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Experiments to Determine the Value of the British Association Unit of Resistance in Absolute Measure

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XIII. *Experiments to determine the Value of the British Association Unit of Resistance in Absolute Measure.*

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[PLATE 48.]

THE present paper relates to the same subject as that entitled "On the Determination of the Ohm in Absolute Measure," communicated to the Society by Dr. SCHUSTER and myself, and published in the Proceedings for April 12, 1881—referred to in the sequel as the former paper. The title has been altered to bring it into agreement with the resolutions of the Paris Electrical Congress, who decided that the ohm was to mean in future the absolute unit (10^9 C.G.S.), and not, as has usually been the intention, the unit issued by the Committee of the British Association, called for brevity the B.A. unit. Much that was said in the former paper applies equally to the present experiments, and will not in general be repeated, except for correction or additional emphasis.

The new apparatus (Plate 48) was constructed by Messrs. ELLIOTT on the same general plan as that employed by the original Committee, the principal difference being an enlargement of the linear dimensions in the ratio of about 3 : 2. The frame by which the revolving parts are supported is provided with insulating pieces to prevent the formation of induced electric currents, and more space is allowed than before between the frame and those parts of the ring which most nearly approach it during the revolution. It is supported on three levelling screws, and is clamped by bolts and nuts to the stone table upon which it stands. The ring is firmly fastened by nuts to two gun-metal pieces which penetrate it at the ends of the vertical diameter, and which form the shaft on which it rotates. The lower end of the bottom piece is rounded, and bears upon a plate of agate, on which the weight of the revolving parts is taken. A little above this comes the driving pulley (9 inches in diameter), and above this again the screw and nut by which the divided card is held. The top piece is hollow, forming a tube with an aperture of $1\frac{1}{4}$ inches, and is held by a well-fitting brass collar attached to the upper part of the frame. On this bearing the force is very small, so that the considerable relative velocity of the sliding surfaces has no ill effect. Notwithstanding its great weight, the ring ran very lightly, and the principal resistance to be overcome was that due to setting air in motion.

In the original apparatus the ring is very light, in fact scarcely strong enough to stand the forces to which it is subjected in winding on the wire. In order to avoid this defect, and also on account of its larger size, the new ring was made very massive. Cast solid, with lugs at the ends of what was to be in use the horizontal diameter, it was cut into two equal parts along a horizontal plane. The two parts were then insulated from one another by a layer of ebonite, and firmly joined together again at the lugs by bolts and nuts, after which the grooves, &c., were carefully turned. As it was intended to use two coils of wire in perpendicular planes, two rings were prepared. The smaller ring fitted into the larger, the end pieces passing through holes along the vertical diameters of both. But for a reason that will presently be given, only the larger ring was used in the present experiments.

In the spring of 1881 the larger ring was wound in Messrs. ELLIOTTS' shop under the superintendence of Dr. SCHUSTER and myself, and the necessary measurements were taken. On mounting the apparatus a few days later in the magnetical room of the Cavendish Laboratory, and making preliminary trials, we were annoyed by finding a very perceptible effect upon the suspended magnet even when the wire circuit was open. The currents thus indicated might have been due to a short circuit in the wire, or more probably (considering that the wire was triple covered, and that the winding had been carefully done) had their seat in the ring itself. Experiment showed that the insulation between the two parts of the ring, as well as between the wire and each part, was very good, so that no currents could travel round the entire circumference; but on consideration it appeared not unlikely that currents of sufficient intensity might be generated in those parts of the ring which lie nearest to the ebonite layer. The width of the ring (in the direction of its axis) was 4 inches, and the least thickness—that at the bottom of the grooves—about $\frac{3}{8}$ inch, so that the operative parts may be compared to four vertical plates $\frac{3}{8}$ inch thick, 4 inches broad, and (say) 6 inches high. In these plates currents will be developed during the rotation, whose plane is perpendicular to that of the currents in the wire.

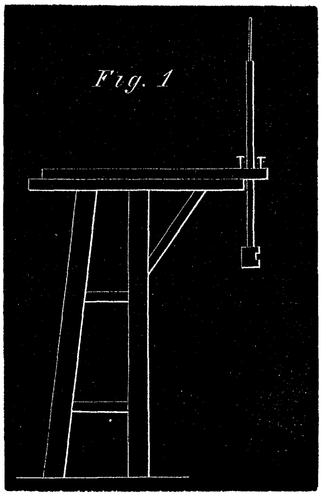
The unwished for currents could doubtless have been much diminished by saw cuts in a vertical plane extending a few inches upwards and downwards from the insulating layer, but it appeared scarcely safe to assume that the ring would retain its shape under such treatment. It would have been wiser to have tried the effect of spinning the ring alone before winding on the wire, but we were off our guard from the fact that the old ring gave no perceptible disturbance.

Theory having shown that these currents, if really formed in the manner supposed, could be satisfactorily allowed for, we decided to proceed with the experiment. At the worst, the differential effect between wire circuit closed and wire circuit open could only be in error by a quantity depending upon the square of the speed, and therefore capable of elimination upon the evidence of the spinnings themselves; while if the view were correct that the disturbing currents were principally in a plane perpendicular to that of the wire, even the correction for induction would not be much affected. A special experiment, in which the ring (with wire circuit open) was oscillated backwards and forwards through a small angle in time with the natural vibrations of the magnet, allowed us to verify the plane of the currents. A marked effect was produced when the plane of the ring was east and west, but nothing could be detected with certainty when it was north and south—the opposite of what would happen with the wire circuit closed. After this, no doubt could remain but that most of the disturbance was due to currents in the ring, and subsequent spinnings after the removal of the wire have proved that no sensible part of it was caused by leakage through the silk insulation. The existence of this disturbance, however, so far modified our original plan as to induce us to omit the second ring as giving rise to too great a complication.

