DISCUSSION ON "DISRUPTIVE STRENGTH WITH TRANSIENT VOLTAGES", "THE ELECTRIC STRENGTH OF AIR" AND "DIELECTRIC STRENGTH OF OIL". JEFFERSON, N. H., JUNE 29, 1910.

D. B. Rushmore: Mr. Whitehead in his paper says something which is perhaps more true than many people realize: "As a result of the increase in values of transmission voltage, however, and of improvements in high voltage apparatus and line insulators, the electric strength of air has become a limiting factor in the long distance transmission of power." It is only of a very recent period that this has been true. It is, however, of great interest and import. In the somewhat old problem of transmitting power, one obstacle after another has been overcome until at present we are confronted with the effect of the insulating properties of air, and what action will be taken with regard to overcoming the obstacles presented by it is a question of much importance at the present time. Until very recently, line insulators were the limiting feature, and at 60,000 volts the standard form of insulator was operating near its limitations. With the new suspension insulators, however, the line can be insulated far beyond the point at which the dielectric strength of air will stand. The question of altitude in transmission lines is also becoming important, because of the lower dielectric strength of air at the higher altitudes. The physical phenomena which take place when corona has formed are extremely interesting, and a considerable number of points arise in connection with it. For instance, it is not quite clear without fuller understanding, why corona should cease to be formed by the lower voltages than that at which it begins.

The phenomena described in the paper are those which are of immediate interest to many members of the Institute. To take an example, a line which was to operate at 60,000 volts had the design of the insulators carefully worked out so that they would always flash over before puncturing. A number of times, however, lightning punctured these insulators and the dielectric time-lag of air was used to explain the occurrence. Also many operating engineers are familiar with the slight blackening of the bright copper wires in their stations, due to the effect of corona formation.

The problem of how best to perform experiments of this kind is a difficult one. We all like to see the conditions surrounding such investigations as nearly as possible like those under which the application of the results will be made, such as the interesting work carried on by Mr. Mershon some years ago on outside commercial lines. It is, however, extremely difficult to obtain exact data in this way, and it would seem that the basis for our engineering recommendations must to a large extent be the result of experiments carried on in the Laboratory.

The relation of power or energy to breakdown or ionization is not clear, and the effect of frequency on such dielectric failure still remains a subject for discussion. The point most deserving of attention is that the future progress of the art depends very largely on the results of the study of the properties of dielectrics. The characteristics of this class of material bring about the limiting features in high voltage power transmission.

V. Karapetoff: Those familiar with the history of the theory of elasticity and of the mechanical strength of materials will find it quite similar to the history of practical electro-statics. During the early stages of the development of civil engineering the engineers who needed to know the strength of columns, bridges, etc., tested them in their actual complicated shapes. The result, therefore, did not characterize the materials, but only certain objects of definite dimensions made out of these materials. Knowing the strength of a column or of a beam of a certain size, it was not possible to predict the strength of a column or of a beam of a different cross-section and of a different length. Then, gradually, a theory of resistance of materials was developed and formulae established into which the elastic coefficient of a material, the dimensions of an object made out of this material, and the given forces entered as independent factors. The next stage in the theory was to combine various stresses and to combine the resultant strains. In most structures and parts of machines there is usually more than one set of stresses (for instance a normal stress and a shear), and unfortunately stresses are combined according to a different law than the resulting strains. Finally the fact became clear that it is the resulting *strain* and not the stress that limits the safe load of a piece of material—I am speaking of mechanical strength now—no matter by what combination of stresses the strain is produced. As the last step, the theory was extended to include the action of *dynamic* loads or what we call in electrical engineering *transient* loads. In bridges, in reciprocating engines, and in the many other cases loads are not steady, they are intermittent; kinetic energy and inertia enter therefore as disturbing factors.

A similar development took place in applied electrostatics. Our predecessors started with a theory of charges acting at a distance. Naturally, with such a wrong assumption, not much progress was made, because the phenomena take place in the dielectric and not on metallic surfaces.

After Faraday and Maxwell had cleared the way for a correct interpretation of electrostatic phenomena, and after it had become necessary for engineers to take into account the dielectric strength of materials, a new impetus was given to electrostatic experiments. But as in the case of early experiments on columns, beams, etc., “practical” electrical engineers still use electrodes of all kinds of fancy shapes, from which the dielectric strength of dielectrics proper cannot be conveniently calculated. Know-

ing, for instance, the striking distance between two pointed electrodes, it is impossible to calculate the striking distance between electrodes of a different shape, or to determine the critical voltage with the same electrodes at a different distance. The question is, however, being gradually put on a more rational basis; it is at present clearly understood by physicists and by many engineers that the striking distance per centimeter of air-gap between needle points really does not mean much, as regards the dielectric strength of air. Experiments must be preferably made on large (practically infinite) parallel plates, in which case the stress in the dielectric is the same at all points. Or, else, electrodes must be used of such a shape that the true strains (critical dielectric flux density) can be calculated from the striking distance. Concentric cylinders are convenient for the purpose.

I wish to refer to the sketches Figs. 1, 2 and 3 of Mr. Tobey's paper, which illustrate my point. The sketches show the difference between a uniform dielectric field in the case of parallel plates, and non-uniform fields in the case of sharp points on spheres.

In a non-uniform electrostatic field it is necessary to consider the actual stresses from point to point, because the air is not broken down simultaneously at all points. It is possible to break down certain portions of the air nearest to the electrodes, to ionize it, to make it a conductor, without producing a jump-spark. The air so ionized becomes a part of the electrode, so that the break-down finally occurs between new shapes of electrodes. This leads to the consideration of the quantity usually called the electrostatic capacity between two electrodes, but which ought to be more properly called the *permittance of the dielectric*. By using proper values of permittance and "permittivity" of the dielectric, calculations of the dielectric strength of insulation are put on the same scientific bases as calculations of conductors of non-cylindrical shapes.

At present we are not yet advanced far enough to be able to calculate the distribution of electrostatic stresses in all cases, but in the simplest cases, for instance, such as is presented by Dr. Whitehead, or in the case of two spheres, it is possible to calculate the actual stresses and their distribution. It is not the average strength of the air, considered as a mass, that interests the engineer, but the *weakest point* at which the air breaks down. The safety of insulation must be considered with regard to this weakest point, the same as the civil engineer figures out the factor of safety of the weakest part in his bridge. And fortunately the electrical problem is simpler than the corresponding problem in the theory of elasticity, because there we have two kinds of stresses, normal stress and shear, while in dielectrics there is but one kind of stress. I do not know what kind it is; perhaps it is some kind of shear between positive and negative electricity; at any rate we have to consider only one kind of stress.

