THE PHYSICAL REVIEW.

ON THE CONVERSION OF ELECTRIC ENERGY IN DIELECTRICS.¹ III.

(Conclusion.)

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IN a particular test the primary potential difference was 105 electrostatic units, and the potential difference across the armatures, as tested by sparking distance, only 10 electrostatic units, instead of 37, as it should be according to this theory. The difference is to be explained by losses from brush discharges and imperfections in the theory.

If we consider the electric force at the center of the armatures as made up of components X and Y at right angles to each other, we may write

$$X = \frac{V_2 - V_1}{d},$$

$$Y = \frac{V_2 - V_3}{d},$$

where d is the distance between opposite armatures. We may write, then,

$$X = Ae^{-\lambda t} \sin(\mu t - \phi),$$

$$Y = Ae^{-\lambda t} \cos(\mu t - \phi),$$

where

$$A = \frac{E_0}{\sqrt{2} \cdot d}.$$

The field between the armatures is therefore a rotating one, and the value of the electric force at the center can be represented by the radius vector of a spiral, the magnitude ρ of the radius vector at any time being given by

$$\rho = Ae^{-\lambda t}.$$

¹ Continued from the PHYSICAL REVIEW, Vol. V., p. 46.

Now
$$\int_0^\infty \rho dt = \frac{A}{\lambda},$$

and if T is the interval between two sparks across the knobs in the primary circuit, the mean value of the force will be $\frac{A}{\lambda T}$.

Putting in the value of A , we get

$$\text{mean force} = \frac{E_0}{\sqrt{2} d \lambda T}.$$

With respect to the above investigation, it is to be noted that it is tacitly assumed (1) that the currents are distributed uniformly throughout the conductors, and (2) that the conductors are short compared with the wave length. An idea of the error introduced by the former supposition is to be obtained from the previous footnote: it practically gives rise to no appreciable difference. With regard to the latter, all the conductors are short compared with the wave length, except the coils of the transformer of the Ebert apparatus. This does not give rise to any appreciable error, however, for I found the secondary potential difference almost exactly what it ought to have been.

With regard to the effect discovered by Jaumann, relating to the influence of a small impressed oscillatory potential difference at the spark gap, we may note that in the present case, either in the primary or in the secondary, the potential difference rises rapidly, but still gradually, to its maximum value, — a state of affairs which does not correspond with that treated experimentally by Jaumann. Other observers, however, have indicated that oscillatory discharges of very high frequency take place more easily than slower ones; but I am not able to discover how far the observations refer to the first spark as distinguished from those following it at very small intervals of time. Suppose, however, that such an effect really exists and operates on the first maximum in the armature (phasing) terminals. The result would be still further to increase the difference between the observed and calculated potential differences. Now the observed potential difference, as deduced from “steady spark length,” was about one-quarter of the calculated potential difference. It will be presently shown that no reasonable reduction of the value thus obtained could possibly account for the absence of any observable rotation in the cases examined, assuming that the losses are the same at these high frequencies as they are at lower speeds. As an illustration of the method of making an experiment in the rapid field, I will select for description some observations on an ebonite cylinder, made from the same kind of ebonite as was examined in the slower fields. A fine thread having been selected, the ebonite cylinder was mounted and dried for several days by phosphorus pentoxide. The Ebert apparatus was fed by sparks from the large induction coil at the rate

of an interval of 0.0014" between each spark. The sparks were examined by a revolving mirror, and the resistance of the primary of the induction coil was so adjusted as to allow the sparks to pass freely. It resulted from a previous experiment that the sparks only occurred on the make and not on the break of the primary circuit. The spheres of the Ebert apparatus were 3 cm. diameter of heavily gilt and polished brass. The rotating mirror (hand turned) disclosed that each spark was multiple. Since the period of oscillation was too rapid for the separate oscillations to be separated by the mirror at the speed at which it was driven, it was inferred that the enormous resistance, self-inductance, and capacity of the secondary of the induction coil was giving rise to discontinuous discharge in the well-known manner. The period quoted is merely that of the spark groups, or breaks of the induction coil circuit; consequently it appears reasonable to infer that the number of effective sparks was about four times as great as the number resulting from the period quoted. We are, however, in search of a minimum possible value for the intensity in the rotating field, and consequently the sparks will be reckoned at one only per break of primary. The spark length between the balls of the Ebert apparatus was 1 cm., corresponding to 105 electrostatic units potential difference.

The spark length between 1 cm. balls attached to the secondary condenser was such as to indicate a potential difference exactly half of this, within the limits of errors of observation. The potential differences were deduced by a reference to Baille's results (J. J. Thomson, *Recent Researches in Electricity and Magnetism*, p. 77).