The suspended magnet was made of four pieces of steel attached to the edges of a cube of pith and of such length (about $\frac{1}{2}$ inch) as to be equivalent in their action to an infinitely small magnet at the centre of the cube. Before the pieces were put together the approximate equality of their magnetic moments was ascertained. The resultant moment was between six and seven times as great as that used in our former experiments. In virtue of the greater radius of the coil, this important advantage was obtained without undue increase of the correction for magnetic moment, which amounted to about ·004, only twice as great as before. The effect of mechanical disturbances, such as air currents, was still further reduced by diminishing the size of the mirror, particularly in its horizontal dimension. On both accounts the influence of air currents was probably lessened about 15 times, and, in fact, no marked disturbance was now caused by the proximity of a lamp to the magnet box.* In consequence of these changes, however, it was found necessary to introduce an inertia ring in order to bring the time of vibration up to the amount (about $5\frac{1}{2}$ seconds from rest to rest) necessary for convenient observation. The diameter of the ring was about $\frac{3}{4}$ inch, and the whole weight of the suspended parts was not too great to be borne easily by a single fibre of silk. A brass wire passing between the spokes of the ring prevented the needle from making a complete revolution.

The enlarged scale of the apparatus allowed us to introduce a great improvement into the arrangement of the case necessary for screening the suspended parts from the mechanical disturbance of the air caused by the revolution of the coil. A brass tube of an inch in diameter was not too large to pass freely through the hollow axis. At its lower extremity (fig. 1) it was provided with an outside screw, to which the magnet box was attached air-tight. By unscrewing the box, whose aperture was large enough to allow the inertia ring to pass, the suspended parts could be exposed to view, and by drawing up the brass tube they could be removed altogether, so as to allow the coil to be dismounted, without breaking the fibre. The upper end of the fibre was attached to a brass rod sliding in a socket at the upper end of the tube, by which the height of the magnet could readily be adjusted. The whole was supported on three screws

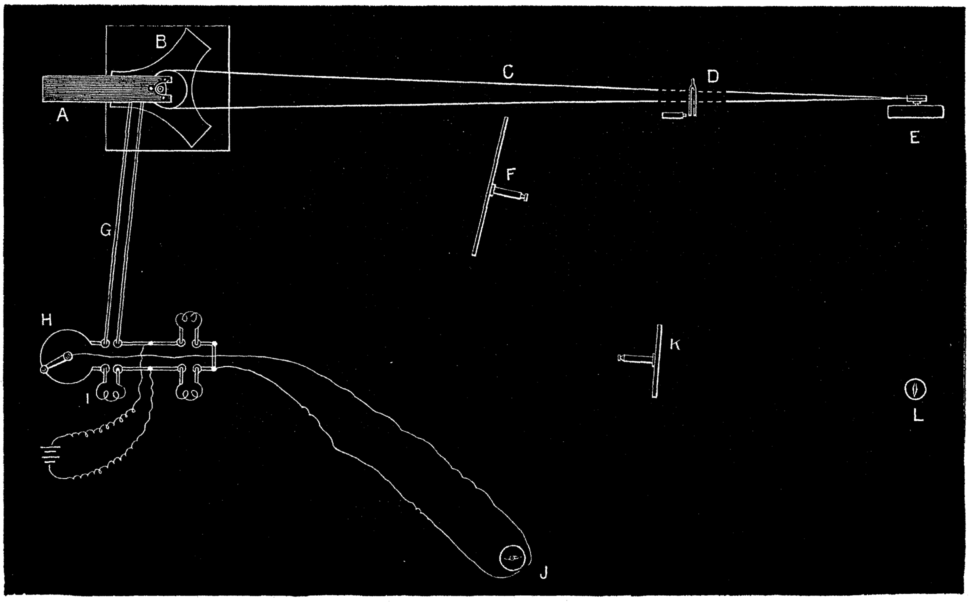
* See pp. 115–132 of the former paper.



passing through the corners of a brass triangle attached to the tube not far above the place where it emerged from the hollow axis. The points of the screws rested upon the same overhanging stand as in the former experiments (p. 113).*

The larger diameter of the tube made the system so rigid that no mechanical disturbance of the kind formerly met with was to be detected at the highest speed to

* June, 1882. The general disposition of the apparatus is shown in fig. 2.



A. Stand for suspended parts.
B. Frame of revolving coil.
C. Driving cord.
D. Electro-magnetic fork and telescope.
E. Water engine.
F. Principal telescope and scale.
G. Copper connecting bars.
H. FLEMING's bridge.
I. Platinum-silver standard.
J. Bridge galvanometer.
K. Telescope and scale of auxiliary magnetometer.
L. Auxiliary magnetometer needle and mirror.

which we could drive the coil. Even a tap with the finger-nail upon the magnet-box produced but a small disturbance.

No change was required in the arrangements for regulating and determining the speed of the coil, which worked, if possible, more perfectly than before, in consequence of the greater inertia of the revolving parts. The divided card was, however, on an enlarged scale, and the numbers of the teeth in the various circles were so arranged that each circle was available for a distinct pair of speeds according as it was observed through the slits in the plates carried by the electric fork or over the top of the upper plate. The speeds actually used corresponded to 80, 60, 45, 35, and 30 teeth, seen through the slits, *i.e.*, about 127 times per second.

The greater resistance of the copper coil (23 instead of 4·6) rendered necessary a modification in the method of making the comparisons with the standard. The whole value of the divided platinum-iridium wire on FLEMING's bridge being only $\frac{1}{20}$ ohm, a change of temperature in the copper of not much more than a degree would exhaust the range of the instrument. To meet this difficulty it was only necessary to add resistances to the copper circuit so as to compensate approximately the temperature variations, for it is evident that it can make no difference whether the change of resistance of the entire revolving circuit is due to a rise of temperature, or to the insertion of an additional piece. The platinum-silver standard was therefore prepared so as to have a resistance (about 24 ohms) greater than any which we were likely to meet with in the copper, and the additional pieces were relied upon to bring the total within distance. As at first arranged, the additional resistance was inserted at the mercury cups, instead of a contact piece of no appreciable resistance. During the comparison with the standard it was transferred to another part of the circuit.