Now we come to the last stage, the same as civil engineers had to face, namely, the effect of dynamic loads, of transient voltages. The paper by Mr. Hayden and Dr. Steinmetz represents the results of a pioneer investigation in this field. Let us consider their experiments in the light of a mechanical analogy, that of an elastic rod or a column suddenly subjected to a normal stress by a released weight. Is it correct to say that the weight is instantly balanced to its full extent by elastic forces in the rod? To me such an assumption contradicts the laws of mechanics of elastic bodies.

In order to produce an elastic reaction a strain or a deformation must first occur in the rod. The rod can resist only (1) as the result of such a strain (elasticity), or (2) by opposing an acceleration of its parts (inertia). In the case under consideration there is no initial strain in the rod, and in order to produce it parts of the rod must be accelerated layer by layer. At the very first instant the load is balanced by a very high acceleration of a very small part of the mass of the rod adjacent to it; then the next layers are accelerated so that we get the effect of a travelling compression wave. Only after a certain displacement has taken place the load is balanced in part or in full by the elastic forces. Hence this paradox that with a very short application of the weight, that corresponds to our transient voltage, the column or the rod can seemingly support without being destroyed a heavier load than with a load applied gradually. Is it not more logical to suppose that the molecular strain, the *actual strain* at which the material breaks down, is the same in both cases? Only in the case of a transient application a part of the load is balanced by the acceleration of the mass and has no time to produce elastic stresses, before the load is removed.

It seems to me that the results obtained by Mr. Hayden and Dr. Steinmetz must be interpreted in the same light. I cannot conceive how a voltage of say 100 kilovolts can be applied suddenly to a neutral dielectric, that is to say to a dielectric which is not strained. The action must be equal to the reaction, and a dielectric which is not strained can produce no electrostatic reaction. It can react only electromagnetically, by means of displacement currents in it, or in the other parts of the circuit. I am willing to grant that the voltage calculated by the authors was actually induced in the high-tension windings of the transformer, but it does not follow from this that at the very instant of the closure of the primary switch this full voltage acted across the air-gap, producing *static* stresses in the dielectric. The electromagnetic inertia effects are predominant at the first instant.

The slope of the voltage wave is very steep at the first instant; therefore, even a very small magnetic leakage between the two windings of the transformer, a leakage negligible with ordinary frequencies, must have produced an enormous counter-electromotive force. I am inclined to think that at the first instant the voltage between the electrodes was practically equal to

zero. The unstrained dielectric acted as a short-circuit, and the applied voltage was balanced by the reactance counter e.m.f. We must remember that the rush of current at the first instant is tremendous, in order to produce the required displacement in the dielectric. There could have been also a considerable magnetic field in the dielectric proper. We have in reality a much more complicated phenomenon, *electromagnetic* as well as *electrostatic*, than it appears on the surface. It is hardly scientific to say that we simply have 100 kilovolts momentarily applied to air or oil, and that momentarily these dielectrics can support more than with a steady application of the voltage.

We must be particularly careful now, when we just begin to understand the full meaning of these phenomena, not to describe them in a "short-hand" way, but to specify carefully the actual physical relations. Without this precaution, busy practitioners are only too prone to misunderstand the results, and to apply them in cases for which they were not intended at all.

Percy H. Thomas: Professor Whitehead's paper suggests many questions that I presume he is not in position to answer as yet, but there are a few points which I think would clear up some matters.

Professor Whitehead has apparently made a very desirable advance in having devised a method for the study of corona and allied phenomena in which he can eliminate all variables which he does not control. His tests, being laboratory tests, and made under somewhat limited conditions, do not tell us everything about corona, but apparently he has succeeded in controlling all the variables actually present in his tests, so that he can absolutely reproduce his results on different days and under quite different circumstances. With such a result realized, it is possible to add another known variable, and get its effect, and so on step by step.

I ask Dr. Whitehead if he will tell us something concerning the electrical conditions at the ends of his tube.

I would ask Dr. Whitehead if it is possible that the peculiar action of small wires in giving the apparently high dielectric strength for air could be due to the fact that near the surface of the small wire the strain is changing rapidly. That is, the volume of air that is subjected to the maximum stress is very much more limited than where the wire is larger; the potential gradient being much sharper in the case of the small rods.

I would ask whether the air which passes through the tube coming from the wire at the time of the formation of corona is ionized in the sense that there are positive and negative charges therein equally balanced, or whether it comes away with a predominant positive or with a predominant negative charge, and if so, which? Also whether he has tried any other gases than air.

Another point, in the visual observation of the corona through oscillographs, has he observed both positive and negative waves,

direction being taken with reference to the test wire—that is, were observations made with the positive potential on the inner wire or the negative potential on the inner wire, or with both? If so did any difference appear?

One other question—I would ask how the actual arc, that is, the complete break down, follows after the ionization first begins, that is how wide a range of voltage, for example, exists between the point where you can first observe a rapid dropping of the electroscopes, and formation of the actual arc.

A. E. Kennelly In regard to one particular point in these very interesting papers, Dr. Whitehead emphasizes the fact the electric intensity at which disruption appears is greater for small wires than for large wires. A suggestion for the reason of this remarkable phenomenon may be offered tentatively. We know, referring to the properties of the magnetic field for a moment, that a small magnetically polarized iron particle, or iron filing, is not acted upon by any magnetic force of translation—any bodily moving force—when subjected to a uniform magnetic field, or a magnetic flux distribution acting in parallel straight lines. There are forces of rotation, couples, or torques, but no forces of translation. When, however, the magnetic field is not uniform but divergent, the iron filings are subjected to translatory forces as well as couples, and are pulled bodily towards the denser parts of the field. This effect may roughly be described by saying that the magnetic pull, in the denser part of the field, on the attracted pole, is less than the opposing push, in the weaker part, of the field on the repelled pole. In fact, we know that the pull of a divergent field $\mathcal{H}$ upon a spherical iron particle or spherical iron filing depends upon the product

$\mathcal{H} \cdot \frac{d\mathcal{H}}{ds}$, where $\frac{d\mathcal{H}}{ds}$ is the greatest space rate of change of the field.

Now returning to the electric field, if we may assume that a molecule of gas subjected to an electric field is polarized thereby, then the molecule will be subjected to a couple or aligning force; but if the electric field is uniform, there will be no pull, or force of translation, on the molecule. If, however, the field is convergent, a force of translation will exist as well as the couple, and the molecule will be pulled towards the denser part of the field. In the case of an electrified round wire, the field will be radially divergent from its surface, and the divergence will be numerically greater, the smaller the wire's diameter. We should, therefore, expect air-molecules to be drawn in towards the surface of the wire, and to crowd together near that surface, just as though the atmospheric pressure were locally increased in this vicinity. If such a local increase of air-density occurred around and near the surface of a wire, the electric intensity required to produce disruption should be increased, as it increases with the atmospheric pressure, and the increase should

be greater the smaller the wire. I tried the experiment recently, with Dr. G. W. Pierce, since reading Dr. Whitehead's paper, of electrifying a two-millimeter wire running down the axis of a glass tube and observing whether the pressure of the air, within the tube as a whole, became thereby altered with respect to the pressure of the air outside the tube. We were unable to detect any such effect. Consequently, although the effect above suggested might occur, and might yet be masked in this experiment by opposing actions, yet it must be admitted that thus far, the suggestion is not supported experimentally.

