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ON THE DETERMINATION OF THE OHM [B. A. UNIT] IN ABSOLUTE MEASURE. By Lord RAYLEIGH, F.R.S., and ARTHUR SCHUSTER, Ph.D., F.R.S.

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Part I.—By Lord Rayleigh.

It is generally felt that considerable uncertainty still attaches to the real value of the ohm, or British Association unit of resistance. The ohm was constructed to represent 10^9 C.G.S. absolute units, but according to Kohlrausch* it is nearly 2 per cent. too great, and according to Rowland† nearly 1 per cent. too small. On the other hand, H. Weber‡ has obtained by more than one method results very nearly in harmony with those of the British Association Committee. Influenced partly by the fact that the original apparatus (though a good deal out of repair) and the standard coils themselves were in the Cavendish Laboratory, I determined last June to repeat the measurement by the method of the Committee, which has been employed by no subsequent experimenter, and sought permission from the Council of the British Association to make the necessary alterations in the apparatus. In this way I hoped not merely to obtain an independent result, but also to form an opinion upon the importance of certain criticisms which have been passed upon the work of the Committee.

The method, it will be remembered, consists in causing a coil of insulated wire, forming a closed circuit, to revolve about a vertical axis, and in observing the deflection from the magnetic meridian of a magnet suspended at its centre, the deflection being due to the currents developed in the coil under the influence of the earth's magnetism. The amount of the deflection

* *Phil. Mag.* vol. XLVII. p. 294, 1874.
† *American Journal of Science and Arts*, 1878.
‡ *Phil. Mag.* vol. v. p. 30, 1878.

is independent of the intensity of the earth's magnetic force, and it varies inversely as the resistance of the circuit. The theory of the experiment is explained very fully in the reports of the Committee* and in Maxwell's *Electricity and Magnetism*, section 763. For the sake of distinctness, and as affording an opportunity for one or two minor criticisms, a short statement in the original notation will be convenient:—

H = horizontal component of earth's magnetism.

γ = strength of current in coil at time t.

G = total area inclosed by all the windings of the wire.

ω = angular velocity of rotation.

$\theta = \omega t$ = angle between plane of coil and magnetic meridian.

M = magnetic moment of suspended magnet.

ϕ = angle between the axis of the magnet and the magnetic meridian.

K = magnetic force at the centre of the coil due to unit current in the wire.

L = coefficient of self-induction of coil.

R = resistance of coil in absolute measure.

$MH\tau$ = force of torsion of fibre per unit of angular rotation.

The equation determining the current is—

$$L\frac{d\gamma}{dt} + R\gamma = HG\omega\cos\omega t + MK\omega\cos(\omega t - \phi), \quad \text{...........(1)}$$

whence

$$\gamma = \frac{\omega}{R^2 + L^2\omega^2}\{GH(R\cos\theta + L\omega\sin\theta) + KM(R\cos(\theta - \phi) + L\omega\sin(\theta - \phi))\}. \quad \text{......(2)}$$

If L were zero, or if the rotation were extremely slow, the current would (apart from KM) be greatest when the coil is passing through the meridian. In consequence of self-induction, the phase of the current is retarded, and its maximum value is diminished. At the higher speeds used by the Committee, the retardation of phase amounted to 20°.

To find the effect of (2) upon the suspended needle, we have to introduce MK and the resolving factor $\cos(\theta - \phi)$, and then to take the average. This, on the supposition that the needle remains on the whole balanced at ϕ, must be equal to the force of restitution due to the direct action of the earth's magnetism and to torsion, *i.e.*, $MH\sin\phi + MH\tau\phi$. Thus—

$$\frac{\frac{1}{2}MK\omega}{R^2 + L^2\omega^2}\{GH(R\cos\phi + L\omega\sin\phi) + KMR\} - MH(\sin\phi + \tau\phi) = 0.$$

* Collected in one volume. Spon, London, 1873.

In the actual experiment τ is a very small quantity, say $\frac{1}{1000}$; and the distinction between $\tau\phi$ and $\tau \sin\phi$ may be neglected.

$$R^2 - R\frac{\frac{1}{2}GK\omega\cot\phi}{1+\tau}\left(1+\frac{MK}{GH}\sec\phi\right) + L^2\omega^2 - \frac{\frac{1}{2}GKL\omega^2}{1+\tau} = 0. \quad\ldots\ldots(3)$$

If we omit the small terms depending upon τ and upon MK/GH, we get on solution and expansion of the radical—

$$R = \tfrac{1}{2}GK\omega\cot\phi\left\{1 - \frac{2L}{GK}\left(\frac{2L}{GK}-1\right)\tan^2\phi - \left(\frac{2L}{GK}\right)^2\left(\frac{2L}{GK}-1\right)^2\tan^4\phi\ldots\right\}. \quad\ldots\ldots(4)$$

The term in $\tan^4\phi$ is not given in the report of the Committee; but, as I learn from Mr Hockin through Dr Schuster, it was included in the actual reductions. But the next term in $\tan^6\phi$, and one arising from a combination of the correction for self-induction with that depending on M, are not altogether insensible, so that probably the direct use of the quadratic is more convenient than the expansion. At the high speeds used by the Committee the correction for self-induction amounted to some 8 per cent., and therefore cannot be treated as very small.