In the course of May, 1881, a complete series of spinnings were taken, the arrangements and adjustments being (except as above-mentioned) in all respects the same as with the old apparatus. Five different speeds were used, and each of them on three different evenings. The work of observing was also distributed as before, Dr. SCHUSTER taking the readings of the principal magnetometer, and Mrs. SIDGWICK the simultaneous readings of the auxiliary magnetometer, while I observed the divided card and regulated the speed. At each speed on each evening four readings were taken with wire circuit closed, two with positive and two with negative rotation, and in like manner four readings were taken with the wire circuit open. Observations on the zero with the coil at rest were for the most part dispensed with, as it was thought that the time could be better employed otherwise; in fact, the mean of the two not very different positions of equilibrium obtained with positive and negative rotation when the wire circuit was open, gives all that is wanted in this respect. In the actual reductions we only require the *difference* of readings with positive and negative rotations.

It was hoped that these observations would have been sufficient, but on the introduction by Dr. SCHUSTER of the various corrections for temperature, for the beats between the two forks, and for the outstanding bridge-wire divisions, the necessity for which disguises the significance of the numbers first obtained, it was found that the agreement of the results corresponding to a given speed was by no means so good as we had expected in view of the precautions taken and the accuracy of the readings. What was worse, there was evidence of a decided progression, as if the absolute resistance of the standard had gradually diminished during the time occupied by the spinnings.

It is not impossible that there really was some change in the standard which had then been newly prepared; but the discrepancies were not, as according to this view they ought to have been, proportional to the speeds of rotation. I am inclined rather to attribute them to shiftings of the paper scales. The principal magnetometer scale was composed of three lengths of 50 centims. each, cemented with indiarubber to a strip of deal. The compound scale thus formed was examined by Dr. SCHUSTER in March, 1881. Between the graduations of the first and of the middle piece there was a gap of about $\frac{1}{4}$ millim., and another of nearly the same magnitude between the middle and the third piece. When I re-examined the scale in July, the gap at 500 divisions had increased to $\frac{9}{10}$ millim., and that at 1000 to $\frac{1}{2}$ millim. Curiously enough, there were no observable errors in the equality of the divisions of the three parts taken separately; but the changes above-mentioned are sufficient to throw considerable doubts upon the value of the first series of spinnings. They have, however, been reduced by Dr. SCHUSTER, and the result is given below for the sake of comparison.

To be free for the future from uncertainties of this kind, I replaced the paper scale by a long glass thermometer tube by CASELLA, graduated into millimetres. The divisions were fine and accurately placed, but the imperfect straightness of the tube has rendered necessary certain small corrections in the final results. Probably a straight strip of flat opal would have been an improvement.

The second series of spinnings was made in August, 1881, and this, it was fondly hoped, would be final. To guard against possible change in the platinum-silver coil a careful comparison with the standard units was previously instituted by Mrs. SIDGWICK, of which the details are given later. As we had unfortunately lost the advantage of Dr. SCHUSTER's assistance, the observations at the principal magnetometer devolved upon Mrs. SIDGWICK. The much easier post at the auxiliary magnetometer was usually occupied by Lady RAYLEIGH; occasional assistance has been rendered by Mr. A. MALLOCK and by Mr. J. J. THOMSON.

In the conduct of the second series one or two minor changes were introduced. In order to know the temperature of the standard tuning-fork more accurately, a thermometer was placed between its prongs and read at the same time as the number of beats was taken. The insertion of the small resistances necessary to bring the copper coil within range of the standard was also arranged in a different manner. Some trouble had been experienced in getting a sufficiently good fit between the contact pieces used in the first series and the mercury cups. It is necessary that the stout copper terminals should press down closely upon the bottoms of the cups, and also that the mercury should not be liable to escape at high speeds from the effect of centrifugal force. Bits of indiarubber tubing were placed round the copper legs, by which a fair fit with the sides of the cups was effected; but I thought that it would be an improvement to revert to a single contact piece for the mercury cups of no sensible resistance, whose fit could be carefully adjusted, and to insert the extra resistances at the connexion of the other (outer) ends of the component coils. For this purpose binding screws were employed, pressing firmly together the flat copper terminals of the copper wire and of the German-silver resistance pieces. It is almost unnecessary to say that these short lengths of German-silver wire were doubled upon themselves before being coiled, and that the pieces were not touched between a spinning and the associated resistance comparisons. Used in this way the screwed up contacts seemed unobjectionable, even though the surfaces were not amalgamated.

On each night and for each speed a set of twelve spinnings was made, six with wire circuit open, and six with wire circuit closed. It was usual to take, first, two of the former (one with positive and one with negative rotation); secondly, to compare the resistances of the revolving circuit and the standard; thirdly, after inserting the contact piece and adjusting the indiarubber strap by which it was held down, to make the six closed contact spinnings; fourthly, to compare the resistances again; and lastly, to complete the open contact readings. Each spinning, it will be understood, involved the reading of several elongations (about six for the open contact and ten for the closed), from which the position of equilibrium was deduced.

Table II. (p. 691) gives all the results of the second series, except one for 35 teeth on August 27th, which was rejected on the ground that it exhibited such large *internal* discrepancies, as to force us to the conclusion that the contact piece had been inserted improperly. It will be seen that the agreement is good except on August 29th, in which case the deflections are as much as four or five tenths of a millimetre too small. These discrepancies, though not very important in themselves, gave me a good deal of anxiety, as they were much too large to be attributed to mere errors of reading, and seemed to indicate a source of disturbance against which we were not on our guard.

