V. *On the Constant of Magnetic Rotation of Light in Bisulphide of Carbon.*

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1. THE phenomenon, to which the present investigation relates, is FARADAY'S discovery of the "Magnetisation of Light," or in more usual language the rotation of the plane of polarisation of light in traversing certain media exposed to powerful magnetic force. One of the characteristics of this rotation is that it takes place in the same absolute direction which ever way the light may be travelling, differing in this respect from the rotation which occurs without the operation of magnetic force in quartz and many organic liquids. Advantage of this property has been taken by FARADAY and others in order to magnify the effect. By reflecting the light backwards and forwards it is possible to make it traverse several times a field of force whose length is limited.

A consequence remarkable from the theoretical point of view is the possibility of an arrangement in which the otherwise general optical law of reciprocity shall be violated. Consider, for example, a column of diamagnetic medium exposed to such a force that the rotation is 45°, and situated between two NICOLS, whose principal planes are inclined to one another at 45°. Under these circumstances light passing one way is completely stopped by the second NICOL, but light passing the other way is completely transmitted. A source of light at one point A would thus be visible at a second point B, when a source at B would be invisible at A; a state of things *at first sight* inconsistent with the second law of thermodynamics.

2. It is known that the rotation may be considered to be due to the propagation at slightly different velocities of the two circularly polarised components, into which plane polarised light may be resolved; and it is interesting to consider what difference of velocity our instrumental appliances enable us to detect. A retardation amounting to one wave length (λ), of one circularly polarised component relatively to the other would correspond to a rotation of the plane of polarisation through 180°. If we can observe a rotation of one minute, we are in a position to detect a retardation of $\lambda/10800$. If l be the thickness traversed, v and $v+\delta v$ the two velocities of propagation, the relative retardation is $l\,\delta v/v$. To take an example, suppose that $l=20$ inches, $\lambda=\frac{1}{40,000}$th inch; so that if $\delta v/v$ exceed 10^{-8}, the fact might be detected.* It appears therefore that we are able to observe extraordinarily minute relative differences in the velocities of propagation of the two circularly polarised rays.

* Camb. Nat. Sci. Trip. Ex., 1883.

3. The laws of the phenomenon were investigated in detail by VERDET, who proved experimentally that in a given medium the rotation between any two points on a ray of light of given kind is proportional to the difference of magnetic potential at those points. When the path of the ray is singly or doubly curved, the rotation is to be estimated upon principles similar to those applicable to *twist* * in curved rods.†

4. Absolute determinations of magnetic rotation in bisulphide of carbon have been made by GORDON,‡ and by H. BECQUEREL,§ whose results differ, however, by about 9 per cent. The former obtained his magnetic force by means of an electric current circulating a great many times round the column of CS_2. This column being a good deal longer than the coil, the electromagnetic effect is approximately determined by the strength of the current and the number of turns. Of these data the first was found by a comparison with H (the horizontal component of terrestrial magnetism). The number of windings in the coil was determined, not by a simple counting, but *à posteriori* by an electrical process.

In M. BECQUEREL'S experiments the magnetic force was that of the earth acting on a column of CS_2 more than 3 metres in length. The very small effect (obtained by reversal of the apparatus in azimuth) was augmented by causing the light to pass the tube 3 or 5 times, but even with 5 passages the double rotation amounted to only about 30 minutes. M. BECQUEREL regards his determination for sodium light as accurate to within 1 per cent., which would be indeed a wonderful result considering the smallness of the rotation.

5. It is important to observe that great care is required in order to define with sufficient accuracy the kind of light employed. Since the rotation is approximately proportional to λ^{-2}, a change from one sodium line to the other would make a difference of two parts per thousand. Both of the above-mentioned experimenters started with white light. GORDON threw a spectrum upon a screen, perforated with a slit, the position of which was adjusted to correspond with the thallium line; while BECQUEREL corrected his results indirectly by a subsequent comparison between the effects of the more mixed light used by him and that emitted by sodium.

Considering that the employment of white light involved very elaborate arrangements for analysis (according to wave length), in order to avoid errors exceeding in magnitude those likely to be encountered in the polarimetric or electric determinations, I decided to use light actually emitted from sodium vapour. The sodium chloride was held by a spoon of platinum gauze in the flame of a small ordinary

* THOMSON and TAIT'S 'Natural Philosophy,' §§ 119–123.

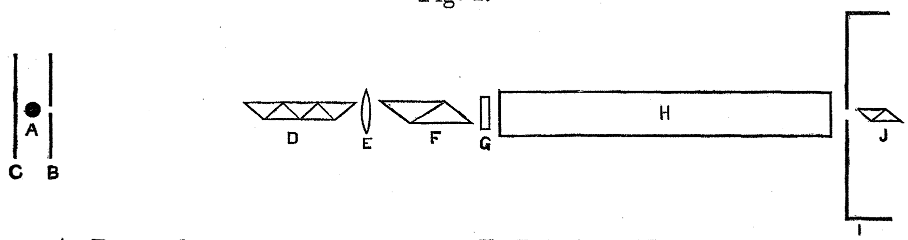
† When polarised light passes from one medium to another, *e.g.*, from air to glass, the plane of polarisation is in general twisted without the operation of any magnetic force. This effect, however, depends upon a part of the light being diverted by reflection, and would disappear if the transition from one medium to the other were gradual, *i.e.*, occupied a stratum a few wave-lengths thick. (See Proc. Math. Soc., vol. xi., No. 159).

‡ Phil. Trans. 1877, p. 1.

§ Ann. d. Chimie, 1882.

BUNSEN burner (fig. 1, A). As in Mr. GLAZEBROOK's optical investigations, the evaporation of the salt and the temperature of the flame were stimulated by a jet of oxygen gas, brought in laterally and caused to play round the gauze.

Fig. 1.



A. BUNSEN burner.
B. Mirror with slit.
C. Back mirror.
D. Direct vision prism.
E. Collimating lens.
F. Polarising NICOL.
G. Sugar cell.
H. Tube of bisulphide of carbon.
I. Screen (blackened inside).
J. Analysing NICOL.