If the axis of rotation be not truly vertical, a correction for level is necessary. In the case of coincidence with the line of dip, no currents, due to the earth's magnetism, would be developed. If the upper end of the axis deviate from the vertical by a small angle β towards the north, the electromotive forces are increased in the ratio $\cos(I+\beta) : \cos I$, *i.e.*, in the ratio $1+\tan I.\beta$, I being the angle of dip. A deviation in the east and west plane will have an effect of the second order only. The magnetic forces due to the currents will not act upon the needle precisely as if the plane of the coil were always vertical, but the difference is of the second order, so that the whole effect of a small error of level may be represented by writing $G(1+\tan I.\beta)$ for G in (3) or (4).

The next step is to express GK in terms of the measurements of the coil. In order that there may be a passage for the suspending fibre and its enveloping tube, it is necessary that the coil be double, or if we prefer so to express it, that there be a gap in the middle. If [see figure]

a = mean radius of each coil,

n = whole number of windings,

b = axial dimension of section of each coil,

c = radial dimension of section of each coil,

b' = distance of mean plane of each coil from the axis of motion,

α = angle subtended at centre by radius of each coil, so that $\cot\alpha = b'/a$, then—

$$G = \pi n a^2 \left(1 + \tfrac{1}{12}\frac{c^2}{a^2}\right), \quad \text{......(5)}$$

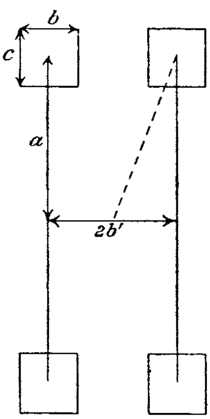
$$K = \frac{2\pi n}{a}\sin^3\alpha\left\{1 + \tfrac{1}{24}\frac{c^2}{a^2}(2 - 15\sin^2\alpha\cos^2\alpha) + \tfrac{1}{24}\frac{b^2}{a^2}(15\sin^2\alpha\cos^2\alpha - 3\sin^2\alpha)\right\}, \quad \text{......(6)}$$

so that

$$GK = 2\pi^2 n^2 a \sin^3\alpha\left\{1 + \tfrac{1}{6}\frac{c^2}{a^2} + \tfrac{5}{8}\frac{b^2 - c^2}{a^2}\sin^2\alpha\cos^2\alpha - \tfrac{1}{8}\frac{b^2}{a^2}\sin^2\alpha\right\}. \quad \text{......(7)}$$

The correction due to the finiteness of b and c is in practice extremely small, but the factor $\sin^3\alpha$ must be determined with full accuracy.

In order to arrive at the value of MK/GH, which occurs in (3), we observe that the approximate value of K/G is $2\sin^3\alpha/a^3$; so that MK/GH is equal to $\tan\mu$, where μ is the angle through which the needle of a magnetometer is deflected when the suspended magnet (M) is placed at a distance from it $a/\sin\alpha$ to the east or west, with the magnetic axis pointing east or west. In practice the difference of readings when M is reversed is taken in order to double the effect, and any convenient distance is used in lieu of $a/\sin\alpha$, allowance being easily made by the law of cubes.



The correction for torsion is determined by giving the suspended magnet one (or more) complete turns, and observing the displacement. If this be δ_1, reckoned in divisions of the scale, *i.e.*, in millimetres, and D be the distance from the mirror to the scale reckoned in millimetres,

$$\tau = \frac{\delta_1}{4\pi D}. \quad \text{......(8)}$$

The correction for scale reading, necessary in order to pass from $\frac{1}{2}\tan 2\phi$ to $\tan\phi$, will be explained under the head of reductions.

Corrections depending upon irregularity in the magnetic field, and in the adjustment of the magnet to the centre of the coil, are given in the report. They are exceedingly small. The same may be said of errors due to imperfect adjustment of the coil with respect to the axis of rotation.

