ART. XLII.—*Research on the Absolute Unit of Electrical Resistance;* by HENRY A. ROWLAND,* Professor of Physics in the Johns Hopkins University, Baltimore, Md.

Preliminary Remarks.

SINCE the classical determination of the absolute unit of electrical resistance by the Committee on Electrical Standards of the British Association, two re-determinations have been made, one in Germany and the other in Denmark, which each differ two per cent from the British Association determination, the one on one side and the other on the other side, making a total difference of four per cent between the two. Such a great difference in experiments which are capable of considerable exactness, seems so strange that I decided to make a new determination by a method different from any yet used, and which seemed capable of the greatest exactness; and to guard against all error, it was decided to determine all the important factors in at least two different ways, and to eliminate most of the corrections by the method of experiment, rather than by calculation. The method of experiment depended upon the induction of a current on a closed circuit, and in this respect resembled that of Kirchhoff, but it differed from his inasmuch as, in my experiment, the induction current was produced by reversing the main current, and in Kirchhoff's by removing the circuits to a distance from each other. And it seems to me that this method is capable of greater exactness than any other, and it certainly possessed the greatest simplicity in theory and facility in experiment.

In the carrying out of the experiment I have partly availed myself of my own instruments and have partly drawn on the collection of the University, which possesses many unique and accurate instruments for electric and magnetic measurements. To insure uniformity and accuracy, the coils of all these instruments have been wound with my own hands and the measurements reduced to a standard rule which was again compared with the standard at Washington. Unlike many German instruments, quite fine wire has always been used and the number of coils multiplied, for in this way the constants of the coils can be more exactly determined, there is less relative action from the wire connecting the coils, and above all we know exactly where the current passes.

The experiment was performed in the back room of a small house near the University, which was reasonably free from

* I am greatly indebted to Mr. Jacques, Fellow of the University, who is an excellent observer, for his assistance during the experiment, particularly in reading the tangent galvanometer.

magnetic and other physical disturbances. As the magnetic disturbance was eliminated in the experiment, it was not necessary to select a region entirely free from such disturbance. The small probable error proves that sufficient precaution was taken in this respect.

The result of the experiment that the British Association unit is too great by about ·88 per cent, agrees well with Joule's experiment on the heat generated in a wire by a current, and makes the mechanical equivalent as thus obtained very nearly that which he found from friction: it is intermediate between the result of Lorenz and the British Association Committee; and it agrees almost exactly with the British Association Committee's experiments, if we accept the correction which I have applied below.

The difference of nearly three per cent which remains between my result and that of Kohlrausch is difficult to explain, but it is thought that something has been done in this direction in the criticism of his method and results which are entered into below. My value, when introduced into Thomson's and Maxwell's values of the ratio of the electromagnetic to the electrostatic units of electricity, caused a yet further deviation from its value as given in Maxwell's electromagnetic theory of light: but experiments on this ratio have not yet attained the highest accuracy.

History.

The first determination of the resistance of a wire in absolute measure was made by Kirchhoff* in 1849 in answer to a question propounded by Neumann, in whose theory of electrodynamic induction a constant appeared whose numerical value was unknown until that time. His method, like that of this paper, depended on induction from currents: only one galvanometer was used and the primary current was measured by allowing only a small proportion of it to pass through the galvanometer by means of a shunt, while all the induced current passed through it. But, owing to the heating of the wires, the shunt ratio cannot be relied upon as constant, and hence the defect of the method. At present this experiment has only historical value, seeing that no exact record was kept of it in a standard resistance. However we know that the wire was of copper and the temperature 0° R. and that the result obtained gave the resistance of the wire $\frac{1}{7}$ smaller than Weber found for the same wire at 20° R. in 1851.

In 1851, Weber published† experiments by two methods, first by means of an earth inductor, and second by observing

* Bestimmung der Constanten von welcher die Intensität inducirter elektrischer Ströme abhängt. Pogg. Ann., Bd. 76, S. 412.

