in fig. 4. An incandescent electric lamp with a ground glass globe was clamped behind a metal screen pierced with a round hole, five millimetres in diameter. Across this opening fine platinum wires were fastened horizontally, to serve for the deflected image. The light from the lamp, after reflexion at right angles by a totally reflecting prism, passed through an achromatic lens to a tilting mirror mounted vertically on a little tripod table. Upon reflexion, the light again passed through the lens and on, above the prism, to a micrometer microscope.

The mirror, 1.5 cm. $\times$ 2.0 cm., furnished by Brashear, was silvered on its front face and plane to $\frac{1}{4} \lambda$. The tilting table of brass, 1.6 cm. square, had three legs made of the finest needle points, one of which rested on the brass cylinder and the other two on the ebonite tooth. By slightly raising or lowering the cylinder, the beam of light could be directed at a

Fig. 4.

desired angle. Small weights which hung below the level of the table from an arm increased the stability to such a degree that the image showed no oscillation from outside disturbances. The prism and lens were both of the best construction, and the microscope was a new instrument obtained especially for these experiments. The platinum wires and the microscope were each placed in the principal focus of the lens; the image was then very sharp and distinct. The least change in length which can be observed is calculated from the following dimensions:—

Dimensions for Magnifying Power.

Focal length of lens	100 cm.
Distance between feet on tripod I	6 mm.
Distance between feet on tripod II	3 mm.
One division on micrometer of microscope.	0.002 mm.

A deflexion of one division of the micrometer, using tripod I, is equal to a change of length in the condenser

of $\frac{6}{2 \times 1000} \times 0.002 = 6 \times 10^{-6}$ mm., and with tripod II

3×10^{-6} mm. This minimum deflexion could be easily observed. In compiling readings, they were reduced to the basis of 6×10^{-6} mm. for one division, whichever tripod was used*.

The criticisms made on this type of apparatus have been a surprise to me, as I supposed such a standard form would impress all with its capabilities for delicacy and accuracy. In the first place, it is as much a form of optical device as the interferometer, and it has just about the same refinement as all optical devices, namely, its limit involves the wave-length of light just as interference methods do, and on the other hand it seems to me better adapted to the experiments under discussion.

During preliminary experiments made to test the insulation and the dielectric strength of the condenser, I inserted a sheet of soft rubber between the lower surface of the top plate of the frame and the upper plate of the dielectric, using no oil. With this arrangement I noticed small deflexions of the mirror, but in every case the deflexion showed an elongation of the dielectric, and not the contraction the theory calls for. This elongation was also much smaller than the theories of electrostriction demand. I thought at first that it was due to

* I wish here to express my thanks and indebtedness to Mr. P. B. Evens, the University mechanician, for the skill and patience he has exercised in the making of all this apparatus which made these experiments possible.

some action in the dielectric, but later found that it was caused by charges induced on the rubber which attracted the tilting mirror statically.

During the final experiments which continued through two months, the rubber sheeting was replaced by pure lard oil. The dielectric constant is almost exactly that of the shellac mixture. The height of the frame was carefully adjusted to make the electrical forces a balanced system, as I have previously shown to be necessary.

When these precautions were taken the mirror showed absolutely no deflexion when the plates were charged to any potential up to the breaking point for the dielectric.

The conditions were varied as far as possible. Sometimes the charging was done slowly and sometimes quickly, ranging from five seconds to a minute or more, and at times Leyden jars were included in the circuit and sometimes not, so that the quantity of charge might be varied.

From the nature of the construction of the condenser surface leakage was obviated, as little or no metallic surface was exposed, and in my former work I showed that surface currents were principally responsible for the heating. The currents through the dielectric are always small, and their effect was so much retarded in time by the large mass of dielectric through which the heat must diffuse, that the condenser could be charged and discharged before the slow creep of the image due to this occurred.

Potentials which produced a spark of 4 millimetres between balls 1 centimetre in diameter were first used, and then increased gradually a millimetre at a time until a spark of 16 millimetres was reached. This latter corresponds to a potential of about 130 c.g.s.

In not a single case did the mirror show either an elongation or a contraction.

When the spark passed, there was usually a slight tremor or jump of the mirror of not more than six or seven divisions of the micrometer, and this was sometimes in the direction of an elongation and sometimes a contraction. These were found to be due to the explosion of the spark transmitted through the air. To prove this, the condenser was disconnected from the electric circuit and sparks passed between the balls of the micrometer; in every case the same jump of the mirror was observed, although the condenser was not electrified. Shearer mentions this same disturbance while using an interferometer as a measuring device.

That my apparatus was amply sensitive is seen from the following calculation.