At the close of the experiments I examined the light thus obtained with a powerful spectroscope, and found that under the influence of the oxygen the originally narrow bright lines dilate almost to the point of contact, thus forming a bright field upon which the dark D-lines are seen with beautiful definition. Although the distribution of light appeared to be tolerably symmetrical, it is a question to what degree of accuracy the mean quality of this light can be identified with that coming from midway between the D-lines. Probably we shall be safe in estimating that the error from this cause is well below $\frac{1}{1000}$.

The bright part of the flame being much larger than is required, a screen (B), perforated with a slit, may conveniently be interposed. In this course there are two advantages. It allows us to purify the light from rays of other refrangibilities (of which there is always a sensible accompaniment, both red and blue), by use of a direct-vision prism (D). Again, by making this screen of looking-glass, from which a narrow strip of silvering is removed, and by backing the flame with a parallel mirror (C), we gain by repeated reflections to and fro, an important increase of illumination. The success of the polarimetry is very dependent upon the intensity of the light, but there must be also a reasonable steadiness. Several arrangements of flame which at first promised well failed in the latter requirement.

6. The rays from the slit, after purification by the direct vision prism, are rendered parallel by a collimating lens (E) and pass into the polarising NICOL (F). The polarimeter employed is on the principle of LAURENT, but according to a suggestion of POYNTING* the half-wave plate of quartz is replaced by a cell (G) containing syrop, so arranged that the two halves of the field of view are subjected to small rotations differing by about 2°. The difference of thicknesses necessary is best obtained by introducing into the cell a piece of thick glass, the upper edge of which divides the

* Phil. Mag., July, 1880.

field into two parts. The upper half of the field is thus rotated by a thickness of syrop equal to the entire width of the cell (say $\frac{1}{2}$ inch), but in the lower half of the field part of the thickness of syrop is replaced by glass, and the rotation is correspondingly less. With a pretty strong syrop a difference of 2° may be obtained with a glass $\frac{3}{16}$ inch thick. For the best results the operating boundary should be a true plane nearly perpendicular to the face. The pieces used by me, however, were not worked, being simply cut with a diamond from thick plate glass; and there was usually no difficulty in finding a part of the edge sufficiently flat for the purpose, *i.e.*, capable of exhibiting a field of view sharply divided into two parts. I had expected to be troubled with depolarisation, especially in the thick glass, but a small piece thus cut out of a large plate is relieved from most of the strain to which it was originally subject. Probably more care would be required in experiments where a strong white light could be used; but by previously testing the rather thin plates used for the sugar cell and for closing the CS_2 tube, I was able to secure a field of view either half of which under the actual circumstances could be made quite dark by suitable orientation of the analysing NICOL.

By this use of sugar half-shade polarimeters may be made of large dimensions at short notice and at very little cost. The syrop should be filtered (hot) through paper, and the cell must be closed to prevent evaporation.

7. On leaving the sugar cell the light entered the column of bisulphide of carbon (H). To contain the liquid two tubes of brass were employed at various times, the ends being closed with plates of worked glass cemented to the metal with a mixture of glue and treacle. Near one end these tubes were provided with a lateral (vertical) branch, closed with a cork, through which passed the stem of the thermometer used for observing the temperature of the CS_2. The length of the larger tube (used in Series I. and II.) was 31·591 inches, and the diameter about $1\frac{1}{2}$ inch. The length of the smaller tube (used in Series III.) was 29·765 inches, and the diameter 1 inch.

When, as in Series I., it was wished to cause the light to traverse the tube more than once, mirrors were necessary at the ends of the tube. They consisted of plates of thin looking-glass, from which part of the silvering was removed, and by means of a little glycerine they were brought into optical contact with the plates by which the tube was closed. This arrangement was simple, and had the further advantage of practically annulling some troublesome reflections; but the want of means of adjustment rendered it necessary that the closing plates should themselves be pretty accurately parallel.

8. The internal diameter of the ebonite tube, upon which the helix was wound (§ 13), was about $1\frac{7}{8}$ inch, and it was intended to utilise the annular space between the ebonite and the brass as a jacket, through which water at the temperature of the room might be made to circulate. This arrangement, however, failed utterly. Within about 10 minutes of the closing of the circuit of the helix, the definition was lost, and nothing further could be done until after a long interval of repose. The water-jacket

was then abolished, and the available space filled with paper wrapped pretty tightly round the tube. This effected a great improvement, enhanced still further in the later experiments of Series III., in which, by reduction of the diameter of the tube, a wider space became available for heat insulation. The disturbance by conduction of heat from the wire to the CS_2 remained, however, the worst feature of the experiments, and could not be obviated without a fundamental alteration in the apparatus. Probably the best arrangement would be a water-jacket next the wire, and a good thickness of paper or other insulator between the water and the CS_2.

9. The bisulphide of carbon was purified by treatment with corrosive sublimate and grease with subsequent distillation (according to the procedure advocated by BECQUEREL), until most of the unpleasant odour had disappeared. The transparency is much greater than is readily (if at all) obtainable with water, provided proper precautions are taken to avoid exposure to light. After being acted upon by light, the CS_2 attacks brass and becomes rapidly opaque. In this respect it would be an advantage to replace the metal tube by one of glass.

10. The analyser consisted, in some experiments, of a NICOL (J), and in others of a double image prism, and was mounted in a circle made by the Cambridge Scientific Instrument Company. In order that a rotation of the plane of polarisation may be correctly indicated by the difference of the two circle readings, it is necessary that the axis of rotation should coincide with the direction of the light. This requirement is, however, not very easily satisfied. At the commencement of a series of experiments the adjustment was made with the aid of a telescope and cross wires temporarily substituted for the NICOL, but during the course of a set of readings the passage of heat into the liquid tended to make the upper strata warmer than the lower, and thus to bend the rays into a different direction. It is known* that the error arising from maladjustment in this respect is in great part eliminated by reading the NICOL always in both the positions (differing by about 180°) which give extinction or (in the half-shade arrangement) equality of illumination. This plan was constantly followed, but it is not clear that the whole error can be thus got rid of. It occurred to me that another term in the harmonic expansion of the error would be destroyed by use of a double image prism read in *four* positions distant about 90°. Experiment showed that in spite of the glare of the unextinguished image, good readings could be obtained after a little practice, and the comparison of the results arrived at in this way tends to show that the error is not wholly eliminated in the mean of two readings taken in positions differing by 180°. But the matter could be much better investigated with a simplified apparatus and the use of a strong white light.

