DISCUSSION ON

"THE DETERMINATION OF THE EFFICIENCY OF THE TURBO-ALTERNATOR." *

NORTH-WESTERN CENTRE, AT MANCHESTER, 25 FEBRUARY, 1919.

Dr. Miles Walker: I quite agree with the authors that the method of determining the losses by measuring the temperature rise of a known flow of air is a sound one. The main difficulty seems to lie in the accurate measurement of the air. The method used by the authors is good, but it is open to the objection that we are not quite sure of the shape of the curve which gives the velocity at different parts of the duct. Especially near the edge of the duct the velocity curve is not known, and as the perimeter is considerable the error is not negligible. We cannot be quite sure whether the rectangle which gives the area of the curve should be 17 inches, 18 inches, or 16 inches wide. The difference between the square of 16 and the square of 18 is very considerable I have little doubt that improved methods will be used for measuring the air, and, even as it is, the method is much more satisfactory than other methods of determining these stray losses. In a number of experiments that we have been carrying out at the College of Technology we have found that

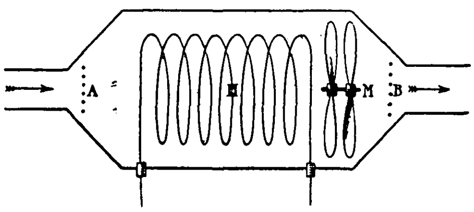

FIG. D.

A. Thermal junctions at entrance.
B. " " " exit.
H. Heater.
M. Fan blades rotating in opposite directions for mixing the air.

so reliable is the measurement of temperature in the measurement of losses that we have inverted the process for the purpose of measuring the air. The current of air to be measured is passed through a special calibrating box (Fig. D) which contains a heater consisting of a spiral of wire, the amount of power supplied to this being known. A number of thermal junctions of uniform resistance are connected in parallel and distributed over the entrance so as to take the mean temperature of the air coming in, and a number of similar junctions measure the mean temperature of the air going out. Knowing the watts lost in the spiral and the temperature rise, the flow of air is easily calculated. With a constant flow of air through various parts of our apparatus which represent various parts of the ventilating system of a turbo-generator, we know that the loss in any part is proportional to the rise of temperature in that part.† We can measure the temperature to 1/100th of a degree; and if we have 5 degrees rise in temperature in the air going through we get a very good measure of the flow of air. It will be quite possible in turbo-generators to have some such arrangement in the ducts feeding the air into the machine; and in that way we shall have, I believe, a very much more accurate measurement of the amount of air going into the machine than could be obtained with an anemometer. With regard to measurement of the temperature of the air going out, we have found that thermometers are rather troublesome. If one is dealing with a large area one has to use a number of thermometers in order to get anything like the mean temperature. A very much better method is to use a large number of thermo-couples of equal resistance connected in parallel. They are much cheaper than thermometers, and not so easily broken. With their aid it is possible to get the average temperature of the whole area at one reading, and the result is much more accurate than that obtained with an ordinary thermometer. This paper will certainly be of very great service in drawing the attention of engineers to the importance of measuring stray losses.

Mr. J. S. Peck: With the increase in size and speed of turbo-generators it becomes more and more difficult to measure the losses in the usual way, that is by means of a calibrated motor, and some method such as that described by the authors which will enable losses to be measured under running conditions on site, is likely to be generally adopted if it is found reliable. The methods described in the paper have previously been discussed on several occasions: they were dealt with in the paper by Professor Threlfall and in a paper read before the American Institute about five years ago. On this occasion the general subject of alternator efficiencies was discussed, and emphasis was laid upon the difficulty of measuring the volume of air. This seems to be the critical point of the whole scheme. About a year ago the Standardization Committee of the B.E.A.M.A. considered this general question of measuring alternator efficiencies, and various manufacturers were asked to submit their opinions upon it. Among others Messrs. Brown, Boveri pointed out that they had been using the air-measurement method for some time and obtained very satisfactory results. But they also stated that, as they were manufacturers of fans and compressors, they were quite accustomed to making exact measurements of air volumes, and therefore would probably not experience all the difficulties which were encountered by manufacturers unfamiliar with such measurements. Nearly all the other manufacturers recommended the "calibrated motor" method, and it was felt that until some method of measuring air velocities and air volumes had been perfected it

* Paper by S. F. Barclay, Ph.D., and S. P. Smith, D.Sc. (see page 293).
† H. D. SYMONS and M. WALKER: "Heat Paths in Electrical Machinery," *Journal I.E.E.*, 1912, vol. 48, p. 674.

was better to adhere to the method of using calibrated motors. The final recommendation of this Committee was that the separate losses should be measured by means of a calibrated motor and that the total loss on short-circuit should be taken and not a compromise between the I^2R loss and the total loss on short-circuit. The most important feature in the paper seems to me to be the design which the authors have worked out for securing a uniform rate of flow of air in the exhaust. The closeness with which the results obtained by different methods agree is really remarkable; it is better than one would expect from commercial measurements, but until further tests have been made I should hesitate to recommend the immediate adoption of the air-heating methods for determining efficiencies. If, however, further tests show that accurate results can be obtained by their use I think there is no doubt that they will be generally adopted for measuring the efficiencies of large turbo-alternators. If this comes about we shall owe a great debt of gratitude to the authors.

Mr. J. Frith: I have brought for the inspection of members some pieces of apparatus which Dr. Cramp and I have developed for the experimental and commercial measurement of air quantities. These are shown in the illustration (Fig. E) herewith. On the right is a portable micro-manometer reading directly in air velocities from 5 to 100 feet per second. For ordinary pressures the two ends of this inclined gauge are connected by rubber tubes to one of the forms of combined facing and side gauges shown in the foreground. The gauge is also suitable for pressures up to 100 lb. per square inch. The whole arrangement shuts up into a box 22 in. × 11 in. × 4 in. and is an extremely portable, accurate, and rapid instrument. To the left is shown a less portable differential gauge reading by a pointer on a dial having a double scale of from 5 to 70 feet per second air velocity and 0 to 1 inch differential water column by which differences of pressures of 1/1,000th of an inch (water gauge) can be measured. I gather that the authors favour the electrical method of Morris because it measures the air velocity independently of its direction. I think this is wrong, as the air may be travelling in any direction, possibly inwards at some points, and what is wanted is to measure the outward component of the air velocity only; such a measurement is given by a facing gauge kept normal to the opening. There is another electrical method developed by Thomas in America in which a measured amount of energy is used to heat the air, the temperature before and after being measured by a platinum thermometer in the form of a wire grid stretched across the air duct. Then the energy lost in the alternator is to the measured energy as the temperature rise in the alternator is to the temperature difference before and after the heater. This method is independent of the quantity or specific heat of the air used. I suggest that the platinum-grid thermometer would be more accurate than Dr. Walker's thermo-couples in parallel, in which arrangement the hotter thermo-couples would send current into the cooler couples and alter their temperature.

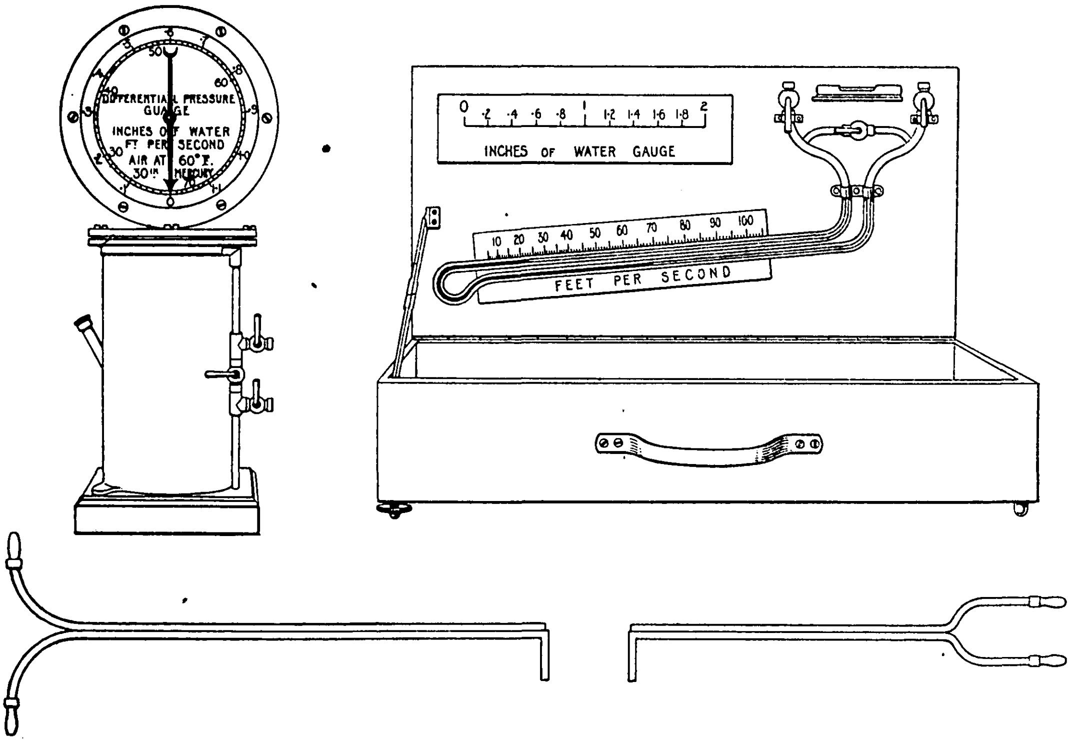

FIG. E.