The least unlikely explanations seemed to be (1) a change in the distance of the mirror from the scale, which unfortunately had not been remeasured at the close of the spinnings, though this would require to reach 3 millims.; (2) imperfect action of the contact piece from displacement of mercury or otherwise; (3) a change of level in the axis of rotation. The anomalous result of August 27th seemed to favour (2), while on behalf of (1) it must be said that the stand of the telescope and scale as well as the support for the suspended parts of the principal magnetometer were of wood. It was just conceivable that under the influence of heat or moisture some bending might have occurred.

On my return to Cambridge in October we proceeded to investigate these questions with the closest attention. As repeated direct measurements of the distance of the mirror and scale were inconvenient, measuring rods (like beam compasses) were provided to check the relative positions of the telescope stand and of the upper end of the suspending fibre with regard to fixed points on the walls of the room. But no changes comparable with 3 millims. were detected, even under much greater provocation than could have existed during the August spinnings. The next step was to examine the action of the contact piece. For this purpose the coil was balanced against the standard as usual, except that the contact piece was inserted and connexion with the bridge made at the other ends of the double coil. It was presently found that the resistance *did* depend upon the manner in which the contact piece was pressed, and that to an extent sufficient to account for the August discrepancies. Eventually it was discovered that one of the legs of the contact piece, which by a mistake had been merely rivetted and not soldered in, was shaky.

After this there could be no reasonable doubt that the faulty contact piece was the cause of our troubles. In all probability the leg became loose on the 27th, in which case the earlier results would be correct. Moreover, the final means are not very different, whether the spinnings of August 29th are retained or not. This being the case, we might perhaps have been content to let the matter rest here; but in view of the importance of the determination, and the desirability as far as possible of convincing others as well as ourselves, we thought that it would be more satisfactory to make a third and completely independent series of spinnings.

In this series the faulty composite contact piece was replaced by a horse shoe of continuous copper, and a check was instituted upon the distance between mirror and scale. The opportunity was also taken to make a minor improvement in connexion with the auxiliary magnetometer. The somewhat unsteady table on which the telescope and scale had stood was replaced by one of stone, and the arrangements for illumination were improved by throwing an image of a gas flame on the part of the scale under observation. The same number of readings were made as in the second series, but we found it more expeditious to take the six open contact spinnings together. At the beginning of the evening it was desirable to commence with these open contact spinnings in order to give more time for the coil to acquire the temperature of the room, which always rose somewhat, although the lamps and gas were lit a couple of hours beforehand. Later in the evening we sometimes took the closed contact readings for two speeds consecutively, in order that the intermediate resistance comparison might serve for both. In other respects the arrangements were unaltered.

Full details of the observations and reductions are given below. It will be sufficient here to mention that the maximum discrepancy between any two deflections at the same speed amounts only to $\frac{7}{100}$ of a millimetre, so that the agreement on different nights is more perfect than could have reasonably been expected. At the lowest speed the above-mentioned discrepancy is less than one part in 3000, and at the highest speed less than one part in 6000. No spinnings in the third series were rejected, except on one or two occasions when it appeared *at the time of observation*, from the behaviour of the auxiliary magnetometer, that there was too much earth disturbance. The spinnings were then suspended, and the observations already obtained were not reduced.

At the close of the spinnings, Mrs. SIDGWICK made a further comparison of our platinum-silver coil with the standard units.

The value arrived at for the B.A. unit (·9865 ohm) differs nearly three parts in a thousand from that which we obtained with the original apparatus. This difference is not very great, and may possibly be accounted for by errors in the measurement of the coil (see p. 114 of former paper). If a coil be imperfectly wound, the mean radius, as determined by a tape, is liable to be too great. At any rate, this discrepancy sinks into insignificance in comparison with that which exists between either of these determinations and that of Professor KOHLRAUSCH,* according to whom the B.A. unit would be as much as 1·0196 ohms. With respect to the method employed by KOHLRAUSCH, I agree with ROWLAND† in thinking it difficult, and unlikely to give the highest accuracy; but how in the hands of a skilful experimenter it could lead to a result 3 per cent. in error, is difficult to understand. The only suggestion I have to make is that possibly sufficient care was not taken in levelling the earth-inductor. Although estimates are given of the probable errors due to uncertainties in the various data, nothing is said upon this subject. In consequence, however, of the occurrence of the horizontal intensity as a square in the final formula, in conjunction with the largeness of the angle of dip, the method is especially sensitive to a maladjustment of this kind. I calculate that a deviation of the axis of rotation from the vertical through 21′ in the plane of the meridian, would alter the final result by 3 per cent.‡

According to ROWLAND's determination, the value of the B.A. unit is ·9912 ohm. The method consists essentially in comparing the integral current in a secondary circuit, due to the reversal of the battery in a primary circuit, with the magnitude of the primary current itself. The determination of the secondary current involves the use of a ballistic galvanometer, whose damping is small, and whose time of vibration can be ascertained with full accuracy; and it is here, I think, that the weakest point in the method is to be found. The logarithmic decrement is obtained by observation of a long series of vibrations, and it is assumed that the value so arrived at is applicable to the correction of the observed throw. I am not aware whether the origin of damping in galvanometers has ever been fully investigated, but the effect is usually supposed to be represented by a term in the differential equation of motion proportional to the momentary velocity. This mode of representation is no doubt applicable to that part of the damping which depends upon the induction of currents in the galvanometer coil, under the influence of the swinging magnet. If this were all, a correction for damping would be accurately effected on the basis of a determination of the logarithmic decrement, made with the galvanometer circuit closed in the same

* POGG. Ann., Ergänzungband VI. Phil. Mag., April, 1874.

† American Journal, April, 1878.

