

DISCUSSION ON "EFFECT OF TEMPERATURE ON THE HYSTERESIS LOSS ON SHEET STEEL", "COMMERCIAL TESTING OF SHEET IRON FOR HYSTERESIS", NEW YORK, APRIL 14, 1911.

**C. H. Sharp:** It may not be amiss to consider very briefly some of the improvements in method and apparatus for magnetic testing and also some of the difficulties which are met with in this class of work.

Testing by the ballistic galvanometer method has been greatly simplified by the use of a long period D'Arsonval galvanometer as ballistic instrument in place of the old undamped moving needle galvanometer which made such a record for itself as a troublesome and time-wasting instrument. The D'Arsonval galvanometer is given any required degree of damping so that it may be used without a loss of time in bringing it to rest. It is calibrated by reference to a standard of mutual induction and its constant is expressed no longer as a ballistic constant, but rather in terms of line of force cut per centimeter throw. The long period is required not so much to enable the throws to be read with accuracy as to obviate the effects of magnetic viscosity in the specimen.

The errors involved in the use of yokes in testing straight specimens have been taken account of and yokes with compensating windings which carry sufficient current to overcome the magnetic reluctance of the yokes, joints, etc., have been devised and put into practical use.

The Picou permeameter, an instrument designed on this principle, has been found in our work to have a very limited range of usefulness and not to be a successful practical instrument. The investigations of the Bureau of Standards along these lines have been most thorough and painstaking and have resulted in the development at the Bureau of Standards of practical forms of compensated yokes wherewith really accurate permeability and hysteresis tests can be made with a reasonable expenditure of time.

Among instruments intended for giving a relatively quick determination of permeability curves and hysteresis loops, by substituting a more convenient device in place of the ballistic galvanometer, the Koepsel instrument has probably a pre-dominate place. The compensation of the yoke reluctance in this instrument, however, is very imperfect and the results obtained for it require so much correction to take account of the unknown reluctance of the yoke, and of the joints, that it can hardly be considered as giving very accurate results in any work except in the comparison of samples of similar material.

Practically, not only the hysteresis loss but also the eddy current loss should be known. This fact leads directly to the wattmeter method of testing samples. If the iron is tested at the same induction with two different frequencies of the

alternating current or if it is tested with the same frequency but at two different values of the induction, the eddy current and hysteresis losses can be separated from each other. There is no practical difficulty in measuring the losses in even quite a small sample of iron if the proper instruments are available. The wattmeter used must be one which is accurate on low power factors. If the sample is quite a small one, some special sensitive wattmeter is required such as for instance, the suspended type, using a mirror and spot of light for reading. It is advisable to use a separate winding for the potential measurements as Professor MacLaren has done. Sometimes a third winding is put on which is connected to an electrostatic voltmeter. Under these conditions the electrostatic voltmeter indicates the total e.m.f. set up in the winding, and consequently the maximum induction; that is no correction is necessary for fall of potential in the windings, due to the voltmeter current. The temperature of the windings and of the sample should be accurately controlled and the temperature at which the losses are measured should be stated. This is very seldom done, but in as much as the eddy current loss must have the same temperature coefficient as the resistance temperature coefficient of the iron; namely about 0.5 per cent per degree, the necessity for stating the temperature is obvious. Inasmuch as the sample may tend to rise considerably in temperature during a test, it has been found in our work advisable, at any rate when working with small samples, to immerse the entire arrangement in an oil bath. Furthermore, if the temperature of the oil bath is kept at some definite value, as for instance 50 deg. cent. the losses are measured at a temperature comparable with the temperature which the iron will actually have in use. It would be advisable therefore that a standard temperature at which these tests are to be made should be agreed upon.

\*The test should be made with a sine wave form. This is required because the losses vary with the wave form. It is not sufficient simply to assume from the fact that the wave form of the generator is practically sinusoidal, that the form factor, which enters directly into the computation of the maximum induction produced in the iron, is exactly 1.11. The actual form factor of the wave of e.m.f. set up in the iron should be investigated and its actual value determined. Any error in the determination of the form factor affects the value computed for the maximum induction by the same percentage, and consequently affects the value for the losses at a given induction by a considerably larger percentage. The form factor may be determined either from an actual plot of the wave, or by rectifying the wave by means of a commutator attached to the shaft of a synchronous motor, and comparing the value of the rectified e.m.f. as indicated by a permanent magnet voltmeter with the value of the alternating e.m.f. as measured by a dynamometer type of voltmeter containing no iron. In this arrangement it is not always easy to be sure

of perfect contact between the commutator and the brush, so that a good deal of care needs to be used, to avoid error from this cause.

The pieces from which the magnetic circuit is built up need to be carefully insulated from each other. The exact method of building a magnetic circuit does not seem to be a matter of great importance. In the Bureaus of Standards apparatus the joints at the corners of the magnetic square are more perfect than in the Epstein apparatus so commonly used in Germany. The result of this is that the wattmeter is used at a better power factor. In the Möllinger apparatus the samples are continuous, ring-shaped stampings and the windings are quickly put on by a special arrangement. This gives a more perfect magnetic circuit than either of the other forms, but requires a less economical form of stamping and gives practically the same results as the Epstein apparatus. Wild, in a recent paper before the Institution of Electrical Engineers, reported finding several per cent higher loss in the Bureau of Standards' style of magnetic square than in the Epstein style. He attributes this to the increased hysteresis in the corner pieces due to bending, and considers the results of the Epstein apparatus to represent more nearly the true values.

The question of the sample seems to be the most difficult one in connection with the wattmeter method of testing. In the Epstein apparatus 10 kilograms of iron are used. The quantity is so considerable and the individual pieces are of such size that the resultant sample can scarcely fail with proper selection, to be representative of the iron. Where smaller samples are used, the doubt increases as to the representative quality of the sample, and this seems to be the chief difficulty in the use of small samples for the wattmeter test.

Mr. Robinson's method presents the wattmeter test reduced to the greatest possible simplicity. The only question about it seems to be that of the size of the sample and in as much as Mr. Robinson has found it to give satisfactory results in the immense amount of work which he has done with it, and the results of which have been of the very greatest commercial importance, it is hardly open to doubt that as a workshop method his is a most excellent one.

**Edwin F. Northrup:** I have been interested in Professor MacLaren's paper on methods of hysteresis testing because of the importance of the subject, and also, for what the paper tells and suggests.

While I do not feel as competent to discuss methods of iron measurement as I would methods of electrical measurement, nevertheless I have some thoughts regarding the subject of the paper which may be worth expressing.

In addition to the losses in a transmission system which result from depreciation of the equipment there are two sources of loss which have received a searching study, namely, the

copper losses on the line and the iron losses in the generators and transformers. The control of the copper losses has become so good that it is not unreasonable to specify conductivities for the copper, or aluminum conductors, used to within a fraction of a per cent, and conductivity measurements can be made by a single observer with great rapidity to within a quarter of one per cent. Yet, without data at hand to confirm the suggestion, I should suppose that it is costing thousands of dollars to overcome unnecessary hysteresis losses, to hundreds of dollars spent in overcoming unnecessary copper losses. Severe specifications respecting the quality of conductor materials, in the present state of engineering development, can be made with much better justification than they can respecting the qualities of transformer irons and steels. This is in part due to the fact that conductivity measurements are so much more easily and precisely made than iron measurements. Any contribution, therefore, to our knowledge of practical methods for making iron measurements is welcome.