† Elektrodynamische Maasbestimmungen; or Pogg. Ann., Bd. 82, S. 337.

the damping of a swinging needle. Three experiments gave for the resistance of the circuit $1903 \cdot 10^8$, $1898 \cdot 10^8$, and $1900 \cdot 10^8$ $\frac{mm.}{sec.}$, but it is to be noted that a correction of five eighths per cent was made on account of the time, two seconds, which it took to turn the earth-inductor, and that no account was taken of the temperature, although the material was copper. He finds for the value of the Jacobi unit, $598 \cdot 10^7 \frac{mm.}{sec.}$. Three years after that, in 1853, Weber made another determination of the specific resistance of copper.* But these determinations were more to develop the method than for exact measurement, and it was not until 1862† that Weber made an exact determination which he expected to be standard. In this last determination he used a method compounded of his first two methods by which the constant of the galvanometer was eliminated, and the same method has since been used by Kohlrausch in his experiments of 1870. The results of these experiments were embodied in a determination of the value of the Siemens unit and of a standard which was sent by Sir Wm. Thomson. As the old Siemens units seem to vary among themselves one or two per cent, and as the result from Thomson's coil differs more than one per cent from that which would be obtained with any known value of the Siemens unit, we cannot be said to know the exact result of these experiments at the present time. Beside which, it was not until the experiments of Dr. Matthiessen on the electric permanence of metals and alloys, that a suitable material could be selected for the standard resistance.

The matter was in this state when a committee was appointed by the British Association in 1861, who, by their experiments which have extended through eight years, have done so much for the absolute system of electrical measurements. But the actual determination of the unit was made in 1863–4. The method used was that of the revolving coil of Sir William Thomson, the principal advantage of which was its simplicity and the fact that the local variation of the earth's magnetism was entirely eliminated and only entered into the calculation as a small correction. The principle of the method is of extreme beauty, seeing that the same earth's magnetism which causes the needle at the center of the coil to point in the magnetic meridian also causes the current in the revolving coil which deflects the needle from that meridian. Whenever a conducting body moves in a magnetic field, currents are generated in it in such direction that the total re-

* Abh. d. Kön. Ges. d. Wissenschaften zu Göttingen, Bd. 5.

† Zur Galvanometrie; Göttingen, 1862. Also Abh. d. K. Ges. d. Wis. zu Göttingen, Bd. 10.

sultant action is such that the lines of force are apparently dragged after the body as though they met with resistance in passing through it: and so we may regard Thomson's method as a means of measuring the amount of this dragging action.

But, however beautiful and apparently simple the method may appear in theory, yet when we come to the details we find many reasons for not expecting the finest results from it. Nearly all these reasons have been stated by Kohlrausch, and I can do barely more in this direction than review his objections, point out the direction in which each would affect the result, and perhaps in some cases estimate the amount.

In the first place, as the needle also induced currents in the coil which tended in turn to deflect the needle, the needle must have a very small magnetic moment in order that this term may be small enough to be treated as a correction. For this reason the magnetic needle was a small steel sphere 8 mm. diameter, and not magnetized to saturation. It is evident that in a quiescent magnetic field such a magnet would give the direction of the lines of force as accurately as the large magnets of Gauss and Weber, weighing many pounds. But the magnetic force due to the revolving coil is intermittent and the needle must show as it were the average force, together with the action due to induced magnetization. Whether the magnet shows the average force acting on it or not, depends upon the constancy of the magnetic axis, and there seems to be no reason to suppose that this would change in the slightest, though it would have been better to have made the form of the magnet such that it would have been impossible. The induced magnetism of the sphere would not affect the result, were it not for the time taken in magnetization: on this account the needle is dragged with the coil, and hence makes the deflection greater than it should be, and the absolute value of the Ohm too small by a very small quantity. The currents induced in the suspended parts also act in the same direction. Neither of these can be estimated, but they are evidently very minute.