Mr. H. C. Lamb: This paper is of special interest to station engineers because everyone who puts in plant wishes to test it himself after it is installed. We are able to do that with almost everything except generators, for which up to the present we have had to take the figures furnished by the makers who test at their own works. Although not wishing to cast any doubt upon their results, we think that all tests should be made by independent parties. There have also been a few cases in which disputes have arisen because the performance of a set has not come up to the guarantee and the turbine builder has wished to put the blame on the builder of the alternator. The method described in the paper would settle such disputes very easily on site. A couple of months ago it occurred to me that the efficiency of a certain alternator might be checked in this way and a comparison made with the figures obtained by the builders. It was a 20,000 kw. alternator and it had been very fully tested before delivery. When the test by the air method was made the load was kept constant for several hours at 17,000 kw., 0·85 power factor, 6,600 volts. The air was measured by an anemometer similar to the one illustrated in the paper, and it had been specially put in order for the test by the Manchester Instrument Department. The discharge duct was divided by cast-iron webs into 16 compartments. The velocity was taken in each one of the 16 divisions, and the air volume worked out at 67,840 cubic feet per minute. The inlet air temperature was 68·20° F. and the outlet air temperature—which was taken by specially calibrated thermometers on the average of 24 readings—was 110·67° F., showing a temperature rise of 42·47 degrees. Taking the weight per cubic foot for 110° at 0·07 lb. it gave the energy carried away by the air as 855 kw. The loss through bearing friction was assumed to be 100 kw., making a total of 955 kw. I had no idea how to estimate the radiation loss, so it was neglected. Those two losses gave an efficiency of 94·7 per cent and the efficiency taken from the curve of the tests at the maker's works was 94·6. After reading the paper I measured the surface of the carcase, which was 210 sq. ft., and allowing 0·006 watt per square inch it gives $7\frac{1}{2}$ kw. of loss by radiation. When that was allowed for it brought the efficiency to 94·65 per cent, against the 94·6 per cent obtained by the manufacturers—a remarkably close result. It did not occur to me at the time that this was a method which would be recommended by scientific people, but after hearing the paper and thinking over my own results I feel quite sure it will be very widely used. It removed from my mind any lurking suspicion regarding the figures given by manufacturers—at any rate by the particular manufacturer in question.

Mr. J. A. Kuyser: The two methods described by the authors for measuring the losses on turbo-alternators have been in use for some years, but no results of tests have been published up to the present. One reason for this is perhaps that the results have been rather uncertain, at least in the case of the air-heating method. For the turbo designer who is anxious to obtain the stray load losses as accurately as possible, the calibrated air method looks the most attractive; it is very simple and requires very few precautions. It is only necessary to use a sufficient number of thermometers to obtain the correct average temperature. As regards the air-heating method, the general opinion is that air-velocity readings are very unreliable; the results obtained by the authors, however, check remarkably closely with the calibrated method and, if these results can be duplicated, the air-heating method should be very useful for the power-station engineer to check the results obtained on the manufacturer's test floor. I have used the calibrated air method for several years to obtain a comparison between load losses and short-circuit losses. The following table gives some of the results :—

Normal k.v.a.	20,000	19,000	18,000
Normal r.p.m.	1,800	1,800	1,800
Normal volts	13,200	2,700	12,000
k.v.a. during load test ..	15,000	13,000	14,500
Load loss using no-load test for calibration, kw. ..	145	91	—
Load loss using short-circuit test for calibration, kw. ..	141	139	169
Short-circuit loss measured by calibrated motor, kw. ..	150	139	152

In these tests the no-load and short-circuit losses were measured by means of a driving motor. The load test was made at zero power factor by means of an under-excited synchronous motor. The tests are ordinary factory tests and the results are therefore not very accurate. The figures give, however, an indication that the short-circuit stray loss appears nearly in full during load conditions. In connection with these tests I would draw attention to the method proposed on page 299 where the authors recommend two temperature runs to obtain the calibration, namely, one run without excitation and one run fully excited. The calibration loss consists in this case of the sum of core losses and rotor I^2R loss, while the fan losses and windage losses are eliminated. I should like to ask the authors the reason for this. The fan and windage losses are known as accurately as the core loss and have the same effect on the air temperature, and to obtain the calibration a single test fully excited on no load, or a short-circuit test should be sufficient, more accurate, and less expensive.

Mr. G. A. Juhlin: I think we should not overlook the necessity of obtaining data as regards the individual losses when considering the method of measuring efficiencies of large turbo-alternators. It is of extreme value to the designer to be able to segregate the losses in a machine with as much accuracy as possible. Unless we increase our knowledge in this direction, progress in design will be slow. The "calibrated motor" method has very distinct advantages, as it is possible to obtain accurately the various losses, with the exception of the load losses. It is of course possible to obtain readings of the individual losses by what the authors term the "air heating" method, but when we consider that it is necessary to run the machine until perfectly steady temperatures are obtained for each reading, it is clear that the time required in making these tests will be very great. My experience has been that as soon as a set is ready for running it has to go into service, and I am afraid operating engineers will object to having a set tied up for any length of time for the purpose of carrying out tests. For this reason I feel it would be a mistake to substitute the air heating method for the calibrated motor method. The air methods are very valuable for determining the total losses under load which would make it possible to determine the actual load loss if the no-load losses are known. With regard to the comparison between the total losses obtained by the " air heating " method and by measuring the separate losses, we carried out tests on a 5,000-kw. machine some time ago and obtained good results. A chimney with two layers of wire gauze was used, and the resistance drop was not found to be excessive. I believe Professor Threlfall used very open muslin in his tests with satisfactory results. The total losses as obtained by the " air heating " method were about 8 per cent higher than the summation of the separate losses, taking the total short-circuit loss as equivalent to the load loss. The air speed was very uniform over the whole outlet of the chimney. In connection with the statement on page 294, that the heating of the lamination has been found to be less on full load than on short-circuit, it would be interesting to hear from the authors whether this is a usual occurrence or if it is an isolated case. It is difficult to accept this as a general experience and one would hesitate to draw the conclusion that the stray losses on load and on short-circuit are materially different. With reference to measurements of air volumes, the Thomas meter to which reference has been made is claimed to be very accurate. The principle underlying this meter has been applied for checking the air volumes in turbo-alternators. It is part of the routine testing to run the machine on field charge by means of a calibrated motor and measure the temperature rise of the air. As the energy heating the air and the temperature rise are known, the volume is easily and accurately obtained.

Mr. R. E. Grime: I agree with Mr. Frith in his criticism of Professor MacGregor-Morris's electrical method of measuring the air velocity, as applied to the present problem. It is desired to measure the average air velocity at the chimney outlet, normal to the plane of the outlet. If the air at any point is not moving normally to this plane there must be a side component, and the absolute velocity is then necessarily greater than the normal component. Professor MacGregor-Morris's ingenious instrument measures the absolute velocity at each point explored, irrespective of direction, and therefore gives too high a reading unless the air is everywhere moving in stream lines normal to the plane of the outlet. On page 298 wet air-filters are mentioned. If the proposed tests are carried out on a machine provided with this type of filter, it will be important to ensure that no free suspended moisture remains in the air entering the machine. The authors show that variations in humidity do not appreciably affect the results, but if any free moisture gets past the thermometer at the intake and is evaporated in passing through the machine, serious errors may result. The present paper puts the art of measurement of stray losses in turbo-alternators in a position very much ahead of that of their calculation. In view of the magnitude of these losses combined with their extreme undesirability, a wide field for further investigation is thus opened up.

