DISCUSSION.

Papers by Percy H. Thomas, Miles Walker and Charles E. Skinner, at Great Barrington, June 20, 1902.

DR. EDWARD L. NICHOLS:—I should like to say in reference to this last paper, that in some recent observations by Dr. Shearer which were intended for quite a different purpose, that of studying the change of length of tubes subjected to electrostatic pressure either alternating or direct, our conclusions with reference to what took place were very similar to those described in this paper. It was found that before the breaking down of the dielectric, this heating effect was noticed, even in glass and rubber, making it exceedingly difficult to carry the tests up to the neighborhood of the breaking point. I should say that the observations which were made were entirely corroborative of the view just suggested; that the breaking down of the dielectric even where the stress is direct and not alternating, was to be ascribed to heating. It seemed to be a mechanical breaking down, due to the extreme local heating of the dielectric, but in no case a rupture traceable to the mechanical forces of the electrostatic field itself. I mention these observations because they were made with the direct electrostatic stress, and not with alternating potentials, and may serve therefore, to broaden and make more general the conclusion reached by the author of the paper to which we have just had the pleasure of listening.

MR. CHARLES F. HOPEWELL:—I would like to ask whether the author has made any tests on underground cables.

MR. SKINNER:—I know of no test made directly for that purpose, but I think the results are entirely applicable to the insulation of such cables.

MR. W. S. ANDREWS:—I should like to ask Mr. Skinner if he has ever noticed the effect of oil on mica, in the way of weakening its resistance to puncture. I once made some tests in this line and found that a sheet of mica which would resist a pressure of 10,000 volts for an unlimited time in air would break down almost instantly under oil at a pressure of 4,000 or 5,000 volts.

MR. SKINNER:—I have not made tests under oil, but I have done so with a film of oil on the surface of mica and found that to be the result.

MR. W. S. ANDREWS (*Communicated after adjournment*):—The following tests were made with alternating current delivered at 118 volts, 60 cycles. The voltage was raised by a small step-up transformer with necessary regulating devices, and measured by a G. E. static voltmeter reading up to 10,000 volts. A good quality of clear white India mica was used, a dry sheet of which, .002" thick, would stand a pressure of 8,000 volts for an indefinite time in air.

The accompanying sketch (Fig. 1) shows the general arrangement of the testing apparatus. A and B are two brass rods supported by a piece of hard rubber, c, and bent towards each other at right angles at their lower ends. One of these rods terminates in a brass disk about $\frac{3}{8}$ " diameter and the other rod is finished with a rounded point touching in the center of this disk. This rounded point rests against the disk with sufficient pressure to

FIG. 1.

support the piece of mica, D, that is to be tested. These parts are attached to a stand so that the holder with mica may be lowered into a vessel, E, containing oil. Preliminary experiments were made with various kinds of oil, but no difference in results could be observed, so it was decided to use a good quality of transil oil for the present tests.

The figures in the following table show the average puncturing voltage derived from a number of tests made on each thickness of mica used.

THICKNESS OF MICA.	AVERAGE PUNCTURING VOLTAGE.
.001"	3,800
.0015"	4,500
.002"	4,600
.0025"	4,750
.003"	5,300
.004"	5,570
.00475"	5,950
.005"	6,050
.006"	6,700
.0065"	6,930
.007"	7,220
.0075"	7,400
.008"	7,700
.0085"	8,550
.01"	8,900

It is not necessary to place the mica under the oil in order to reduce its puncturing resistance. If a piece of mica, say .002" thick, is supported in the holder shown in sketch, the holder and mica being quite free from oil, the pressure may be raised to about 8,000 volts, and, if the mica is of good quality, it will stand the strain indefinitely, but if a drop of oil is applied to the blunt metal point, the mica will be instantly punctured.

It is curious that oil does not seem to produce any weakening effect on artificial insulators, such as paper coated with boiled linseed oil, copal varnish, etc. The results of many tests show that the resistance to puncture of these materials remains about the same, whether tested in air or under oil.