The mere fact that this small magnet was attached to a comparatively large mirror which was exposed to air currents could hardly have affected the result, seeing that the disturbances would have been all eliminated except those due to air currents from the revolving coil, and which we are assured did not exist from the fact that no deflection took place when the coil was revolved with the circuit broken. In revolving the coil in opposite directions very different results were obtained, and the explanation of this has caused considerable discussion. As this is of fundamental importance I shall consider it in detail.

The magnet was suspended by a single fiber seven feet long, and the deflection was diminished by its torsion ·00132. No mention is made of the method used for untwisting the fiber, and we see that it would require only 2·11 turns to deflect the needle 1° from the meridian. To estimate the approximate effect of this, we may omit from Maxwell's equation* all the other minor corrections and we have

$$R=\tfrac{1}{2}\frac{GKw\cos\varphi}{\sin\varphi+t(\varphi-\beta)}=\tfrac{1}{2}\frac{GKw}{\tan\varphi(1+t)}\frac{1}{\left(1-\dfrac{\beta t}{\sin\varphi(1+t)}\right)}\text{ nearly,}$$

where we have substituted $\varphi-\beta$ for φ in Maxwell's equation in the term involving t. In this equation φ is measured from the magnetic meridian; but let us take ψ as the angle from the point of equilibrium. Then $\psi'=\varphi'+\alpha$ and $\psi''=\varphi''-\alpha$, where ψ' and φ' are for negative rotation and ψ'' and φ'' for positive rotation and $\alpha=\text{arc}\sin\frac{\beta t}{1+t}$.

Let $$C=\frac{2}{GKw}.$$

Then $$CR'=\frac{1}{\tan\psi'(1+t)}.$$

$$CR''=\frac{1}{\tan\psi''(1+t)}.$$

$$R_{,}=\tfrac{1}{2}(R'+R'').$$

Where R' and R'' are the apparent values of the resistance as calculated from the negative and positive rotations, and $R_{,}$ is the mean of the two as taken from the table published by the British Association Committee. If R is the true resistance,

$$CR=\frac{1}{\tan\varphi'(1+t)\left(1+\dfrac{\sin\alpha}{\sin\varphi'}\right)}=\frac{1}{\tan\varphi''(1+t)\left(1-\dfrac{\sin\alpha}{\sin\varphi''}\right)}.$$

We shall then find approximately

$$R=\frac{1+\tan\psi'\tan\alpha}{R'\left(1+\dfrac{\sin\alpha}{\sin\varphi'}\right)\left(1-\dfrac{\tan\alpha}{\tan\psi''}\right)}=\frac{1-\tan\psi''\tan\alpha}{R''\left(1-\dfrac{\sin\alpha}{\sin\varphi''}\right)\left(1+\dfrac{\tan\alpha}{\tan\psi''}\right)}.$$

When α is small compared with ψ'' or ψ', and when these are also small, we have

$$R=R_{,}(1+\alpha^2(\alpha^2-\tfrac{1}{2}\psi^2)+\&c.).$$

So that by taking the mean of positive and negative rotations, the effect of torsion is almost entirely eliminated. Now α is the angle by which the needle is deflected from the magnetic meridian by the torsion and its value is $\frac{1}{2\psi}\left(1-\frac{R'}{R''}\right)$ nearly,

* "Reports on Electrical Standards," p. 103.

when α is small, and this, in one or two of their experiments, exceeds unity or α exceeds $28°\cdot6$, which is absurd. Taking even one of the ordinary cases where $\frac{R_{\prime}}{R_{\prime\prime}}=1\cdot02$ and ψ is about $\frac{1}{20}$, we have $\alpha=12°\cdot$ nearly, which is a value so large that it would surely have been noticed. Hence we may conclude that no reasonable amount of torsion in the silk fiber could have produced the difference in the results from positive and negative rotation, as has been stated by Mr. Fleming Jenkin in his "Report on the New Unit of Electrical Resistance."*

The greatest value which we can possibly assign to α which might have remained unnoticed is $\frac{1}{10}$, which would not have affected the experiment to any appreciable extent.