Dr. W. Cramp: It seems to me that the unknown quantities in connection with the methods which the authors have put forward are two. The first of these is the amount of heat which goes into the machine itself owing to the specific heat of the material of which it is made. In particular with the calibrated air temperature method it seems certain that the machine cannot have reached its final temperature at the time the loss is measured ; and nothing is said about the amount of heat which goes into the machine during the calibration period. Similarly with the first method it will not be possible to measure the efficiency of the machine unless it has first reached a final temperature. It would seem to follow that the measurement must be taken at the end of a long run, and that a correspondingly long run must always intervene between the time the test is taken on one load and the time the test is taken on another load, which makes the whole method rather slow. The other unmeasured quantity is the loss due to radiation, conduction and convection from the exterior of the machine. It seems to me that the the rule given in Appendix I for ascertaining this loss is too rough to be of any service. The authors suggest that the external surface should be measured and multiplied by a constant to give the total number of watts lost. Anyone who has tried to estimate the temperature of an ordinary direct-current machine knows how difficult it is to arrive at the value of the external surface. What is the external surface to include ? Take the case of a turbo-alternator. Are the feet to be taken as part of the external surface ? Are the bearings ? It is surely not reasonable to take every square inch of the external surface and multiply it by the same constant. Nor is it possible to adopt the same constant for all types of turbo-generators. For example, some machines are smaller in diameter and of greater length than machines built by other makers, and it would not be reasonable to take the same specific loss in both cases. Speed and frequency also will affect the question. To get this allowance accurately we need a method better than that put forward on page 304 where 0·006 watt per square inch of surface per degree rise is mentioned. In connection with that I should like to call the authors' attention to some figures which are given in the current number of *Science Abstracts*, showing the loss of heat from plain iron pipes when steam passes through them at different temperatures. The variation of the loss with temperature is very considerable. Thus even where the surface is settled, it would not be satisfactory to take a single figure for all temperatures. I am very surprised that the authors refer to the anemometer as a satisfactory instrument, as I have found it entirely unreliable. To measure velocity with an anemometer we have to take not only the anemometer reading but also a reading of the time, so that we have a possibility of two errors. The authors propose to take a number of readings over a large area divided into a number of sections. It seems to me that that process will be very slow and possibly very inaccurate. For instance, in the diagram shown there are 81 squares and it is proposed to take anemometer and thermometer readings in every one of those sections. The authors have commented upon the time that is taken in getting readings with the Brabée tube and micro-manometer, but surely that is nothing compared with what they recommend in connection with the anemometer. They say that the reading in each section should occupy at least 15 seconds, and as there are 81 sections they will require for a single reading at least 20 minutes. During those 20 minutes a number of things may happen. It is very difficult indeed on the ordinary test to keep the load absolutely steady for 20 minutes and the temperature in various sections may change. The method therefore leads to inaccuracies from the time taken to get a single reading. It is said on page 296 in connection with the anemometer that the frictional effect is not more than 3 per cent of the vane-wheel speed. I should like to know how that test was made, as it is an exceedingly difficult one. A little later there is a reference to the use of the Pitot tube to obtain the average velocity across the discharge opening. The authors say, "The mean air velocity was measured by traversing the Pitot tube across the discharge opening." Such an arrangement as that cannot possibly give by means of a Pitot tube the average velocity. With a suitable manometer it might conceivably give the mean square of the velocities in a rectangular pipe; but even that is unlikely and in any case would be very different from the mean velocity. If the anemometers were checked by this method the calibration would be of little use. Reference has been made to the question of the accuracy of the static gauge compared with the dynamic gauge. The static gauge is more likely to give trouble than the dynamic, but there does not seem to be any particular reason why the static tube should not be in the side of the duct square with the duct and flush with it. An advantage of the type of gauge illustrated by Mr. Frith's exhibit is that the static tube is quite close to the point at which dynamic pressure is measured. Now it is known that with a circular tube the pressure along a radius is practically constant. On the other hand it is known that the velocity across any section of the tube varies, being higher in the middle and going down almost to zero at the boundary. As the pressure does not vary with the radius of the tube the static tube may be put wherever convenient, and a safe place is at the boundary of the duct. A small hole quite flush with the boundary will give the best results, because the velocity along the boundary is low and probably below the critical velocity. I would also remind the authors that the shape of the velocity curve is well known in connection with round pipes. It has been shown by very careful experiments that, not only is the ratio of the mean to the maximum velocity almost constant, but also the position in the tube at which that mean velocity occurs is very nearly constant; that is to say, the radius of mean velocity is very nearly independent of the velocity and of the diameter of the tube. Therefore for accurate measurement it is desirable to use a round pipe for the air to pass through and not a rectangular pipe such as the authors used. There would be no difficulty I think in extending the tube slightly and making it round. I notice that although the losses are given in some detail, the sizes of the machines to which these losses refer are not mentioned. I hope that the authors will give these in their reply, and that they will include also the dimensions of the tube shown in Fig. 2. Among instruments for measuring the velocity of fluids, the authors do not mention the pneumometer,* which has been developed and used with considerable success in Germany. The action of the instrument depends upon the condition existing on the two sides of a disc placed at right angles to the direction of the stream the velocity of which is to be measured. On that side of such a disc which faces the current and close to the centre of the disc, the pressure of the fluid is given by the formula $p=v^2 \rho/2g$, where ρ is the density of the fluid. On the opposite side of the disc it has been found by careful experiments that the pressure is lower than the above in the ratio of 1 to 0·37. Fig. F shows two forms of the

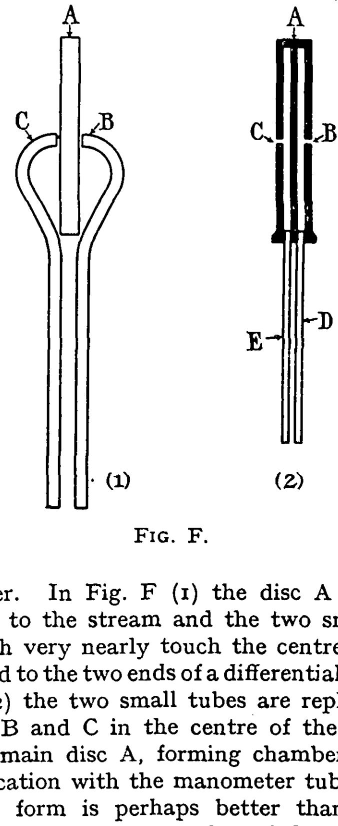

FIG. F.

pneumometer. In Fig. F (1) the disc A is placed at right angles to the stream and the two small tubes B and C, which very nearly touch the centre of the disc, are connected to the two ends of a differential manometer. In Fig. F (2) the two small tubes are replaced by the small holes B and C in the centre of the discs which enclose the main disc A, forming chambers which are in communication with the manometer tubes D and E. This second form is perhaps better than the other. In either case when the two tubes of the pneumometer are connected to the two ends of a manometer the pressure which the latter reads is $p=1{\cdot}37\, v^2 \rho/2g\, \rho_1$, where ρ_1 is the density of the manometer fluid, from which the velocity of the fluid can easily be determined. The great advantage of the instrument is that inaccuracies due to variations in the static head are entirely eliminated, and that the velocity is measured practically independently of any other condition.

Mr. F. J. Teago (*communicated*): This paper de-

* *Proceedings of the Manchester Literary and Philosophical Society*, 1914, vol. 58, part 2.

scribing a method of calculating the losses in turbo-alternators is of great interest to me especially as I was concerned with similar experiments, using thermometers and anemometers, some 8 years ago. Retardation tests were also being used at the time and the object of the tests was to determine the magnitude of the stray loss. The authors are to be congratulated for having put the advantages of air-heating methods so clearly before us, and for having taken the trouble to show fully the derivation of all formulæ used. Table 3 shows that, taking the air-heating method as correct, the calibrated air-heating method gives results about 3 per cent high and the American method gives results about 4·5 per cent low. Now, to say with any certainty that the air-heating method is more exact than either of the other two, one would have to be certain that the measurements involved were capable of being made with great accuracy. Since the authors had to baffle the discharge trunk to obtain satisfactory anemometer readings it would probably have paid to use a Pitot tube in a trunk baffled at both ends as described by me in a short account* of some experiments made in the Electrotechnical Laboratories at Liverpool University. Most people are convinced that the Pitot tube, when correctly proportioned and properly handled, is a very exact instrument and much superior to the anemometer. The main objection to the use of baffles is the increased resistance they offer to the flow of the air. Provided, however, that the amount of baffling employed was as small as was consistent with precision in the readings, a Pitot tube arranged in a suitable trunk would constitute an almost ideal piece of apparatus for accurate commercial testing. One thing the authors do not emphasize is the objection to the use of mercury thermometers on alternating-current machinery where reliable readings are desired. It would be of great value, since the constructional details of turbo-alternators vary so little and since forced ventilation is so widely used, if the authors could extend the range of their experiments and obtain a curve similar to that shown in Fig. 16 of my paper.

* *Journal I.E.E.*, 1914, vol. 52, p. 563.

(The authors' reply to the discussion will be found on page 488.)

North-Eastern Centre, at Newcastle, 24 March, 1919.

Mr. J. Rosen: I am pleased that the authors pay particular attention to stray losses on load—at present there is usually some discussion between the alternator designer and the turbine builder as to the efficiency of the alternator. As is well known, when the efficiency is taken by the "summation of losses" method it is higher than the actual efficiency. A tolerance of 1 per cent is usually allowed on the guaranteed figures. I think it is preferable to give the actual efficiency including all load losses. This is, after all, the desired efficiency, as the steam consumptions are based on these figures. The stray losses are dependent on many factors. Machines designed some years ago for good regulation had comparatively low stray losses, and in some cases these perhaps would not exceed 10 per cent of the no-load iron loss, but under these conditions the exciting energy was greater with the corresponding increase in the size of the alternator. The authors state that the stray loss may become a limiting factor in the size of large turbo-alternators; with this I am in agreement, not necessarily owing to the magnitude of the loss, but that on load local heating occurs causing "hot spots." It is therefore most necessary not only to measure the magnitude, but also to locate the source of heating and take steps to minimize or eliminate the loss. This is a point I would emphasize. Several sources of loss will at once come to mind, and perhaps the most important are the eddy-current losses in conductors—more particularly the joints—in the pole face, rotor end caps, stator teeth, winding clamps, and studs. There may be a large loss in the casing and end shields, but although this should be kept as low as possible, the results are not serious from a reliability point of view, as the heating in these parts does not affect the insulation, the most vulnerable point of an alternator. The separation of the losses is of great assistance in the design. One case occurs to me of a 500-kw. single-phase alternator, the temperature rise being found to be higher than expected. On careful testing it was found that the windage loss was 31 kw. or $6\frac{1}{4}$ per cent of the output. By the fitting of suitable baffles this loss was almost entirely eliminated. The retardation method was used in this instance. This method is not referred to in the paper, but I think it should not be overlooked.