In Series II. and III., when the light traversed the tube but once, no magnification was necessary, and the eye was applied immediately behind the analyser. In

* "Zur Theorie des Polaristrobometer und des drehenden NICOLS." V. D. SANDE BAKHUYZEN. POGG. Ann., cxlv., 259, 1872.

"Notes on NICOL'S Prism," GLAZEBROOK, Phil. Mag., October, 1880.

Series I., the apparent magnitude of the field was much less, and an opera-glass, magnifying about twice, was employed between the analyser and the eye.

11. The setting of the NICOL (or double-image prism) by adjustment of the match between the two parts of the field presented by the half-shade apparatus was facilitated by a device that may be found useful. "In addition to the principal helix, the tube was embraced by an auxiliary coil of insulated wire, through which could be led the current from a LECLANCHÉ cell. This current was controlled by a reversing key under the hand of the observer, who was thus able to *rock* the plane of polarisation backwards and forwards through a small angle about its normal position. The amount of the rocking being suitably chosen, the comparison of the three appearances (two with auxiliary current, and one without) serves to exclude some imperfect matches that might otherwise have been allowed to pass." *

12. Apart from the effect of heat upon the CS_2, the working of the optical parts was fairly satisfactory. The following zero readings taken without the current on June 4, 1884, will give an idea of the sort of accuracy attained. The analyser was a double image prism, and was read in all four positions, the circuit being made three times.

TABLE I.

	103° 2′	193° 4′	283° 0′	13° 2′
	102 55	193 5	283 2	12 59
	102 58	193 3	283 2	13 4
Mean	102 58	193 4	283 1	13 2
Subtract	90	180	270	
	12 58	13 4	13 1	13 2

It appears that an error of 3 or 4 minutes may occur in a single setting.

13. I now pass to the description of the electrical arrangements. The magnetic force depends upon the helix and upon the strength of the current, and we will take these elements in order.

The helix.

The wire is wound upon an ebonite tube, the outside surface of which was turned true in the lathe, and is kept in its place laterally by ebonite flanges screwed upon the tube. The distance between the flanges, equal to the length of the helix, is 9·990 inches; but the tube itself projects some inches beyond the flanges, and when it was desired to use an internal water-jacket, could be further prolonged by additional lengths of brass tube.

In order to give better opportunity for testing the insulation, on which the correctness of the final results is entirely dependent, it was decided to wind on two wires simultaneously, which should be in contact with one another throughout their entire

* "Preliminary Note on the Constant of Electro-magnetic Rotation of Light in Bisulphide of Carbon." Proc. Roy. Soc., vol. 37, p. 146 (June 19, 1884).

length. The operation was performed on December 14–15, 1883, with triply-covered wire of diameter about $\frac{1}{20}$ inch, and no particular difficulty was experienced. The revolutions of the ebonite tube, mounted in the lathe, were taken with all care by an engine counter, and amounted to 1842, so that the total number of windings is 3684. The internal diameter of the helix is 2·188 inches, and the external diameter is 4·13 inches.

By endeavouring to force a current from one wire to the other we obtain a very severe, though of course not absolutely complete, test of the insulation. The resistance between the two wires varied with the hygrometric condition of the silk, which was not impregnated with paraffin. At first it was not much over 2 megohms, but latterly reached 6 or 8 megohms, and was thus abundantly sufficient.

14. As a further test observations were made of the external effect of the helix upon a suspended magnet, when a powerful current was passed in one direction through the first wire, and in the opposite direction through the second. If the positions of the two wires could be treated as identical, the external effect ought everywhere to vanish. In consequence, however, of the fact that one wire lies throughout on the same side of the other, the compensation could not be expected to be complete, except when the suspended magnet is equidistant from the two ends. Experiment with the magnet of a reflecting galvanometer showed that the effect, in fact, varied as the magnet was displaced, but even in the symmetrical position there was a perceptible outstanding differential effect. In order to eliminate the influence of other parts of the circuit, the readings referred only to the deflection of the needle as the current was reversed in the *helix;* and the scale of sensitiveness was obtained by repeating the observations after altering the connexions of the two wires, so that the current circulated the same way round both, and after insertion of a high resistance by which the intensity of the current was reduced in a known proportion. From this it appeared that the differential effect of the two wires (with a given current) was $\frac{1}{2500}$ of the combined effect.

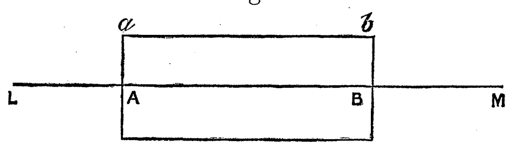
This fraction is tolerably small, but I had expected to find it smaller still. It seems probable that the incompleteness of compensation is due to a small difference ($\frac{1}{5000}$) in the mean diameter of the windings in the two cases. To throw light upon this I took careful measures of the resistances of the two wires. Although they had originally formed one length, their resistances differed by as much as $\frac{1}{70}$th part, that of the wire which had shown itself *least* effective being 7·075 B.A., and of the other 6·965. If, as it seems plausible to do, we attribute the difference of resistance to difference of diameter, this actual difference must amount to $\frac{1}{2800}$ inch. The mean diameter of the windings is about three inches; and if the two wires were wound upon a smooth cylinder of this diameter, the difference in the diameter of the windings would be $\frac{1}{8400}$ of the whole. As this estimate would be increased were we to take into account the fact that each winding really sits upon two windings of the layer underneath, and that these cannot be practically in actual contact, we may perhaps

consider the small anomalous differential effect upon the external magnet to be sufficiently explained by the observed difference of resistances.

Correction for finite length.

15. If the tube were infinitely long the difference of potentials at its ends due to the unit current in one winding would be 4π. But on account of the finiteness of the length a correction is required, whose approximate amount is given in GORDON's paper.