The phasing condenser plates were 15 cm. diameter, and when the phasing potential differences were equal they were 2.3 cm. apart. When the rotating field cell was connected with the wires and keys, readjustment was obtained by keeping the resistance constant and increasing the plate distance to 3.1 cm. This shows that the capacity of the cell was by no means to be neglected in reality, though we are more or less forced to neglect it in our calculations. This disturbance constitutes the most serious objection to the interpretation of the results, but it is not, after all, very large. The potential difference of the phasing terminals, with cell attached, was tested by observing the spark length between spheres 0.4 cm. diameter. One of these spheres was mounted on the slide of a measuring microscope (with, of course, the necessary insulation), and was found to yield a spark length of 0.06 cm. This corresponds to about 10 electrostatic units of potential difference. When the cell was disconnected, the potential difference rose so as to give a spark length of 0.14 cm., the difference being attributable to the inevitable loss by brush discharge, which it is practically impossible to avoid with the complexity of wires and keys actually employed.

Calculating the mean force by help of the formula arrived at in the mathematical discussion, we have

$$F = \frac{E_0}{\sqrt{2} d \lambda t}.$$

The data, besides those already given, used in the calculation of λ , are :

$$\begin{array}{ll} E_0 = 105 \text{ (electrostatic).} & \lambda = 366. \\ t = 14 \times 10^{-4} \text{ seconds.} & d = 1.3 \text{ cm.} \end{array}$$

The resulting value of F is about 56 electrostatic units. Applying an "internal screening factor," this reduces to about 16.8 electrostatic units actually active in the mass of the cylinder of ebonite.

But we have seen that the want of uniformity in the distribution of the currents in the conductors may increase λ rather less than three times. Hence, taking 3λ instead of λ , the external force becomes 18, say, and the internal force, say, 5.

But the observed external force is $\frac{10}{1.3} = 7.7$, say, instead of 18, and the resulting external force is 2.3, say.

Using these latter numbers, we have the energy loss per unit volume by the previous investigation in slow fields

$$W = 0.03 F^{1.85},$$

using the formula applicable to the smaller sphere. In this case the volume of the ebonite cylinder was 0.36 cc., so that the energy expended and lost becomes 0.4 erg per revolution. This implies a possible rise of temperature sufficient to melt paraffin in, say, about ten minutes, but the ebonite was cemented on by paraffin, and the field was often in action for ten minutes and more, but the ebonite never fell off. It is quite a question whether a temperature method might not yield instructive results if used to check the dynamometric method. Of course, as the apparatus was arranged, no temperature tests were possible. In order to compute what angular displacement of the cylinder is to be expected, the following data are necessary.

Time of double vibration of cylinder of moment of inertia, 0.08 (g.cm.²); with thread employed = 43", hence coefficient of torsion of thread is $1.7 \times 10^{-3} = \tau$, and if ϕ is the angular rotation of the cylinder in radians, we have $2 \pi \tau \phi = W$.

This gives, with above minimum value for W ,

$$\phi = 37 \text{ radians.}$$

Now, as the scale was set up at a distance of 150 cm., a 5 cm. deflection, which I shall show directly could not have been overlooked, would correspond to a rotation of the ebonite of 0.016 radian.

I will now show that though low frequency results would lead to an angular motion of the light spot of 74 radians, the observed angular motion was either absent or less than 0.032 radian; that is, $\frac{1}{2312}$ of the deflection which would have been observed had the low frequency loss persisted.

There was some fear that the water resistance might introduce a sensible capacity at the terminals, and consequently, in this case as in all others, three experiments were made.

(1) With the resistance and capacity in the phasing arrangement as described.

(2) With the resistance augmented by a small impedance, viz. the coil of a low resistance galvanometer.

(3) With two dead resistances of water — one the resistance employed in the other arrangements, and the second adjusted to equality by adjusting the spark lengths.

The results of many sets of measurements are collected for reference in Table A.

It may reasonably be inferred from the irregularity of the results that the deflections, such as they were, were the result of unequal electrifications of the ebonite, and this is supported by the fact that the deflections were not generally produced with the suddenness which should have characterized the real effect when the coil was suddenly started. On several occasions, also, observations were interrupted by the cylinder becoming electrified permanently to such an extent as to throw the reading right off the scale, where it remained long after the coil was stopped.