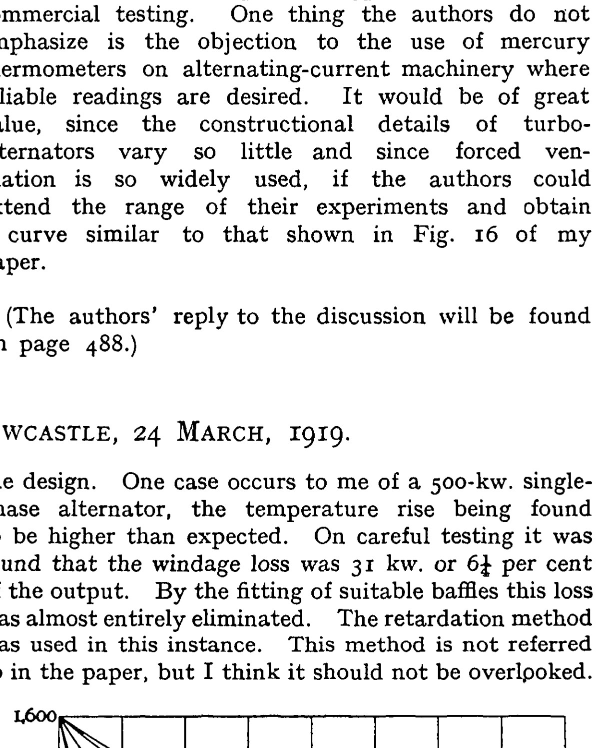

Fig. G.—Retardation curves for 2,500-kw. 1,500-r.p.m. 100 ∿ single-phase turbo-alternator.

It is necessary to know the stored energy of the rotating parts, and this figure can be readily checked by repeating a test with a known load on the alternator either at normal voltage or, better still, at a fraction of the voltage only, when the stray losses under this condition are negligible. In the case mentioned before, it was not possible to measure the quantity of air passing, as the machine was of the open type, and the results are the more interesting as the tests were made on a single-phase set; if no accurate means had been available for locating the loss, it is quite possible that this loss would have been put down to other causes and no steps taken to eliminate it. Fig. G shows a typical set of retardation curves. The difference in the losses calculated from curves 1 and 2 gives the windage and friction losses and from curve 3 the no-load iron loss is calculated: allowance is made for the excitation loss. Curves 4 and 5 give the retardation at loads of 252 kw. and 450 kw. and this result acts as a check to the calculation of the stored energy of the plant. No reference has been made to obtaining the loss by the measurement of the steam pressure at the initial row of blades of the turbine and obtaining the Willans line. This has given good results and the loss can be obtained without a lengthy run, and the method is found to be very accurate, but precautions must be taken, as in the other methods,

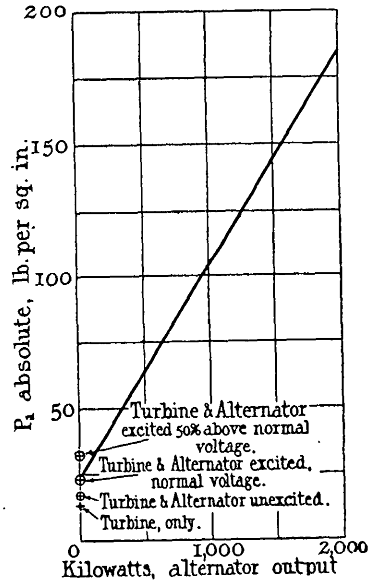

FIG. H.—Curve for 2,000-kw. alternator.

more especially to see that the steam conditions are identical throughout the test. Fig. H illustrates a Willans line obtained on a 2,000-kw. reaction turbine. P_1 is the pressure measured at the initial row of blading and is directly proportional to the total quantity of steam passing; as the turbine efficiency ratio measured on P_1 is found in practice to be constant, it follows that P_1 plotted on the base of kilowatts gives a straight line which is the well-known Willans line. By measuring P_1 with the alternator unexcited, excited and uncoupled from the turbine, it is an easy matter to estimate and separate the alternator no-load losses. Results have shown that the anemometer is the best instrument to be used for measuring the velocity of the air. The authors have already shown the disadvantages of the Venturi and Pitot tubes. The electrical methods have similar disadvantages, as it is practically an impossibility to eliminate air eddies, which will certainly give a false reading. I had occasion recently to measure the quantity of air on a test in which the air was heated electrically and then cooled. The quantity of air was checked by means of an anemometer, and also by the heat passed into the air and absorbed by the cooling medium—the results were uniform and gave the air quantity within 4 per cent. I do not agree with the authors that a temporary trunk should be provided, and its use should be avoided if possible. Some of the most informative tests have been carried out with the machines on steady commercial load, and readings taken by the engineer, who was given instructions how to obtain the temperatures under the various load conditions. All he has to do is to measure the temperatures of the air inlet and outlet and take a copy of the station log. Previous to this, of course, the quantity of air will have been accurately measured. I have been able to obtain most interesting figures under these conditions at steady loads up to full load and at varying power factors. In the case of the larger sets now under consideration, separate motor-driven fans will no doubt be employed. With a solid type of rotor where the disturbance due to the rotating parts is small, the quantity of air could be checked by passing direct current through the rotor at standstill, or through the stator, or through both these parts. There are disadvantages attached to this method, but I have no doubt that it will at least be a useful check on the other tests.

Mr. E. Fawssett: The author's methods appeal strongly to the practical man, since they allow of the machine being tested under commercial conditions on the actual load corresponding to the specified performance. This is very necessary as the "stray loss" which is the indeterminate factor in other methods certainly varies largely with conditions that occur in normal running, e.g. I have found the stray loss in end-bell heating vary very considerably through the normal range of power factor (unity to 0·7) at constant kilovolt-amperes. I am surprised at the considerable praise given to the anemometer, and also at any maker guaranteeing 1 per cent on any such instrument. I should like to know over what velocity range this held good. Though not seriously affecting its use for the purposes in this paper, the remarks on its inaccurate registration of varying velocities are interesting. I have found such error to be of the order of 5 per cent on a ¼ minute run with velocities varying from zero to a maximum: but I disagree with the authors when they ascribe it to "inertia." Certainly inertia is involved in the meter's overrunning, but it is not the cause of the acceleration and deceleration not balancing, since an induction watt-hour meter has a rotor with inertia but next to no air-braking, and does not exhibit this defect within the limits of accuracy of a careful laboratory test, say 0·1 per cent; rather is the anemometer's failing due to the varying conditions in which the rotor has to work when accelerating in the stream of air and decelerating, the acceleration being dependent on the difference between the wind and vane speeds. I have no confidence in the anemometer, and even curve A in Fig. 4 looks to me as if we may expect considerable errors in its use, wherefore I much prefer the calibrated air-temperature method, especially as no previous data are really necessary since either (1).

the machine can be run up from turbo end, synchronized in, turbo slipped by Messrs. Parsons' method, and the input to the machine measured near unity power-factor with special small ratio current transformers, and the rotor loss added in; or (2) when the fan is separate the machine may stand and have its rotor fully excited from another source, though as this would not exceed one-fifth or so of the whole the temperature rise will not be very large and it will take a long time for conditions to become quite steady.

Mr. L. H. A. Carr: I do not care for the use of the term "stray loss" in the sense in which the authors use it. To my mind the indefinite or true stray loss may be divided into two parts, first, the stray loss at no load which is measured with the core loss, and secondly the increase of stray loss when load is put on. The authors use the term "stray loss" to denote the latter part only, and I suggest the term "incremental loss" would be better. In a standard direct-current machine this incremental loss is known to be largely due to the distortion of the flux wave on load, this distortion being accompanied by a higher maximum flux density, and empirical methods can be used with a fair degree of accuracy to predetermine it. With a cylindrical rotor, this distortion should be very much less than with a direct-current machine, since the flux wave can be displaced without much distortion, and I should like to know whether the figures given in Table 6 refer to machines with salient or cylindrical rotors. I should be glad to know if the authors could give any indication of the cause of the stray loss, and whether they are able to give away the secret of how to avoid it by careful design. For example, if a turbo-alternator be imagined, and then the design altered by doubling the air-gap and approximately doubling the rotor ampere-turns, would the stray loss be reduced? The agreement in Table 2 seems to be rather too good, for surely the losses measured by the driving motor include exciter losses which do not enter into the losses obtained from the air heating. I would suggest that there is considerable practical difficulty in holding the anemometer for one minute (as suggested by the authors) over each of 81 squares, due to fatigue of the experimenter and consequent liability to inaccurate placing of the instrument. Has any mechanical arrangement been used for this purpose; and if so, would it not baffle the air? I should be glad to know if the air temperature varies much over the chimney, that is whether the air is thoroughly mixed. This is doubtless interlinked with the uniformity of the velocity over the chimney. I am interested to know that the velocity can be obtained so uniform as shown in Fig. 5. Some years ago I carried out some tests on small fan-ventilated motors with a 6-ft. wooden chimney, but it was found impossible to get rid of a partial negative flow of the air, and so these particular experiments were abandoned. One would be glad to see standard methods of efficiency measurements adopted, as some customers prefer summation of losses methods, and some customers prefer other methods, and while the figure expected for each method is known to the manufacturer it is usually unknown which of them is desired by any individual customer.