Fig. 2.



Considering, in the first place, one layer of windings of radius Aa, we know that the external effect is the same as would be produced by a uniform distribution of imaginary magnetic matter over the ends, positive (say) over Aa and negative over Bb, the superficial density being equal to the number (m) of windings per unit length. The potential at L of the matter on Aa is

$$2\pi m\,(\mathrm{L}a - \mathrm{LA}),$$

or approximately

$$\pi m\left(\frac{\mathrm{A}a^2}{\mathrm{LA}} - \frac{1}{4}\frac{\mathrm{A}a^4}{\mathrm{LA}^3}\right).$$

Similarly the potential at L for the matter on Bb is

$$-\pi m\left(\frac{\mathrm{A}a^2}{\mathrm{LB}} - \frac{1}{4}\frac{\mathrm{A}a^4}{\mathrm{LB}^3}\right),$$

so that altogether the potential at L for this layer of windings is

$$\pi m\ \mathrm{AB}\left(\frac{\mathrm{A}a^2}{\mathrm{LA.LB}} - \frac{\mathrm{A}a^4}{4}\,\frac{\mathrm{LB}^2 + \mathrm{LA.LB} + \mathrm{LA}^2}{\mathrm{LA}^3.\mathrm{LB}^3}\right),$$

in which mAB denotes the whole number of windings in the layer. This result has now to be integrated so as to represent the effect of the helix, whose inner and outer radii we may call Aa_1 and Aa_2. The mean value of Aa^2 is

$$\frac{\mathrm{A}a_2{}^3 - \mathrm{A}a_1{}^3}{3(\mathrm{A}a_2 - \mathrm{A}a_1)},$$

and that of Aa^4 is

$$\frac{\mathrm{A}a_2{}^5 - \mathrm{A}a_1{}^5}{5(\mathrm{A}a_2 - \mathrm{A}a_1)}.$$

Thus if n be the whole number of windings on the helix, the difference of potential from L to M corresponding to the unit current is

$$4n\pi-4n\pi\left[\frac{Aa_2^3-Aa_1^3}{12\,a_2a_1}\left(\frac{1}{LA.LB}+\frac{1}{MA.MB}\right)\right.$$
$$\left.-\frac{Aa_2^5-Aa_1^5}{80\,a_2a_1}\left(\frac{LB^2+LA.LB+LA^2}{LA^3.LB^3}+\frac{MA^2+MA.MB+MB^2}{MB^3.MA^3}\right)\right]$$

In the present case

$$Aa_2=2{\cdot}065 \text{ (inches)},\quad Aa_1=1{\cdot}094,\quad a_2a_1={\cdot}971,$$

from which we get

$$\frac{Aa_2^3-Aa_1^3}{12\,a_2a_1}={\cdot}6433,\quad \frac{Aa_2^5-Aa_1^5}{80\,a_2a_1}={\cdot}4632.$$

In the remainder of the calculation we have to distinguish the two tubes. For the first

$$LA=MB=10{\cdot}800 \text{ inches}$$
$$LB=MA=20{\cdot}790 \text{ inches};$$

and for the second

$$LA=MB=\ 9{\cdot}887 \text{ inches}$$
$$LB=MA=19{\cdot}877 \text{ inches}.$$

Hence for the first tube we have

$$4n\pi(1-{\cdot}00573+{\cdot}00006)=4n\pi\times{\cdot}99433\,;*$$

and for the second

$$4n\pi(1-{\cdot}00655+{\cdot}00008)=4n\pi\times{\cdot}99353,$$

the correction for finite length thus somewhat exceeding one-half per cent.

16. We have now obtained the difference of potential at the ends of the column of CS_2 due to the passage through the helix of unit current. It yet remains to describe the means adopted for the measurement of the actual current in absolute measure.

In a former paper "On the Electro-chemical Equivalent of Silver, and on the Absolute Electromotive Force of CLARK Cells,"† it was shown how the E.M.F. of a CLARK cell was obtained by comparison with the difference of potentials at the extremities of a wire of known resistance, due to the passage of a current known either directly from its effect upon a current measuring apparatus, or indirectly through the deposition of silver. For the purposes of the present investigation this process was reversed, the

* In the Preliminary Note the reducing factor for this tube was given as ·99449. The alteration is due to the use of more precise data in place of some quite rough measurements in round numbers on which, by an oversight, the first calculation was founded.

† Phil. Trans., 1884, Part II., §§ 35, 36, 38.

CLARK cell itself being treated as a standard of E.M.F., by which to determine the value of the current, which traversed the known resistance, and also the helix by which the magnetic rotation was produced. The arrangements differed so little from those elaborately described in the paper referred to, that it seems unnecessary to enter into the matter at length. If the reader will refer to Plate 17, fig. 1, he will understand the electrical connexions, and he may suppose the current-measuring apparatus EGF, replaced by the magnetising helix. In point of fact this helix was situated in another room at a distance from the E.M.F. compensator and its galvanometer T. The direction of the current in the helix was reversed by a mercury key of the rocker pattern, and care had to be taken that at this moment the galvanometer contact Q was open. The general nature of the arrangement will be sufficiently understood when it is said that the want of balance between the E.M.F. of the CLARK and that at the terminals of the resistance R was made up by E.M.F., taken from an auxiliary circuit, the value of which was afterwards expressed in terms of the CLARK. Denoting the force thus added or subtracted by r, upon a scale according to which the force of the CLARK was ρ, the actual difference of potential at the terminals of R may be written

$$\left(1 \pm \frac{r}{\rho}\right) \times \text{CLARK}.$$

17. As it was intended to use currents of about one ampère, the resistance R was made about [$1\frac{1}{2}$] ohms. The construction was somewhat similar to that of the [4] described in § 33 of the former paper, but on account of the increase in the current to be carried, three wires of German silver were used in parallel. The amount of heating was unimportant for the purposes of the present investigation.