Iron measurements have directly in view a commercial end and in devising methods for making measurements one should work from the commercial view point. Laboratory precision, as applied to many electrical measurements, is of relatively small importance in making iron measurements as compared with simplicity, rapidity and the use of few observers.

Professor MacLaren presents us with two methods of testing iron, and with the first of these methods he obtains results which are very interesting. His first method, the wattmeter method is not new but his second method, the direct measurement of hysteresis, is, as far as I know, described in print for the first time. I have had the opportunity of examining the construction of his samples, the apparatus used in his tests, and myself have made a trial of his second method. I am convinced that his work was done with painstaking care and that all his statements are conservative. His construction of a furnace for regulating the temperature of the samples, I think is particularly well adapted to what it has to do.

The curves, giving the relation, between watts lost per kilogram of steel, and degrees centigrade, are interesting in the extreme. It would appear from these curves that a reduction in the hysteresis loss to one fourth its original value, by an elevation of temperature of some 500 deg., is a physical fact and one of much significance. It looks as if the molecules turn about under the directive force of the magnetic field with less friction as they become further separated by elevation of temperature. One should also expect considerable diminution in the eddy-current losses, with elevation of temperature, from the increase in the specific resistance of the iron. Referring to tables I, II, III, of the article we find this not to be true throughout the entire ranges of temperature used. In table I, for any given induction, say 10,000, we have the eddy-current loss, starting at 24 deg.

equal to 0.312 and falling at 390 deg. to 0.166, but then rising again at 535 deg. to 0.246 and continuing to rise at 708 deg. to 0.92. Sample No. 1 was thought to be defective in insulation, but there is no regular decrease beyond 597 deg. even in sample No. II, table II. If both the hysteresis loss and the eddy-current loss in transformers decidedly decreases with elevation of temperature, and if the decrease in loss should prove to exceed the increase in copper losses, and if the elevated temperature does not gradually diminish with time the permeability of the steel, it might actually result that transformers would show a total higher efficiency when run hot than when run cold. It is doubtful if experiment would justify this conclusion, but I suggest it as a research for some one, to make a prolonged test of the relative efficiencies of properly insulated transformers operated at, say 40 deg. temperature and 200 deg. temperature. It is not improbable that the insulation problem could be cared for with asbestos covered wire and some of the new insulating compounds.

The data at the bottom of page 548, in which Professor MacLaren finds that the curves, showing change in induction as the iron passes through the magnetic point, practically overlap, both for an increasing temperature and for a decreasing temperature, have, to my mind, an important physical significance. On many samples of steel tested by me, to determine their heating and cooling curves, I found that in all cases the recalescent point lay some 26 deg. or more below the decalescent point. In one low carbon steel the decalescent point was at 724.4 deg. C. and the recalescent point at 676.4 deg. C., In this case the two points were separated by 48 deg. In the case of a high carbon steel the decalescent point was at 728.4 deg. C. and the recalescent point at 702.4 deg. or a difference of 26 deg. Now, as iron or steel is supposed to lose its magnetism, when it passes through the decalescent point and to regain its magnetism when it passes through the recalescent point, it is quite remarkable that Professor MacLaren should find the induction on heating the sample to be 6010 at 727 deg., and to find the induction on cooling the sample to be 6015 at 726 deg., which is practically the same thing. Perhaps a sample of steel when under the influence of an alternating magnetomotive force will have its recalescent point and decalescent point located at the same temperature. This is worth a trial for the physicists.

Professor MacLaren's second method of the direct measurement of hysteresis has the very great advantages of being applicable to an extremely wide range of induction, of giving the data, for plotting the hysteresis loop, in about ten minutes, and of being, to a certain extent, self checking (as the sum of the deflections for change of induction in one direction must equal the sum of the deflections for an equal change of induction in the opposite direction). It has the disadvantage from a commercial standpoint of requiring from three to four observers. Then,

also, the reduction of the data and the plotting of the curves require considerable time. I believe the method could be made very much more rapid, as well as more accurate, by making it partly automatic.

It would be a simple matter to construct a recorder, whereby the galvanometer deflection would be recorded as a sinuous line upon a cylinder revolving uniformly with the time. The same recorder, or another one, could be made to record synchronously on another revolving cylinder, the change in the magnetizing force. From the two curves so drawn automatically, to a proper scale, the data could be taken for drawing the hysteresis loops. The delicacy of the galvanometer required would be no impediment to such automatic recording, as much more difficult problems, in the automatic recording of temperatures registered with very sensitive galvanometers, have been already solved commercially by myself and others.

It seems to me that progress in iron measurements should follow along the line of seeking greater rapidity in taking the data in methods of rapidly reducing the results, and in making automatic, processes now performed manually. To accomplish these results I can say with a certainty only requires capital and enterprise.

**J. A. Capp:** Mr. Robinson has described two sets of apparatus for measuring the hysteresis loss in sheet iron samples. A brief statement of about ten years' experience with the apparatus, using the bundle of 10 in. x  $\frac{1}{2}$  in. strips, in the commercial testing of sheet iron may be of interest.

The first and most obvious use of the test method was in the inspection of raw material and its testing to determine whether or not it conformed with the standard established in the specifications. Consideration of the many points which have bearing upon the preparation of purchasing specifications soon indicated that such specifications must define the required chemical composition of the steel and its manipulation in the rolling mill. At the time when this work was begun, the rolling mills were generally not equipped to make tests to determine the magnetic quality of their product but they did understand the chemistry of the steel and, of course, had control over mill processes. Experience shows that relatively hard sheets can be punched with greater ease to a given degree of accuracy than softer or thoroughly annealed sheets. It had long been recognized that heat treatment has fully as great an influence upon the quality of the sheets as composition. The use of the relatively hard sheets immediately then requires that the final heat treatment be done by the purchaser and it is obvious that the manufacturer cannot be held accountable for the results of a treatment applied by the purchaser and over which the manufacturer can have no control.

The next, and what has since turned out to be the most important use of this convenient means of commercial testing

was the determining of the best treatment to apply to sheets to put them in a state having the maximum permeability with minimum hysteresis. This work was undertaken by selecting typical lots of steel from the standard commercial output of several manufacturers and from which lots of steel a very large number of 10 in. x  $\frac{1}{2}$  in. strips were cut. These strips well mixed, so as to be broadly representative of the lots from which they were taken, were made up into the one pound samples required in the hysteresis testing outfit. The hysteresis loss and permeability were determined for each of the samples and different samples were then carried through a long series of heat treatments, designed to indicate the influence of the rate of heating, the rate of cooling, the time the maximum temperature was maintained and of all the other variables which necessarily would enter into heat treatment of this character. In this series, thousands of samples were tested and the results when completed, indicated the desirable heat treatment of steels of the compositions and characters included in the lots selected.

This work provided the basis for the preparation of standard specifications which could be used by the purchasing department in their contracts for the sheet steel purchased. These specifications as already indicated, state the required composition of the steel and then define its manipulation by the producer, hence inspection testing substantially resolves itself into a matter of chemistry. But the question of proper mill manipulation still can be determined with reasonable dependence by the application of the prescribed heat treatment to the steel, when if the manipulation has been that specified, the hysteresis loss and permeability should equal at least a certain standard.