Another source of error which may produce the difference we are discussing is connected with the heavy metal frame of the apparatus, in which currents can be induced by the revolving coil. The coil passes so near the frame-work that the currents in it must be quite strong and produce considerable magnetic effect. Kohlrausch has pointed out the existence of these currents, but has failed to consider the theory of them. Now, from the fact that after any number of revolutions the number of lines of force passing through any part of the apparatus is the same as before, we immediately deduce the fact that, if Ohm's law be correct, the algebraical sum of the currents at every point in the frame is zero, and hence the average magnetic action on the needle zero. But although these currents can have no direct action, they can still act by modifying the current in the coil; for while the coil is nearing one of the supports the current in the coil is less than the normal amount, and while it is leaving it it is greater; and although the total current in the coil is the normal amount, yet it acts on the needle at a different angle. By changing the direction of rotation, the effect is nearly but not quite eliminated. The amount of the effect is evidently dependent upon the velocity of rotation and increases with it in some unknown proportion, and the residual effect is evidently in the direction of making the action on the needle too small and thus of increasing R. If these currents are the cause of the different values of R obtained with positive and negative rotation, we should find that, if we picked out those experiments in which this difference was the greatest, they should give a larger value of R than the others. Taking the mean of all the results† in which this dif-

* "Reports on Electrical Standards," London, 1873, p. 191.

† In the table published by the Committee the different columns do not agree, and I have thought it probable that the last two numbers in the next to the last column should read 1·0032 and 1·0065 instead of 1·0040 and ·9981, and in my discussion I have considered them to read thus.

ference is greater than one per cent, we find for the Ohm $1{\cdot}0033\ \frac{\text{earth quad.}}{\text{sec.}}$, and when it is less than one per cent, $\cdot9966\ \frac{\text{earth quad.}}{\text{sec.}}$, which is in accordance with the theory, the average velocities being $\frac{100}{26}$ and $\frac{100}{43}$ nearly. But the individual observations have too great a probable error for an exact comparison.

But whatever the cause of the effect we are considering, the following method of correction must apply. The experiments show that R is a function of the velocity of rotation, and hence, by Taylor's theorem, the true resistance R_0 must be

$$R_0=R(1+Aw+Bw^2+\ \&c.),$$

and when R is the mean of results with positive and negative rotations,

$$R_0=R(1+Bw^2+Dw^4+\&c.).$$

Supposing that all the terms can be omitted except the first two, and using the above results for large and small velocities, we find $R_0=\cdot9926\ \frac{\text{earth quad.}}{\text{sec.}}$. But if we reject the two results in which the difference of positive and negative rotations is over seven per cent, we find

$$R_0=\cdot9934\ \frac{\text{earth quad.}}{\text{sec.}}.$$

The rejection of all the higher powers of w renders the correction uncertain, but it at least shows that the Ohm is somewhat smaller than it was meant to be, which agrees with my experiments.

It is to be regretted that the details of these experiments have never been published, and so an exact estimate of their value can never be made. Indeed, we have no data for determining the value of the Ohm from the experiments of 1863. All we know is that, in the final result, the 1864 experiments had five times the weight of those of 1863, and that the two results differed ·16 per cent, but which was the larger is not stated. Now the table of results published in the report of the 1864 experiments contains many errors, some of which we can find out by comparison of the columns. The following corrections seem probable in the eleven experiments: No. 4, second column, read 4·6375 for 4·6275. No. 10, fourth and fifth columns, read 1·0032 and +0·32 in place of 1·0040 and +0·40. No. 11, fourth and fifth columns, read 1·0065 and +0·65 in place of 0·9981 and −0·19. Whether we make these corrections or not the mean value is entirely incompatible with the statement with respect to the 1863 experiments.