Mr. J. R. Beard: From the plant user's point of view the paper is very valuable, since there is no doubt that if a simple and accurate method is available for the determination of alternator efficiencies under working conditions the result will be more accurate predetermination of performance and reduced losses. The classic example of this is, of course, the enormous improvement in fan and pump design when the electric motor drive enabled the power inputs to be easily determined. As a result of more accurate predetermination of efficiency it is to be hoped that some reduction will be possible in the considerable margin asked for on the steam-consumption guarantees of turbo-alternators before the penalty and bonus clause takes effect. Increased alternator efficiency is, however, the more important point now that higher load factors and coal costs make economical generation so essential. In rough figures I estimate that in a modern 50,000 kw. station 1 per cent improvement in alternator efficiency would enable at least £4,000 extra per annum to be earned with the same capital and running costs, and in addition there would presumably be a saving in the capital cost due to the less material required for the more efficient machines. Turning to the actual methods of measurement I have three suggestions on which I would be glad to have the authors' comments. (1) In the calibrated air-temperature method would it not be possible to make the calibration by measuring the increase in temperature caused by heating an external resistance (say of the iron grid type) placed in the inlet. The heat losses in such a resistance could be measured with much greater accuracy than the core loss and the method would be equally applicable to tests on site. (2) I have always been attracted by the method of measuring the flow in water turbines by admitting a measured steady flow of salt solution of known strength at the inlet and analysing the outlet water for the amount of salt per cubic foot. Would it not be possible to apply a similar principle to air measurements by admitting a known quantity of inert gas into the inlet? There is no doubt that the passage through the alternator would thoroughly mix the gas with the air and, I believe, the amount of small quantities of such a gas as CO_2 can be determined with very great accuracy. (3) I incline to the view that in the future a closed air-circuit method of cooling alternators is, for various reasons, likely to be frequently used, the same air being circulated continuously and passed over some form of water cooler. If this is so, simple measurements of the water circulation through the cooler and its temperature rise would give at once an exact measure of the total losses in the alternator. I was interested in the description of the Roberts micromanometer, since some 12 years ago I tried to construct an instrument on this principle for the measurement of coke-oven gas. I found, however, that the surface tension at the surface of the bubble in the small connecting tube vitiated all my results. In addition to an air bubble I experimented with bubbles formed of various liquids which were not miscible with the main liquid, but without success. Perhaps means have now been devised for overcoming this difficulty with the surface tension. I think it would add to the value of the paper if the authors could describe the methods adopted for air volume measurements by the National Physical Laboratory in their extensive aeronautical researches.

Mr. A. Q. Carnegie: This method of determining the efficiency of an alternator by the temperature rise of the cooling air has been used by Messrs. Parsons at various times for quite a number of years, and although we have often felt that it would be better to use an orifice for the measurement of the air, we have been forced to the same conclusion as the authors, namely, that an anemometer is about the only convenient instrument available. The use of an orifice would be extremely difficult, owing to the very cumbersome air ways which would have to be provided, and it would be ruled out by the fact that the fan system attached to the alternator would not be capable of giving the additional pressure required to pass the air through the orifice. I must say I do not like an anemometer—as an instrument—it is extremely delicate, and it requires very special apparatus for its calibration. I think that in any test such as the authors describe, the anemometer should be calibrated just before use, and should be returned for recalibration immediately after. This, I think, is necessary to provide against the possibility of the vanes having been accidentally disturbed from any cause. The instrument should, further, be calibrated in air of the temperature at which it has to work, as I have known cases in which the instrument ran slower and slower as its bearings became warmed up. The second method, which the authors call the "calibrated air-temperature" method, avoids the necessity for directly ascertaining the weight of air passing, and seems to me to have considerable advantages.

Mr. W. G. Turner: The paper gives a very practicable solution of the otherwise almost impossible problem of ascertaining the total losses in large turbo-alternators, and should prove to be of great service to both manufacturers and users of such plant. The remarks which I am about to make are from the standpoint of the latter. A turbo-alternator set is purchased on a guaranteed steam consumption, the accuracy of which can be checked most readily. From this the combined efficiency of the machine is calculated by comparing the steam consumption in lb. per kilowatt-hour with the work which would be theoretically obtainable in an ideal engine working substantially on the Rankine cycle. Hitherto, the checking of the alternator efficiency guarantee has been almost impracticable, with the result that the purchaser has been more or less in the hands of the manufacturer. By the application of the "air-heating" method, which enables the otherwise indeterminable stray losses to be included, the alternator efficiency can be accurately calculated. Further, the mechanical efficiency of the turbine can be deduced from the actual alternator efficiency, and the combined efficiency of the machine calculated as above. The method will be undoubtedly of great importance in cases where the turbine and alternator are purchased from separate contractors, in particular the settling of any dispute which may arise regarding efficiency guarantees. In arriving at the efficiency, however, allowance must be made not only for the bearing losses and the loss due to heat dissipated from the outside of the stator, both of which are mentioned, but also for the exciter losses as well as the kinetic energy in the air leaving the alternator. These losses, whilst being small individually, are of appreciable magnitude when taken collectively. It should be noted that a correction has been made in formulæ (2) and (2A) to cover the loss due to heat radiated from the stator. If the plant is tested after erection a deduction must be made for the extra air-friction losses due to the ducts and air filter. In the testing of large machines we are still left with the problem of a full-load run at the manufacturer's works, which to a great extent depends on the boiler capacity and also the facilities for dissipating the electrical energy generated. The test will generally have to be carried out after the machine has been delivered and erected. The apparatus required is simple, but the test is rather laborious, involving, as it does, a great number of separate readings to ascertain the average velocity and temperature. If instead of the separate readings some means could be employed for one reading only in each case, a great improvement would thereby be effected. When, however, it is remembered that the test will not often be required, possibly only once in the life of each machine, the amount of work involved is excusable. The authors do not mention the alternative of measuring the velocity of the air at the alternator inlet, where one would expect a more equal velocity distribution and direction of flow. By means of a circular opening here the velocity readings might be reduced to a series along a diameter only. Measuring at the inlet might be more practicable in the case of machines discharging at the stator bottom which are usually fitted with ducts leading to the outside of the building. This would not, of course, eliminate the necessity of taking a number of thermometer readings at the outlet to obtain the average outlet temperature. It is interesting to note that an appreciable percentage error in arriving at the heat carried away by the air becomes of decreasing importance as the output of the alternator increases, that is as the efficiency rises, and in any case only affects the efficiency by a very small percentage. For instance, if we assume that the actual loss of 336 kw. as obtained by the American Institute method for machine No. 5802 in Table 6 is the correct loss, then the loss of 352 kw. given by the air-heating method for this machine is 4·8 per cent high. Assuming the load on the machine to be 5,000 kw. the true efficiency would be 93·7 per cent, and the efficiency obtained by the air-heating method 93·42 per cent, which is an error of only minus 0·28 per cent. Now that a simple method of obtaining the total losses has been applied it would be interesting to know whether the authors have arrived at any definite conclusion as to the laws of the variation of the stray losses with the load. To enable the formulæ for Method (1) to be quickly evaluated, I have prepared an alignment chart (see Fig. J) from which the kilowatt loss as calculated by the air-heating method, under any condition and for any size of machine, may be obtained by the simple expedient of drawing crossing lines. These charts are very simple to prepare and might be more often applied. They can be constructed to evaluate complicated formulæ involving a number of variables by a combination of charts connected in threes, the method adopted in this case. In order to explain the working of this chart I give the following example: Suppose V=16,800 cubic feet per minute, and $T_o-T_i=71\cdot6$ degrees F. and we wish to find P in formula (1). We join 1,680 on the V axis to 71·6 on the T_o-T_i axis, and find that the line passes through 36·4 on the P axis, which multiplied by 10 gives 364 kw. loss. If now we wish to correct this for the variation of air density with temperature and barometric pressure as obtained with formula (2), H being, say, 29 inches of mercury, and the average absolute temperature at the alternator discharge say 570° F., we join 36·4 on the P axis to 29 on the H axis and let the line intersect the ungraduated axis at a point which we will call A. We then join 570 on the T axis to A and produce it to cut the P axis, when we get $35\cdot2\times10=352$ kw. loss. The lines must join values in the order given, because the chart was so constructed. The Centigrade portion of the chart is for use only with formula (2A). I have not dealt with the calibrated air-temperature method as this admittedly is not so accurate as the air-heating method, and, generally, would be used only as a rough test of efficiency under service conditions, rather than in checking the guaranteed efficiency.

Mr. H. B. Poynder: A few points occur to me in connection with the paper. In the first place, Tables 6 and 7 show the stray loss or, as I, like an earlier speaker, should prefer to call it, the incremental loss, determined for three different machines at full load by the air-heating method. The incremental loss as thus obtained is, especially in the smallest machine, the difference between two relatively large quantities, so that it will be very sensitive to comparatively small errors in the latter. If now the authors could submit figures showing a number of different values between no load and full load, the plotting of a curve connecting the incremental losses with the current passing would give a very clear indication of the degree of accuracy