The equation for elasticity introduced into Maxwell's formula gives the result:

$$F/S = \mu \frac{\delta d}{d} = \frac{nKV^2}{8\pi d^2} \text{ or } \delta d = nKV^2/8\pi d\mu,$$

where

- n , the number of plates = 30
- K , the dielectric constant = 2.5
- V , max. potential difference = 130 c.g.s.
- d , distance between electrodes ... = 0.3 cm.
- μ , Young's modulus = 10^{10}

substituting,

$$\delta d = 168 \times 10^{-7} \text{ cm.}$$

This value corresponds to 56 divisions of the micrometer, using tripod II, or 28 divisions using tripod I.

These results certainly are difficult to ignore and can lead but to one interpretation, and that is the lack of influence of the æther on matter in this respect.

Dielectric Strain Perpendicular to the Lines of Force.

Before the experiments already described in this paper were begun, a series of tests for the dielectric strain perpendicular to the lines of force was undertaken with this same shellac mixture for a dielectric. The method of experimentation and the apparatus, with the exception that the cylindrical condensers were of the shellac mixture instead of glass, were exactly the same as described in the Philosophical Magazine, vol. vi. p. 1, 1903. Cylinders of the shellac mixture were cast in a mould made of two coaxial brass cylinders. Condensers of paraffin were also cast in the same mould.

Dimensions of Cylinders.

No.	Composition.	Length.	Outer diam.	Inner diam.	Thickness.	μ .	K.
7.	Shellac.	64.6 cm.	4.7 cm.	3.9 cm.	0.4 cm.	10^{10}	2.5
9.	Shellac.	64.8	4.7	3.9	0.25	10^{10}	2.5
8.	Paraffin.	64.8	4.7	3.9	0.4	10^{10}	2.5

These condensers were mounted in the same manner as the glass ones in my earlier experiments. The values for Young's modulus were obtained in the usual manner by placing weights on the ends of the cylinders and measuring their contraction. They are fairly accurate for the shellac, but the value for the paraffin is not so reliable as the substance is decidedly viscous. The values for both are given rather too large than too small.

Tube No. 7 was a little irregular in thickness, as it distorted somewhat when taken from the mould. But the results are of importance as they show the effect of a slightly non-uniform field. Tube No. 9, after removal from the mould, was turned in a lathe and was quite uniform in bore and thickness.

Tube 7.—Adherent Armatures.

Spark-length.	Potential.	Mean elong. micr. div.	Mean elong. $\delta l \times 10^4$.	Time of charging.
5.0 mm.	60 c.g.s.	27.0	1.62	0.25 min.
7.5	85	35.0	2.10	0.25
7.5	85	70.0	4.20	1.00
10.0	100	55.0	3.30	0.25
10.0	100	100.0	6.00	1.00
10.0	100	180.0	10.80	5.00
12.0	110	133.5	8.01	0.25
12.0	110	242.5	14.55	1.00

The elongations, δl , are the mean in each case of a series of readings. Since one division of the micrometer equals 6×10^{-6} mm., the absolute value of the deflexion is obtained by multiplying the observation in divisions of the micrometer by that number.

In the first place, for the same time of charging the elongation does not increase as the square of the potential. Secondly, the dependence of the deflexion on the length of time employed in charging is so pronounced as to make it imperative to account for it by the heat evolved by the electrical charge. When the condenser was discharged the return of the image, as Cantone also noticed, is quite irregular; at times the image returned quickly to the original zero, very often about half way, but most often there was no recovery. This is contrary to theory, as there should be no residual effect of the charge.

Tube 9.—Adherent Armatures.

Spark-length.	Potential.	Mean elong. micr. div.	Mean elong. $\delta l \times 10^4$.	Time.
5.0 mm.	60 c.g.s.	35.0	2.10	0.25 min.
5.0	60	62.5	3.75	1.00
5.0	60	67.5	4.05	2.50
7.5	85	67.5	4.05	0.25

Greater potentials could not be used as the tube was ruptured with that spark-length. The tube could of course be repaired and used again.

According to the theory, the elongation varies inversely as the square of the thickness, thus for the same potential the ratio of the deflexions of tubes 7 and 9 should have been three to eight. This is not verified, as the deflexions for equal potentials and time of charging are about equal. In fact, the effect I ascribe to heating is so preponderant as to overshadow all others.

Tube 8.—Paraffin, Adherent Armatures.

While experimenting with paraffin tubes the potentials were carried to the breaking point, which occurred at a spark-length of 10 mm., or 110 c.g.s. I observed no changes of length at all. I think that experiments with this substance have little significance as it is so viscous that not much could be relied upon.