With the corrections the mean value of the 1864 experiments is 1 Ohm $= 1{\cdot}00071 \frac{\text{earth quad.}}{\text{sec.}}$, and without them, using the fourth column, it is 1·00014. With the corrections the difference between fast and slow rotation is ·6 per cent.

In the year 1870 Professor F. Kohlrausch made a new determination of Siemen's unit in absolute measure, the method being one formed out of a combination of Weber's two methods of the earth inductor and of damping, by which the constant of the galvanometer was eliminated, and is the same as Weber used in his experiments of 1862. His formula for the resistance of the circuit, omitting small corrections, is

$$w = \frac{32S^2T^2t_0(\lambda-\lambda_0)AB}{\pi^2K \quad (A^2+B^2)^2} \text{ approximately,}$$

where S is the surface of the earth inductor, T is the horizontal intensity of the earth's magnetism, K the moment of inertia of the magnet, t_0 the time of vibration of the magnet, λ the logarithmic decrement, and A and B are the arcs in the method of recoil.

One of the principal criticisms I have to offer with respect to this method is the great number of quantities difficult to observe, which enter the equation as squares, cubes, or even fourth powers. Thus S^2 depends upon the fourth power of the radius of the earth inductor. Now this earth inductor was wound years before by W. Weber, and the mean radius determined from the length of wire and controlled by measuring the circumference of the layers. Now the wire was nearly 3·2 mm. diameter with its coating, and the outer and inner radii were 115· mm. and 142 mm. Hence the diameter of the wire occupied two per cent of the radius of the coil, making it uncertain to what point the radius should be measured. As the coil is wound, each winding sinks into the space between the two wires beneath, except at one spot where it must pass over the tops of the lower wires. The wire must also be wound in a helix. All these facts tend to diminish S and make its value as deduced from the length of the wire too large; and any kinks or irregularities in the wire tend in the same direction. And these errors must be large in an earth-inductor of such dimensions, where the wire is so large and many layers are piled on each other. If we admit an error of one-half a millimeter in the radius as determined in this way, it would diminish the value of S^2 1·4 per cent, and make Kohlrausch's result only ·6 per cent greater than the result of the British Association Committee.

Three other quantities, T, λ and K, are very hard to determine with accuracy, and yet T enters as a square. It is to be noted that this earth-inductor is the same as that used by Weber in his experiment of 1862, and which also gave a larger value to the Ohm than those of the British Association Committee. *Indeed, the results with this inductor and by this method form the only cases where the absolute resistance of the Ohm has been found greater than that from the experiments of the British Association Committee.*

There seems to be a small one-sided error in A and B which Kohlrausch does not mention, but which Weber, in his old experiments of 1851, considered worthy of a ·6 per cent correction, and which would diminish $\frac{AB}{(A^2+B^2)^2}$ by 1·2 per cent. This is the error due to loss of time in turning the earth-inductor. As Kohlrausch's needle had a longer time of vibration than Weber's, the correction will be much smaller. In Weber's estimate the damping was not taken into account, and indeed it is impossible to do so with exactness. To get some idea of the value of the correction, however, we can assume that the current from the earth-inductor is uniform through a time t''', and the complete solution then depends on the elimination of nine quantities from ten complicated equations, and which can only be accomplished approximately. If γ is the true value of the angular velocity, as given to the needle by the earth-inductor, and γ is the velocity as deduced from the ordinary equation for the method of recoil, I find

$$\frac{\gamma}{\gamma_{\prime}}=1+\tfrac{1}{24}\left(\pi\frac{t}{T}\right)^2\frac{\left(1-9\left(\frac{\lambda}{\pi}\right)^2\right)(1+\varepsilon^{4\lambda})}{(1+\varepsilon^{2\lambda})^2}+\ \&c.,$$