‡ See p. 684.

manner as when the throw is taken. In all galvanometers, however, a very sensible damping remains in operation even when the circuit is open, of which the greatest part is doubtless due to aerial viscosity; and it is certain that the retarding force arising from viscosity is not simply proportional to the velocity at the moment, without regard to the state of things immediately preceding.

In particular, the force acting upon the suspended parts as they start suddenly from rest in the observation of the throw, must be immensely greater than in subsequent passages through the position of equilibrium, when the vibrations have assumed their ultimate character. I calculate that in the first quarter vibration (*i.e.*, from the position of equilibrium to the first elongation) of a disc vibrating in its own plane and started impulsively from rest, the loss of energy from aerial viscosity would be 1·373 times that undergone in subsequent motion between the same phases. From this it might at first appear that in this ideal case the logarithmic decrement observed in the usual manner would need to be increased by more than a third part in order to make it applicable to the correction of a throw from rest; but in order to carry out this view consistently we should have to employ in the formula the time in which the needle would vibrate if the aerial forces were non-existent instead of the actually observed time of vibration. Now since the action of viscosity is to increase the time of vibration, the second effect is antagonistic to the first, so that probably the error arising from the complete neglect of these considerations is very small.

There is another point in which it appears to me that the theory of the ballistic galvanometer is incomplete. It is assumed that the magnetism of the needle in the direction of its axis is the same at the moment of the impulse as during regular vibrations. Can we be sure of this? The impulse is due to a momentary but very intense magnetic force in the perpendicular direction, and it seems not impossible that there may be in consequence a temporary loss of magnetism along the axis. If this were so, the actual impulse and subsequent elongation would be less than is supposed in the calculation, and too high a value would be obtained of the resistance of the secondary circuit in absolute measure. In making these remarks I desire merely to elicit discussion, and not to imply that ROWLAND's value is certainly four parts in a thousand too high.

Determinations of the absolute unit have been made also by H. WEBER,* whose results indicate that the B.A. unit is substantially correct. In the absence of sufficient detail it is difficult to compare this determination with others, so as to assign their relative weights.

The value of the B.A. unit in absolute measure is involved in the two series of experiments executed by JOULE on the mechanical equivalent of heat.† The result from the agitation of water is 24868, while that derived from the passage of a known absolute current through a resistance compared with the B.A. unit was 25187. The

* Phil. Mag., Jan., Feb., March, 1878.

† Phil. Trans., Part II., 1878. Brit. Ass. Rep., 1867; Reprint, p. 175.

latter result is on the supposition that the B.A. unit is really 10^9 C.G.S. If we inquire what value of the B.A. unit will reconcile the two results, we find—

$$1 \text{ B.A. unit} = \cdot 9873 \text{ ohm},$$

in very close agreement with the measurement described in the present paper. It should be remarked that in the comparison of the two thermal results some of the principal causes of error are eliminated; and it is not improbable that an experiment in which heat should be simultaneously developed in one calorimeter by friction, and in a second similar calorimeter by electric currents, would lead to a very accurate determination of resistance, more especially if care were taken so to adjust matters that the rise of temperature in the two vessels was nearly the same, and a watch were kept upon the resistance of the wire while the development of heat was in progress.

[*June*, 1882.—Since this paper was sent to the Society, Mr. GLAZEBROOK has worked out the results of a determination of the B.A. unit in absolute measure by a method not essentially different from that adopted by ROWLAND. The final number is practically identical with that of the present paper; and the agreement tends to show that the difference between ourselves and ROWLAND is not to be attributed to the use of a ballistic galvanometer.

Reference should have been made to the results of LORENTZ.* He finds as the value of the mercury unit *defined* by SIEMENS

$$1 \text{ mercury unit} = \cdot 9337 \frac{\text{earth quadrant}}{\text{second}}$$

The corresponding number calculated from the results of the present paper with use of the value of the specific resistance of mercury lately found (Proc. Roy. Soc., May 4, 1882) is ·9413. If we invert the calculation, we find that according to LORENTZ the value of the B.A. unit would be ·9786 absolute measure. The method of LORENTZ is ingenious, and apparently capable with good apparatus of giving a result to much within 1 per cent. Mrs. SIDGWICK and myself are at present making a trial of it.]

It will be desirable here to consider briefly some of the criticisms of KOHLRAUSCH and ROWLAND upon the method of the original British Association Committee, which has been adopted in the present investigation without fundamental alteration. The difficulty, remarked upon by KOHLRAUSCH, of obtaining a rapid and uniform rotation, has not been found serious, and I believe that no appreciable error can be due either to irregularity of rotation or to faulty determination of its rapidity. It has also been brought as an objection to the method that a correction is necessary on account of the magnetic influence of the suspended magnet upon the revolving circuit. The theory of this action is, however, perfectly simple, and the application of the correction requires only a knowledge of the ratio of the magnetic moment to the earth's

* POGG. Ann., 1873.

horizontal force. If the magnetic moment is very small, the correction is unimportant; if larger, it can on that very account be determined with the greater ease and accuracy. It is probable that in the original experiments too feeble a magnetic moment was used, and that in consequence the suspended parts were too easily disturbed by non-magnetic causes; but this might have been remedied without increasing objectionably the correction in question. At any rate the larger coil of the new apparatus allows the use of any reasonable magnetic moment.

Perhaps the least advantageous feature in the method is the necessity for creating a violent aerial disturbance in the immediate neighbourhood of a delicately suspended magnet and mirror. If, however, any deflection occurs in this way, very little error can remain when the open contact effect is subtracted from the closed contact effect. The difficulty of avoiding a sensible deflection, due to currents in the ring, when the wire circuit is open, is connected with a special advantage—*i.e.*, the possibility of assuring ourselves that there is no leakage from turn to turn of the coil. In the method followed by ROWLAND, for instance, such a leakage would lead to error, and could not be submitted to any direct test.