The value of the [$1\frac{1}{2}$] was determined by comparison with a combination of three standard units, one (taking the whole current), and two in parallel (giving the $\frac{1}{2}$). At 13° the resistance is 1·4945 B.A. At 15°, which was adopted as the standard temperature for R and for the CLARK, we have

$$R = 1{\cdot}4958 \text{ B.A.}$$

18. In consequence of the heating of the copper wires, the current (usually obtained from secondary cells) fell off somewhat rapidly during a set of observations, and it was found convenient to take readings of the E.M.F. compensator simultaneously with the adjustment of the polarimeter. The former readings were taken by myself and the latter by Mrs. SIDGWICK, while the flame (at which the optical observer should not look) was regulated by an assistant, who also recorded the circle readings.

The procedure will be most easily explained by an example, for which purpose I take at random the observations of July 25, recorded in Table II.

TABLE II.—July 25, 1884.

Time.	r.	Circle reading. +	Time.	r.	Circle reading. −	Time.	r.	Circle reading. +	Time.	r.	Circle reading. −
h. m.		° ′	h. m.		° ′	h. m.		° ′	h. m.		° ′
6 3½	1525	261 44	6 5	1515	269 19	6 7½	1500	351 56	6 8½	1490	359 24
6 14½	1450	261 45	6 15½	1445	269 18	6 18½	1425	351 56	6 20½	1415	359 22
6 29½	1365	261 47	6 31½	1355	269 16	6 33	1350	351 57	6 35	1340	359 23
Mean	..	261 45·3	..	..	269 17·7	..	..	351 56·3	..	..	359 23·0
6 10	1480	81 48	6 11	1475	89 23	6 12½	1465	171 49	6 13½	1460	179 22
6 23	1400	81 48	6 24½	1395	89 19	6 26	1385	171 53	6 27	1380	179 20
6 37½	1325	81 53	6 39	1320	89 16	6 40	1315	171 55	6 41	1310	179 18
Mean	..	81 49·7	..	..	89 19·3	..	..	171 52·3	..	..	179 20·0

One passage. Double-image prism.

$\rho=7017$.

Temperature of CLARK and R=17°·6. Mean temperature of CS_2=18°·3.

It will be seen that the cycle consisted of eight readings, four with positive and four with negative rotation of the plane of polarisation, and that this cycle is repeated three times.

The three readings under any one head vary in consequence of the diminution of the current as well as from errors of observation. The value of ρ was

at the beginning	$\rho=7018$
at the end	$\rho=7016$
Mean	$\rho=7017$

Thus in the first observation at $6^h\ 3\frac{1}{2}^m$, when the cycle reading was 261° 44′, the difference of potentials at the extremities of the $[1\frac{1}{2}]$ was $\left(1+\frac{1525}{7018}\right)\times$ CLARK I., the temperature of CLARK I. and of the $[1\frac{1}{2}]$ being 17°·6.

For the mean double rotations in the four positions of the double image prism we have

$$
\begin{array}{rrrrrr}
\overset{\circ}{269} & \overset{\prime}{17\cdot7}-\overset{\circ}{261} & \overset{\prime}{45\cdot3} & =\overset{\circ}{7} & \overset{\prime}{32\cdot4} \\
359 & 23\cdot0-351 & 56\cdot3 & =7 & 26\cdot7 \\
89 & 19\cdot3-\ \ 81 & 49\cdot7 & =7 & 29\cdot6 \\
179 & 20\cdot0-171 & 52\cdot3 & =7 & 27\cdot7 \\
 & \text{Mean} & . \ . \ . & 7 & 29\cdot1
\end{array}
$$

Since all the effects are proportional to the current, it is sufficient to compare the mean rotation with the mean value of r, viz., 1413; so that the double rotation 7° 29′·1, or 449′·1, corresponds to a difference of potentials equal to

$$\left(1+\frac{1413}{7017}\right)\times \text{CLARK I.}=\frac{8430}{7017}\times \text{CLARK I.}$$

The double rotation that would have been found if the current had been just strong enough to balance CLARK I. (at the actual temperature) is

$$\frac{7017}{8430}\times 449'\cdot1=373'\cdot8.$$

19. This result is a function of the temperatures of the cell and of R as well as of the CS_2; and it is rather unfortunate that all three temperature corrections tell in the same direction. A rise of the thermometer involves a rise in R and a fall in the force of the standard cell, so that on both accounts the current giving the balance is diminished. At the same time the smaller current acts less advantageously in

producing rotation in consequence of the properties of the CS_2. It will be convenient to postpone the last correction, and take first the corrections for temperature in R and the E.M.F. of CLARK, which relate rather to the machinery for measuring the current, and which can be made from data obtained in previous investigations. For this purpose 15° C. is adopted as the standard temperature; and the proportional corrections per degree are ·00082 for the E.M.F. of CLARK and ·00044 for the R, making altogether ·00126 per degree. For the observations of July 25, the correction is therefore

$$+2{\cdot}6\times{\cdot}00126\times373'{\cdot}8=+2{\cdot}6\times{\cdot}471=+1'{\cdot}2.$$

If we take as a standard current that which in traversing R at 15° would balance CLARK I. at 15°, the double rotation of July 25 reduced so as to correspond with the standard current will be

$$373'{\cdot}8+1'{\cdot}2=375'{\cdot}0.$$

This rotation corresponds to the temperature 18°·3 of the CS_2. To obtain comparable results we must reduce to a standard temperature, for which purpose we will select 18°. According to BICHAT the rotation at $t°$ may be expressed by

$$1-{\cdot}00104t-{\cdot}000014t^2,$$

the rotation at 0° being taken as unity. To obtain a more convenient formula, applicable in the neighbourhood of 18°, we may write $t=18+t'$. Thus

$$1-{\cdot}00104t-{\cdot}000014t^2={\cdot}9767-{\cdot}00154t'={\cdot}9767(1-{\cdot}00158t');$$

so that the coefficient for the correction is ·00158. Hence, if the CS_2 on July 25 had been at 18°, we should have had

$$375'{\cdot}0+375'{\cdot}0\times{\cdot}00158\times{\cdot}3=375'{\cdot}0+{\cdot}592\times{\cdot}3=375{\cdot}0+{\cdot}2=375'{\cdot}2.$$

Thus reduced the results for the observations of different days should agree together.