The necessity for heat treating the steel purchased at once requires that there be means of determining whether the prescription for heat treatment is actually being carried out in practice. The direct measurement of the temperatures used in the heat treatment is possible at least in certain parts of the furnace by pyrometers. It is obvious that a pyrometer can only measure the temperature of the particular point in the furnace at which it is placed and furthermore it is well understood that the indication of the pyrometer is more or less an average of the conditions surrounding its point of location. These considerations led to the use of hysteresis testing outfit as a check upon furnace work and an elaborate scheme was worked out for the exploration of the annealing furnaces by pyrometers and hysteresis test samples. Large numbers of the test samples were distributed throughout the annealing pots, as many as five or six hundred samples being used in one annealing heat. These samples had been taken from lots of material the history of which was thoroughly known and upon which the influence of varying heat treatment had been extensively studied. Hence the results obtained in the hysteresis outfit upon the test samples which had been distributed throughout the annealing pots

enabled us, with reasonable accuracy, to determine the distribution of temperature in the furnace and pointed to the way to correct inequalities in this distribution, and to establish a routine for the furnace manipulation. This routine, when followed, is expected to yield uniform results, but hysteresis test samples are regularly used as a check upon furnace work, and the heat treatment operations are controlled through the joint use of pyrometers and the hysteresis testing outfit. Samples are placed in the pots at what experience has shown are apt to be the hottest and the coldest parts, and when the test samples show results below normal, the punchings in the particular part of the pot represented by these poor samples may be sent back for re-annealing. Obviously the samples which are used in this way must be taken from stock which previous testing has shown will respond properly to the prescribed heat treatment, though it is but fair to say that almost no selection is required for this purpose, since the specifications provide a grade of steel which can be turned out by the producers with a highly satisfactory degree of regularity. Results are checked by tests upon samples taken from the actual punchings themselves and these confirm closely the results from the regular heat samples.

Still another commercial use may be made of a method of testing such as has been described. There are times when by reason of unusual service conditions, or because of unusually stringent specifications on the part of a customer, apparatus must be turned out which must be classed as special, because the core losses are required to be lower than in standard apparatus of similar design. In such circumstances, it is not always possible for the designing engineer to take care of the special necessities of the case in any other way than by the use of special material. Standard specifications provide material which, after proper heat treatment, may be expected not to exceed a certain maximum hysteresis loss. The average, however, is, of course, considerably below this maximum and there may be found exceptional lots which will run at a very low minimum. By having available a means of testing which requires but small quantities of material and the results from which can be quickly obtained, it is possible to go through stocks which could be used for the production of such special apparatus and select those particular lots which will run at or below the average. Advantage has been taken of this possibility in some of the rare cases where the designing engineers have been forced to require the use of specially selected material.

Mr. Robinson has discussed at some length the question of the general accuracy of the results obtained with the outfit using the 10 in. x  $\frac{1}{2}$  in. specimens. Experience has shown that when a line of machines has been designed using material which has been tested extensively by this method and which machines after completion have been put through the usual tests, the designing engineer is provided with data upon which



he may determine within close limits the core loss of machines of new design. We were at one time able to determine from tests made on the 10 in. x  $\frac{1}{2}$  in. samples that the manufacturing processes of one of the sheet steel mills had gone astray. Had we not been able, at least with a very close approximation to accuracy, to make tests upon a large number of samples so as to determine that it was actually temporarily characteristic of the output of that mill that we could not get the expected results, we would have been forced to work in the dark and probably would not have been able to prevent the use of a good deal of poor material in commercial apparatus. We were able so to impress the mill representatives with the results shown them that they made an investigation only to find rather to their astonishment that a supposed improvement had been made in the process by a man in the mill who had not thought it necessary to bring the change to the notice of his superiors.

When the apparatus was first set up for use, there was some doubt whether readings could constantly be duplicated on the same samples. Therefore, a number of samples representing a wide range of results was tested and laid aside under lock and key. These were taken out at irregular intervals and tested for a period of some two or three years. The greatest variation found was not over one per cent which, of course, is considerably less than the normal variations to be expected in a large number of tests representing one particular lot of material.

The foregoing has shown that a very considerable commercial use is being made of the testing outfit described. This extensive use, however, is made possible only by reason of the speed with which results may be obtained and by the comparatively small weight of material which is required in the test. Mention has been made of the use of the method as a check upon the material received to indicate conformity with specifications, and as a check upon the heat treatment applied in the annealing furnaces, which check is accomplished not only by the testing of the samples which are prepared and placed at predetermined points in the annealing pots but also by tests upon samples punched from the material actually annealed. Tests of the samples are made not only before and after treatment, but also upon a considerable proportion of them after subsequent exposure to moderately elevated temperatures to determine the amount, if any, of increase in hysteresis due to what is commonly called aging. This together with experimental work, requires the testing of an average of about 600 samples per week. The samples are delivered to the laboratory in the form of loose strips 10 in. x  $\frac{1}{2}$  in. These must be assembled into the one pound bundles, the top strip of which is stamped with an identifying number. The bundles are weighed and the necessary data entered in a record book. The samples are then turned over to the furnace men by whom a record is delivered to the laboratory showing the location of the several samples in the various pots. This data is

added in the record book, together with the results of tests before and after heat treatment and aging. Results are then entered on a blank report form which is sent to the parties who must know the detailed results.

Mr. Robinson has briefly referred to the necessity for economy in the use of material where a large number of tests is to be made. This is obvious when it is considered that the test samples are not of suitable size or shape for productive use, and hence may become an item of expense worthy of consideration. Furthermore, room in annealing pots may not be taken up unduly by test samples without reduction of productive capacity, hence the test samples must be small in volume and of such shape that they may readily be placed in the annealing pots without interference with the regular production material. Special shearing devices have been provided for cutting the 10 in. x  $\frac{1}{2}$  in. strips so that the waste of iron is at a minimum. We have stated that there are an average of 600 tests per week made and reported. Some of these tests are repeated on the same sample so that there are actually required about 500 samples or an equivalent number of pounds. Each sample consists of an average of 52 strips of fourteen mil iron. Hence, there are about 26,000 strips of iron handled per week in making up the samples. Obviously, if the sample were larger than that now used, the weight would be increased proportionately as well as the cost of material consumed, and labor of handling.

From all this, there may be formed an idea of the amount of labor involved in the testing and clerical work. Perhaps the best indication of the speed with which the work may be done is the statement that all of this work is carried on by two men who have time left to do the laboratory annealing experiments which are constantly in progress.

**L. W. Chubb:** The magnetic testing of sheet steel had been a very active subject for the last few years, especially during the development of the silicon steels. I have no formal notes on these papers to-night, but some points have come to my attention that I would like to ask some questions about. Is it not true that the use of windings of iron wire, referred to in Professor MacLaren's paper, is a source of error. If, when the temperature gets high, the permeability is very much reduced, excessive exciting current in the iron wire would cause distortions of the induced voltage and the flux in the sample. Such distortions would apparently lower the hysteresis loss and be caused by decreased permeability, even though there would be no decrease in the area of the true loop of the steel. It would seem as though the combined resistance and inductance of the iron winding would cause drops and distortions, resulting in hysteresis errors of several per cent. No data has been given for the high induction points at high temperatures, and I therefore assume that reduction of permeability was a limiting feature. I would like to ask Professor MacLaren if any results

were taken showing the relation between permeability and temperature.