The correction for self-induction cannot be made very small without a disadvantageous reduction of the whole angular deflection; but so far as the wire is concerned it can be calculated *à priori*, or determined by independent experiment, with the necessary accuracy. There is reason, however, to think that the best method of treatment is to determine this correction from the spinnings themselves, combining the results of widely different speeds so as to obtain what would have been observed at a small speed. At small speeds it is certain that all effects of self-induction and of mutual induction between the wire circuit and other circuits in the ring will disappear.

Measurements of coil.

The mean radius of the coil, being the fundamental linear measurement of the investigation, must be found with full accuracy. There has been some difference of opinion as to the best method of effecting this. The greatest accuracy is probably attained by the use of the cathetometer. The measurement of the circumference of every layer by a steel tape has the advantage that the subject of measurement is three times as large, and is much less troublesome. The disadvantage is that if a layer be not quite even, there is danger of measuring the maximum rather than the mean outside circumference. In the present investigation the coil was so large that the tape could be employed without fear.*

Each of the component coils marked A and B had $18 \times 16 = 288$ windings, but in

* The original Committee also employed the tape method. Their measurement of the length of the wire when unwound was not in order to find the mean radius, as SIEMENS and KOHLRAUSCH suppose, but to verify the number of turns.

consequence of variations in the thickness of the triple silk covering, there was a difficulty in getting exactly 18 turns into each layer. In the eleventh layer of A it was necessary to be content with 17 turns, and to place an extra turn on the outside, so as to form the commencement of a seventeenth layer—a circumstance which of course was taken into account in calculating the mean. The number thus arrived at, after correction for the thickness of tape, is the mean *outside* circumference. What we require is the mean circumference of the axis of the wire; it may be derived from the first by subtraction of half the difference between the tape readings for the first layer, and for the bottom of the gun-metal groove.

The results obtained by Dr. SCHUSTER and myself when the coils were wound are:

	Coil A.	Coil B.
Mean of readings in millims.	1489·3	1487·5
Correction for tape	·6	·6
Mean outside circumference	1488·7	1486·9
Correction for thickness of wire	3·4	3·4
Mean circumference	1485·3	1483·5
Mean radius	236·39	236·11
Mean circumference of A and B	1484·4	
Mean radius of A and B (a)	236·25	
Axial dimension of section in millims.	19·9	19·9
Radial	15·9	15·4
Distance of mean planes ($2b'$)	65·95	

Two or three readings were taken of the circumference of every layer, and to prevent mistakes in the number of turns, the plan described by MAXWELL,* of simultaneously winding string on wooden rods, was followed. Without some such device, there is great risk of confusion.

In estimating the degree of accuracy obtainable, we must remember that the circumference of each layer is measured before the outer layers are wound on; any change produced by the pressure of these outer layers is a source of error. We had already observed a tendency in the measurements to be less during the unwinding of a coil than during the winding, and we fully intended to remeasure the coil after the spinnings were completed. This was done on December 6, 1881, by Mrs. SIDGWICK and myself. As we expected, somewhat smaller readings (by about $\frac{3}{4}$ millim.) were obtained for the circumference of the middle layers. The results were:

* 'Electricity and Magnetism,' II., § 708.

	Coil A.	Coil B.
Mean radius	236·31	236·02
Mean of both	236·16	

or nearly one part in 2000 less than before. Of the two values, it would appear that the latter is more likely to represent the actual condition of the coil during the spinnings, and is therefore entitled to greater weight. If we give weights in the proportion of two to one, we get

Mean radius=23·619 centims.*

Calculation of GK.

We have

$$\mathrm{GK}=2\pi^2n^2a\sin^3\alpha\left\{1+\tfrac{1}{6}\frac{c^2}{a^2}+\tfrac{5}{8}\frac{b^2-c^2}{a^2}\sin^2\alpha\cos^2\alpha-\tfrac{1}{8}\frac{b^2}{a^2}\sin^2\alpha\right\}$$

in which

a =mean radius =23·625 (1st measurement)
b =axial dimension of section = 1·990
c =radial dimension of section = 1·565
n =total number of turns = 576
$2b'$=distance of mean planes = 6·595

$$\sin\alpha=a\div\sqrt{(a^2+b'^2)}$$

From these data we find

$$\begin{aligned}\log 2\pi^2n^2 &= 6\cdot81617\\ \log a &= 1\cdot37337\\ \log\sin^3\alpha &= \bar{1}\cdot98744\\ \log\{\ldots\} &= \bar{1}\cdot99995\\ \hline \log \mathrm{GK} &= 8\cdot17693\end{aligned}$$

But if we substitute the adopted value of a, *i.e.*, 23·619 centims., we have by subtraction of ·00011

$$\log \mathrm{GK} = 8\cdot17682$$

Calculation of L.

We may write

$$\mathrm{L}=16^2\times18^2\,(\mathrm{L}_1+\mathrm{L}_2+2\mathrm{M}),$$

where L_1, L_2 are the coefficients of self-induction of the two parts, and M the

* [August, 1882. At the time of use the tape was compared with a measuring rod, which again has been compared with a standard metre verified by the Standards Department of the Board of Trade. For the purposes of this investigation the differences observed are altogether negligible. I may add that the clock with which the standard tuning-fork was compared (see p. 137 of former paper) was rated from astronomical observations.]

coefficient of mutual induction without regard to the number of turns. L_1 and L_2 may be calculated from the formula

$$L=4\pi a\left[\log_e\frac{8a}{r}+\tfrac{1}{12}-\tfrac{4}{3}(\theta-\tfrac{1}{4}\pi)\cot 2\theta-\tfrac{1}{3}\pi\operatorname{cosec}2\theta-\tfrac{1}{6}\cot^2\theta\log_e\cos\theta-\tfrac{1}{6}\tan^2\theta\log_e\sin\theta\right]$$

in which r is the diagonal of the section, and θ the angle between it and the plane of the coil. With this formula and with the dimensions as measured when the coil was wound, we get

$$L_1\ (\text{for A})=1029{\cdot}3\ \text{centims.}$$
$$L_2\ (\text{for B})=1031{\cdot}9\ \text{centims.}$$