W. H. Pratt: There is one suggestion which comes to me in connection with the second paper, which has read. The results quoted with moist air are apparently in disagreement with certain other measurements which have been made. May it not be that moist air, as we find it in the open, at times contains electric nuclei due to the partial re-evaporation of condensations that have occurred around these electric nuclei, and hence the moist air, at such times, is really somewhat conducting, whereas in the experiments that were outlined, the moisture was added to the air in such a way that it certainly would not be expected to be conducting.

E. E. F. Creighton: Two of the big problems in the transmission and distribution of electrical energy are the lightning protection of the transmission circuit and the suppression of troubles from internal surges of electrical energy. The dielectric spark lag and the spark energy enter into both problems and most vitally in the latter. Speaking of internal surges only, it has been noted here and there all over the country that transformers and generators would fail at some internal part of the winding. On any particular system these failures are rather infrequent, but the aggregate is considerable. Some time ago this problem of internal surges was confused by the presence of end turn effects. Modern designers have eliminated the end turn failures by heavily reinforced insulation. This leaves the problem of caring for internal surges distinct and prominent. The conditions of cost are not such that a designer may place everywhere in the windings, as he does on the end turning, an insulation that will stand a test pressure of 6,000 to 20,000 volts when the dynamic voltage normally present is only ten to twenty volts. The amount of energy in these internal surges is usually small. The excellence of a design taking these factors into consideration would never be casually apparent, yet they would be vital. With these practical features in view, the theoretical and experimental studies made by the authors assume a great importance. Careful measurements are the life of further progress. A simple method of test is given which can be extended easily to further studies.

The speaker has had occasion to study a particular feature of the dielectric spark lag of protective apparatus, with the object in view of getting rid of it. We were surprised to find lags extending to several seconds. To the speaker's knowledge

this long lag has never been observed before. These data have a strong bearing on the subject of the paper. Among several other things, the dielectric spark lag depends on the excess voltage above the voltage which would cause a spark to pass if applied for a long time, theoretically for an infinite time. For brevity, call this, excess voltage the super-spark potential.

As would naturally be expected, the relation between the super-spark potential and the time, is hyperbolic. For very great superspark potentials the time interval before the spark passes becomes only a few milliseconds, or may even go down into the microseconds.

In the curve herewith reproduced the measurements were taken by the simple method of the oscillograph. Direct current potential was applied to a gap and the time interval measured between the application of the potential and the beginning of the current in the spark. The points of slight super-spark potential deviate very little from hyperbolic curve—in this particular

FIG. 1.—Super-spark potential curve. The points are taken by measurement and the curve is the hyperbola $x(y-23)=1780$

FIG. 2

case $x(y-23) = 1780$. (See Fig. 1). But the points at considerable super-spark potential are somewhat variable, showing an erratic behavior not understood at present. At only 35-volt super-spark potential, the lag is 200 milli-seconds. At 250-volt super-spark potential, it is 20 milliseconds. At 300-volt super-spark potential it is 6 milliseconds.

The asymptote to the curve is 23 volts above the spark potential at an infinite time of application. This may have been due to difference in conditions of tests as this value was taken some time previous to the other readings.

The dielectric-spark-lag was demonstrated by another test using alternating potential. The spark-potential for continuous application was less than the peak value of the alternating potential, yet under these conditions it was not possible to spark across the gap. Successive alternations apparently deionized the gap. At any rate the time of application of super-

spark-potential was not sufficient to cause a spark and the successive impulses were not cumulative.

The obnoxious effect of the spark lag on the operation of lightning arresters was noticed first, years ago, in taking equivalent needle-gaps and other direct measurements of the value of lightning arresters.

The gap at G of a static machine requires a certain pressure to spark. (Fig. 2). This determines the value of pressure that will appear at the protector. At the terminals of the protector is attached the insulation to be protected. Between the protector or lightning arrester and the insulation a small amount of inductance is placed to imitate circuit conditions. When the pressure at G was as high as 100,000 volts, the insulation was not injured as the spark took place quickly at the protector. When, however, the electric pressure at the gap (G) was relatively small, in one case about 3,000 volts, the insulation was punctured. The greater time lag of the protector allowed the charge to pass beyond into the insulation and although the spark sometimes took place in the protector, it occurred too late to give protection.

In regard to Dr. Whitehead's paper, I wish to say that in the study of the dielectric spark-gap, we came across the same thing that he has studied in what he has named dirty wires, or the oxidation of the wire. We had ionization taking place on wires which allowed a spark to pass at less than fifty per cent of the normal spark potential, and I felt that it could not be explained by any roughness of the surface. It seemed evident from the tests we made, that the ionization was due to the oxide on the surface, in some inexplicable manner. In other words, the oxide apparently causes a considerable degree of auto-ionization. After we removed the oxide it disappeared, although the surface was equally rough. The tests were made in hydrogen vapor, and consequently the oxide on the copper was reduced by the passage of an arc over the surface. These results are suggestions although the phenomenon may be different from the effect of duty copper wires in air.

J. C. Lincoln: There is one question I would ask Dr. Whitehead. In the diagram of his apparatus, he shows a wire in the medium which is controllable, and I take it from the diagram that there is passed through the medium an alternating voltage, and therefore that the ionization, such as there would be, would tend to produce positive and negative ions in the tube, and in that way the ions are passed across the indicating instrument and some kind of an indication is given. Apparently either the negative or the positive ions are in preponderance, and I would like to know which did preponderate.

Charles F. Scott: I want to say a word in appreciation of the three papers which have been presented, and the work they represent, and the valuable part which these papers will play in the progress of the art.

In going into a new field like this high voltage field, there is

good deal of pioneer work to be done of two kinds—work in the laboratory and work in the field, theoretical work and practical work. Very often these alternate. First we take one kind of step and then the other. Dr. Whitehead enumerated several papers which have been presented to the Institute, which are preliminary to his, and form a series. In running over these papers, I note the first one is on the dielectric strength of air by Dr. Steinmetz, a laboratory paper; next is the Telluride test by Mr. Mershon, which was a field test; next is the paper by Professor Ryan, a laboratory test; next follows a Mershon paper, giving field tests, and now the Whitehead paper, a laboratory paper follows. The next thing on the regular schedule would be to have another paper from Mershon.

Harris J. Ryan: Dr. Whitehead's paper is a most valuable contribution to the subject.