DR. PERRINE:—Mr. Thomas's paper is very interesting to me. In some of the installations with which I am familiar, there have been installed an enormous number of gaps, and they have been found to fail utterly when certain conditions of discharge were produced. This is a decided step in advance that Mr. Thomas has made. It seems to me there is something still to be done in that direction. Mr. Thomas depends on rupturing the current, if I understand the theory of the paper, by the introduction of the shunt resistance in addition to the series resistance, and that means that the shunt and series resistance combined must be comparable to the total resistance of the circuit. It is possible to do that, but that amount of resistance may run up to a very large amount in many cases where we have in series with the generator a long-distance line in which the resistance has become considerable. Then there is another point that seems to me not to be covered by this particular device, and that is the fact of the change of frequency which occurs when the lightning arresters discharge, and the line is short-circuited through the lightning arresters. For power-house conditions I should be inclined to think that Mr. Thomas's plan would be applicable, because there

we have simply the inductance of the machine to take care of, and the resistance if made high enough will take care of every form of the charge, but when we put the same apparatus at the end of a long line or end of a heavy cable, we get entirely changed conditions. Mr. Thomas's paper is one of the most difficult to raise any points of objection to, because it is an experimental paper, and a man who has performed the experiments knows what his apparatus will do. At the same time I have had a recent remarkable instance of what I suppose is resonance in a cable line going into a substation where there was a break in the wire in the cable, opening only after high currents began to pass. The cable was closed, and moderate currents flowed. When large currents began to flow there was an arc produced within the cable,

FIG. 2.

and the effect of that was to let such currents through, that the fuses were blown out and fuse blocks destroyed, and switchboards blown out, and an appearance of short-circuited currents entirely beyond the capacity of the machine to produce. If the self-induction of the machine had not been counteracted by the capacity of the cable that is producing this resonant condition, by reason of the fact that the arc was of an immensely higher rate of alternations than the normal rate of alternations of the machine, allowing currents to flow by, putting the line in the condition of being for those frequencies practically distortionless. I should think the same effect would be found in producing an arc between the gaps of an arrester at the terminal of a long line, or where we had long cables. This can hardly be overcome by any ordinary value of non-inductive resistance, and it might be

necessary to carry on this work a step further, using possibly a number of selective resistances, some of which would stop the normal rate of frequency, and others would be resistances when the frequencies were very much increased. That is a condition that we are likely to meet in all lines where we have large capacity in series with generating apparatus or other apparatus where the inductances are large; and whether the arc is of true alternating character in the small gaps of a lightning arrester, I don't know; but if it is of that nature, we undoubtedly would have under certain circumstances the induction of the circuit; and I would be inclined to think that in such circumstances these arresters will not act, and until this system has been thoroughly applied to such cases, I think it will be impossible to say that the result that Mr. Thomas has reached is the complete solution.

Regarding Mr. Skinner's paper, I notice in the curves on Figs. 2 and 4 a remarkable resemblance to similar curves that have been derived from high-potential tests of long cables, and particularly with the curves that are derived in working with gutta-percha at varying temperatures. The energy loss at varying voltage and varying temperature seems to be, from the appearance of the curves, almost the same as the character of curve which is obtained with gutta-percha. Now the work with cables has also shown the correctness of Mr. Skinner's observations, that a slight amount of moisture in the insulation will materially increase the heating, and that heating of this character almost invariably results in final breakdown; and the fact that the heating is greater than the $C^2 R$ loss, has also been observed in cables, and my idea has always been that that is due to electrolytic conduction. In experiments with cables that are at all damp, with a direct current, I have invariably found that where a cable was subjected to high-potential direct-current strain, and then disconnected, that cable acting as a storage battery would furnish a current for a very considerable length of time. In cable working I found many instances similar to that, and invariably where I found the temperature of the insulation rising beyond what it should attain, due to the strain from the potential applied, that it was due to the presence of moisture, that there was a distinct appearance of electrolytic conduction through the cable, heating it more than would be reasonable to suppose, and ultimately breaking it down. Mr. Newbury, the electrician of the Roebings, has also found a number of interesting things in connection with thoroughly dry cables. His experiments seem to indicate that with fibre cables you can increase the dielectric strength of the cable by increasing the thickness of the insulation, up to a certain critical e.m.f.; beyond that point the cable seems to break down at almost the same potential, irrespective of increase in thickness; and in discussion with him I came to the conclusion that it was impossible to so thoroughly saturate the fibre cable that there are no vacuous spaces in the insulation, and in certain cases those vacuous spaces distinctly acted as a conductor,