It would not be difficult to calculate an approximate correction for the curvature of the coil, but this is scarcely necessary. (See p. 119 of former paper.) Adding the above, we have

$$L_1+L_2=2061{\cdot}2\ \text{centims.}$$

The value of M was found from the tables given as Appendix I. to § 706 of the new edition of MAXWELL's 'Electricity.' If we suppose each coil condensed into the centre of its section, we find $M=4\pi\times 33{\cdot}061$. A more exact calculation by the formula of interpolation explained in Appendix II. gives $M=4\pi\times 33{\cdot}140$, so that

$$2M=832{\cdot}88\ \text{centims.}$$

The final result is accordingly

$$L=16^2\times 18^2\times 2894{\cdot}1=2{\cdot}4004\times 10^8\ \text{centims.}$$

These calculations of the coefficients of induction have been made independently by Mr. NIVEN and myself, and are so far reliable; but we must not forget that the accuracy of the result depends upon the accuracy of the data, and that in the present case the diagonal of the section (r) on which the most important part of L depends is an element subject to considerable relative uncertainty. It is probable that the effective axial dimensions of the section is somewhat less than the width of the groove, and therefore that the real value of L may be a little greater than would appear from the preceding calculation.

Theory of the ring currents.

If the circuits are conjugate, the currents in the wire and in the ring are formed in complete independence of one another, a circumstance which simplifies the theory very materially. In the same notation as was used in the former paper (p. 105), and with dashed letters for the ring circuit, we have as the equation determining the angle of deflection (ϕ) when the wire circuit is closed,

$$\tan\phi+\tau\frac{\phi}{\cos\phi}=\frac{\frac{1}{2}\mathrm{GK}\omega}{\mathrm{R}^2+\mathrm{L}^2\omega^2}\{\mathrm{R}+\mathrm{L}\omega\tan\phi+\mathrm{R}\tan\mu\sec\phi\}$$
$$+\frac{\frac{1}{2}\mathrm{G'K'}\omega}{\mathrm{R'}^2+\mathrm{L'}^2\omega^2}\{\mathrm{R'}+\mathrm{L'}\omega\tan\phi+\mathrm{R'}\tan\mu\sec\phi\}$$

When the wire circuit is open the equation determining the angle of deflection (ϕ_0) is

$$\tan\phi_0+\tau\frac{\phi_0}{\cos\phi_0}=\frac{\frac{1}{2}\mathrm{G'K'}\omega}{\mathrm{R'}^2+\mathrm{L'}^2\omega^2}\{\mathrm{R'}+\mathrm{L'}\omega\tan\phi_0+\mathrm{R'}\tan\mu\sec\phi_0\}$$

Since τ is an extremely small quantity it is unnecessary to keep up the distinction between $\tau\phi/\cos\phi$ and $\tau\tan\phi$. By subtraction

$$(1+\tau)(\tan\phi-\tan\phi_0)$$
$$=\frac{\frac{1}{2}\mathrm{GK}\omega}{\mathrm{R}^2+\mathrm{L}^2\omega^2}\{\mathrm{R}+\mathrm{L}\omega\tan\phi+\mathrm{R}\tan\mu\sec\phi\}$$
$$+\frac{\frac{1}{2}\mathrm{G'K'}\omega}{\mathrm{R'}^2+\mathrm{L'}^2\omega^2}\{\mathrm{L'}\omega(\tan\phi-\tan\phi_0)+\mathrm{R'}\tan\mu\,(\sec\phi-\sec\phi_0)\}$$

The last term is small, and we may neglect ($\sec\phi-\sec\phi_0$) in combination with $\mathrm{R'}\tan\mu$.

Moreover

$$\frac{\frac{1}{2}\mathrm{G'K'}\omega}{\mathrm{R'}^2+\mathrm{L'}^2\omega^2}=\frac{(1+\tau)\tan\phi_0}{\mathrm{R'}+\mathrm{L'}\omega\tan\phi_0}$$

so that

$$(1+\tau)(\tan\phi-\tan\phi_0)=\frac{\frac{1}{2}\mathrm{GK}\omega}{\mathrm{R}^2+\mathrm{L}^2\omega^2}\{\mathrm{R}+\mathrm{L}\omega\tan\phi+\mathrm{R}\tan\mu\sec\phi\}$$
$$+(1+\tau)(\tan\phi-\tan\phi_0)\frac{\mathrm{L'}\omega\tan\phi_0}{\mathrm{R'}+\mathrm{L'}\omega\tan\phi_0}$$

If now we write (GK) for GK/($1+\tau$), we get

$$\tan\phi-\tan\phi_0=\frac{\frac{1}{2}(\mathrm{GK})\omega}{\mathrm{R}^2+\mathrm{L}^2\omega^2}\{\mathrm{R}+\mathrm{L}\omega\tan\phi+\mathrm{R}\tan\mu\sec\phi\}\{1+\frac{\mathrm{L'}\omega}{\mathrm{R'}}\tan\phi_0\}$$

The effect of L′ would therefore be to *increase* disproportionately the deflections at high speeds, *i.e.*, contrary to the effect of L. It appears, however, that in these experiments it could not have been sensible. At the highest speed $\tan\phi_0$ was about $\frac{1}{670}$, and ω about 26 per second, so that $\omega\tan\phi_0$ would be about $\frac{1}{26}$. The value of L′/R′ is difficult to estimate with any accuracy. But the value of L/R for the wire circuit is about ·01 second, and that for the ring circuit must be much less, so that the terms involving L′ may safely be omitted.