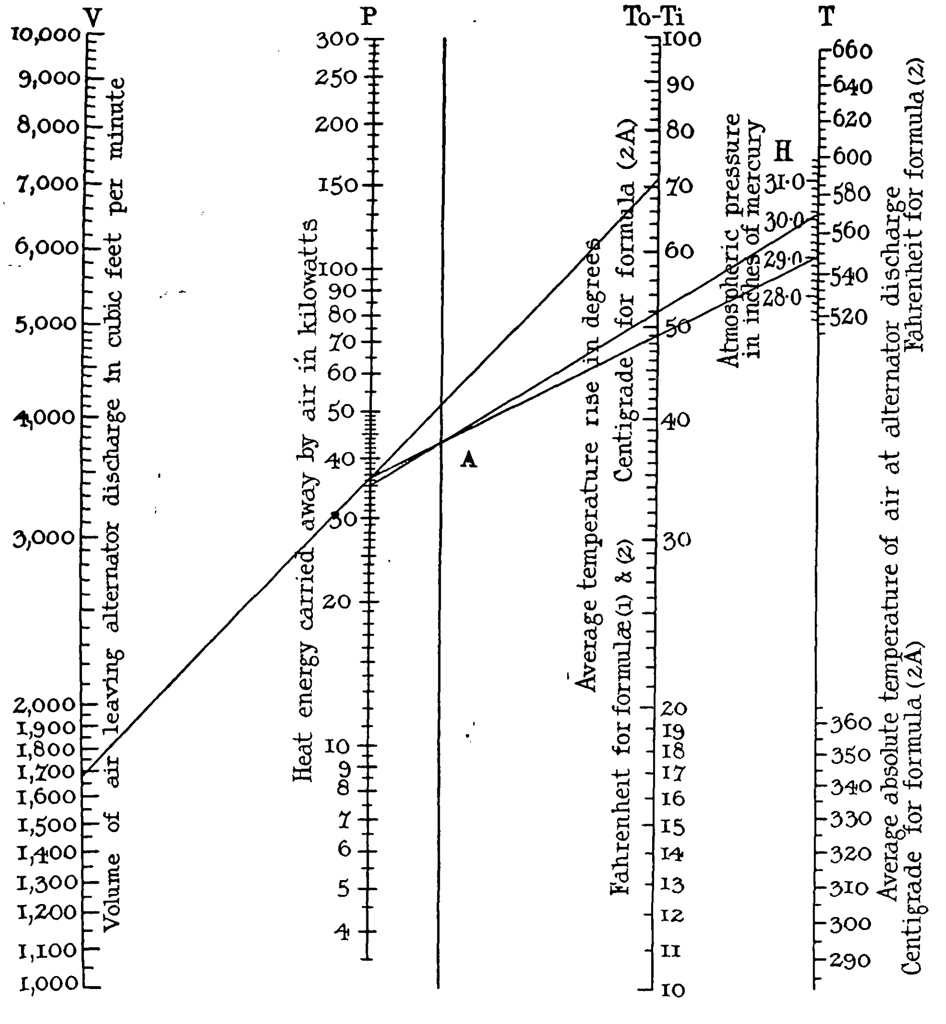

FIG. J.—Chart giving losses in kilowatts as measured by the "air-heating" method.

of the air-heating method ; it would, in fact, exaggerate any errors. If the points fall well in a reasonably smooth curve and show zero value for the increment at zero current, we should then have a stronger vindication of the accuracy of the method than could, I think, be obtained by any other means. In addition, the shape of the curve would give us valuable information about the incremental losses. In direct-current machinery they have been found to vary directly as the current squared, and they presumably follow a similar law in an alternator. I think that, from some points of view, an unnecessary mystery is made of the incremental loss. Taking first the case of a direct-current machine, imagine an armature fitted with brush-gear but without a field frame ; a certain applied voltage is necessary to force a given current through the armature when at rest. If it is now rotated a higher voltage will be necessary, quite apart from any change in the contact resistance of the brushes. The loss will not exert any resisting torque, which is perhaps more clearly seen if one assumes that, instead of rotating the armature, the brushes are rotated. We have here a relative movement between the armature and the magnetic poles, with consequent hysteresis loss in the iron and eddy-current losses in the iron and copper. If now a field frame is superimposed, unexcited, with the poles symmetrically disposed midway between the armature poles, the reluctance of the magnetic circuit will be considerably decreased and the loss consequently increased, but provided that the number of lines of force are all cross-magnetizing, or in other words that the armature current does not produce any flux through the yoke, there will still be an absence of external torque, and the increased loss demands an increase in the voltage applied. Similarly we may consider an alternator stator without a rotor. If a three-phase voltage is applied to the stator, the hysteresis and eddy-current losses do not set up an external torque ; and if the rotor be now inserted and driven synchronously, unexcited, and free to set itself at what angle it likes, there will still be no external torque exerted, although the losses will have been increased through the reduction in the reluctance of the magnetic path. In direct-current machines I have been in the habit of considering the incremental losses as equivalent to a resistance loss for a given winding, speed, and air-gap length, determining the equivalent value in ohms and adding it to the ohmic resistance of the armature, using this convention not only for calculating the loss but also for calculating the voltage drop in the armature. This is based upon the assumption that the main flux is of normal value, and it will be very interesting to hear from the authors how the incremental loss varies with the main flux, that is, whether and to what extent the incremental loss in the unexcited machine is affected by superimposing the main flux. In my opinion if the incremental loss with zero main flux is proportional to the square of the current and causes a voltage drop rather than an external torque, then it is to be expected that the incremental loss with full excitation will have the same characteristics. It should be noted that, if this is so, the real incremental loss is less than the apparent, since the voltage drop due to the real loss should be taken into account in calculating the true value of the main flux. The incremental loss can also be considered to be the difference between the losses due to the main flux and that due to the resultant of the main and armature or stator fluxes, but to calculate it on this assumption requires an exact knowledge of the distribution and maximum values of the resultant flux, which is probably much more difficult than to treat it as a function of the armature ampere-turns, assuming that the latter method gives sufficiently accurate results. The incremental loss has been recognized, and empirical formulæ based upon (armature ampere-turns $\times$ speed)^2 have been in use for nearly 20 years by machine designers, and it appears very remarkable that it has not previously been more fully investigated. I trust therefore the authors of this paper wll give us an authoritative statement which will carry our present knowledge a very substantial step forward. The only other point which occurs to me is in connection with the method adopted for measuring the temperatures and the flow of air. A previous speaker has referred to the difficulty and trouble involved in taking so many thermometer and anemometer readings. It would undoubtedly be an advantage to be able to get accurate results with a single reading, and it would appear possible to obtain this by the use of three similar multi-wire grids each composed of a suitable resistance material. Calling these A, B, and C, A would be fixed at the inlet and B and C at the outlet, one above the other. A and B would only carry the minute currents necessary to obtain resistance readings, and the difference in their resistances should give a very accurate measure of the air temperature-rise. A current which would be passed through C would be adjusted to such a value that its resistance, and therefore its temperature, are some convenient definite amount above that of B. Grid C being thus maintained at a constant temperature above the air passing through it, the velocity of the air can then be calculated from the number of watts expended in C. This method would not necessitate the use of the screened wire used by Professor MacGregor-Morris, an objection to which seems to be the obstruction caused by the screening, if a multi-wire grid is adopted with the purpose of obtaining true average results. I realize, however, that the above method is subject to error due to obliquity of the stream lines, and it is only by actual experiment that it would be possible to determine whether this disturbing cause has in fact any serious effect provided that the screen is kept well up the trunk and baffles fitted as described by the authors.

Mr. W. T. Maccall : Has iron been tried for the electrical method of measuring air velocity, and if so is it satisfactory ? In case it is, a network could be used to give the average velocity of the air by a single reading, whereas platinum might be too expensive for this purpose. The Wheatstone bridge method could be used by dividing this network into two sets of wires at right angles and having two similar resistances in still air. Similarly the average temperature of the air could be found by a single measurement of the resistance of a network. In using the anemometer, what difference is caused in the readings when the air is not moving perpendicular to the cross-section, and is any allowance made for this? Does not the allowance for radiation (and other forms of heat dissipation) from the stator frame vary with the size of the alternator? In particular would it not be a considerably greater percentage for a small machine?

(The authors' reply to the discussion will be found on page 488.)

NORTH MIDLAND CENTRE, AT LEEDS, 8 APRIL, 1919.

Mr. R. H. Campion: Undoubtedly this paper will help to bring about a standard method of determining heat losses. The method of measuring the temperature rise and the air flow is a good one. It will help with tests when taken on site. The fact that the losses in some 20,000-kw. sets are something like 1,000 kw. shows that it is necessary to agree upon some standard of measurement; and although this paper gives results on a 5,000-kw. set that has forced ventilation, with the exception of a relatively small amount of heat dissipated from the external surface of the stator all the losses except the bearing losses are carried off by the cooling air. The alternative method is described as the "calibrated air temperature" method. There is another point, that the bearing losses may be determined fairly accurately by measuring the volume and temperature rise of the lubricating oil, and by making a suitable allowance for the heat dissipated in other ways. The method advocated in the paper should be adopted to provide a continuous record of a turbo set when in service, and any foreign matter getting into the air ducts would be detected. There is a point I should like further information about, namely, the effect of filters in connection with the determination of the efficiency of the turbo-alternator. I have tried the cloth and the wet type and have come to the conclusion that a combination of the two will give the best results, but I have not been able to get any further load out of the sets by supplying cooler air.

Mr. W. M. Selvey: This paper is long overdue. The method of air calorimetry was first accurately explored by Professor Threlfall in the paper referred to by the authors. As regard other methods, I should like to associate myself with Captain Law's remarks (see page 306). We have both been closely interested in a line of alternators the efficiency of which was adequately determined by these methods. The ordinary acceptance test, however, is often the only opportunity for determining the efficiency of the alternator while running at an ascertained load, and moreover is probably the only occasion in the life of the machine in which it is run under absolutely steady and defined conditions. I have long felt it necessary to make some test, however approximate, of the alternator efficiency which can be determined at the same time as the steam consumption tests. The method of air calorimetry plus that of water calorimetry for the bearing losses is practically the only method available, and what therefore has to be considered is whether this can be carried out with any degree of approximation without entailing elaborate apparatus. I think it can. Fortunately, one has to determine the inefficiency, i.e. the losses, and therefore an error of 10 per cent in the actual measurement only signifies an error of ½ per cent in the actual efficiency ascertained. I do not think it is necessary to have so high an error as 10 per cent in the method outlined by the authors, which I have used myself for a number of years without, however, going into quite such refined details. This refinement can only be carried out if the importance of the determination justifies considerable expense, in which case one would run a test lasting 8 or 10 hours under dead steady load for the purpose. The only occasion I can imagine where such a test would be needed by commercial requirements would be where the turbine and the alternator were supplied as separate contracts, and the total result not being entirely satisfactory it became necessary to determine to what extent each contractor had fulfilled the specification. I have brought with me some curves and figures showing the actual results of anemometer measurements on arrangements similar to Fig. 3. In the cases I have taken I have given the velocities measured by anemometer at 16 places. In the first case the plain opening was used for the discharge, but it was of a fair depth. In the other case an additional trunk was placed on the outlet as shown in Fig. 3. There is certainly a good deal of steadying motion by adding the trunk, but no alternator maker would, I imagine, agree to place the obstructions of expanded or perforated metal in the path of the air current unless the air were supplied by an external fan and the actual water-gauge pressure drop across the alternator had been specified. The simplest way of steadying the flow would be to put a few division plates on very thin tin across the flow, edge on, two sets being arranged at right angles and not necessarily immediately one on top of the other. In the figures I have given for the case without the trunk the greatest variation of velocity from the mean is 26 per cent, while with the trunk added the greatest variation from the mean is still the same, although the general flow is steadier. It may be of interest to state that in a recent test where the actual efficiency of the alternator was stated the following are the figures obtained by the air calorimetric method of measuring the sum of the iron, copper, and windage losses, and by the oil-water calorimetric method of measuring the bearing losses.