Mr. Robinson's paper treats of testing methods that need to be carried on very extensively in a large commercial factory. In our work we use a single method with a sample which is between the one pound sample and the standard Epstein sample described in the paper.

In certain work it is the custom to use steel without annealing after punching, and the sample consisting of small strips 10 in.  $\times$   $\frac{1}{2}$  in. does not give a representative result for the steel. The variations in a commercial sheet of steel are as great within the sheet as between separate sheets, and therefore the test with the small sample is not satisfactory, not only because of the damage due to cutting small strips, but also because such a sample does not sufficiently cover the area of a commercial sheet. To get a representative one-pound sample from a sheet or lot of steel, it is necessary to resort to some complicated scheme of cutting, which is laborious and wasteful of material.

Our Epstein apparatus uses samples of fifteen pounds of strips  $15\frac{1}{2}$  in. long by  $1\frac{3}{16}$  in. wide. The results obtained with the 15-pound sample check results obtained with the standard Epstein samples. Several details in the method are different. The first I will mention is the use of a secondary winding. In our apparatus we take the potential for the wattmeter coil and for the voltmeter directly over the exciting winding. This includes in the wattmeter reading the  $I^2 R$  loss in the windings. The voltage drop in the winding also tends to reduce the measured core loss.

At first sight it would seem inadvisable to introduce such errors in the testing method, but we have found by very careful ballistic exploration of the Epstein sample that the error due to the leakage at the corners is greater than the resultant error caused by the  $I^2 R$  loss and the  $IR$  drop in the winding. Since resultant copper error and the leakage error are in the opposite direction, and are both functions of the permeability of the steel, they will always cancel the effect of each other to a certain extent, and when the coils are properly proportioned the method will give better results than when a secondary winding is used.

Dr. Sharp brought up the point that the results in Professor MacLaren's paper given by the ballistic method and by the wattmeter method do not agree exactly. Mr. Robinson shows in his paper a rather close agreement between the two. Many researches on the comparison of the wattmeter and the ballistic methods have generally shown the ballistic results to be lower than the hysteresis part of the wattmeter results, which seems to be correct.

When flux grows in a lamination of finite thickness the eddy currents will cause a skin flux, and the induction will not grow with uniform rate throughout the lamination. This will result in a variable maximum induction and increased hysteresis loss.

Work done by Dr. Lloyd shows that only when the laminating of the steel is very fine will there be a close agreement between the hysteresis loop taken ballistically and the hysteresis per cycle, taken by the wattmeter test. I believe that the reason Mr. Robinson's results check as close as they do is because he takes his voltage from a secondary winding, and the leakage error causes a reduction in the measured loss.

Mr. Robinson states in the paper that the flux variations due to leakage amount to not more than eight per cent. I assume that he means variation at an induction of 10,000 lines. Exploration of the sample under direct current excitation has shown that the leakage at the corners is very much greater and the resultant error to be more than one per cent. The design of Mr. Robinson's apparatus, the Epstein apparatus, is novel, and I believe it is a very fine scheme. We use corner blocks and clamps similar to those shown on the Epstein set in Fig. 4.

Repeated tests have shown variations between the results obtained by different operators, while the same operator can test a sample repeatedly and get the same result. The variations were found to be caused by differences in clamping the corners and pounding the corner blocks. I believe the clamping scheme shown in Fig. 5 is a great improvement over the usual Epstein corner blocks, and will do a great deal toward eliminating the personal equation in testing with a given set of apparatus. The other advantages mentioned in the paper are also of great importance, and make the adoption of the new clamping method seem advisable for standard Epstein apparatus.

**R. B. Treat:** A comparison of the two pieces of apparatus indicates that testing by means of the one pound sample is economical only for sheet makers or users who require many tests per week. The 10 kilogram apparatus is the more economical for a few tests per week, or infrequent tests, because of the use of commercial instruments which are available for other tests and are returnable to the makers for repairs and good results can be gotten by the average operator. The one pound apparatus requires the maintenance in usable condition of infrequently used special instruments, a thing very annoying and expensive. The high cost of material in the 10 kilogram samples is a disadvantage, but is to be preferred to the special instruments of the one pound apparatus.

Mr. Robinson takes it for granted that we are familiar with many of the minor details involved in the tests. It may be well to mention the importance of a few of them. Users of steel find it necessary to check the quality of incoming material with the supplier's guarantee and with specification. The samples should be sheared but unannealed, unless there is an agreement with the supplier that he will accept annealed tests. Ordinarily the purchaser of sheet steel must be able to test the supplied material without subjecting it to any questionable process, such as punching or annealing. Some so-called annealing is ruinous to the material.

Users of steel require also to know the properties of the material when incorporated in the finished apparatus. For this information the samples may be either sheared or punched, but should receive an annealing identical to the usual manufacturing process.

Mr. Robinson chooses a width of sample such that the effect of shearing is small, but while small in a sheared sample it may be very large in a punched sample, for these reasons: Shearing must be a clean cut, or there will be no cut at all, while a punch may cut cleanly, or it may mechanically disturb the metal for a considerable distance from the edge of the sample.

Samples can be japanned or shellac coated at a very small cost, so it is advisable to do this and thus minimize the effect of fins on the edges. Japanning increases the weight of the sample about one-half of one per cent, depending upon the thickness of the coat. Annealing may change the specific gravity of the sample, under some conditions it increases the weight about two-tenths per cent both dependent upon the exposure of the sample to the annealing heat.

It would be well to settle upon a method of determining the cross-section of the sample, and consequently the value of "B". This should be calculated from the weight and the longest dimension, for the percentage of error in taking this dimension is least of all.

It is very unreliable to micrometer individual sheets or even a squeezed pack.

Area determined by weight, and that by linear dimensions, may differ by ten per cent. The weight per cubic inch of silicon steel is about 0.271 lb. and common steel is about 0.275 lb., one and one-half per cent difference.

The butt joint with its paper insulation, has the advantage over the lap joint in that the flux is less likely to pass through the sheets at the joint, which might set up local eddy currents. The butt joint however causes leakage which has the effect of apparent higher core loss. This is easily demonstrated by successive tests with varying insulation in the joints and has been discussed by the Washington Bureau of Standards.

The Magnetic Committee of The American Society for Testing Materials has given the subject of sheet steel tests considerable study and we may hope it will soon settle upon a standard method for the use of both the maker and user of electrical sheets.

**W. J. Wooldridge:** Several speakers in referring to Prof. MacLaren's paper have spoken of the probable errors due to the use of iron wire for winding. The paper as a whole seems to fill in a gap in our knowledge in regard to steel.