where λ is the logarithmic decrement, ε the base of the natural system of logarithms, T the time of vibration of the needle, and t the time during which the uniform current from the earth-inductor flows. In the actual case, the current from the earth-inductor is nearly proportional to sin t, and hence it will be more exact to substitute

$$4\left(\frac{t}{\pi}\right)^2\int_0^{\frac{1}{2}\pi} t\sin t\,dt=4\left(\frac{t}{\pi}\right)^2$$

in the place of t^2. The formula then becomes

$$\frac{\gamma}{\gamma_{\prime}}=1+\tfrac{1}{6}\left(\frac{t}{T}\right)^2\left(1-9\left(\frac{\lambda}{\pi}\right)^2\right)\frac{1+\varepsilon^{4\lambda}}{(1+\varepsilon^{2\lambda})^2}+\ \&c.$$

This modification is more exact when λ is small than when it is large, but it is sufficiently exact in all cases to give some idea of the magnitude of the error to be feared from this source. Kohlrausch does not state how long it took him to turn his earth-inductor, but as T=34 seconds, we shall assume

$\frac{t}{\mathrm{T}}=\frac{1}{10}$, and as $\lambda=\frac{1}{2}$ nearly, we have

$$\frac{\gamma}{\gamma_1}=1{\cdot}0008,$$

which would diminish the value of the resistance by ·16 per cent.

As the time we have allowed for turning the earth-inductor is probably greater than it actually was, the actual correction will be less than this.

The correction for the extra current induced in the inductor and galvanometer, as given by Maxwell's equation,* has been shown by Stoletow to be too small to affect the result appreciably.

We may sum up our criticism of this experiment in a few words. The method is defective because, although absolute resistance has the dimensions of $\frac{\text{space}}{\text{time}}$, yet in this method the fourth power of space and the square of time enter, besides other quantities which are difficult to determine. The instruments are defective, because the earth-inductor was of such poor proportion and made of such large wire that its average radius was difficult to determine, and was undoubtedly overestimated.

It seems probable that a paper scale, which expands and contracts with the weather, was used. And lastly, the results with this inductor and by this method have twice given greater results than anybody else has ever found, and greater than the known values of the mechanical equivalent of heat would indicate.

The latest experiments on resistance have been made by Lorenz of Copenhagen,† by a new method of his own, or rather by an application of an experiment of Faraday's. It consists in measuring the difference of potential between the center and edge of a disc in rapid rotation in a field of known magnetic intensity.

A lengthy criticism of this experiment is not needed, seeing that it was made more to illustrate the method than to give a new value to the Ohm. The quantity primarily determined by the experiment was the absolute resistance of mercury, and the Ohm will have various values according to the different values which we assume for the resistance of mercury in Ohms.

One of the principal defects of the experiment is the large ratio between the radius of the revolving disc and the coil in which it revolved.

* "Electricity and Magnetism," Art. 762.

† Pogg. Ann., Bd. cxlix, (1873), p. 251.

In conclusion I give the following table of results, reduced as nearly as possible to the absolute value of the Ohm in $\frac{\text{earth quad.}}{\text{sec.}}$.*

Date.	Observer.	Value of Ohm.	Remarks.
1849	Kirchhoff	·88 to ·90	Approximately.
1851	Weber	·95 to ·97	"
1862	Weber	1·088	From Thomson's unit.
		1·075	From Weber's value of Siemen's unit.
1863–4	B. A. Committee	1·0000	Mean of all results.
		·993	Corrected to a zero velocity of coil.
1870	Kohlrausch	1·0196	
1873	Lorenz	·970	Taking ratio of quicksilver unit to Ohm =·962.
		·980	Taking ratio of quicksilver unit to Ohm =·953.
1876	Rowland	·9912	From a preliminary comparison with the B. A. unit.

[To be continued.]