In remounting the apparatus the first point for consideration was the driving gear. The Committee used a Huyghens' gearing, driven by hand, in conjunction with a governor. This, it appeared to me, might advantageously be replaced by a water-motor; and Bailey's "Thirlmere" engine, which acts by the impulse of a jet of water upon revolving cups, was chosen as suitable for the purpose. As the pressure in the public water pipes is not sufficiently uniform, it was at first intended to introduce a reducing valve; but on reflection it seemed simpler to obtain a constant head of water by connecting the engine with a small cistern at the top of the building. This cistern is just big enough to hold the ball-tap by which it is supplied, and gives at the engine a head of about 50 feet.

The success of this arrangement depends upon attention to principles, as to which it may be well to say a few words. The work done by many prime movers is within practical limits proportional to the speed. If the work necessary to be done in order to overcome resistances, as in overcoming solid friction, or in pulling up weights, be also proportional to the speed, there is nothing to determine the rate of the engine, and in the absence of an effective governor the motion will be extremely unsteady. In general the resistance function will be of the form—

$$Bv + Cv^2 + Dv^3 + \dots,$$

in which the above-mentioned resistances are included under B. The term in C will represent resistances of the nature of viscosity, and that in D a resistance such as is incurred in setting fluids in motion by a fan or otherwise. By these resistances, if present, the speed of working will be determined.

In the water impulse engine, however, the work is not proportional to the speed. At zero speed no work is done; neither is any work done at a speed such that the cups retreat with the full velocity of the jet. The speed of maximum efficiency is the half of the last, and the curve representing work as a function of speed is a parabola with vertex directed upwards. If we draw upon the same diagram the curve of work and the curve of resistance, the actual speed will correspond to the point of intersection, and will be well or ill defined according as the angle of intersection is great or small. At the higher speeds of the coil (four to six revolutions per second) so much air is set in motion that the resistance curve is highly convex downwards, and no difficulty is experienced in obtaining a nearly uniform motion. But when the speed of rotation is as slow as once a second, the principal resistance is due to solid friction, and the requisite curvature in the diagrams must be obtained in the curve of work. It was necessary in order to obtain a satisfactory performance at low speeds to introduce an additional reducing pulley, so that the engine might run fast, although the coil was running slow.

The revolving coil with its frame, and the apparatus for suspending the magnet, were at first arranged as described by the Committee. This description, with drawings, is to be found in the report, and it is reproduced in Gordon's *Electricity and Magnetism*, vol. I. The water engine was ready about the middle of June, and towards the end of the month the apparatus was mounted by Mr Horace Darwin. During July and August preliminary trials were made by Mr Darwin, Mrs Sidgwick, and myself, and various troubles were encountered.

The only point in which the arrangement adopted by the Committee was intentionally departed from was in the connexion of the magnet and mirror. The magnet is necessarily placed at the centre of the revolving coil, but in their arrangement the mirror is on the top of the frame and is connected to the magnet by a brass wire. In order to save weight, I preferred to have the magnet and mirror close together, not anticipating any difficulty from the periodic and very brief interruption caused by the passage of the coil across the line of sight. A box was, therefore, prepared with a glass front, through which the mirror could be observed, and was attached to the end of a brass tube coming through the hollow axle of the coil. This tube itself was supported on screws resting on the top of the frame. The upper end of the suspension fibre was carried by a tall tripod resting independently on the floor.

The first matter for examination was the behaviour of the magnet and mirror when the coil was spinning with circuit open. At low speeds the result was fairly satisfactory, but at six or more revolutions per second a violent disturbance set in. This could not be attributed to the direct action of wind, as the case surrounding the suspended parts was nearly air-tight, except at the top. It was noticed by Mr Darwin that even at low speeds a disturbance was caused at every stroke of the bell. This observation pointed to mechanical tremor, communicated through the frame, as the cause of the difficulty, and the next step was to support the case surrounding the suspended parts independently. A rough trial indicated some improvement, but at this point the experiments had to be laid aside for a time.

From the fact that the disturbance in question was produced by the slightest touch (as by a tap of the finger nail) upon the box, while the upper parts of the tube could be shaken with impunity, it appeared that it must depend upon a reaction between the air included in the box and the mirror. It is known that a flat body tends to set itself across the direction of any steady current of the fluid in which it is immersed, and we may fairly suppose than an effect of the same character will follow from an alternating current. At the moment of the tap upon the box the air inside is made to move past the mirror, and probably executes several vibrations. While these vibrations last, the mirror is subject to a twisting force tending to set it at right angles to the direction of vibration. The whole action being over in a time very small compared with that of the free vibrations of the magnet and mirror, the observed effect is as if an impulse had been given to the suspended parts.