It would perhaps be interesting to bring to your attention again the fact brought out by Ewing in his book on Magnetic Induction of Iron and other Metals, in which he points out that the permeability at very low inductions increases with increasing

temperature up to  $775^{\circ}$  C. and then decreases very suddenly between 775 and 786 deg. cent. At medium intensities that is less marked, while at high intensities the permeability falls off continuously, it does not increase as it does at low intensity—but falls off continuously, and falls off more rapidly at the end. In this connection it has been a question in my mind, and the minds of several people I have talked with from time to time, as to whether there was really some relation between hysteresis loss of high density and permeability. We know that at low densities there is no such relation, because with an alloyed steel, the permeability at very low densities is very much better than in standard iron, but these permeabilities cross somewhere about 12,000  $B$ , if I remember rightly, and it has been a question whether on densities over that there is or is not some relation between the hysteresis and the permeability. The results of this test would apparently show that there is no such relation, and therefore this paper fills a gap on a point concerning which we did not have definite knowledge before.

As near as I can make out in table III, the logarithmic relation between the hysteresis loss at various densities does not follow the 1.6 law. One of the speakers has mentioned that the eddy current loss does not vary as everybody supposed it would, *viz.*, that it should be stated as varying inversely as the specific resistance. The eddy loss in ordinary iron, at normal densities and frequencies is, roughly, 30 to 35 per cent. Silicon steel has between four and five times the specific resistance of ordinary iron, but the eddy loss is reduced only to 18 or 20 per cent, which does not follow at all the supposed law in regard to specific resistance.

**W. R. Whitney:** In the Research Laboratory we have found, after considerable experimental work, that it is desirable to employ as small samples as practicable for test. The reasons are almost obvious. Small samples of new material are usually much easier to obtain than large ones. Experimental work on these samples, such as heat treatments, compression and bending experiments, etc., can be more easily performed than on large samples. A relatively large number of separate samples can be taken from a small sheet of iron, and by taking the average of measurements on many small samples, results are sometimes obtained which would be concealed by the measurement of the hysteresis of the same quantity of iron in a single sample. For this reason we have often used a Ewing tester, where separate measurements were made on each ounce of iron, and have followed up the leads opened by such tests, by means of the standard factory one pound samples. In experimental work it is very important that all unnecessary barriers be removed from the way of quick and easy production of samples. It is preferable if the iron can be cut, annealed and tested in the same room and by one man.

Samples must also be capable of quick and simple measure-

ment, and finally, large numbers of the identical tested samples should be kept on hand for later reference.

This is all very easy with one ounce samples, but the Ewing tester, in our hands, is little better than a qualitative instrument, so that we have always referred to one pound samples for accurate conclusions. Such samples are also still small enough so that care and storage of them is not a serious matter. When the tests are made on twenty-two pound samples, it becomes evident that even the cost of the samples themselves is a factor where much testing is to be done. To make many tests in one day or to store identical samples in any quantity becomes a burden, if they are too heavy.

**C. J. Fechheimer:** The methods which have been described this evening for testing iron are applicable principally to transformers, in which the distribution of flux is very similar to that which obtains in the samples. They cannot, however, be used directly in dynamos or motors because in these there are heavy pulsations of flux and the distribution of flux in the core is by no means uniform and is quite different from that which we generally assume. The rapid change of flux which takes place when the edges of the poles pass under the armature teeth (or the armature teeth pass under the poles, whichever the case may be) also materially affects the loss occurring in the teeth. The result is that the core loss is two or three times as great as we would determine ordinarily by the constants obtained from transformer tests.

One of the speakers said this evening that the tests made in the factory on samples of new grades of steel are used for laying out a new line of machines. This, I believe, should be the case to a limited extent only, but I would be inclined to question it, because the transformer tests are not necessarily indications of what may be expected when the same steel is used in the armature cores of generators or motors.

For example, we know that while silicon steel has an eddy current loss, measured by means of transformer tests, which is considerably less than that of common steel and has also a slightly lower hysteresis loss, yet this steel does not have any appreciably less loss in generators or motors than the common steel. I questioned the results of the tests for sometime and had them repeated and was finally convinced when the temperatures indicated that the core loss as measured was correct.

There were six turbo generators of a certain size built, three with silicon steel and three with common steel, and the loss seemed to be about the same in all cases. There was, of course, some slight variation, but this variation was not any greater than that which usually occurs in duplicate machines built with the same quality of iron. I have never been able to account for this discrepancy and I should very much like to have an expression of opinion from some of the members to help clear this up.

Then there is another point which we find in practice does not

agree with theory. We usually assume, and without doubt correctly, that the eddy current loss varies with the square of the frequency and the square of the induction density. The hysteresis loss varies with the first power of the frequency and approximately with the 1.6 power of the density. However, I have found that the total core loss in machines varies from 1.9 to 2.2 power of the density, which would indicate that all the loss is due to eddy currents. Nevertheless when we compare the loss at different frequencies on the same machine we find that the loss is proportional to about the 1.4 power of the frequency, which would indicate that a great deal of the loss is due to hysteresis. This is another discrepancy for which I have never been able to account.

I should like to ask Professor MacLaren whether the iron did not age when starting with 25 cycles, repeating the tests at 60 cycles and then going back to 25 cycles. It would seem that the alternate heating and cooling of the samples causes the constants for the losses to be affected; that is, the iron would have aged.

**C. A. Adams:** At the recent mid-year convention at Schenectady, Mr. W. J. Wooldridge presented experimental data showing that the exponent of "*B*" in the Steinmetz formula for hysteresis loss was not a constant for modern silicon steels, but varied from about 1.5 at low densities up to 2.5 or more at high densities. The results of Prof. MacLaren's experiments seem to show that up to the highest density used by him, the exponent was 1.6 and practically constant throughout.

It is certainly very desirable that this apparent discrepancy be explained, for if both exponent and coefficient vary over a wide range, the Steinmetz law is not applicable to silicon steels.

**W. S. Franklin:** If the engineers concerned in making measurements would make a distinction between and use two distinctly different words for the two classes of errors, it would help us very much in regard to discussions of measurements of such things as hysteresis loss in iron. There are two distinct kinds of errors which physicists recognize, namely, errors due to the wobbling of the measuring instrument, and errors due to the inherent lack of constancy or definiteness of the thing you measure; and I think we should call the first of these errors, speaking of the probable error of a set of observations; that is, the things which are inherent in our methods and instruments, and the other things we should call departures. We could speak of "probable departures", etc. Take, for example, the main load factor of the consumer of electric lights who has fifty lights installed. We will say that the main load factor of that kind of a customer is 60 per cent. Now, as a matter of fact, there are enormous departures from that. If you were to take a large number of individual cases, you could determine what the probable departure is, for that probable use, if used in the same sense as we use probable errors. I wish we could get into the



habit of distinguishing clearly these two kinds of errors which enter into numerical measurements; one is external error and the other is internal error, and we might call one "error" and the other "departure".

**Malcolm MacLaren:** In answering Mr. Sharp's question, regarding the use of iron wire, I would say that that was taken as a matter of convenience. It did not seem as though copper would be suitable to stand the high temperatures, and iron was the most convenient substitute. The wire was about one-sixteenth of an inch away from the sample, No. 18 wire used, the turns being spaced about one-eighth of an in. apart, so that with the large cross section of the sample it would seem that the error due to the additional iron in the wire would not be considerable. If extreme accuracy was aimed at, it would be probably better to use some other material such as nickel.

As to the ease of manipulating the apparatus in making the loop, I believe that it will be found that taking men who have not made loops before, they will find it much easier to accommodate themselves to this manipulation of the resistance than to making the observations with the ballistic step by step method. As an indication of the flexibility of the method we recently took loops varying from an induction of about 200  $B$  up to about 14,000  $B$ , in which the only change required was to vary the resistance in the galvanometer circuit, so as not to take too many points in tracing the loop through its cycle.