Non-Adherent Armatures.

The tinfoil was removed from the tubes and the non-adherent brass-tube armatures were mounted in place. The space between the armatures and the dielectric was filled with the lard-oil. Thus the fall of potential between the armatures was uniform, and there was neither a free charge on the surface of the shellac tube nor any discontinuity in the lines of force. The elongation due to the mechanical pressure of the electrodes was eliminated, and according to the value assumed for Poisson's ratio, a decrease of from one-fourth to one-third of the deflexion for adherent armatures of the same dimensions should result. Again, the distance between the armatures is now increased to 7 millimetres, instead of the former less amount. For number 7 it should reduce the deflexion in the ratio of sixteen to forty-nine; for number 9 of six and a quarter to forty-nine.

The results actually obtained are given in the table.

Tube 7.—Non-adherent Armatures.

Spark-length.	Potential.	Mean elong. micr. div.	Mean elong. $\delta l \times 10^4$.	Time.	Date.
10 mm.	100 c.g.s.	33.5	2.01	0.25 min.	Apr. 13
10	100	64.5	3.87	0.25	14
12	110	58.0	3.48	0.25	13
12	110	80.5	4.83	0.25	14
16	130	61.0	3.66	0.25	13
16	130	98.5	5.91	2.00	13
16	130	132.0	7.92	0.25	14
16	130	170.0	10.20	2.00	14

The table shows that the deflexions, though somewhat less, are not nearly so small as they should be. This would be accounted for satisfactorily if we believe most of this elongation to be caused by the heat evolved. Again, the influence of time of charging is clearly shown. The values on different days seem to be consistently different. I cannot account for this except to suppose that the humidity was so different on the two days as to cause a great variation in leakage currents and so a difference in the heating of the tubes.

Tube 9.—Non-adherent Armatures.

Spark-length.	Potential.	Elongation micr. div.	Elongation $\delta l \times 10^4$.	Time of charging.
10 mm.	100 c.g.s.	80	4.8	1.00 mm.
10	100	55	3.3	0.75
10	100	35	2.1	0.50
10	100	25	1.5	0.25
10	100	75	4.5	1.00
10	100	30	1.8	0.25
10	100	35	2.1	0.25
10	100	42.5	2.6	0.25
12	110	130	7.8	1.00
12	110	115	6.9	1.00
12	110	115	6.9	1.00
12	110	85	5.1	0.50
12	110	77	4.6	0.50
12	110	82.5	5.0	0.50
12	110	70	4.2	0.50
14	120	150	9.0	0.50
14	120	150	9.0	0.50
14	120	165	9.9	0.50
14	120	145	8.7	0.50
14	120	165	9.9	0.50
14	120	160	9.6	0.50
14	120	150	9.0	0.50

The most convincing results were obtained with tube 9, mounted with non-adherent armatures.

The separate readings are here given of a number of observations. With a 10-millimetre spark the elongation for a charging of about one minute is approximately 4.5×10^{-4} mm., and this is reduced to one-third of its value when the tube is charged four times as quickly. This same effect of the time is noticeable throughout the table. Evidently very little of the effect would remain if the charging were made instantaneously. The times of charging are only given approximately, and this accounts for the variations in the readings when the potentials and the times are the same. There is no evidence of the law that the deflexions are proportional to V^2/d^2 .

In concluding this work on electrostriction, which has extended over years, I have some hope that the results given will be sufficiently convincing to prove that the effects which have been observed by myself and others can be fully accounted for by extraneous effects, and that when these are eliminated the distortion of dielectrics by an electrical charge also vanishes.

University of Cincinnati,
June, 1905.

LXXVI. *Plan of an Atom to be capable of Storing an Electrion with Enormous Energy for Radio-activity.* By Lord KELVIN*.

§ 1. **I**N a communication to the Philosophical Magazine of October 1904, I described combinations of atoms and electrions having certain definite qualities of radio-activity; holding myself, for the time, bound to the precise description of the electric property of an atom of ponderable matter, which I had suggested in § 4 of "Aepinus Atomised" (Baltimore Lectures, Appendix E). In that description each atom of ponderable matter is supposed to have ideal electric matter of the vitreous kind distributed uniformly through it. No longer binding myself by this limitation to uniformity of vitreous electric density, I now propose to consider an atom of ponderable matter intrinsically charged with concentric strata of electricity, vitreous and resinous, of equal electric density at equal distances from the centre; and with an excess of the total quantity of the vitreous above the total quantity of the resinous. I still suppose (with, I believe, all at present concerned with radio-activity) that free resinous

* Communicated by the Author.