	Half Load %	¾ Load %	Full Load %	Over-Load %
Alternator efficiency (excluding bearings)	90·9	92·5	94·7	95·3
Alternator efficiency (including bearings)	89·6	91·6	94·0	94·7
Efficiency of alternator stated in contract	90	92·5	94·0	—

I give the figures both excluding and including bearings, the amount added for bearings being one-half of the total for the four bearings. I think, however, the heat from the turbine conducted along the shaft really makes the loss in the turbine bearings to be higher than in the alternator bearings, and therefore it is rather against the alternator to add one-half of the bearing losses. It will be seen that the figures show very good agreement. I do not think, however, that this is entirely a perfect method, and had I to make a test in which great importance was attached to accuracy I should endeavour to develop the method used in Thomas' gas meter, viz. of adding a known number of watts electrically to the "unknown" air either before or after the alternator and of measuring the rise of temperature produced by this added energy. Many alternators are now fitted with wet air filters, and it would, I think, be necessary to use two heater resistances, one just past the eliminator plates to make certain of evaporating all the moisture carried over, and a second one fairly near to the alternator for the purpose of adding the "known" watts to produce the required rise in temperature. Temperatures before and after the heater and at the exit from the alternator would have to be taken by a resistance thermometer, say by stretching a number of very fine copper wires on a frame in such a way as to offer no appreciable resistance to the air current, and at the same time well spread about the section to get the average temperature of the air. Such thermometers could be quite well home-made. All that is necessary is some fine copper wire and a good Post Office bridge with a fairly sensitive galvanometer. Not a very large rise in temperature need be caused by the preheat in the air, since temperature measurements by using resistance thermometers are so easy to make that probably only one-tenth of the main rise would have to be caused by the heating coils; that is to say for a 5,000-kw. alternator probably about 25 kw. would make quite a workable test. Very simple calculations would therefore give the actual alternator losses from three temperatures and one volt-ampere measurement of the added watts. I do not think this method could be developed for ordinary commercial testing on site, but it could be very well used for research work or for any special case which may arise, in which the matter is of importance. It would seem by the comparison of the results I have given with the calculations that there is not as a rule much difficulty in predicting with reasonable accuracy what the efficiency of the alternator is likely to be.

Mr. F. A. Lauper: Most of us younger designers have not the chance to work on big machines of 1,000 k.v.a. output, and therefore we cannot have so much to say about how to determine these efficiencies. But I am sure we all take great interest in these new and improved methods and we can make use of such information always to some extent also for much smaller units. I think that for pipe-ventilated or forced-ventilated motors, alternators and transformers, the given methods can be used with much advantage. One of the most interesting points of course is that this method enables us to determine more accurately the stray losses. The stray losses are very complicated, especially to a young man with little experience. We cannot find much published information about these losses, although probably many designers have their own approximate methods of how to get at them. We can use a certain coefficient such as is given in textbooks or, better, as found from recent tests on actual machines, but we find that it alters very much even for the same kind of machine built up to the same specifications but at different times. I have tested personally some 20 machines ranging between 200 kw. and 2,000 kw., and found that for motors and alternators of these sizes these stray losses were not so important as those given in this paper for big turbo-alternators, i.e. up to 5 or 6 times the stator copper losses. They usually amounted to only from 20 to 40 per cent of the ohmic losses in the armature. Of course single-phase alternators showed such losses to be as high as 100 and 180 per cent of the armature copper losses. We made our usual tests to get at these stray losses with the short-circuit run; that is to say, we excited the short-circuited alternator until full-load current flowed in the armature. When we have had the chance we have driven the alternator with a calibrated motor also on full load. We found that the figures arrived at in both cases generally agreed fairly well; sometimes they were far out, especially on single-phase alternators and motors. Still they seemed to agree very well for the smaller size. We found, however, that these stray losses on short-circuit must be differently distributed from those occurring on full load and also if the alternator is run as an over-excited synchronous motor. I remember a particular case where the local heating was very marked. The machine in question was a high-speed single-phase alternator with solid poles, and we ran it on short-circuit. For some reason we had to shut it down and dismantle it; I think it was due to bearing trouble. We found then that on one and always the same side the fore-running corners of the pole-shoes were heating excessively, in fact got quite blue, which would indicate a local temperature rise of approximately 250 to 300 degrees C. We polished the pole-shoes again and replaced them. The machine was then at about full load for several hours driven by a calibrated motor. After this run the alternator was examined and the pole-shoes were found to have remained absolutely bright. On repeating the short-circuit test, after only a very short run the pole-shoes showed local heating as previously observed. The stray losses determined from both tests agreed fairly well, but obviously these losses were quite differently distributed in each case and caused local heating on the short-circuit run. I think these stray losses are so complicated and important that those members who have more opportunity should give a little more information about such losses so as to help us younger men to obtain better and more accurate results in the future.

Mr. A. Imbery: It would appear that by the addition of the baffle plates and by the reduction in area of the outlet owing to the use of the truncated body, the resistance to the flow of air through the outlet would be increased, thus producing an increase in the temperature of the air expelled from the machine. If we increase the temperature of the outgoing air, the loss in the iron and copper would more than compensate for the increase in efficiency obtained by the use of the truncated body. Another point is that by reducing the temperature of the air on the inlet side, we should be able to reduce the velocity of the air through the machine and probably save something on the present windage losses, in addition to reducing the iron and copper losses to some extent. I should like to know if any system has been used in this country for ventilation of alternators, where the air before entering the machine is passed through a special form of refrigerator or other suitable apparatus, so that the temperature of the incoming air can be maintained at an extremely low figure. I think that if this arrangement can be carried out satisfactorily the iron and copper losses could be kept at a minimum and it might be possible to obtain an increased overload capacity from the machine with the same temperature rise as at present.

Dr. S. F. Barclay and Dr. S. P. Smith (*in reply to the discussions before the North-Western Centre, the North-Eastern Centre and the North Midland Centre*) : As far as possible, the questions will be replied to collectively, and repetition of replies to the discussions before the Institution and the South Midland Centre (see page 313) has been avoided. Further, the remarks of many of the speakers do not call for a reply, but we should like to express our thanks to those gentlemen for their valuable contributions.

Stray loss.—Mr. Fawssett's experience that the heating of the end-bell varies with the power factor tends to confirm our view that the difference between the distribution of the heating on load from that on short-circuit is largely due to the phase displacement of the current. This view is further supported by the results of Mr. Lauper's tests. Messrs. Carr and Poynder object to our use of the term "stray loss," but we only employ the term in its generally accepted sense. All the machines of which test results are given had cylindrical rotors with the pole-centres unslotted—in such machines the flux wave may be considerably distorted on unity power factor. The very interesting matter discussed by Mr. Poynder falls outside the scope of the paper, which is intended to deal with the measurement of, rather than with the cause of, stray loss.

Criticism of anemometer.—The contributions by Dr. Cramp and others are very interesting. With uniform air flow there is no difficulty in measuring the air velocity very accurately, but the whole problem is whether perfectly uniform flow must be obtained or whether results sufficiently accurate for the purpose can be obtained when an anemometer is used in an air flow that is not quite uniform. Some speakers do not appear to have grasped that great accuracy, say within 1 per cent, is not required. Mr. Fawssett's surprise at the statement that the anemometer we employ is guaranteed not to vary more than 1 per cent, may be due to the fact not being appreciated that this is the error after the correction for friction has been made. This correction is practically constant over the whole range of the instrument and is determined by calibration at the National Physical Laboratory. If this constant correction is ignored, the inaccuracy of the instrument appears to be very considerable, varying in accordance with a straight line law from 100 per cent at about 60 feet per minute to 3 per cent at about 2,000 feet per minute in the type of instrument we employ. With regard to Mr. Carnegie's remarks, it should be mentioned that the anemometer can be readily calibrated and, as seen from Table 3, the one used by us gave very reliable results after two years' use.

Resistance grids.—Messrs. Walker, Frith, Beard, Poynder and Selvey appear to favour the use of resistance grids, but only Dr. Walker pointed out the special arrangements needed for mixing the air in the heater. The insertion of the apparatus between the filter and the alternator would often entail considerable expense and inconvenience ; while a rapid variation of 1 degree in the temperature of the air near the power station might cause very misleading results when the loss in the grid increases the air temperature by only 5 degrees, as proposed by Dr. Walker. While we fully appreciate the value of a resistance grid under the steady conditions obtainable in the laboratory, the calibrated air-temperature method is to our mind much better for commercial testing.