The quadratic in R then becomes

$$\mathrm{R}^2-\mathrm{R}\frac{\frac{1}{2}(\mathrm{GK})\omega(1+\tan\mu\sec\phi)}{\tan\phi-\tan\phi_0}+\mathrm{L}^2\omega^2-\tfrac{1}{2}(\mathrm{GK})\mathrm{L}\omega^2\frac{\tan\phi}{\tan\phi-\tan\phi_0}=0$$

whence

$$R=\frac{\frac{1}{2}(GK)\omega}{\tan\phi-\tan\phi_0}\left[\tfrac{1}{2}(1+\tan\mu\sec\phi)+\sqrt{\left\{\tfrac{1}{4}(1+\tan\mu\sec\phi)^2-U(\tan\phi-\tan\phi_0)^2\right\}}\right]$$

where

$$U=\frac{2L}{(GK)}\left\{\frac{2L}{(GK)}-\frac{\tan\phi}{\tan\phi-\tan\phi_0}\right\}$$

L *by direct experiment.**

Although the calculated value of L was the result of two independent computations, I considered that it would be satisfactory still further to verify it by an experiment with WHEATSTONE'S balance. The statement of this method and the final formula, as given on p. 116 of the former paper, being approximate only, it will be convenient here to repeat them with the necessary corrections.

The four resistances in the balance are two equal resistances (10 units each), that of the copper coil P, and a fourth resistance Q (nearly equal to P) taken from resistance boxes, of which P is the only one associated with sensible self-induction. When P and Q are equal, there is no permanent current through the galvanometer; but if the galvanometer circuit be first closed and then the battery current be made, broken, or reversed, the needle receives an impulse, whose magnitude depends upon L.

If x denote the change of current in the branch P, the action of self-induction is the same as that of an electromotive impulse in that branch of magnitude Lx, and the effect upon the galvanometer is that due to this electromotive impulse acting independently of the electromotive force in the battery branch.

In order now to get a second quantity with which to compare the induction throw, the resistance balance is upset in a known manner. If while Q remains unaltered, P be increased to $P+\delta P$, there is a steady current through the galvanometer, which we may regard as due to an electromotive force $\delta P.x'$ in the branch $P+\delta P$, x' being the current through the branch. If θ be the deflection of the needle under the action of the steady current, α the angular throw, and T the time of swing from rest to rest, we have by the theory of the ballistic galvanometer as the ratio of the instantaneous to the steady electromotive force

$$\frac{T}{\pi}\frac{2\sin\frac{1}{2}\alpha}{\tan\theta},$$

subject to a correction for damping; so that this expression represents the ratio of $Lx:\delta P.x'$. If the induction throw be due to the make or break of the battery circuit, x represents simply the current in the branch P. In the case where the battery

* In consequence of the necessity which ultimately appeared of introducing an arbitrary correction proportional to the square of the speed of rotation, the result of the present section does not influence the final number expressing the B.A. unit in absolute measure. The method, however, is of some interest, and (it is believed) has not been carried out before with the precautions necessary to secure a satisfactory result.

current is reversed, we may write $2\mathrm{L}x$ for $\mathrm{L}x$, understanding by x the same as before. As this method was the one actually adopted, we will write the result in the appropriate form

$$\frac{\mathrm{L}.x}{\delta\mathrm{P}.x'} = \frac{\mathrm{T}}{\pi}\frac{\sin\frac{1}{2}\alpha}{\tan\theta}.$$

In the formula as originally given by MAXWELL, and as stated in the former paper, the distinction between x and x' (the currents before and after the resistance balance is upset) was neglected. This step is legitimate if $\delta\mathrm{P}$ be taken small enough, to which course however there are experimental objections. In order that $\tan\theta$ might be of suitable magnitude, it was found necessary to make the ratio of $\delta\mathrm{P} : \mathrm{P}$ equal to about $\frac{1}{300}$, a fraction too large to be neglected.

In carrying out the experiment it was found more convenient to insert the additional resistance in the branch Q, leaving P unaltered. By the symmetry of the arrangement it is evident that this alteration is immaterial, and that we may take the formula in the form

$$\frac{\mathrm{L}}{\mathrm{Q}} = \frac{\mathrm{L}}{\mathrm{P}} = \frac{\delta\mathrm{Q}.x'}{\mathrm{Q}.x}\frac{\mathrm{T}}{\pi}\frac{\sin\frac{1}{2}\alpha}{\tan\theta},$$

x being the current in the branch Q when the resistance balance is perfect, x' the diminished current when the additional resistance $\delta\mathrm{Q}$ is inserted.

The principal difficulties in carrying out the experiment arose from variation in the battery and in the resistance balance. From these causes the results of two days' experiments were rejected, as unlikely to repay the trouble of reduction. On the last day (December 3, 1881) the first difficulty was overcome by using three large DANIELL cells (charged with zinc sulphate) in multiple arc. As precautions against rapid change of temperature the copper coils were wrapped thickly round with strips of blanket and deposited in a closed box. The delicacy of our arrangements was such that about $\frac{1}{1000}$ of a degree centigrade would manifest itself, so that it was hopeless to try to maintain the resistance balance absolutely undisturbed. The mode of applying a suitable correction will presently be explained. On December 3, partly by good luck, the necessary correction remained small throughout. In order to avoid a direct action of the current upon the galvanometer needle, the coil was placed at a considerable distance, at the same level, and with its plane horizontal. Any outstanding effect of the kind would, however, be eliminated from the final result by the reversals practised.

The induction throws were always taken by reversal of the battery current. A reversal has two advantages over a simple make or break. In the first place the effect is doubled and is therefore more easily measured; and in the second the battery is more likely to work in a uniform manner, the circuit being always closed except for a fraction of a second at the moment of reversal. The key was of the usual rocker and mercury cup pattern.

The galvanometer was one belonging to the laboratory of about 80 ohms resistance.