DISCUSSION ON "THE TESTING OF TRANSFORMER STEEL."
FRONTENAC, N. Y., JUNE 28, 1909

L. T. Robinson: As the subject of iron testing has been of great interest to me for a number of years I am prompted by Dr. Lloyd's paper to ask what sort of method is this that he presents? Is it to be a standard method? Is it to be a workshop method for ordinary use, or will it necessitate the sending of samples of iron to a testing laboratory? I do not know whether there is a similarity between the methods used or not, but I believe that all users of iron have methods for testing it that have proved satisfactory. By means of one method, at any rate, with which I am quite familiar we can make an apparently satisfactory test at a labor cost of something less than 5 cents per sample.

With the advent of German activity in iron, etc., it becomes necessary to know what the results obtained in Germany mean, and we must, therefore, interpret our results by means of the Epstein apparatus. From my point of view I think it exceedingly unfortunate at this time, so long after the present methods have become well established and familiar to those most interested, that we have to consider the adoption, or the use, or the interpretation of results obtained by still another method.

Will Dr. Lloyd explain why the Epstein method is not perfectly satisfactory as a commercial standard method, and why some modification of that method, which may mean an appreciably different result, should be introduced. I believe the objections to the lack of uniform distribution of the flux in the sample are fairly well met, by the introduction, in accordance with the German specifications, of the cardboard pieces in the corners.

As to his method being accurate within one per cent, I would say that we have always been accustomed to refer our tests to ballistic measurements and from them to judge whether or not any method is a good one. Now, a question arises, is it the intention to substitute Dr. Lloyd's method for the standard method of employing a ballistic galvanometer, or does the one per cent mean that ballistic measurements have been made on these identical samples, and that the results obtained by the two methods agree within one per cent?

It seems to me that before we adopt a new method it should be demonstrated on the basis of what we have always considered to be the standard method of test, that the old method is not good and that the new one is good. Or if the new method can not be so compared, something definite should be said to convince us that what we have always considered the absolute method, namely, suitable manipulation with ballistic galvanometer, is not correct and should not be used.

V. Karapetoff: Referring to the sketch, I see the advantage over the Epstein method of assembling the strips

on edge; but is not there a disadvantage in using separate corner pieces, making two joints instead of one joint as in the Epstein method? Could not a better joint be obtained by cutting the strips longer and bending them over the corner? This would do away with one of the joints, and, moreover, the joint would be made of the same material, instead of being made of a piece of iron that does not belong to the same sample.

I would also like to ask Dr. Lloyd with reference to Table XI, how he separated the hysteresis loss from the eddy-current losses? Did he assume any exponents beforehand, such as 2 for eddy currents and 1.6 for hysteresis, or did he determine the exponents experimentally from tests at different frequencies and different flux densities? As far as I am aware the values of the eddy-current loss and of the hysteresis loss are different, according to whether we assume the exponents beforehand, or determine them from the tests on the samples themselves.

C. E. Skinner: These tests have a degree of refinement rarely attempted in a commercial way, yet such refinement is necessary for the exact study of materials of this class.

After about two and one-half years' use of what is essentially the Epstein method we have found it to be the most satisfactory of any that we have tried. The insertion of bent corner pieces, as described in the paper, seems to me to be unnecessary in commercial work. The added accuracy of the corner-piece method when all necessary corrections are made, is unquestionable I think, but for commercial work the accuracy with the butt joints seems to be all that is really necessary, if inductions are kept relatively low. For inductions above 10,000 the joints may be lapped as in transformer work. We use a sample three or four times as large as that used by the authors of the paper, and the sample is of such width that material, if of satisfactory quality, can be used in commercial apparatus later, so that there is little or no actual loss of material due to the relatively large sample. While the strip is not as wide as that used by the authors we can detect little or no error from shearing provided the shear is kept sharp.

The accuracy claimed for the tests by the authors is within one per cent. I do not know the accuracy of our own method, but many checks lead me to believe it is within two per cent. On account of the ease and rapidity with which samples can be built and tested by our method I feel that it is to be preferred for commercial work where a large number of samples must be tested each day.

J. C. Lincoln: The first four samples *A*, *B*, *C*, and *D* have iron loss per pound given, and the annealed samples, which follow, have a very much decreased iron loss, sometimes as much as 50 or 60 per cent. I wish to inquire as to the method of annealing which gives this result.

Clayton H. Sharp: Is the wattmeter used in this test one of the ordinary commercial type, or is it something else? Is

any special provision made to maintain the temperature of the iron constant, or to determine the temperature of the iron, and how is it done?

Andrew Pinkerton: In testing iron as it is received we usually want the result of a large part of one or more whole sheets and not of a small sample, as the user of the material is interested in the results from whole sheets and not from small samples. That is my main objection to Dr. Lloyd's method.

Clayton H. Sharp: Regarding a remark of the latest speaker, I would say that Dr. Epstein once remarked to me that he had found that any test of iron made on a small sample had no value whatever.

E. E. F. Creighton: Some years ago we made tests of samples of iron cut from large sheets, and found a great difference in the result, depending on what part of the sheet the sample was cut from. The gases in the furnace have a great effect on the annealing of the iron. I have no doubt also that small differences in the temperature affect the results.

M. G. Lloyd: In regard to Mr. Robinson's question of the method itself and the comparison with other methods: the method that we have developed, it is true, is not quite as rapid as some of the workshop methods already in use, and it is not intended that it will replace them. For factory purposes it is often sufficient to use a method which is perhaps not so accurate, but which will serve when simply comparative results are desired, to determine the uniformity of material, or something of that kind.