As to the lack of agreement in the measurements by the loop method and the two-frequency method, I can not explain slight discrepancies that occur except to say that at the lowest induction where the greatest discrepancy occurs the wattmeter deflection was too small to read accurately. I believe that the results presented by Mr. Robinson this evening are certainly a sufficient justification for the use of the two-frequency method in making such measurements.

Dr. Northrup speaks of the number of observers required to make the loop measurements. The loops presented in the paper were taken with two observers, it is a little easier, however, to have three, one to take the time, and read the ammeter, one to change the resistance, and the third to read the galvanometer.

Mr. Chubb speaks of the losses that would occur in the use of iron wire, but I think these have been largely eliminated by the use of the second coil which avoids the necessity of correcting for the  $I^2 R$  loss and for the  $I R$  drop. In addition, the currents were kept very low throughout the experiment, so that the distorting effect was small.

With regard to the change in permeability, this was not observed with any particular care. It was noted, however, that the change at the higher inductions was very small, until the higher temperatures were reached. Above 650 or 700 deg. cent., the permeability fell off very fast.

In reference to Mr. Fechheimer's question regarding the

method of making the measurements, the temperature was not allowed to fall at any time during the measurements, it was simply held constant for a sufficient time to take a set of readings, first at 25 cycles, then at 60 cycles, and then a few check readings at 25 cycles, so that the check readings would generally be taken less than ten minutes later the original readings. The sample was then allowed to heat up to the next point.

With reference to Dr. M. G. Lloyd's method of deriving values for the exponent of  $B$  in the expression for hysteresis loss,  $w = a B^x$  I would say that the usually accepted interpretation of this expression is that the hysteresis loss in any magnetic material may be represented by a *constant* multiplied by the induction raised to an unknown power. If however the exponents given by Dr. Lloyd be used for the determination of  $a$  its value is found to be variable even with constant temperature. For example, using his figures in Table II for 218 deg. cent.  $a$  varies from  $0.635 \times 10^{-6}$  to  $1.49 \times 10^{-8}$ . These revised tables which he presents appear therefore to have no physical significance. My reason for taking 1.6 as the value of the exponent at the lower inductions was that these samples were admittedly not as advantageously designed for obtaining extreme accuracy as would have been possible if the windings had not been arranged to stand temperature up to 800 deg. cent. and numerous other investigators had obtained this value for ordinary steel at inductions near 6000  $B$ . Also the results that had been published upon silicon steel while not entirely convincing indicated that this value of 1.6 was correct for inductions near 8000  $B$ .

With regard to the possible effect of eddy currents in the determination of the hysteresis loop I would call attention to the fact that the samples were well laminated and that the time of reversal was over five minutes so that the eddy current could not be large. It was possible however to detect their presence and estimate their effect by holding the magnetizing current constant for a few moments during a reversal and noting the galvanometer deflection. The correction due to this cause could not well amount to more than the width of the line bounding the loop.

**L. T. Robinson:** With reference to the points that have been brought up about the variation of the hysteresis exponent, it was certainly very interesting to see that Professor MacLaren got the original 1.6 power results, whereas some of the rest of us have got something quite different. Whether the explanation is that which Mr. Chubb gave, that there may be certain irregularities in the tests which Professor MacLaren made, and we are right, or that the 1.6 is right and we are wrong, I do not know, but I think that is something that perhaps can be straightened out within a short time, at least I hope so. I have a great many results, and I am trying to work them up now and see if I can make anything out of them.

About the ballistic method, and the agreement, etc., between

the two methods of measurement, I think I covered that in a fair way in my paper. I do not know whether it should agree, or whether it should not agree. There are so many things about the behavior of iron in magnetic fields that we do not know, that I do not think we should be too positive about our statements. I am satisfied to say that we usually have looked on the ballistic method as a good way to make the test. It was the standard method, and in developing another method we simply looked back to the old method to see how it agreed. There is a fairly good agreement, whether it means anything or not, I do not want to say. I hope it does, but we are not by any means sure of it, until such time as we can explain some of the vagaries with reference to the separation for eddy currents at different densities. The whole thing is founded on certain assumptions, some of it I hope is true. They all of necessity cannot be true. If you start in to discuss the whole subject and proceed to the final analysis along known lines, you cannot go very far. You assume that the eddy current loss varies as the square of the frequency. When you say that you say that it varies as the square of the voltage produced. When you say that it varies as the square of the density, you say the same thing, but still when you try to separate using various densities, it does not work. You cannot separate by change in density, although you can separate and get a straight line by changing frequency. Whether the fact that you get the straight lines means that you get the correct result or not, I think we can by no means be sure we are correct in saying. There is an opportunity for something to be said about this subject that has some definiteness about it that our present statements must lack, because of our want of absolute knowledge.

With reference to the long period D'Arsonval galvanometer, with considerable damping, old and new are only comparative words, I would say that we have used such an arrangement since 1898, to the best of my recollection, and I believe it to be good.

The Koepsel apparatus referred to is perhaps not directly connected with the subject matter of the evening.

I believe that it was brought out first in this country and published in the *Electrical World* some time in 1894. It is said to have been brought out sooner than that by the one whose name is now applied to it—but I think it was not published until some time after.

With reference to using insulation between the sheets, I think it is not necessary to insulate alloy iron but it may be necessary to insulate standard sheets.

I believe Dr. Sharp spoke of the disagreement between tests made by another method and by the apparatus that has been developed by the Bureau. My remarks on the elegance of that method were based almost entirely on the results that were obtained with it regardless of the comparison with other results—that is, it seems to be very good, because they get very nice

separations, and the things that vary more in other apparatus seem to vary less in it, and therefore I thought it was very good.

I think the matter of the standard sample has been quite generally referred to as being desirable. I was much interested in Dr. Northrup's remarks about the transformer running at 200 deg. cent.—we may have something like that some time, but the most interesting thing is what would he use for the insulation. That information is somewhat incomplete.

With reference to Dr. Franklin's remarks about errors and departures, I think that is a very good distinction, but it is hard to tell which is which. You do not get anything just right—sometimes you know you are wrong, but do not know whether it is an error or a departure.

With reference to Mr. Chubb's remarks, there is nothing more than a slight difference of opinion on what seems to be a minor point. I appreciate the fact that results can be obtained accurately by the method that he prefers. I may be wrong in thinking so, but it seems to me it is better to do something that is more generally applicable than to choose some arrangement for matching one error against another in a specific device. However, the specific point brought up as it relates to any apparatus except that described in the paper I have not covered experimentally, and cannot, therefore, be very definite in expressing an opinion on it at this time.

With reference to Mr. Fechheimer's remarks, it is interesting to know that some one has found some things that he cannot explain. We have been finding them right along, and it would be very easy to write a paper on the things about iron that you have tried and seen, and could not explain.