It may, perhaps, be urged that owing to the capacity of the water resistance, or of the leads, or of the armatures, a phase difference of 90° was not in reality set up. This is, of course, possible, but it is hardly likely that in the three cases examined the phase difference would have been 0° or 180° in each case, and yet this is the position we must take up if we assume that no deflection was observed, because in fact the field did not rotate. In fine, the margin is so great that we may admit that the vibration was nearly plane polarized in all three cases without affecting the conclusion.

Similar experiments were performed on flint glass and on sulphur, containing a great deal of the insoluble variety, and therefore suitable for the detection of any effect, but the results with both these substances were practically identical with the results as quoted.

These experiments were made before the unique properties of selenium had been discovered, and the apparatus was in use for other purposes, or I should, most certainly, have examined this curious substance.

We may now summarize the results of this investigation in Tables B and C.

TABLE A.
EBONITE CYLINDER IN HIGH FREQUENCY FIELD.
Clockwise rotations marked +; counter-clockwise marked —.

Position of the two six-point keys.	Position of the rotation reversing key.	Expected direction of rotation. Clockwise marked +.	Deflections in centimeters.											
			1	2	3	4	5	6	7	8	9	10	11	12
A	V	—	0.0	+ 2.5	+ 0.8	0.0	— 2.0	— 1.0	— 2.0	— 1.0	— 1.0	— 0.62	— 0.1	+ 0.2
A	H	+	— 13.2	— 13.5	— 11.6	— 18.0	— 25.0	— 20.0	— 17.0	— 16.0	— 15.0	— 14.0	— 20.0	— 16.0
B	H	—	+ 21.5	+ 21.0	+ 20.0	+ 35.0	+ 20.0	+ 14.0	+ 13.0	+ 12.0	+ 10.0	+ 18.1	+ 22.0	+ 15.0
B	V	+	0.0	— 0.6	+ 0.3	— 1.5	— 1.5	— 2.0	— 1.0	— 1.2	— 1.4	— 0.2	— 1.4	— 1.3

TABLE B.

Description.	Energy loss and electric intensity relation.	Percentage loss $F=1$.	Percentage loss formula.	Frequency.	Effect of change of frequencies.
Ebonite "Long A"	$W = 0.029 F^{1.765}$	21.2	$21.2 F^{-0.235}$	0.15	Very slight increase with speed.
Silvertown best.					
Large sphere	$W = 0.017 F^{1.85}$	—	—	—	} Nil.
Smaller sphere	$W = 0.03 F^{1.85}$	—	—	—	
Flint glass spheroid	$W = 0.038 F^{1.92}$	14	$14 F^{-0.08}$	18.8	Nil.
Sulphur spheroid:					
A, aged monoclinic; not especially dried	$W = 0.0139 F^{1.91}$	11.05	$11.05 F^{-0.09}$	22	Large decrease.
C, dried	Index $n = 1.5$	Too small for measurement			
K, 0.7 per cent insol.: perfectly dry	$W = 0.00048 F^{1.857}$	0.0038	$0.0038 F^{-0.143}$	18	Small increase.
Paraffin M.P. 50°: Spheroid A	Too small to measure by thread used for ebonite				
B, M.P. 52°	Very small loss indeed				
C {	Index 1.95				
	$W = 0.00138 F^{1.56}$	1.51	$1.51 F^{-0.44}$	12.5	} Considerable decrease.
	$W = 0.0008 F^{1.56}$	0.88	$0.88 F^{-0.44}$	27.3	
Various spheres	Index 1.5 to 1.6		Amplitude not reduced.		

TABLE C.

Description.	Energy loss and electric intensity relation.	Per cent formula.	Change with speed variation.
Selenium:			
Commercial A	Immeasurably large loss.		
Commercial B	Index 1.96	Similar to next, but 3.4 times larger at 5000 volts.	Nil.
Commercial C	Index 1.96	Amplitude about same as next, D .	Small increase.
Pure D	$W = 4.6 F^{1.85}$	More than 400 per cent.	No observations.
Resin:	Index varied from 2.8 to 1.86.	Loss $F = 1$ from	Small increase?
Many spheres.	4 to 18 per cent.		
	Rapid field rotating between 10^6 and 10^7 per second. Ebonite, glass, and complex sulphur gave no loss at all, even though the electric intensity employed was considerably greater than in the other experiments here summarized.		
	Heterogeneous conductors. Paraffin and resin, mixed with large quantities of graphite, gave results not much greater than when the pure dielectrics were employed. The index in these cases was about 2.		
	Conductors: aluminium, paraffin coated with tin-foil, arc light carbon. Resin and paraffin, made conducting with graphite, gave no effect whatever in slow fields.		
	Bisulphide of carbon, benzene, and kerosene were not set in rotation by the rotating field.		