The writer agrees with the author that "secondary ionization is the cause of initial breakdown and formation of corona." For out-door conditions he prefers the terms "native" ionization to "antecedent" ionization as used in the paper. He differs from the author in the conclusion that native ionization enlarges the effective terminal until the stress (intensity) at its surface falls to the ionizing value. *Native ionization strengthens the stress near the terminal and diminishes the distance to the ionizing zone. This distance is required to accomplish the process of general ionization by collision, i.e. the formation of corona.* At a stress of 76 kilovolt-in., or 30 kilovolt-cm., ions travel nearly one inch in 0.0002 second. The region about the electrode enclosed by the zone in which the corona starts contains mostly native ions of opposite polarity. Such ions will strengthen the stress and lessen the striking distance to the corona forming zone.

Recent studies of the normal in-door air show that the value of J. J. Thomson and the one in which the author of this paper expressed confidence, a year ago, is correct, namely, 76 kilovolt-in. There is much evidence that 40 kilovolt-cm. as given now by the author is too high for normal in-door air.

The rapid outward spread of the stress from the surface of a small conductor and the greater concentration of native ions combine to make the striking distance to the initial corona zone shorter. Since the initial corona zone always forms in the normal air at 76 kilovolt-in., the net result is that the surface stress of the conductor rises as the initial corona striking distance falls.

In regard to the statement "In view of these discrepancies one naturally turns to Mershons' method for measuring the loss as a possible source of error." A careful study of the methods used at Niagara and recent laboratory tests have convinced the writer that Mershon's methods and results are correct. There are no real discrepancies. The surprises are due mainly to the larger native ionization at the Falls. Mershon's Niagara corona zone stresses go below 76 kilovolt-in., because of irregular

distribution of stress about parallel conductors worked with alternating high-tension; this irregularity is produced and enhanced by the larger supply of native ions occurring at Niagara Falls. The vapor-product is an indirect evidence of their quantitative variation.

R. D. Mershon: Referring to the paper by Mr. Hayden and Dr. Steinmetz, I would ask them to make clear how they determined the energy available at the spark gap; how they found out what energy was effective.

C. P. Steinmetz: We did not determine it, do not know it.

R. D. Mershon: Then it seems to me it is pretty hard to arrive at any definite conclusion.

C. P. Steinmetz: The only conclusion is the total energy supplied to the apparatus, hence the energy at the spark gap must be less than that. We give only the energy limit.

R. D. Mershon: The question of energy is related to that of voltage. I do not see how you can have any idea of the voltage finally impressed upon the dielectric.

C. P. Steinmetz: I will answer that in the final discussion.

R. D. Mershon: Referring to Dr. Whitehead's paper, the questions and doubts he raises in regard to the accuracy of my high-voltage measurements at Niagara are all of them questions and doubts which I myself raised at the time the results were obtained, and which I feel were resolved by the careful tests made of the accuracy of measurement. We tested out the method of measurement in every way possible and in consequence of these tests I am well satisfied that the method of measurement is a reliable one, and that the results obtained are substantially correct. I feel confident the accuracy of the measurement is within 5 per cent, and probably much closer than that. Reference to my paper will show the kind of tests made, and the method of making them.

The apparatus used at Niagara was not that used at Telluride. It was of the same kind, but better designed and executed. At Telluride I had a 1,000,000-ohm resistance with which to test out the accuracy of the measuring apparatus. At Niagara no such resistance was available, but in view of the closely checked results obtained at Telluride making use of the resistance, together with the fact that the Niagara apparatus was an improvement on that at Telluride, and in view of the results of such check-readings as could be taken at Niagara, I have no suspicion whatever of the accuracy of the Niagara apparatus and the results obtained with it.

Dr. Whitehead says:

"Since Mershon's observations appear to have been taken in all seasons, it may be stated that a range of temperature of 35 deg. cent. and a pressure of 25 mm. of mercury, and an error of 5 per cent in the critical point as taken from the curves, would give a range of critical voltage as wide as that attributed by him to moisture".

I judge from this that Dr. Whitehead thinks the relation I found between critical point and vapor product may be the re-

sult of a series of errors. If this is the case, it is curious that these errors should have occurred so consistently over a year and a half, during which time measurements were taken almost every day.

I do not see how it is possible that any errors in our observations or in our method of measurement could account for the variation of critical point with vapor product. The effect was entirely too marked to be so accounted for. Before we discovered the relation between loss and vapor product, we would sometimes get consistent results over a period of several days, and then the results of the next day would be all out of line. It even happened at times that of a set of observations all taken on the same day the results would be erratic and entirely inconsistent. But all these readings, when interpreted by means of the vapor product, came together and were consistent; the results of different days came together; discrepancies occurring on the same day were reconciled. So I cannot conceive of the relation referred to as being in any way due to errors in either measurements or apparatus.

In this connection I would call your attention to the facts brought out in one of the other papers here, relative to the very small amount of moisture necessary in oil in order to enormously change its dielectric qualities. In what condition does that moisture exist in the oil? Is it water, or vapor? It certainly must be very finely divided, at any rate, for it is uniformly distributed through the oil. This may have a considerable bearing upon the loss in air due to moisture; a loss entirely aside from the so-called ionization loss.

The only error that I can conceive of as possible, due to the method of measurement, would arise from phase displacement in the measuring apparatus itself. The tests made of the apparatus, as detailed in my paper, seem to me to pretty clearly show that there was no such error, or at any rate it was not great enough to account for the discrepancies, if you may call them such, between my results and those of Professor Ryan and Dr. Whitehead.

I would call your attention to the fact that the results obtained by Dr. Whitehead, and to a considerable extent, the results obtained by Professor Ryan, are not necessarily comparable with my results. I did not investigate the matter of ionization. I investigated the point at which the loss between outdoor transmission line conductors begins to rise rapidly, which point I called the critical point. I do not think this loss is necessarily all ionization loss. It seems to me it may include losses other than those due to ionization. The results of Professor Ryan and Dr. Whitehead were obtained under laboratory conditions. My results were obtained under engineering conditions. I do not believe we are safe in applying the results of laboratory investigation to the engineering problem, unless we can check up these results under engineering conditions.

There are too many other elements which may enter to modify laboratory results as applied in practice.

What I have said applies to the conclusions. Conclusion 5 says, "Water vapor or moisture has no influence on the electric strength of air. Increasing moisture content probably lessens the loss above the critical voltage". That depends on what you mean by the critical point. If you mean the point at which ionization begins, the statement is possibly true, but if you mean the point at which the loss between the wires of a transmission line begins to rapidly ascend, I do not agree with that at all. Similarly with regard to Conclusion 6—"There is no loss through air until the critical voltage accompanied by ionization and corona is reached". I believe there may be a loss below the critical point, as I have just defined it, and that this critical point does not necessarily correspond to the inception of ionization.

Now, as regards Conclusion No. 10: "The corona has high conductivity, and most of the loss takes place beyond it". I do not remember anything in the paper which would appear to justify this conclusion. If there is, I shall be glad if Dr. Whitehead will bring it out more strongly.