**Henry Pikler:** We are engineers, ultimately interested in the performance of sheet steel in the finished apparatus. If different makes of electrical machinery were built of exactly the same quality of sheet steel, we would find that the total iron loss per pound of sheet steel in these machines would not be the same, although the magnetic densities and frequencies are kept the same. This is due to differences in the design and construction between the electrical machinery of the various makers. In other words, if a given quality of sheet steel be used for building the transformers of makers "A", "B" and "C", we would find that the total watts loss per pound, at the same magnetic density and frequency, would be different in these three transformers. If instead of transformers we consider generators or induction motors, we would find that these differences would be much greater.

From this it follows that in order to foretell the total watts loss in the finished electrical apparatus, it is not sufficient to know only the core loss watts per pound in a sample of sheet steel tested by some standard method; the figure obtained by the standard method must be multiplied by a coefficient.

The difficulty lies in establishing a method of testing samples which can be used as a standard. I wish to emphasize that, for the reasons pointed out above, I do not consider that it is of primary importance at all that by the standard method physically accurate core loss watts per pound may be determined, but that by using this standard we should always get exactly the same result, whether the test is made by Peter, or by Paul, in the United States, in Japan, or in Germany. I would go even further and state that it is not a core loss—that is, a watt loss measurement—which is necessarily the criterion of the quality of the steel. To illustrate my point by a drastic example, I mean that if for instance a certain sample of steel, under the conditions created by the standard method, be struck and it vibrates say so many times per second and between this number of vibrations and the total watts loss there is a definite relation and finally *this* standard method can be exactly duplicated everywhere and at any time for the purpose of obtaining uniform results, then this method is more desirable than one which is based on watt measurements and gives not so uniform a result.

Probably the most important function of a standard method is to establish a basis in the transactions between the purchaser and supplier of sheet steel which excludes any chance of dispute. It is my opinion that there is a greater necessity for establishing a standard to this effect, one that gives uniformly correct results within say 2 per cent than to have this method for the purpose of enabling the designer to predict the core loss in his machine within that limit. Because I consider the uniformity of the results the most important, I naturally do not favor the introduction of any element into the standard method which is liable to introduce variations. For this reason I am very strongly opposed to any method of testing iron for the above purposes for core loss where the magnetic circuit of the test piece is broken by joints of any kind.

In reference to several remarks and questions brought up in connection with "core loss", which pointed out that the core loss of induction motors or turbo generators, etc., cannot be predicted, and furthermore the relations between watts, magnetic density and frequency do not follow the law given by the equation  $\dot{W} = M (B \propto)^2 + N B^{1.6} \propto$ , I wish to give my opinion as follows:

In order to investigate these problems the engineer must become a physicist. Unfortunately, according to my observation, very few become such in their investigations. One of the most fundamental principles of physical investigation consists in attacking the problem step by step and these steps should be extremely small. A phenomenon, as it appears to us at first glance, is in reality a complexity of very many phenomena and we will not be able to understand the whole if we fail to recognize the details.

Take the simplest electromagnetic apparatus, the trans-

former. There the engineer assumes a uniform flux density as the underlying basis of his calculations, derived from the terminal voltage, frequency, number of turns and the dimensions of the core. The truth, however, is that in any commercial transformers, even with the best magnetic joints, such is not the case. The magnetic flux is the densest at the magnetic center, which is the center of the magnetizing winding and there the volts per turn are also the greatest. In either direction, away from this center, the density of the magnetic flux, and with it the induced electromotive force per turn of the winding, will diminish. Whereas in the neighborhood of the magnetic center the lines of force travel in the same direction as is the direction of the "mechanical" magnetic circuit in that region, in the neighborhood of the joints there will be many lines of force which will depart from the steel and will cut it transversely. Right at the joints this transverse cutting of the plates by the lines of force leaving them will be a maximum. This will introduce additional losses in the steel, mainly eddy losses, the magnitude of which depends upon the perfectness of the joint and the distribution of the winding over the core.

In considering, instead of a closed magnetic circuit of a transformer, the magnetic circuit of a generator or motor, we find the situation much more complicated. In these machines the irregular distribution of the magnetic flux is established even on purpose (low regulation, small slip, etc.). In these machines the transverse cutting of the core plates by the magnetic flux is very greatly pronounced. It is therefore a great mistake to expect to derive any conclusions as to core loss watts from measurements based on conditions where the magnetic flux travel in the direction of the mechanical magnetic circuit. Before trying to establish a relation between watts core loss and magnetic density derived from impressed or induced e.m.f.s., turns and frequency, it is of primary importance to investigate the geometrical configuration of the path of the magnetic flux. This can be done either by exploring coils, or even by simple reasoning, based on experience, going step by step over the entire magnetic circuit and predicting the probable path of magnetism in order to get the geometrical configuration of the lines of force.

When we look at magnetic circuits, flux densities and core losses in this light, I think we shall be surprised to hear anybody expect a connection between core loss and magnetic density as derived from volts impressed, turns, etc.

**M. G. Lloyd:** It is highly desirable that some method of measuring hysteresis suitable for commercial conditions should be standardized in this country, for the convenience of manufacturers and buyers. The American Institute of Electrical Engineers could well take the initiative in this matter, as has been done by its sister society in Germany. There the Epstein method has been adopted and seems to give general satisfaction,

the standard test being at 50 cycles and 10,000 gaussess. Perhaps the details of the Epstein method could be revised so as to be more acceptable in this country, but its predominant features should be retained for a standard comparison method. Where shop conditions make other methods desirable, they can of course be used, the standard method being resorted to only for occasional comparisons, and to check up results against those obtained by outside parties. The more accurate methods available by the use of properly proportioned rings or such apparatus as that in vogue at the Bureau of Standards are more especially suitable to investigations aimed at a correct value of the physical constants, and similar tests where accuracy is more important than economy in time, in materials or in the necessary apparatus. It is important, however, that any method adopted as a commercial standard should use a specimen of such dimensions that the hardening effect of the cut edges is small, since it is not always feasible to anneal after cutting, and indeed, unless annealing can also be standardized (a difficult matter), results obtained by different observers at different places would not be comparable. It is well recognized that the process of annealing is quite as important a factor in determining the properties of the material as any other factor involved. It is desirable also that the method should be adapted to the use of portable measuring instruments. This requires the use of a large sample, and it suggests the use of a good magnetic circuit in order to keep a fairly high power-factor. The Epstein method seems to have best met conditions generally found, but where it is necessary to make a large number of determinations in a short time, such a method as that described by Mr. Robinson is decidedly superior for the workshop. It gives comparative values with sufficient accuracy and has the advantage of using less time and material.

It is to be regretted that Mr. Robinson has said nothing about the application of his method to silicon-steel. Silicon-steel is usually used at higher flux densities than ordinary steel, and at the higher flux densities it requires a much larger magnetizing current, making a lower power-factor. In Mr. Robinson's method the magnetizing current must be large enough to induce the flux at the center of the specimen, and this may be 40 per cent higher than the nominal value of flux. The conditions are therefore much less favorable with silicon-steel than with ordinary steel.

The paper by Professor MacLaren is of great interest in showing the manner in which hysteresis varies with temperature. Caution should be observed, however, in attaching too much weight to the accuracy of his measurements. When the form-factor is not determined in the wattmeter method the results must be viewed with suspicion. Evidently the form-factor was not determined in these experiments. Even supposing that the voltage wave of the generator was sinusoidal, it is not likely

that the secondary electromotive force, whose form-factor enters into the computation of flux density, would have the assumed value. This is especially true since iron wire was used in the magnetizing coil and its resistance was probably high therefore, and moreover variable with temperature. Since the voltage used and the resistance of this winding are not given, it is impossible to judge of the amount of distortion probably present.