and beyond those potentials it was not possible to use that type of insulation. The same thing we generally found in breaking down rubber cables; that in nearly all instances where the rubber cable was broken down below the normal e.m.f., which should break the dielectric, that there were vacuous spaces within the rubber, not necessarily any moisture present, but simply rarefied gas. Through vulcanization of the cable the air would be heated, a small bubble of air would increase to a large one, the rubber became vulcanized, and would not contract when the temperature was relieved, and that would make a vacuous space which would invariably locate the breakdown of the cable at and below the normal dielectric strength represented by the thickness of the insulation and character of the material; and it seems to me that this work of Mr. Skinner's has shown very clearly that for perfect insulations we can gather a great deal of valuable information by comparing the work that has been done by the cable engineers in the very careful study which they have made of gutta-percha. Vulcanized rubber has never been studied so carefully, at any rate studies have not been published, because vulcanized rubber is such an indefinite material; but gutta-percha is a perfectly definite material, has been carefully studied, and seems to bring out an entire confirmation of the class of results that Mr. Skinner has brought before us.

PROF. THOMSON:—I am inclined to agree with Dr. Perrine with regard to the necessity for carrying out tests concerning lightning protection under practical conditions; that is, we must gain our experience not so much in the laboratory with limited appliances, but with our lines. We must look for damage on or from the actual lines, see what caused it, and then attempt to find the remedy. We can, of course, reproduce to a certain extent the conditions by laboratory operations, but when we have a long line with capacity and inductance to take into account, and high potentials in operation, the conditions seem to change the results very materially. In regard to the point which Dr. Perrine drew attention to, the insulation breaking down notwithstanding the increase of thickness, I think there exists a very simple explanation for that. Given the diameter of the wire of the cable, a certain thickness of dielectric surrounding the same, it is manifest that the stress is a radial stress, and most highly concentrated around the wire, and so an increase of thickness may not change the radial stress particularly. It is akin to the fact that with a certain diameter of wire in the air under electrification up to a certain point, the air breaks down near the wire. There is a blue discharge over the wire in the air at certain potentials. The only remedy is to increase the diameter of your metallic portion so as to reduce the gradient of stress, reduce the steepness of the gradient at the wire surface.

DR. PERRINE:—I do not think that explains it altogether, for the reason that experiments were made with wires not smaller than $\frac{1}{4}$ " in diameter. While that is altogether true, it does not

seem we are getting up to the point or condition with a $\frac{1}{4}$ " wire and 10,000 volts strain.

PROF. THOMSON:—I agree that it undoubtedly is provoked by some lack of homogeneous character in the dielectric; but my point is you do not gain such a great deal by the addition to thickness; that the stresses remain high on those weak points. In the same way, the mica plate being broken down when covered with oil or an oil film, the tendency there is, the oil being thick and thin in spots, to produce again the radial stress upon the thin portion—the concentration of stress will be at the portion where there is no oil, or at least where the oil may be the thinnest, and there is protection of the surrounding area by the layer of oil.

DR. PERRINE:—That is invariably true with any compound dielectric.

MR. THOMSON:—Yes, it is undoubtedly.

MR. MAILLOUX:—It was pointed out in a paper read before the Institution of Electrical Engineers in London by Mr. O'Gorman, that sometimes a piece of very highly insulating material placed between two conductors subjected to a high e.m.f. would, instead of increasing the resistance to puncture, actually decrease it. The explanation given in that paper is that a piece of glass, for instance, while it is a high insulator, yet is really a lower insulator than dry air, and consequently it tends to really increase the difference of potential across the actual air gap remaining; in other words, if there be no glass, we have a certain fall of potential between the two points, but if we put a piece of glass there, which really acts as a high resistance conductor, the effect is the same as if the actual difference of potential were increased. This may perhaps have some relation to the question here discussed.