Referring to Mr. Tobey's paper, Fig. 7 would be much more interesting if the melting point of the oil were shown, and it is to be hoped that in closing Mr. Tobey will give us the melting point. This curve presents some facts in regard to which I have often wondered. I have been afraid sometimes that if the oil in transformers was allowed to solidify, due to cold, we might get into trouble, but according to this curve we are safer with the oil solidified than with it in its normal condition.

C. P. Steinmetz: First, to answer some questions regarding our paper—the energy values, are, as stated in the paper, not the energy appearing at the spark-gap, but are the total amount of energy supplied to the system through the transformer, and, as stated in the paper, the actual energy supplied to the spark-gap probably is only a small part thereof, so that these values merely mean the upper limit of energy, which is sufficient to break down the gap, and probably much less energy will be enough.

As regards the question whether the voltage was actually at the spark gap or not, we carefully looked into the matter and came to the conclusion that this voltage is there, at least approximately this voltage, as near as it was possible to judge. Mathematically, I gave the reasoning in the appendix, which shows how we calculated the voltage which appears at the secondary terminals and thereby at the spark-gap. We know if there is one thing where experiment and calculation agrees, it is in transient phenomena, provided you consider all the factors involved in the problem.

There are two factors which have not been included in that mathematical calculation. One is the variation of the in-

ductance; the second is the capacity of the secondary circuit. It can easily be seen that the variation of the inductance with the voltage has no effect on the voltage, but merely modifies the shape of the voltage curve of the secondary. There remains then the consideration of the secondary distributed capacity. From the constants of the transformer, its estimated resistance, inductance and capacity, it appears obvious, that the effect of capacity could result only in an oscillation, not in a steady change, that is, that at the beginning of the impulse there might appear and probably does appear an oscillation superimposed on the steady discharge curve, calculated in the appendix. Such oscillation may be either limited to the high voltage circuit, and then be of very short duration or small energy, or it may extend into the primary circuit.

In the paper we gave evidence that such oscillation did not extend into the primary circuit, by changing the resistance of the primary circuit, increasing the non-inductive resistance of the primary supply circuit nearly a hundred-fold, and getting the same result. It is obvious if there is any oscillation taking effect in the primary, that by increasing the resistance of the oscillating circuit nearly one hundred-fold, from about $\frac{1}{2}$ ohm to 26 ohm, the oscillation must be enormously reduced and damped out, and we found the same transient discharge. I believe, however, there was in the high potential secondary circuit an oscillation superimposed on the exponential curve of the calculated voltage ratio, but of extremely short duration, and as the entire effect is a transient, you see that an oscillation of still much shorter duration at the beginning of the effect, could not well be expected to have an appreciable effect. It is, however, desirable, and we hope some time to be able to carry out corresponding investigations by some other method of producing the transient voltage, and thereby being able to determine exactly the amount of energy existing at the spark-gap.

A most interesting paper we have in the next paper, that by Dr. Whitehead, which I consider as extremely valuable and important at present. I may say, indeed, that we had intended to make the same investigation, even assembling apparatus of the same kind, a conductor in the center of a tube, and measuring the beginning of the breakdown by an electroscope. We expected to carry that investigation a little further, that is, to lower air pressure, so as to study more carefully the effect of higher altitudes, since we know that transmission lines at the present time extend to altitudes where the air pressure is appreciably reduced.

There is only one point in Dr. Whitehead's paper, concerning which I do not agree with him, and that is his opinion that the explanation of the apparently greater breakdown strength of air with small wires cannot well be given by the assumption of the zone of condensed air around that wire. We know, and it can easily be shown physically, that all solids are surrounded

by a zone of condensed air, and a rough calculation will show that the densities of this air at the immediate neighborhood of the conductor, by gravitational or molecular attraction, may reach considerable values. This phenomenon of the great increase of disruptive strength of air close to the solid terminals I observed and gave the explanation, that it is due to the condensed layer of air, some years ago, I believe in the paper which I read on disruptive strength before the Institute in 1893. There I found that in testing very small air gaps the disruptive strength very greatly increased, beyond that observed in the case of larger air-gaps between parallel plates.

As we know, Professor Ryan has very carefully looked into this effect, and in his paper of 1904 gave it a numerical value. I believe it would be very interesting, first, to take his numerical value of the layer of condensed air, and reduce Dr. Whitehead's tests by this assumption, thereby seeing whether we do not get a greater constancy, and then, inversely, from the data of this test, calculate how thick a layer of condensed air we have to assume to get constant break-down strength of the air. It is reasonable to assume that the layer of air is not the same, but decreases with decreasing diameter of the wire, due to the effect of divergence.

If there exists such a layer of condensed air, the result would be that the initial break-down occurs, not at the surface of the conductor, but at a small, though finite distance from the conductor surface, and with the increasing voltage the break-down extends outwardly, but also extends inwardly towards the conductor, and at a second higher voltage reaches the conductor.

This gives a second point in the curve where a change of the phenomenon might be expected to occur. It is very interesting to realize that with our increasing knowledge of the phenomena of disruptive strength, we are more and more impressed with the similarity of the electro-static strength, and its laws and phenomena, with the laws and phenomena existing in the case of mechanical forces, as pointed out also by Dr. Karepetoff. Only I do not agree that the electrostatic phenomena are simpler than the mechanical, due to the absence of shear, but there is very good evidence which makes it probable that electrostatic shear also exists, because at the edge of the electrostatic field, where the electrostatic field very abruptly changes, for instance, at the edge of two parallel plates, there is a very much greater disruptive force; although the potential gradient would be the same, and there is some evidence qualitatively, though not sufficient quantitatively, that not only the disruptive potential gradient, but also the abrupt variation of the potential gradient causes a breakdown. This was discovered first in the old Ferranti system in London, England, in bygone ages, when the cables had the unhappy habit of breaking down at the edge of the insulation—where there is an abrupt change in the electrostatic field, that is an electrostatic shear, and it is so well known

in the industry that we always carefully avoid abrupt changes of the electrostatic field, for instance, at the end of lead covered cables taper the field, etc. Experience has shown that if we allow too sudden variations of the static field, we break the insulation down, although the insulation is just as thick as where the field is uniform.

Originally when electrostatic phenomena were first investigated, they did not appear to agree with a constant breakdown strength of air, but gradually the phenomena lined themselves up in the direction of law and order, pointing towards a definite strain which air will stand, and beyond that will break down, and what we here are mainly interested in is the range beyond the break down, that is the phenomena beyond the electrostatic elastic limit of insulating material, where the phenomena have ceased to be reversible and become irreversible. It is the same thing in mechanical engineering, and we shall listen still to a paper dealing with an analogous problem in the mechanical force. This is an extremely interesting field, but the difficulty of exploring it is the same as in the case of almost all phenomena—you never find in nature a clear cut case of the action of one phase or one form, you always have it complicated by numerous other conditions, forces or phenomena which are existing as secondary actions, and practically most of the work of the electrical engineer and other engineers consists of a very simple calculation of the theoretical condition, and then he must calculate and estimate the secondary actions which modify, and sometimes entirely obscure the primary action of the phantom apparatus under phantom conditions.