The values given for the exponents which represent the power of flux density which is proportional to the hysteresis are very misleading. In the first place they depend upon the assumption of 1.6 for one condition, and a computation based on this value for the other conditions. Moreover a very undesirable method has been used in making the computation. As a matter of fact the exponents vary through a wide range, from values less than unity to as high as 1.99, as will be seen in the accompanying tables, which have been computed for specimens two and three. In fact, at a temperature of 700 deg. the value is actually negative for silicon-steel, since it is to be observed that the hysteresis found at 8,000 gaussses is greater than the value found at 10,000 gaussses.

Professor MacLaren has computed his values by assuming 1.6 at the lowest flux density and used this value to determine the constant  $a$ , and the resulting value for  $a$  is used in computing the exponents for all other flux densities. In the tables which I have computed the exponent is found by means of the equation

$$x = \frac{\log w_1 - \log w_2}{\log B_1 - \log B_2}$$

and the value of the exponent therefore depends only upon the values found for the hysteresis at two values of flux density and the coefficient  $a$  is assumed to be constant only between these two points.

In the tables each value applies to the range of flux density between the value which it is opposite and the next preceding value. It will be seen from these tables that there is no regular variation in the value of the exponent, except that for silicon-steel at all but the highest temperatures the exponent decreases with increasing flux density. This result is in contradiction to most of the other experiments which have been made; most observers have found it to vary in the opposite direction. Reference may be made, for instance, to the paper by Mr. Wooldridge, at the Schenectady convention. As experience in this work has shown me what large errors may be produced in the result by small variations in the form-factor, it seems likely that these results are not very accurate and they are principally valuable in showing the general trend of hysteresis loss with temperature. In this connection it should be remembered, too, that at high flux densities the permeability decreases with increasing tempera-



ture. The magnetizing current would therefore increase and at the same time the resistance of the magnetizing circuit is increasing, so that the distortion would increase at a rapid rate.

The table given in the paper, showing the variation in induction with change in magnetizing current near the recalescence point, is chiefly valuable in showing the great rapidity of variation in that region. The differences found with ascending and descending temperatures are not significant. The two curves start very sharply at the same temperature of 737 deg. and cross at about 721 deg. An error in the measurement of temperature of three deg. would account for any difference found between these points. Since the variation of permeability with temperature is very different for different magnetizing forces, it is necessary to use a wide range of magnetizing forces to make such experiments of much value.

It is interesting to observe that the method used by Professor MacLaren for hysteresis loops can be used with small samples, as it has been generally supposed that this method was only applicable to cases in which the cross-section of the material, and consequently the flux, was very large. This method can hardly be claimed to have the accuracy, however, of the step-by-step method. Since the continuous change in flux will induce eddy currents which continue during the entire half loop, and since they oppose the main magnetizing field, the observed value of  $H$  will be too large. The extreme points of the loop will be correctly found, since the current becomes constant at these two points, but in between them the loop will be broadened, and the value for hysteresis will be too large. The result is similar to that found by the wattmeter method in a case where the eddy currents are small and are neglected. It is to be noted that in two of the three cases given, the loss is larger by this method than by the two-frequency method, although in the third case it is smaller by five per cent.

Dr. Sharp has called attention to the recent paper in the PROCEEDINGS of the Institution of Electrical Engineers in which the method used at the Bureau of Standards was reported to give results too high. I should like to call attention to the fact that the comparison was not made with the Epstein method, but with a different method whose accuracy was entirely unknown, although it was used as a standard of comparison. The only value of that research is in showing the differences found in the two methods.

It is sometimes wondered by those designing and testing electric machinery that the values of the exponents of flux density which are found in laboratory experiments, or computed on theoretical grounds, do not apply in practice. It would really be more wonderful if they did. Such an exponent as 2.0 for eddy currents, for instance, is computed on the assumption of uniform density in one definite direction in a sheet of infinite width. In dynamo and motor cores none of these conditions

are fulfilled, and even in transformer cores we do not have uniform flux density. The demagnetizing effect of the eddy currents makes the flux less at the center than near the surface. With increasing flux density or increasing frequency this effect is intensified and results in the eddy currents increasing at a less rate than the square of either of these quantities. As shown in Mr. Wooldridge's paper at the recent Schenectady convention, the exponent for eddy currents is actually found to decrease with increasing flux density, and such a decrease is qualitatively explained by the above consideration. It must be remembered, however, in connection with the determination of eddy-current losses, especially in silicon-steel, that they form a small part of the total loss, and accurate determinations are extremely difficult. Small errors in observation, or small departures in the wave form, may produce large errors in the value of exponent found.

In the cores of generators and motors, the flux density does not simply alternate in direction, and the relative distribution of flux is not constant. The flux pulsates in space as well as time. The laws of hysteresis and eddy-current loss which obtain with the usual testing apparatus cannot be expected to apply even approximately under such conditions.

TABLE II

<i>B</i>	Temperature in deg. cent.							
	218	237	358	445	527	597	660	707
6000								
8000	1.53	1.60	1.64	1.63	1.77	2.08	1.92	1.99
10000	1.87	1.68	1.68	1.69	1.70	1.54	1.57	1.25
12000	1.81	1.72	1.60	1.61	1.62	1.24	0.85	0.94
14000	1.94	1.72	1.62	1.38	1.31	1.14	1.41	0.60

TABLE III

<i>B</i>	Temperature in deg. cent.					
	47	300	402	508	597	659
8000						
10000	1.79	1.66	1.48	1.04	0.74	0.52
12000	1.68	1.44	1.36	0.96	0.60	1.425
14000	1.54	1.37				

**J. D. Ball:** In regard to the iron tester for 10 kg. samples 50 cm. by 3 cm., it might be well to add some data as to how satisfactory it has proven in actual practice. Regular checks are made between three laboratories: one located at the steel mill,

one at the transformer factory and one the standardizing laboratory.

The average of 278 samples tested at the mill and factory agree within 1 per cent, the maximum variation of any one sample between the mill and factory measurements was 3.5 per cent. 100 samples tested at a later date show an agreement within 1/20 of one per cent of the same average loss measured in the two places.

Samples tested in all three laboratories show conclusively that average results are well within 1 per cent.

These comparisons are extremely good in as much as the mill test results quoted are results of single observations; the results given at the other points are, in many cases, the average of several observations.

There is one advantage in quoting results as hysteresis loss, as is done when tests are made on the small set (using 1 lb. sample 10 in. x  $\frac{1}{2}$  in.) It has been demonstrated that the eddy current loss is a function of the kind, shape and thickness of material and geometrical construction of the test sample or transformer and is independent of total loss or manner of cutting sample as regards direction of rolling.

The hysteresis loss is a value to which may be added the eddy loss to give total core loss at any desired frequency for any size or shape of test sample or transformer after these eddy losses have been determined by separation tests on a comparatively few representative samples.

As is shown in the paper, this is true for the rings and other samples mentioned. It is also shown by Lloyd and Fisher (U. S. Bureau of Standards reprint 109) and other writers. The writer has found it true for various transformer cores of the same types and material.

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