There is another point upon which I would like to have some information. I had occasion about five years ago to make some experimental tests in a rough way of a certain insulating material. The experiments were conducted at a laboratory in Vienna by Professor E. Grau and myself. The material was a sort of resinous compound which became soft at a certain temperature. This material was placed in thin sheets between two discharge points, connected with a Holtz machine, using a by-pass spark gap of lower potential in the well-known way, so as to control the voltage applied to the sheet of material at the point to be tested. Although the temperature at which this material resisted puncture was quite high, it was observed that in every case before the puncture took place there seemed to have been a thinning action, as if the material was mechanically brushed off radially from the point of greatest stress, and pushed away on each side, the action being quite similar to the displacement or the flow of metal by the action of a die—or other perforating instrument—when we try to perforate a piece of lead with an awl for instance, when the metal flows or is displaced sidewise. There seems to have occurred an action quite similar and comparable to this; and the

conclusion which Professor Grau and I came to, at the time, was that the puncture resulted not only from the increase of temperature, and the consequent actual decrease of the dielectric strength, but also from the actual thinning of the plate. In some cases a thinning in a plate of three or four millimetres thickness occurred to the amount of as much as half a millimetre before the puncture took place. I would like to know whether any similar effects have ever been observed in any other materials which are not viscous, at such low temperature. I think that in any homogeneous substance which becomes softer as the temperature increases, we should have an analogous phenomenon. In this case, the phenomenon seemed to be more of a mechanical character, than of a chemical, electrolytic or other similar nature.

MR. THOMAS:—I would like to make one point in regard to Mr. Skinner's paper. His conclusions are specially valuable as they represent the results of a number of years of practical commercial work, and of a great many tests made under a great many different conditions, a great many different times, by different people and different methods, and the conclusions from all those tests are summed up and brought into a general law. There has been a great deal published in a fragmentary way on this subject by men who have tested a few samples, may be a good many samples of one particular object; and taken all together, something can be derived from these results; but they are not to be compared with the results which are obtained by Mr. Skinner by years of experimenting on practical materials, under working conditions. I would like to bring out one point relating to the continuation of Professor Thomson's discussion in regard to the breakdown of these cables. I think it was Mr. O'Gorman in a recent paper before the British Institution who stated that a cable can be made with materials of different conductive capacity placed on different circles around the core and in this way the strain can be made equal on the outer portion or the inner portion.

MR. WM. MAVER, JR.:—In regard to the point raised by Dr. Perrine, that the pressure resisting quality of the cable does not continue to increase with the thickness of the insulating material, I have heard the explanation offered that it may be due to the fact that, as we all know—I presume the Doctor has referred to oil saturated cables—it is necessary in these fibrous cables to expel the moisture very thoroughly, and to do that the cables are dried in a heating oven, and it has been observed that when the insulation exceeds a certain thickness the amount of heat necessary to drive out the moisture is so great or so long continued that it has a detrimental effect upon the outer layers of the insulating material; and on the other hand, when the cables are then plunged into the insulating compound, the thickness of the material does not permit the oil to permeate the entire mass of the fibrous material, and therefore its dielectric strength is not

so great as might be expected in proportion to the thickness of the insulating wall.

PROF. P. B. WOODWORTH:—I have found when the dielectric strength was very low, a transformer placed in circuit would break down. I tried to account for this condition, by using commercial oil where I obtained a dielectric strength of about 16, put it into a transformer and found a dielectric strength of 2. I carried on quite a series of investigations, tried to find out why the oil dropped from 16 to 2, and for a long time could find no clue to it. I came to the conclusion that there was enough water in the transformer at the time the oil was put in, to furnish sufficient moisture to diminish the dielectric strength. I tried some experiments, and found all I needed was something less than 9/100 of 1 per cent. of water in the transformer oil to change the dielectric strength from 16 to 2. The oil used in the transformer was regular commercial oil. I then carried on some investigations to ascertain where the water came from. I made some calculations as to the amount of moisture there was in the air at the place, and the amount of change in the temperature that would be necessary to form a deposit in the transformer sufficient to supply all the water required. I found a change of temperature of about 10°C. was all that was necessary to form the film on the plates in the transformer, sufficient to change the dielectric strength. It seemed to me, when the transformer is sent out to be filled with oil, great care should be taken that the transformer is warmed up before the oil is sealed in; the immense surface permits a deposit of moisture.

PRESIDENT STEINMETZ:—I can corroborate the statement that the disruptive strength of oil is very largely affected by a trace of water. That is one of the most important matters where oil is used for high-grade insulation. If there is no further discussion, I call upon Mr. Thomas to conclude his paper.