The most important secondary actions in electrostatic phenomena are:

First, the phenomena of the effective terminal. That is, when we have electrostatic fields, the terminals of the field are not the solid terminals, the needle points or spheres, but are made up of that space surrounding the solid terminals within which the air or space is broken down, has passed beyond its elastic limit and is more or less conductive. Between needle points, even at extremely low voltages, the break down strength is exceeded at the points, nevertheless the spark discharge does not occur until much higher voltages are reached, and when it occurs it is not a discharge between needle points, but between these effective terminals, these approximately spherical spaces of conducting or ionized air.

The study of that phenomenon mathematically is rather difficult; is not as simple as it appears. We can easily consider the electrostatic field with its force lines and equi-potential surfaces between needle points or spheres, and then see at a given voltage what is the equi-gradient surface which corresponds to the break-down of air, and all the air space inside of this surface would become conductive. But as soon as the space becomes conductive up to the equi-gradient surface, the equi-gradient

surface becomes an equi-potential surface, while it is not an equi-potential surface in the original electrostatic field. That means the distribution of the lines of force changes and the entire field changes and the problem is to find that equi-gradient surface which at the same time is an equi-potential surface, and has the particular numerical value of the break down strength of air, and that is rather a more difficult problem.

The second phenomenon is the layer of condensed air which surrounds the solid material. The result is that the disruptive strength at the surface of the terminal is different from what it is in the space at some distance. At atmospheric pressure it is very much higher. The result of that would be, as I stated, that the initial break-down occurs at a voltage corresponding to, not the break-down gradient at the conductor surface, but at the space some distance away, or, in other words, it occurs when the ratio of the electrostatic gradient to the density of the air has reached the maximum at some point at some distance from the conductor. This also requires further theoretical investigation, which probably can be done, because we know the law of gravitation, and the gas laws, and the layer of condensed air could be calculated.

When you have electrostatic forces acting, the phenomena may be expected to be complicated still further, as Dr. Kennelly pointed out, by the electrostatic attraction, which changes the density of the layer of air and its volume.

Lastly, and one feature which we usually overlook, is the resistance of the conducting space of the effective terminals. It is not correct to say that the space within which the air is broken down and which forms the effective terminal, is a perfect conductor, but it has some resistance, and therefore quite considerably modifies the phenomena. To illustrate it by the discharge between needle points—we have a discharge occurring between these effective conductors, spherical spaces of ionized air, but these spaces are not perfect conductors—they have a certain resistance, and the resistance has the characteristic of gas resistance, that is, it increases enormously with decreasing current density. Now, the current density depends on the size of the conducting terminals, the volume which has to be ionized, and also on the frequency, and you see, then, within this range, until you have reached so large conducting spheres, effective terminals, that the amount of current required for ionization is large enough to give a negligible resistance, the frequency as well as other features, have no effect. That is very markedly shown in the striking distance curve between needle points. Theoretically, we can calculate it, allowing for the effective terminals, the ionized air space, and no matter what allowance we make we can easily show that the striking distance between needle points should be a straight line going through the origin. Now, the actual curve, as we found it by test, is given in the paper in the 1898 *TRANSACTIONS*, at 125 cycles; it differs from the

straight line at lower voltages. More recent tests at 60 cycles seem to show a curve in which the deviation from a straight line extends to somewhat higher voltages, and there seems to be some evidence which makes it probable that at extremely high frequencies, 100,000 cycles, the curve remains straight practically down to zero, that is, is the true theoretical curve. The cause of this deviation from the true theoretical curve seems to be the resistance of the effective terminals which, within that range, are still so small that the current consumed by them is sufficiently low to give a resistance, consuming a voltage comparable to the voltage between the needle points. At higher voltage the space ionized is so large and the current absorbed is so great that the effect has disappeared. That is another secondary phenomenon which requires consideration.

Again, we really do not observe the break down voltage, or the elastic limit of air directly, but always indirectly by different methods, and there is no evidence that the point we observe by different methods is the same point. We get it by the disruptive discharge. We get it by observing the beginning of ionization, as described in Dr. Whitehead's paper. We get it by the corona. We get it by measuring the energy loss in the conductor as in Mr. Mershon's paper or Mr. Ryan's paper. Then we get it by the increase of capacity of the conductor, at the voltages where the corona spreads out and increases the effective conductor. There is no reason why the different methods should really give you the same point. As soon as the air begins to break down anywhere in space, an irreversible process occurs. The energy consumed by it is not returned. It means at that point the energy loss in the electric system must appear. If we could measure the beginning of the energy loss exactly, which is an extremely difficult problem, we could show the beginning of the energy loss, the beginning of the break down.

As there is a layer of condensed air, that initial break down occurs at some distance from the conductor, and the conductor, therefore, is surrounded by a zone of conducting air which is separated from the conductor by a layer of compressed and insulating air. There is no conduction of current from the conductor into the ionized air region until a higher voltage is reached, when the zone of disrupted air spreads to the conductor and current begins to flow into the zone. It appears possible that the point where the capacity of the conductor increases is not the point where the initial break down occurs, but is the point where the zone of broken down air gas spread up to the conductor and reached it.

It is curious to note that there are also two visible steps in the luminosity which are quite sharply marked, and are also apparently sharply distinguished by their chemical action. First appears the glow, the noiseless or silent discharge. Perhaps this is the break down at some distance from the conductor, not at the conductor—while the air at the conductor still is insulating, but

at some distance therefrom has broken down. This does not appear at needle points, where the break down must occur at the beginning, but it appears very markedly at plane surfaces. Then at higher voltage, the blue glow is superseded by violet streamers, which are noisy and are interrupted, intermittent, and appear to represent conduction of current, when that ionized layer of air has reached the conductor. When conduction occurs into the space, that conduction, following the gas laws, has the effect that as soon as current begins to pass locally the resistance at the space falls, a discharge occurs, the voltage disappears and the current disappears, so by the laws of gas conduction the current must be intermittent and must always be a localized expenditure of energy as a spark or streamer. At this point, apparently, the chemical action changes. Where we have the blue glow, the energy seems to be sufficient to dissociate the oxygen molecules, but not the nitrogen molecules, and ozone formation takes place. As soon as you see these violet streamers, not only the oxygen molecules, but also the nitrogen molecules split, and you get nitric-oxide, but less ozone, because at the high temperature of that streamer discharge the ozone is broken up, so that there seems to be a sharp dividing line. Below that the blue discharge produces ozone, above it the streaming discharge or brush is intermittent, and of different color, noisy, and seems able to fix nitrogen.