In order to illustrate this effect I contrived the following experiment*. A small disk of paper, about the size of a sixpence, was hung by a fine silk fibre across the mouth of a resonator of pitch 128. When a sound of this pitch is excited in the neighbourhood, there is a powerful rush of air in and out of the resonator, and the disk sets itself promptly across the passage. A fork of pitch 128 may be held near the resonator, but it is better to use a second resonator at a little distance in order to avoid any possible disturbance due to the neighbourhood of the vibrating prongs. The experiment, though rather less striking, was also successful with forks and resonators of pitch 256.

It will be convenient here to describe the method adopted for regulating and determining the speed of rotation, which has proved thoroughly satisfactory. In the experiments of the Committee a governor was employed, and the speed was determined by means of the bell already referred to. This bell received a stroke every 100 revolutions, and the times were taken with a chronometer. In this method rather long spinnings (ten or twenty minutes) are necessary in order to get the speed with sufficient accuracy, much longer than are required to take the readings at the telescope. Desirous, if possible, of making the observations more quickly, I determined to try the stroboscopic method. On the axis of the instrument a stout card of 14 inches diameter was mounted, divided into concentric circles of black and white teeth. The black and white spaces were equal, and the black only were counted as teeth. There were five circles, containing 60, 32, 24, 20, 16 teeth respectively, the outside circle having the largest number of teeth.

This disk was observed from a distance through a telescope, and an arrangement for affording an intermittent view. An electric tuning-fork of frequency about $63\frac{1}{2}$ was maintained in regular vibration in the usual way by means of a Grove cell. To the ends of the prongs are attached thin plates of metal, perforated with somewhat narrow slits parallel to the prongs. In the position of equilibrium these slits overlap so as to allow an unobstructed view, but in other positions of the fork the disk cannot be seen. When the fork vibrates, the disk is seen intermittently 127 times a second; and if the speed be such that on any one of the circles 127 teeth a second pass a fixed pointer, that circle is seen as if it were at rest.

By means of the various circles it is possible to observe correspondingly varied speeds without any change in the frequency of the fork's vibration. A further step in this direction may be taken by modifying the arrangement for intermittent view. If the eye be placed at the top or bottom of one of the vibrating plates, a view is obtained once only, instead of twice, during

* *Proc. Camb. Phil. Soc.* Nov. 8, 1880. [1899. For a lecture experiment the paper disc may be replaced by a magnet and mirror, such as are used for galvanometers. See also *Phil. Mag.* vol. XIV. p. 186, 1882.]

each vibration of the fork. This plan was adopted for the slowest rotation, and allowed 60 teeth to take the place of 120, which would otherwise have been necessary.

The performance of the fork was very satisfactory. It would go for hours without the smallest attention, except an occasional renewal of the alcohol in the mercury cup. Pure (not methylated) alcohol was used for this purpose, and a *platinum* point made and broke the contacts. Although, as it turned out, this fork vibrated with great regularity, dependence was not placed upon it, but repeated comparisons by means of beats were made between it and a standard fork of Kœnig's construction, of pitch (about) 128. These beats, at pitch 128, were about 48 per minute, and scarcely varied perceptibly during the course of the experiments. They could have been counted for an even longer time, but this was not necessary. It was intended at first to make the comparisons of the forks simultaneous with the other observations, but this was given up as a needless refinement.

Some care was necessary in the optical arrangements to obviate undue fatigue of the eyes in a long series of observations. In daylight the illumination of the card was sufficient without special provision, but at night, when the actual observations were made, the image of an Argand gas flame was thrown upon the pointer and the part of the card near it. On account of the necessity of removing the electric fork and its appliances to a distance, the card, if looked at directly, would appear too much foreshortened, and a looking-glass was therefore introduced. The eyepiece of the telescope, close in front of the slits, was adjusted to the exact height, and the eye was placed immediately behind the slits. By cutting off stray light as completely as possible, the observation may be made without fatigue and with slits narrow enough to give good definition when the speed is correct.

As governor I had originally intended to employ an electro-magnetic contrivance, invented a few years ago by La Cour and myself*, in which a revolving wheel is made to take its time from a vibrating fork, and it was partly for this reason that the water engine was placed at a considerable distance from the revolving coil. I was, however, not without hopes that a governor would be found unnecessary, and a few trials with the stroboscopic apparatus were very encouraging. It appeared that by having the water power a little in excess, the observer looking through the vibrating slits could easily control the speed by applying a slight friction to the cord connecting the engine and coil. For this purpose the cord was allowed to run lightly through the fingers, and after a little practice there was no difficulty in so regulating the speed that a tooth was never allowed finally to pass the pointer, however long the observation was continued. If from a

* *Nature*, May 23, 1878. [Art. 56; vol. I. p. 355.]

momentary inadvertence or from some slight disturbance a tooth passed, it could readily be brought back again. The power of control thus obtained will be appreciated when it is remembered that the passage of a tooth *per second* would correspond to less than one per cent. on the speed. In many of the observations the pointer covered the same tooth all the while, so that the introduction of a governor could only have done harm.