TABLE III.

Series I.

Date, 1884.	Actual mean rotation (2α).	Mean value of r.	ρ.	$2\alpha\frac{\rho}{\rho\pm r}$.	Temperature of CLARK and R.	Correction to 15°.	Rotation corrected to standard current.	Temperature of CS_2.	Correction to 18°.	Rotation corrected to 18°.	Deviation from mean.
May 5	1123′·0	44	5032	1132′·9	15°·0	+0′·0	1132′·9	16°·8	−2′·2	1130′·7	+2·2
,, 6	1086·7	205	5040	1132·8	15·6	+0·8	1133·6	17·2	−1·4	1132·2	+3·7
,, 7	..	..	..	1132·1	14·8	−0·3	1131·8	17·2	−1·4	1130·4	+1·9
,, 9	1035·0	402	5046	1124·7	16·5	+2·1	1126·8	18·0	0·0	1126·8	−1·7
,, 10	1008·0	515	5033	1122·9	17·0	+2·8	1125·7	19·8	+3·2	1128·9	+0·4
,, 13	1009·0	502	5019	1121·1	17·8	+3·9	1125·0	20·0	+3·6	1128·6	+0·1
,, 15	1040·5	359	5028	1120·5	18·2	+4·5	1125·0	19·3	+2·3	1127·3	−1·2
,, 16	543·6	2582	5026	1117·9	18·8	+5·3	1123·2	20·3	+4·1	1127·3	−1·2
,, 19	589·0	2416	5050	1129·3	16·2	+1·7	1131·0	17·5	−0·9	1130·1	+1·6
,, 21	681·2	1998	5065	1125·0	16·4	+2·0	1127·0	17·2	−1·4	1125·6	−2·9
,, 23	664·8	2070	5062	1124·7	17·0	+2·8	1127·5	18·1	+0·2	1127·7	−0·8
,, 26	755·0	1667	5066	1125·3	17·1	+3·0	1128·3	18·0	0·0	1128·3	−0·2
,, 29	..	..	..	1128·6	14·9	−0·1	1128·5	15·5	−4·5	1124·0	−4·5
,, 31	685·4	2850	7194	1135·1	14·4	−0·8	1134·3	15·0	−5·4	1128·9	+0·4
June 2	640·7	3116	7164	1133·9	15·7	+1·0	1134·9	15·9	−3·8	1131·1	+2·6
Mean	..	..	..	..	..	..	1129·0	17·7	..	1128·5	..

Series II.

Date, 1884.	Actual mean rotation (2α).	Mean value of r.	ρ.	$2\alpha\frac{\rho}{\rho\pm r}$.	Temperature of CLARK and R.	Correction to 15°.	Rotation corrected to standard current.	Temperature of CS_2.	Correction to 18°.	Rotation corrected to 18°.	Deviation from mean.
June 3	518·6	2719	7121	375·3	17·5	+1·2	376·5	16·5	−0·9	375·6	−0·1
,, 5	543·0	3168	7122	375·8	15·8	+0·4	376·2	16·3	−1·0	375·2	−0·5
,, 6	393·5	323	7118	376·4	15·1	+0·0	376·4	15·6	−1·4	375·0	−0·7
,, 9	552·9	3275	7110	378·5	14·3	−0·3	378·2	15·9	−1·3	376·9	+1·2
Mean	..	..	..	..	..	..	376·8	16·1	..	375·7	..

TABLE IV.

Series III.

Date, 1884.	Actual mean rotation (2α).	Mean value of r.	ρ.	$\frac{2\alpha\rho}{\rho \pm r}$.	Temperature of CLARK and P.	Correction to 15°.	Rotation corrected to standard current.	Temperature of CS_2.	Correction to 18°.	Rotation corrected to 18°.	Deviation from mean.
July 23 . . .	447·2′	1358	7017	374·7′	18·4°	+1·6′	376·3′	19·2′	+0·7°	377·0′	+1·2
" 24. . .	449·6	1410	7019	374·4	18·0	+1·4	375·8	18·9	+0·5	376·3	+0·5
" 25. . . .	449·1	1413	7017	373·8	17·6	+1·2	375·0	18·3	+0·2	375·2	−0·4
" 28. . . .	448·2	1390	7017	374·1	17·1	+1·0	375·1	17·9	−0·1	375·0	−0·8
Aug. 2. . . .	389·7	319	6998	372·7	19·1	+1·9	374·6	20·7	+1·6	376·2	+0·4
" 4. . . .	343·9	516	7001	371·3	19·0	+1·9	373·2	20·1	+1·3	374·5	−1·3
" 5. . . .	530·2	2963	7005	372·6	19·0	+1·9	374·5	20·8	+1·7	376·2	+0·4
Mean . . .	..	..	..	..	..	..	374·9	19·4	..	375·8	..

20. The results of all the observations (other than preliminary) which were thought worthy of reduction are exhibited in the accompanying tables, grouped in three series. In Series I., II. the first tube was employed; the principal difference between them being that in Series I. the light traversed the tube *three times*, while in Series II. the light passed but *once*. It will be seen that in Series I. the actual double rotation varied from about 9° to 19°, and the currents from about $\frac{1}{2}$ ampère to 1 ampère. In Series II. stronger currents were usually passed, amounting to about $1\frac{1}{2}$ ampère, but the rotation was only about 9°. The extreme deviation from the mean is only about ·4 per cent., if we exclude the observations of May 29, which owing to interruptions and other causes were marked as unsatisfactory before reduction.

The NICOL was used as analyser in Series I., and on June 3 of Series II. The remaining observations of Series II. and the whole of Series III. were taken with a double-image prism, read in all four positions as already explained by the example of July 25.

For the observations of Series III. the second tube was employed, with some improvements in the provision against the communication of heat. The diminished diameter of the tube was the inducement to pass the light but once, though it would have been possible to work with three passages. But when the rays skirt the walls of the tube, there is more disturbance from heat; and, indeed, generally the advantage of augmented rotation is in great measure paid for by greater sensitiveness to deviation from optical uniformity.