The whole subject is an extremely interesting one, and what I have said here I do not want to have taken as conclusive evidence, as the entire field is altogether too new to accept final conclusions. This is only my present personal opinion of what, from the evidence, appears to be probable. We must all be governed by further evidence, which may and probably will change our opinions more or less.

John B. Whitehead: Before answering the questions which have been asked in connection with my paper, I want to say a few words in general comment on the three papers which have been presented at the morning session.

These three papers all deal with the effects of high voltage on insulation. Each of the papers presents a set of experimental results on this perhaps most important of all electrical questions. All of them fail, however, to explain the observations in terms of simpler phenomena or physical laws at present understood. Nor do the few suggestions of explanations which are offered coincide either in point of view or language of expression. There is here then a sharp indication of present uncertainty surrounding these highly interesting phenomena. The uncertainty, however, is by no means as great as may appear from this confusion. The phenomenon of spark discharge in all its manifestations has been a subject of study by experimental physicists for many years. As a result of this study the ionic theory has not only paved the way for most fruitful investigation, but has been finally established as a medium through which

many electrical manifestations now receive their explanations in terms of the laws of simple mechanics. For no set of phenomena is this more true than for the spark discharge in gases. While there are still many phenomena which have not yet been brought into perfect accord, those which have been studied most widely show conclusively that for a proper understanding of the conditions governing the break-down of insulation whether gaseous, liquid or solid, the way must be through the knowledge of the properties of ions which has been already gained. These comments are suggested by the mode of interpretation used by Messrs. Hayden and Steinmetz in discussing their results. We are to be congratulated upon the presentation of so careful and accurate a set of experiments in a field so little understood. The conclusions drawn from the experiments by the authors, however, invite some comment.

Disruptive discharge is said to require a definite minimum amount of energy. The laws of spark discharge in air which may be said to be firmly established, explain the phenomena in terms of motion of gaseous ions, *i.e.*, charged particles, acted on by electric force. These ions have mass, and therefore acceleration and momentum when in an electric field. The laws of motion and impact of the simple mechanical system thus presented lead in many cases to complete explanation. Spark discharge is the result of secondary ionization or the collision of a charged particle moving under electric force with a neutral molecule, thus breaking up the latter into new ions and so furnishing the conductors for the discharge. Since the free ions have mass they require time to attain speed. If the electric force is transient and of sufficiently short duration the ion may remain practically unaffected. The study then of the phenomena of transient voltages should be extended to that of the shape and duration of the voltage impulse, *i.e.*, the integrated product of intensity and time in relation to the known properties of gaseous ions. Even granting that the initial discharge voltage of the experiments is properly deduced from the ratio of turns, this value cannot exist for more than an infinitesimal time and the appendix concludes that the shortest and longest time durations of a pulse are in the ratio 1 to 100. It is concluded that the discharge does not instantly follow the application of voltage, although the observations leading to this conclusion are not described. It is also stated that during the interval energy is being supplied to the dielectric, and relations are deduced between energy and sparking distance for air and oil, as given on page 770. Although the observed points in the two cases fall on curves of different shape they are assumed to have the same equations, and although the energy is measured at the transformer primary, it is assumed to be a basis of comparison for the secondary spark-gaps. It would be interesting to know what part of the energy delivered to the magnetic circuit is regained upon opening the primary circuit. It would also be

interesting if the authors would present a conception of the mechanism by which a dielectric accumulates energy leading up to disruptive discharge.

Taking up the points that were brought out in connection with my own paper, the first question was that of Mr. Mershon as to the point on the voltage wave at which the corona ceases in relation to the point at which it starts, and he raised the question as to just how you could expect the corona to stop at a lower voltage than that at which it started. The indication is that there is only a small lag, if any, in the point of disappearance of the corona. The corona, however, is a source of heat in the gas, and the raising of temperature has the effect of lowering the corona voltage.

Regarding Mr. Thomas' questions—first, the conditions at the ends of the tube: The wire is carried through a cylindrical metal bushing set in a glass receptacle of the shape very much as indicated in the drawing on page 1063. There is no observation of spark discharge, no corona nor anything else indicating ionization at this point, occurring before that on the length of wire within the tube. If you carried the wire through a rubber or other insulating bushing, ionization would first set in at that point, and would spread to some extent along the wire inside of the tube. The variation of electric field at the end of the tube is quite marked, of course, outside the tube, but does not extend to the region inside for any considerable distance. As soon as corona forms, it is very evident that no error is introduced on this account. The whole apparatus was, of course, enclosed with the idea of conducting any ionization that was generated out through the gap leading to the electrode. I think there was no error on this score.

With reference to the question as to whether in a small wire rapid change of gradient at the surface would explain any of these phenomena: The potential gradient does change very rapidly; however the distances within which the potential gradient varies widely are very much greater than mean free paths of the molecules and ions.

P. H. Thomas: You did not catch my point—the question is whether the volume of air which was subjected to something like the critical strain was not greater in the case of the large wire, not that the mean free path was involved, but the surrounding presence of supporting ionized air might prevent the overwhelming of the very narrow zone there, very narrow particles, with reference to the free paths.

J. B. Whitehead: I am not sure that I get the idea. Ionization in this case is a sharply cumulative effect. As soon as you get a region where there is this secondary ionization, when this second ionization starts, it is a continuous thing, and if you attempt to explain it by any relation between the mean free paths of the ion or the molecule, and the variation in diameter of the wire, there is the widest kind of discrepancy. I have

given the figure 10^5 in the paper showing that the two quantities are of different orders of magnitude.

Referring to the question as to the state of the air as it leaves the wire, whether it has positive or negative ions—I think Mr. Lincoln asked a similar question—a gas ionized in this way has both positive and negative charges—the point of initial break-down will be observed by charging the electroscope either positively or negatively. The only differences which come in are in the shape of the discharge curve of the electroscope. These differences are to be expected in view of the known fact of the difference in the rates at which the positive and negative ions move in the gas and the different rates at which they diffuse. The point of break-down can be observed with either kind of ion. I have not worked with any other gas than air.

The question as to the positive and negative corona is taken up in the paper, rather briefly, and I have pointed out that there is no apparent difference in the positive and negative corona unless it be possibly the somewhat greater brightness and sharper definition of one. I am not prepared to say that there was any noticeable difference.

As to the relation of initial ionization to the following arc, the corona starts evenly, as you are, of course, aware. As you go on increasing the voltage you will finally get to either a spark or an arc. Now then, just what this range of voltage is will depend on the relative diameters of the inner wire and the tube. The potential gradient between a tube and the central wire depends upon the diameter of the wire in such way that under some circumstances an increase in diameter will mean a lowering of the electric intensity and under other conditions it will mean an increase in the electric intensity. With the increase in corona diameter, resulting from increased voltage, when the corona reaches a point so that any further increase means an increase of intensity the spark or arc follows.