Measurement of temperature.—Dr. Walker remarks that the difference between the effective and the actual area of the discharge trunk may be considerable, but we have not found this to be so. Experience shows thermometers to be very suitable for determining the average temperature of the air at inlet or outlet with accuracy ; moreover, thermometers are usually available in fair numbers. We have found close agreement between the mean temperature determined in this way with that obtained from the change in resistance of a coil of wire mounted in the air flow, as mentioned in the paper. The proposal to use a large number of thermo-couples has been considered by us, but we preferred to bring out all the ends to a common terminal board whereby any couple could be connected to the indicating instrument at will. Connecting a large number of thermo-couples in parallel is very simple, but theoretical investigation is necessary to show under what conditions correct readings would result. In response to Mr. Carr's request, we have appended the following table to show what variation of temperature over the outlet may be expected when the air flow has been made fairly uniform.

Manometers.—Mr. Frith and other speakers raised the question of manometers. The reasons which led us to the conclusion that any of the known forms of this instrument was unsuitable for our purpose have been given in the paper, but it may here be added that during these discussions the most conflicting views on this subject have been expressed. Certainly under existing conditions we could not propose the use of micro-manometers for the commercial testing of turbo-alternators.

Pitot tube.—Many speakers have expressed surprise that the various forms of the Pitot tube have been rejected by us in the standard methods of test which we have advocated. We can only repeat, however, that while the Pitot tube can be regarded as an instrument of precision under ideal conditions, it is greatly inferior to the anemometer under the conditions which exist in turbo-alternators.

Duration of test.—This matter has been very prominent in the discussions, but our own experience has not shown the time taken to carry out the tests to be unduly long, nor has the labour involved been incommensurate with the importance of the tests.

Turning now to the other points that have been raised, we think it is regrettable that the method adopted by the B.E.A.M.A., as outlined by Mr. Peck, should not have progressed beyond the conventional method put forward some years ago by the American Institute, and it is hoped that the fears expressed by him will have been expelled by the paper and the discussion thereon. It must not be forgotten that the calibrated-motor method does not provide a means for testing the machine on load. To Mr. Frith we would only the total loss or efficiency is required. The presence of free moisture referred to by Mr. Grime would vitiate the results obtained by any air method, but an error due to this cause is not to be feared with a filter which is working properly. In reply to Dr. Cramp, we would point out that the loss due to radiation is but a small fraction of the whole and that the rule given in Appendix I is good enough for most practical purposes. Mr. Rosen will find that reference to the retardation method was made in the paper (page 301). Without a trunk it was found that the results obtained were often unreliable, consequently we think that a trunk ought to be specified in a standard method of testing where the velocity or temperature of the air has to be measured. Mr. Carr criticized the figures in Table 2. We would point out that these were the figures obtained after the necessary corrections had been made, while in every case the rotor was separately excited. Mr. Beard suggested the addition of a small, known amount of carbonic acid or other gas. As far as we know, this attractive proposal has never been tried, but it is doubtful whether the mixing could be thorough with some schemes of ventilation. In any case, simpler methods might be preferable in practice. The Pitot and other tube methods used in aeronautical laboratories for measuring the air velocities in wind channels are very exact, but quite unsuitable for the conditions which we are now considering. We are much obliged to Mr. Turner for his remarks and the chart reproduced in Fig. J. Measurements at the inlet are seldom practicable—in many cases the only opening available at the inlet is the opening to the filter; also the flow at the inlet is often less uniform than at the outlet. In reply to Mr. Maccall's query, we do not know whether iron has been tried in the electrical method of measuring air velocity. Our reply to Mr. Campion would be that, when a filter cools the air, either the temperature rise of the alternator for a given load would be reduced, or for a given final temperature the output from the machine would be increased. Had Mr. Selvey used

TEMPERATURES TAKEN AT OUTLET OF 5,000-KW. SET.

Turbine End of Alternator.

°F.	°F.	°F.	°F.	°F.	°F.	°F.	°F.	°F.	°F.
115	114·5	113	111·5	109	108	109	110	111	111
117·5	117	114	111	109·5	108	109·5	111	113·5	114
120	120	116·5	113	111	110·5	111	114	117	117
123·5	122	119·5	117	116	115	115	117·5	120	120
123	122	122	121	120	119·5	119	120	121	120
121	122	122	121	120	120	120·5	120	120	117·5
120	122	121	119	115·5	114	114	116·5	116	115
116·5	118·5	117	114	111	110	110	112	113	112
113·5	114	114·5	112	109	105·5	107	109	109·5	109
110	110·5	111	110·5	109	107	106	106	106·5	106·5

Exciter End of Alternator.

NOTE.—All readings taken in about 4 minutes. Temperature did not vary while readings were taken. Discharge outlet of trunk divided into 10×10=100 squares.

say that we have no test results which would enable us to form a definite opinion of the suitableness of Professor MacGregor-Morris's instrument for measuring the velocity of the air leaving a turbo-alternator. With reference to Mr. Kuyser's criticism, it must be remembered that the windage and fan losses are not known apart from the bearing losses, and that further tests are necessary to enable us to separate these losses. Consequently, if this additional information is not available—as is usually the case—it is necessary to carry out two tests to calibrate the machine; for example, one test with the rotor unexcited, and the other with a known iron and excitation loss. We concur with Mr. Juhlin in regard to the importance of measuring the losses separately, but a calibrated motor alone is not sufficient for obtaining the stray loss which occurs on load, or the windage plus fan loss apart from the bearing loss. To find these losses, methods such as those indicated in the paper are essential. Though the air volume can be found by the air-temperature method, this determination is not necessary when the precautions prescribed, we think that he would find no need for the alternative grid method. To Mr. Imbery we would say that the restrictions in the trunk did not affect the air flow appreciably. If a cooler were used, it would doubtless be possible to reduce the volume of the cooling air, but it is questionable whether this would have any marked effect on the losses.

In conclusion we should like to express our appreciation of the very helpful discussions which have followed the reading of the paper before the Institution and four Territorial Centres, and we earnestly hope that the wide interest shown in the subject will result in the adoption of a method for the determination of the efficiency of the turbo-alternator which is based on the measurement of the actual losses and not on conventional quantities.

PROCEEDINGS OF THE INSTITUTION.

631ST ORDINARY MEETING, 10 APRIL, 1919.

(Held in the Rooms of the Institution of Civil Engineers.)

Mr. C. H. WORDINGHAM, C.B.E., President, took the chair at 6 p.m.

The minutes of the Ordinary Meeting held on the 3rd April, 1919, were taken as read, and were confirmed and signed.

A vote of condolence was passed with the family of the late Sir William Crookes, O.M., F.R.S., Past President, the members standing in silence.

Messrs. J. R. Bedford and E. Eugene-Brown were appointed scrutineers of the ballot for the election and transfer of members, and, at the end of the meeting, the result of the ballot was declared as follows :—

ELECTIONS.

Members.

Bell, Frederick John.
Gray, Andrew.
Mason, Charles Edward.

Associate Members.

Adkins, Frederick Sydney.
Austin, Nathan John.
Bayley, Benjamin Croft.
Bentley, George William.
Bowley, Edward Ernest.
Burton, Arthur Cooksey.
Cameron, Ernest Gordon.
Chaytor, Henry Joseph
Christmas, Ernest Napier.
Clark, Frederick George.
Edwards, William Douglas.
Fenton, Herbert.
Gaccon, Evan Charles.
Gerrard, Herbert Rueben.
Grey, Arthur Wallace.
Healy, Henry William.
Hill, Ernest William.
Hudson, James Barcroft.
Hutchings, Arthur.
Kernick, Henry Williams.
Lawrence, Walter.
Lewsley, John Wilfred.
McDonnell, Edward Clement.
McKie, James William L.
McLachlan, Dugald Henderson.
McLare, James Porteous, Capt., R.A.O.C., B.Sc. (Eng.).
Mann, George Henry.
Martin, Thomas Noonan.

Associate Members—continued.

Mason, David Meredith.
Mavor, John Bridie.
Molloy, Vincent L.
Palmer, William George.
Paton, Alfred Maurice, B.A., B.Sc.
Pausey, Ernest Bernard.
Pletts, John St. Vincent.
Rattenbury, Grahame.
Roworth, George Ernest L.
Scott, Ernest William.
Smart, James Henry.
Smith, Albert.
Smith, George Casewell.
Southern, Joseph Heaton.
Stamp, James Ernest.
Sweetinburgh, Alfred Weight.
Townley, James William J.
Waite, James Nixon.
Walker, Laurence Gaston.
Wardell, Thomas.
Warner, Herbert Theobald.
Wintle, Harry.

Associates.

Faraker, Frederick Cecil.
Johns, Morgan Jones.

Graduates.

Adyanthaya, Nitte Mahaling, B.Sc. (Eng.).
Allan, John Edward.
Bakesef, Samuel.
Browne, Clarence Fishburn.
Brydon, Sydney, M.Sc.
Crimp, William Norman, 2nd Lieut., R.A.F.
Crowther, William Reginald D., Commdr. R.N.
Dale, James Floyer.
Davies, William Barron.
Frost, Thomas George.
Hardy, William Cornelius G.
Holliday, Wilfred.
Humphreys, Albert.
Love, Charles Reginald.
McGuire, Alexander Thomas.
Rankin, John Stanley.
Rowe, Benjamin.
Skinner, Leslie Carl.
Stacey, Arthur Cecil.
Vince, Sydney James.
Wilson, John, Capt., M.C.
Woods, Alfred, Capt., R.E.

Student.

Scott-Taggart, John.