Not only does the communication of heat disturb the definition, but it tends also to render the actual temperature uncertain. During some of the more protracted sets of readings with the stronger currents there was a rise of nearly 2° in the temperature of the CS_2; and, although this rise was carefully watched, it is difficult to feel confident that the effective mean temperature can be determined with a less error than say $\frac{1}{3}$ of a degree. Such an error would correspond to about $\frac{1}{2000}$ in the final number. To avoid increasing the uncertainty under this head the readings were often concluded, although the definition still remained satisfactory.

If the apparatus were to be designed afresh I should endeavour to guard more adequately against these disturbances, and it might then be possible to use five passages with advantage, more especially if by increasing the weight of the coil it were practicable to bring the double rotation up to about 90°. The determination of such a rotation with the double image prism would be free in high degree from the polarimetric errors considered in § 10. But it is doubtful whether in the present state of science the additional accuracy would repay the labour involved.

21. It only remains now to work out the results in absolute measure. And first as to the value of the standard current, defined as that which, flowing through the [$1\frac{1}{2}$] at 15°, balances CLARK I. at the same temperature. This value in ampères is expressed by dividing the E.M.F. of CLARK I. in B.A. volts (see Table XI. of former paper) by the resistance of the [$1\frac{1}{2}$] in B.A. units. Hence the standard current is

$$\frac{1{\cdot}4542}{1{\cdot}4958} = {\cdot}9722 \text{ ampère} = {\cdot}09722 \text{ C.G.S.}$$

If the tube were infinitely long, the difference of magnetic potentials at its ends would be $4n\pi$; but in the case of the actual tubes we have to introduce the correcting factors ·99433 and ·99353 (§ 15). Thus for the first tube, if x be the (single) rotation in minutes corresponding to difference of potential 1 C.G.S., the whole actual double rotation for a single passage of the light will be

$$2 \times {\cdot}09722 \times 4\pi n \times {\cdot}99433 \times x.$$

From Series I. at 18° this quantity is found to be $\frac{1}{3} \times 1128{\cdot}5$, or 376·2, so that

$$x = \frac{376{\cdot}2}{2 \times {\cdot}09722 \times 4\pi n \times {\cdot}99433} = {\cdot}04203.$$

In like manner from Series II. we get

$$x = \frac{375{\cdot}7}{2 \times {\cdot}09722 \times 4\pi n \times {\cdot}99433} = {\cdot}04198.$$

For the second tube used in Series III. we have to employ a slightly different correction for finite length. We have

$$x = \frac{375{\cdot}8}{2 \times {\cdot}09722 \times 4\pi n \times {\cdot}99353} = {\cdot}04202.$$

The results of Series I. and III. are thus in precise agreement, while that of Series II. is about $\frac{1}{1000}$ lower. Ascribing a somewhat less importance to Series II. in consequence of the smaller number of sets of observations, we may take as the final result of the investigation

$$x = \mathbf{{\cdot}04202},$$

which gives the rotation in minutes in bisulphide of carbon at 18°, corresponding to a difference of potential equal to 1 C.G.S. It should be noticed that the mean temperature of the observations was so nearly 18° that the result as given depends scarcely at all upon BICHAT'S formula for the dependence of the rotation upon temperature.

22. M. BECQUEREL gives as his result for 0° C. ·0463 minute. To find the rotation at 18°, this must be multiplied by ·9767 according to BICHAT'S formula: and as BECQUEREL'S observations were in fact made at about 18°, this reduction does not introduce, but rather removes, an extraneous element. Thus according to BECQUEREL—

$$x = {\cdot}0452 \text{ minute},$$

differing by about 7 per cent. from the value found by me.

The comparison with GORDON is more uncertain, inasmuch as his observations were

made on light of the refrangibility of the thallium line. The corrected* result for this light is in circular measure $1 \cdot 5238 \times 10^{-5}$, or ·05238 minute. To pass to sodium we may use a formula given by BECQUEREL† and VERDET, according to which the rotation for different wave lengths (λ) is proportional to $\mu^2(\mu^2-1)\lambda^{-2}$, μ being the refractive index. At this rate the ·05238 minute for thallium would be ·04163 minute for sodium. The temperature was not directly observed by GORDON, but was estimated to be about 13° C. Assuming this to be correct, the value for 18° would be ·0413 minute, or about 2 per cent. *less* than according to my determinations.

APPENDIX.

Notes on Polarimetry in general.

The problem of the polarimeter is how best to render evident the rotation through a small angle θ of the plane of polarisation of light of brightness h. The effect of the rotation is to introduce light of amplitude $h^{\frac{1}{2}} \sin \theta$, or $h^{\frac{1}{2}}\theta$, polarised in the perpendicular plane, and it is this which must be made to produce a recognisable change. By the use of a NICOL, or double-image prism, adjusted to the original plane, the light of brightness $h\theta^2$ may be isolated, but, as will be proved presently, this is not the best method of rendering its existence evident.

From the preceding mode of statement it is clear that the accuracy obtainable in determining the plane of polarisation increases indefinitely with the brightness of the light, and is in fact proportional to the *square root* of that brightness.‡ Again we see that little is to be expected from such devices as that of FIZEAU, in which the rotation is magnified by causing the light to pass obliquely through a pile of glass plates. The brightness of the light polarised in the perpendicular plane ($h\theta^2$) can only be diminished by such treatment, and the increase of rotation, being due merely to weakening of the first component, is of no value.

The arrangements to be adopted depend for their justification upon the physiological law of the perception of differences of brightness. If $d\mathrm{E}$ denote the difference of sensations, corresponding to two degrees of brightness, H and $\mathrm{H}+d\mathrm{H}$, we have §

$$d\mathrm{E}=\mathrm{A}\frac{d\mathrm{H}}{\mathrm{H}+\mathrm{H}_0},$$

in which H_0 is a certain constant brightness, supposed to depend chiefly upon the proper or internal light of the eye, but to which may be added the effect of light

* Mr. GORDON's result was originally given at double its proper value.

† Ann. d. Chim., t. xii., 1877, p. 78.