Dr. Kennelly and Dr. Steinmetz have each suggested that an increase due to pressure in the neighborhood of a small wire may offer a means of explaining the increase of electric intensity of corona and break-down. I am not quite sure that I grasp Dr. Steinmetz's idea, in that I cannot see why you should expect any difference between the case of small and large wires. Professor Kennelly's idea is, of course, a possible one, and there is no difficulty about imagining such a thing to occur. I would say, however, that I have made efforts to observe any increase in pressure in the neighborhood of these wires by optical means—the increased density of this air in its relation to the neighboring portions, permits the use of a very simple optical method of observation. Such experiments were negative in my case, although I cannot say I think that is any conclusive proof that some such increase of pressure may not explain the observed facts. Since we are dealing with extremely small distances, and I am not sure that even with the optical method I have used the

layer would be sufficiently thick to manifest itself in this way. The test is known to physicists as the "Schlieren" method.

Professor Creighton made some comments as to the auto-ionization of the air and the necessity of removing ions. I do not know the word auto-ionization. Ions may be generated by secondary ionization, as in coronæ, and recombine very promptly; the air is then to all appearances in quite the same state as it was before secondary ionization or coronæ set in. This is shown very well by the sharpness with which the corona stops and by the absence of any after effects of ionization. There are always a certain number of free ions in the air, and these ions account for the small conductivity which air has. If you generate ions by some external means, and put them in the air, it simply means you can get a greater leakage current, but the presence of these ions does not in any way hasten or lower the point at which secondary ionization steps in, it simply increases the leakage current. There are numerous ways of removing free ions from the air; the use of glass wool, or crude raw cotton and various materials of like nature, through which the air can be drawn to a perfectly pure state, so far as ionization is concerned, but it will always have a certain extremely small number of free ions. The natural conductivity of the air is entirely explainable on this basis. For reasons which will be found in the remarks I have just made, I do not think Professor Ryan's suggestion, as well as I was able to gather it from hearing his letter read in the meeting, that there may be a difference between indoor and outdoor ionization, offers an explanation of the discrepancies that he is calling attention to. The range of natural ionization throughout the regions which have been investigated all over the earth, is about in the ratio of 1 to 4, and as I have said, the presence of more or less free ions in the air, simply increases conductivity, but I do not believe under any circumstances it would hasten the point at which secondary ionization, spark discharge, or corona would start.

Mr. Mershon has asked the effect of the wave form on the observations. The discussion of the wave form is given in the paper, showing the relation of maximum to effective values, and also in the reference to the curve Fig. 5. It is possible to observe the corona very close to the peak of the wave, and it is so sensitive that it was possible by careful voltage adjustment to pick up the three little peaks by the stroboscopic method with intervening dark spaces corresponding to the dimples.

As to Conclusion 10, that the corona has high conductivity and most of the loss takes place beyond it, that is based on the results given in the paper, where it is shown that the intensity at the edge of the corona is the same, practically the same, differing from it by a small amount, as that of a solid wire of the same diameter, and if that is the case it is a fairly reasonable assumption that the corona has high conductivity, and marks the limit at which ionization takes place.

In calling attention to Mr. Mershon's method of measurement I have been led by the process of elimination of all other explanations of the discrepancies between our results. Mr. Mershon has pointed out that his critical point is not necessarily the same as my own, but I am unable to see any other explanation than that of corona and break-down for the sharp, upward bend of his curves. I am unable to think of any means, also, by which there is a possible loss through the air below this point.

It has been suggested by several that laboratory methods present conditions different from those obtained outside and that this may be an explanation of some of these differences. I should be interested to hear some one suggest some means or method by which a loss could take place through air at voltages below those at which it actually breaks down.

M. A. de Chatelain (by letter): The writer and his associate, Professor V. F. Mitkevitch, were led to the question of corona formation and the accompanying losses in connection with the question of possibility of high-tension power transmission in the vicinity of St. Petersburg, Russia.

Laboratory experiments were performed on cylindrical conductors of different diameters, placed at different distances from each other. In view of discrepancies in the values of the critical voltage, as given by Messrs. Scott, Mershon, Ryan, Kapp, and Berg, among themselves and with our results, Professor Mitkevitch was led to investigate the matter theoretically. Based upon J. J. Thomson's researches on conductivity of gases he has found the following formula for the critical voltage:

$$E_{eff} = 35 \frac{H}{273+t} \cdot r \log_{10} \frac{d}{r} \text{ (in kilovolts)}$$

where

H is the barometric pressure in mm. of mercury;

t is the temperature of air in degrees centigrade;

r is the radius of the conductor, in cm.;

d is the distance between the centers, in cm.

Changes in humidity did not produce any appreciable variations in results. The values calculated according to the foregoing formula were checked experimentally in the electrical laboratory of St. Petersburg Polytechnic Institute, and were compared to the results given by Professors Ryan and Kapp for the same conditions. The comparison is given in Tables I and II. It will be seen that the difference between the voltages at which the corona became visible, and those calculated according to Professor Mitkevitch's formula, are not large.

It may be appropriate to mention here that Professor Kapp's adaptation of Mershon's formula (Journal of the Inst. El. Eng., February, 1910) leads to an odd result, namely, that the critical voltage, beyond a certain limit, becomes lower with increasing diameter of conductors (see Table III).

Mr. Mershon's statement that the losses increase with the frequency led us to investigate the effect of frequency. Our results seem to indicate that the loss is practically independent of the frequency, and rather decreases at higher frequencies. Namely, we measured the losses between two parallel planes, 27 x 27 cm. each, one of which was covered with needles of the same length. The distance between the points and the opposite plane was 2 cm. The frequency was varied from 15 to 90 cycles per second. After having taken into account the necessary corrections for the capacity, etc., we found the following values of losses:

Cycles	Watts
15	56
50	54.5
90	54

TABLE I

2 r (mm.)	$\frac{d}{\text{mm.}}$	Critical kilovolts	
		Observed	Calculated*
5 (cable).....	1000	65	60
11 (cable).....	"	120-125	115
11 (tube).....	"	125-130	115
14.3 (tube).....	"	145-150	141

*Formula Professor Mitkevitch

TABLE II
CRITICAL KILOVOLTS

$\frac{d}{\text{mm.}}$	2 r = 10 mm.			2 r = 15 mm.			2 r = 20 mm.		
	Ryan	Kapp	Mitkevitch	Ryan	Kapp	Mitkevitch	Ryan	Kapp	Mitkevitch
500	143	82	98	180	89.5	135	212	92.5	165
1000.....	165	94	110	209	104	155	250	109.5	195
2000.....	187	106	125	236	119.5	177	287	125.5	225
3000.....	200	114	135	256	128	190	310	135	240

TABLE III

$\frac{d}{\text{mm.}}$	2 r mm.									
	5	10	15	20	25	30	50	100	150	200
2000.....	79.3	106	119.5	125.5	128.5	131	130	119.5	—	—
10000.....	98	136	154	164	171	172	177	169	163	156