It results from the above investigation (1) that the effect of moisture is very far from being eliminated by the Arno method, though the circumstances are very favorable in the sense that moisture should have a smaller effect than in experiments involving rapid space variations of the electric forces.

Nevertheless, prolonged drying will bring the insulators examined down to a steady state, and a residual hysteresis effect makes its appearance.

This effect can be represented over a range of potential differences of from 0 to 6000 volts with an accuracy of the order of the accuracy of observation by an expression of the form $W = aF^n$. No physical conclusion, however, can be drawn from such a formula, for it merely sums up the results of many complex processes; and it is notorious that a formula of

the kind quoted can be made to fit almost any curve over a short range. The values of n vary from about 1.5 to about 1.95 for ordinary "homogeneous" dielectrics. When dielectrics are made heterogeneous by the addition of graphite, the index rises to 2 and over, and the effects become irregular. This result was not unforeseen, for I had previously found that sulphur conducts discontinuously, and conduction in heterogeneous dielectrics is probably an important feature of the phenomenon. The rise of the index above 2 is to be explained on Hess's theory by the remark that the conduction does not follow Ohm's law; and in terms of the experiments by the remark that if the rate of subsidence of the polarization of the dielectric due to conduction is comparable with the period of field rotation, the energy put into the dielectric during one rotation is quite indeterminate, and the drag depends on the angle between the intensity and the polarization, which in turn depends on the non-ohmic conductivity.

(2) The hysteresis so discovered is quite constant for any given specimen, but is very variable as we pass from specimen to specimen, the index of the empirical expression of the variation being much more constant than the coefficient. It is known that very small differences in physical condition produce immense variations in the hysteresis loss of iron.

The hysteresis of dielectrics resembles the magnetic hysteresis of iron in this respect, but the phenomenon is on the whole still more strongly marked.

(3) The great differences between the values of specific inductive capacity assigned to certain substances by different observers, as well as the results here obtained, suggest that perhaps it is nearly as futile to treat the specific inductive electric capacity of a "substance" as distinguished from a "specimen" as constant, as to extend the same treatment to the specific inductive magnetic capacity of iron, even under given magnetic forces.

(4) The mechanics of the problem of the rotating field require that there should be an angular interval between the direction of the electric polarization and the direction of the electric force, in order that there may be any drag on the suspended spheroid. The problem is essentially similar to the problem of the couple exerted by the moon on the earth about the axis of the latter, and due to the want of correspondence between the phases of the tide and of the tide-raising force.

The fact that there is a drag on the spheroid exerted by the rotating field implies either (1) that dielectric polarization takes an appreciable time to become established, or (2) that dielectric polarization does not die away in simple proportion as the force is reduced. The present experiments do not enable us to discriminate between the phenomena, and it is most probable indeed that one implies the other. The experiments of

observers on the variation of specific inductive capacity, expressed as a function of the period of charge, lead to the same result, but I believe that they have all been affected to some extent by surface conduction.

(4) When I commenced these experiments I hoped to increase the angle between the direction of polarization and the direction of electric force by increasing the rate of rotation of the electric field. Assuming for a moment that the time required for the establishment of the polarization is merely the time required for the inwards sideways motion of the Faraday tubes, we see that for experiments of the frequency adopted in the mechanically rotating cell, the polarization may be considered as established instantaneously. In this case, the drag is due to the persistence of the polarization, and would become a maximum when the rotation was so fast as to make the angle between the electric force and the polarization vectors exactly 90° . A reference to the summary will make it clear that when the dielectrics were dry the drag was either inappreciably affected by increasing the rate of rotation or was increased thereby. At a frequency about a million the effect vanished. There is clearly a speed of maximum effect somewhere between a frequency of rotation of 30 and of a million per second. If I had had a high frequency alternator at my disposal, I should certainly have attempted to run this down, for I think that the measurement of the period of persistence which would be deduced could not fail to give most important information.

(5) The Maxwell suggestion as to the cause of residual discharge does not, I think, account for the results of these experiments. My former experiments on sulphur (*loc. cit.*) show most unmistakably that the break-down of polarization is of an electro-chemical nature, and this fact is sufficient to account for residual charge if the ions are not free to reach the electrodes. The present series of experiments indicates how the time required for this break-down may be measured by any one fortunate enough to possess a couple of alternators of high and low frequency, respectively.

In conclusion, I desire again to thank those who have assisted me, — Mr. Alfred Pepper, in the earlier experimental work, Professor Lyle, who supplied the substance of the mathematical investigation, and Mr. Pollock, who enabled me to apply the same, and who kindly checked a typical set of observations and reductions.

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Nov. 23, 1896.