Regarding the results obtained, however, I wish to say that our aim was to develop a method that would give as high an accuracy as possible; in other words, to get what we might call absolute values, constants that would apply to the material, and would not depend upon any peculiarities of the method; for that reason we wanted wide strips and uniform flux densities, and some of the other conditions mentioned. It is true that the method described is very similar to the Epstein method, and yet it differs from it materially. At the Reichsanstalt in Germany the Epstein method was compared with several others by what was considered an absolute method, and it was found that on the whole the Epstein method gave very satisfactory results for the total loss at 50 cycles and 10,000 gausses, but that the separate losses due to hysteresis and eddy currents individually were not so close, that the one came high and the other low by three per cent, and that they very nearly balanced up in the total loss. Consequently, in making separations of that kind, as is desirable for many purposes, it is not safe to use a method which is so much in error. It is true the separation cannot be made with nearly the accuracy of the individual observations, because the result at one frequency is often in error in one direction and in the case of a second frequency it may be in the opposite direction, and the errors multiply in making the separation.

It probably would be desirable for the Institute to adopt some definite method which could be universally used and accepted by manufacturers and users of steel, in order that when tests are made they shall be under the same definite conditions, and consequently not lead to any disagreement as to quality or interpretation of results. I do not see any harm in the meantime, however, in developing in practice any number of methods, in order to have the best possible selection when any attempt is made to adopt some definite method.

As regards the comparison with the ballistic method, I do not think we can expect any alternating-current method to give results which will be the same as those given by a ballistic method. If we consider the different methods of taking ballistic curves—such as that known as the step-by-step method, by which we get a ballistic reading for each slight increment in flux, and a ballistic method in which a large change is made in flux from the tip of the hysteresis loop to another point—it is found that the two ballistic methods will not agree between themselves; discordant results are obtained according to the process adopted in carrying out the measurements.

Similarly with alternating-current measurements, it is still an open question whether frequency affects the value of hysteresis, if everything else remains the same. Some investigations in the past have appeared to indicate large differences, as much as 30 per cent, in the value of hysteresis, due to different frequencies. When differences in conditions produce such results, and iron is put through a hysteresis loop with alternating current, it is not fair to take any ballistic measurements and say that because the alternating-current measurement did not agree with them, that the alternating-current measurement is in error. I do not think the ballistic measurement is any criterion of the accuracy of the measurements with alternating magnetization.

The error in our method has been determined simply through a consideration of the separate sources of error and the accuracy of the instruments, and by a comparison of measurements under slight variation of conditions. When I state the accuracy as one per cent, I consider that the results, before the separation of losses, are to be depended on, that is, the constants of the material are known to that degree of accuracy.

Professor Karapetoff's comments on the joints in the corners are partly answered in the paper itself, in the part which I did not take time to dwell upon. Ordinarily, the corner pieces are of the same material as the test pieces, but we have found it is not necessary that they should be. One can get sufficiently good results for all commercial purposes by using stock corner pieces, instead of making a set of corner pieces for each specimen, and for that reason, in practice, we use them very often in that way.

The objection to the second joint does not seem to be justified

by the results, because we find the difference in flux between the center and the end of the test piece to be very small indeed, and consequently any objection to the two joints at the corner is not substantiated.

The exponents have not been assumed in any case, as was intimated. The exponents are determined by obtaining values for hysteresis or eddy currents at two different flux densities, consequently derived from entirely independent measurements. The values of these two losses at any one flux density are determined by runs at two frequencies. Two observations at one flux density determine the separation of hysteresis and eddy-current losses. Then the values taken at two different densities are sufficient to determine the exponent which applies to these two particular flux densities.

In regard to the question on annealing, I may say that we have not done much of that. The specimens we have tested have in almost all cases been tested in the condition in which we received them, and the question of annealing was one of past history with us.

Dr. Sharp's question in regard to wattmeter and temperature conditions is answered in full in the paper.

As to the quantity of material, discussed by several, that is a point on which every person, I take it, must have his own opinion. Some think that the results of tests on samples are not of any value, and the test must be made on the entire material under question, and others seem to have found it satisfactory to use small test specimens. My own opinion is, that once a sufficient quantity is selected to represent a fair average of the material, if the method demands more beyond that, it is an undesirable feature of the method. The Epstein method, as usually employed, requires 10 kg. or 22 lb. of iron. What I take to be slightly better results are obtained with as small a quantity as 4 lb., and I think that is an advantage. Of course, if a person wants to test a whole sheet or larger quantities, it is easy to adapt the apparatus to the larger quantity. The question has been to find the apparatus which can be adapted to smaller quantities. With a method available for small quantities, one can always adapt it to one's own needs for large quantities.

I think I may be permitted at this time to call attention to the magnetization curves in Fig. 5. showing the wattless component of exciting current plotted against flux density for silicon-steel and ordinary steel. At low flux densities the silicon-steel is more highly permeable than the other, whereas at higher flux densities it is less so.

In regard to the effect of the higher magnetizing current, I wish to say one thing. It is of course undesirable for some reasons to allow the exciting current to run too high, and that has limited the design of transformers with this silicon-steel in one direction, that of pushing the flux density too high. It has been

very generally thought, however, that the regulation of the transformer would be affected by a high magnetizing current. I wish to speak of that, because it is an error which is commonly made. Most of the formulas which we see in print for the regulation of the transformer involve magnetizing current. As a matter of fact, magnetizing current has nothing whatever to do with regulation, and any objections to high magnetizing current in transformers are not along the line of inferior regulation. The reason for that is, there is a certain voltage drop in the transformer, due to magnetizing current, but it comes in both at no load and at full load, and it can be easily shown that it does not affect the regulation.