Another, and perhaps still more important, improvement on the original method related to the manner of making correction for the changes of declination which usually occur during the progress of the experiments. The Committee relied for this purpose upon comparisons with the photographic records made at Kew, and they recognise that considerable disturbances arose from the passage of steamers, &c. All difficulty of this kind is removed by the plan which we adopted of taking simultaneous readings of a second magnetometer, called the auxiliary magnetometer, placed at a sufficient distance from the revolving coil to be sensibly unaffected by it, but near enough to be similarly influenced by changes in the earth's magnetism, and by other disturbances having their origin at a moderate distance. The auxiliary magnetometer was of very simple construction, and was read with a telescope and a millimetre scale, the distance between mirror and scale (about $2\frac{1}{2}$ metres) being adjusted to approximate equality with that used for the principal magnet, so that disturbances were eliminated by simple comparisons of the scale readings. During a magnetic storm it was very interesting to watch the simultaneous movements of the magnets.

In the month of September the apparatus was remounted under the direction of Professor Stuart, to whose advice we have often been indebted. In order to examine whether any errors were caused by the circulation of currents in the frame, as has been suggested by more than one critic, insulating pieces were inserted, mercury cups at the same time being provided, so that the contacts could be restored at pleasure. But the principal changes related to the manner of suspending the fibre and supporting the box and tube. In order to eliminate tremor, as far as possible, these parts were supported by a massive wooden stand, resting on the floor and overhanging, but without contact, the top of the metal frame of the coil. The upper end of the fibre was fastened to a rod sliding in a metal cap, which formed the upper extremity of a 2-inch glass tube. Near the other end this tube was attached to a triangular piece of brass, resting on three screws, by which the whole could be raised or lowered bodily and levelled. Rigidly attached to this tube, and forming a continuation of it, a second glass tube, narrow enough to pass freely through the hollow axle of the coil, protected the fibre as far as the box in which the mirror and magnet were hung. This box was cylindrical and about 3 inches in diameter. The top fitted stiffly to the lower end of the narrow glass tube, and the body of the box could be unscrewed, so as to give access to the interior. The window necessary for observation of the mirror was made of a piece of worked glass, and was fitted air-tight.

On my return to Cambridge in October the apparatus was tested, but without the full success that had been hoped for. At high speeds there was still unsteadiness enough to preclude the use of these speeds for measurement. Since it is impossible to suppose that the tremor is propagated with sufficient intensity through the floor and massive brickwork on which the coil is supported, the cause must be looked for in the fanning action of the revolving coil, aggravated no doubt by the somewhat pendulous character of the box, and perhaps by the nearness of the approach between the coil and its frame at three points of the revolution.

At this time the experiment was in danger of languishing, as other occupations prevented Mr Darwin from taking any further part; but on Dr Schuster's return to Cambridge he offered his valuable assistance. Encouraged by Sir W. Thomson, we determined to proceed with the measurements, inasmuch as no disturbance, due to the rotation of the coil with circuit open, could be detected until higher speeds were approached than it was at all necessary to use.

One of the first points submitted to examination was the influence of currents induced in the frame. Without altering the speed or making any other change, readings were taken alternately with the contact-pieces in and out. Observations made on several days agreed in showing a small effect, due to the currents in the frame, in the direction of a *diminished* deflection. The whole deflection being 516 divisions of the scale, the mean diminution on making the top contacts was ·86 division. When the coil was at rest no difference in the zero could be detected on moving the contact-pieces.

In these preliminary experiments very consistent results were obtained at constant speeds, whether the rotation was in one direction or the other; but when deflections at various speeds were compared, we were startled to find the larger deflections falling very considerably short of proportionality to the speeds. There are only two corrections which tend to disturb this proportionality—(1) the correction for scale-reading, (2) the correction for self-induction. The effect of the first is to make the readings too high, and of the second to make the readings too low at the greater speeds. According to the figures given by the Committee (Report, p. 106), the aggregate effect is to increase the readings, on account of the preponderance of (1) over (2), whereas our results were consistently of the opposite character. Everything that could be thought of as a possible explanation was examined theoretically and experimentally, but without success. The coil was dismounted and the wire unwound, in order to see whether there was any false