MR. THOMAS:—I have only a few words to say, though the subject might be discussed almost indefinitely. It is very true, as President Steinmetz, Professor Thomson and Dr. Perrine stated, that experiments with trial plants of large capacity is the only certain way of determining the full effect of any such apparatus. It may turn out there is a difference on whichever end of the line the arresters are placed. In any case, however, we are to look for favorable results as regards oscillations from the use of resistance, for though the resistance may not always be sufficiently great to overcome all tendency of a circuit to oscillate, yet it is in the right direction and at least as regards resonance phenomena, we are better off with it than without it. I feel ~~sure~~, however, that our troubles will not be increased even on the largest lines and highest voltages, by the presence of the arresters, provided they are properly designed.

In regard to one of the points our President brought out I will say that I should have made it clear myself that we have as a result of using the series and shunt resistances, double the num-

ber of gaps to suppress the generator arc. This principle can theoretically be carried further; if we have three times the number of gaps necessary to withhold normal voltage, the second part shunted by a high resistance and the third by a low resistance, we can without increasing the voltage necessary to cause discharge, have three times as many gaps effective as with plain series resistance, and so on. Practically, however, such an arrangement involves rather too much complication.

The discussion on Mr. Skinner's paper suggests a point that I wish to bring out. Very possibly the energy loss in commercial insulating material is due to local high-resistance conduction within the material. If this is true, we should not expect the law that a single cycle has always the same loss of energy in it to hold strictly, independent of frequency. For if we assume local high resistance conductors in the midst of the material, we can analyze the phenomena in this manner; if the local circuits were perfect conductors, they would have the effect of reducing the thickness of the dielectric; if perfect insulators, they would have no effect at all, but as they are somewhere between perfect conductors and perfect insulators, their effect will be intermediate. When the frequency is very low, there will be ample time for all necessary charge to flow through the high local resistances so that it acts nearly as a perfect conductor. If, however, the alternations are very rapid, and a high internal resistance will not allow time for any appreciable charge to flow, then the result would be that local circuits will act nearly as perfect insulators. There will be a certain $C^2 R$ loss in either case, and a difference in the effective thickness of material, so that frequency will evidently have a very decided effect on the energy loss per cycle. Experimentally it is found that differences in temperature, differences in frequency, differences in amount of moisture, etc., affect very materially the loss per cycle.

It is very desirable, of course, that we learn how to build dielectric material perfectly homogeneous and in the very best possible condition electrically, but we must always bear in mind the fact that even though this ideal is attained, it will be very difficult to maintain; the material must be carefully protected, so that no moisture or other harmful matter can be absorbed. The difficulty of maintaining the ideal condition in commercial practice may prevent our making full use of a perfect dielectric, even if we are enabled to produce one.

MR. SKINNER:—I have but a word to say further about Mr. Walker's paper, and that is this, that the author considers, I believe, the chief points in the paper the simple method he has given for calculating the torque on the needle and the elimination of the A^2 term in the formula.

In regard to my own paper, I may say that while the investigation of a theoretically perfect insulator is of very great interest, it has been of infinitely more importance to us to take the insulating materials that we are compelled to use in actual practice, and to

determine their characteristics, and to find out just how they would act under normal operating conditions. In regard to the point raised by Mr. Mailloux, I may say that I have frequently noted an actual mechanical rupture, principally in connection with some tests of glass under oil. In some instances I found actual chipping of the glass to such an extent that pieces as large as pin heads were scattered over the surface of the glass and a groove was cut, in one instance, half way through a piece of glass which formed one of three or four placed together. This was undoubtedly due to mechanical action.

In regard to the question of water in oil, I may say that this may be detected either by measuring the actual energy loss in the oil or by measuring the dielectric strength. We have found that heating almost any commercial oil increases its dielectric strength. This results until the oil is heated almost to the charring point.

PRESIDENT STEINMETZ:—Gentlemen, before proceeding to the reading of the further papers on to-day's progress I call upon Dr. Kennelly, the Secretary of the Committee on Standardization to submit to us the report of this Committee.

DR. KENNELLY:—The report submitted by this Committee, of which Prof. Crocker is Chairman, differs from the report which was adopted by the INSTITUTE in 1899, mainly in the direction of different paragraphs. A few changes have been made in the report as it was submitted originally, but the principal points of interest are the additions which have been made. I will just run over the report briefly and point out the salient features of addition. A few definitions have been added on the second page.