‡ This point is insisted upon in an excellent paper by LIPPICH (Wien. Ber., 85, 9 Feb., 1882), which has lately come to my notice.

§ HELMHOLTZ: 'Physiologische Optik,' § 27.

diffused by imperfect translucency of the optical apparatus. If dE denote the smallest perceptible difference, the value of dE/A is in favourable circumstances as low as $\frac{1}{50}$ or $\frac{1}{100}$, which means that with a sufficient total brightness differences of this amount may be apparent to observation.

Let us now consider the values of dE corresponding to different methods of procedure. If the analysing NICOL be adjusted for extinction of the original light, the comparison is between the brightness which cannot be got rid of (H_0) and ($H_0+h\theta^2$).* Near the limit of discrimination, to which case we may confine our attention, $h\theta^2$ is small relatively to H_0, and thus we may take

$$dE = A\frac{h\theta^2}{H_0}.$$

The procedure just considered is that which would naturally be adopted to render evident a small quantity of light of given amount, viz., to isolate it and compare it with the best attainable darkness. But in the present problem the circumstances are peculiar in that we are able to deal with phases. Now if we regard the amplitude (α) of the feeble light as given, putting $\alpha^2=h\theta^2$, we may produce more effect from it by combining it with other light in the same phase of amplitude (β) than by isolating it. The comparison is then between brightnesses $(\alpha+\beta)^2$ and β^2, or as α is very small, between $\beta^2+2\alpha\beta$ and β^2. Thus

$$dE = A\frac{2\alpha\beta}{\beta^2+\beta_0{}^2},$$

in which $\beta_0{}^2$ is written H_0.

The light of amplitude β is obtained in the simplest possible manner by merely rotating the analysing NICOL through a small angle, and the only question is how to exhibit the comparison light which shall not be affected, when β is changed to $(\beta+\alpha)$. For this purpose we may divide the field of view into two halves with an oblique mirror in which is seen by reflection a feeble light, of the same colour and coming ultimately from the same source.

It is possible that an instrument upon this principle might be made to work satisfactorily,† but the half-shade polarimeters of JELLET and LAURENT seem to be in most respects preferable. In them the comparison is between $(\beta+\alpha)^2$ and $(\beta-\alpha)^2$, so that

$$dE = A\frac{4\alpha\beta}{\beta^2+\beta_0{}^2},$$

representing twice as great a sensibility. The only thing to be said upon the other side is that the division line in these instruments can hardly be made as invisible as the sharp edge of a mirror may be.

* We may imagine the presentation of the two brightnesses to be consecutive, or more favourably that both are seen at once, half the field of view being occupied by a black body seen after reflection in an oblique mirror, whose edge forms the dividing line.

† Readings would of course be taken in both the positions (one on either side of extinction) which give a match with the comparison light.

In these formulæ β may be chosen at pleasure by suitable adjustments of the polarising arrangements. In order to get the best result, $d\mathrm{E}$ must be made a maximum by variation of β, α and β_0 being treated as constants. The maximum occurs when $\beta=\beta_0$, and its value in the last case is

$$d\mathrm{E}=\mathrm{A}\frac{2\alpha}{\beta_0}.$$

Taking $d\mathrm{E}/\mathrm{A}=\frac{1}{25}$, which is probably about as small as can be expected in practice, we have for the least perceptible value of α

$$\alpha=\frac{1}{50}\beta_0$$

whereas without the half-shade arrangement, and with a NICOL simply set to extinction of the original light,

$$\frac{h\theta^2}{\mathrm{H}_0}=\frac{\alpha^2}{\beta_0^2}=\frac{1}{25},$$

so that

$$\alpha=\frac{1}{5}\beta_0.$$

According to these numbers the half-shade arrangement would have a tenfold superiority, a result not fully borne out in practice. In explanation of this it is important to notice that the procedure in the absence of a half-shade arrangement would in reality be very different from what we have tacitly supposed. The experienced operator, in setting a NICOL to the position of maximum extinction, does not judge merely by the degree of darkness attained in the final position, but displacing the analyser alternately in opposite directions, estimates the position which lies midway between those which give similar revivals of light on the two sides; or, endeavouring to retain in his memory a certain degree of brightness, he may take actual readings on both sides, of which the mean will correspond to the desired position. In this way the fundamental advantage of the half-shade method is in a sense attained, the only difference being that the brightnesses to be compared are seen consecutively after a short interval of time, instead of almost simultaneously; and even this difference becomes less important when the line dividing the field of view of the half-shade apparatus is so coarse that it cannot be rendered invisible.

The carrying out of this method is facilitated by a device which is worthy of trial. The NICOL may be mounted loosely, so as to be capable of turning through a small angle (2 or 3 degrees) between two stops. These stops are rigidly attached to a rotating piece carrying the vernier, and it is to the position of this piece (and not that of the NICOL) to which the readings relate. In taking an observation the piece is turned until the degree of brightness is unaltered, when the NICOL is put over from the one stop to the other. It is probable that under these advantageous conditions

more favourable results than hitherto would be obtained with an undivided field of view.

In the application of the polarimeter, with which the present paper is mainly concerned, the free play of the NICOL is advantageously replaced by an equivalent rocking of the plane of polarisation itself through a small angle on either side of its normal position, produced by the action of an auxiliary electric current, embracing the experimental tube a moderate number of times, and reversed at pleasure by a suitable key under the hand of the observer.

In these discussions it has been convenient to take as a basis the fractional difference of brightnesses which can be recognised on simple presentation to the eye,* but it must be remembered that if suitable precautions are taken to avoid asymmetry, there is no theoretical limit of final accuracy. Thus in ordinary photometry with a divided field (*e.g.*, BUNSEN's grease-spot photometer), the match must not be approached from one side only. By combining a large number of observations in which the match is approached as much from one side as from the other, a degree of accuracy may be practically attained far beyond that corresponding to the difference of brightness which can be directly recognised by the eye. It is not necessary actually to take readings on the two sides, though it is sometimes desirable to do so; the essential point is to secure symmetry. Time may be saved by the plan of providing means for instantaneous displacements of given amount on either side, as was done in the experiments of the present paper by the auxiliary reversible current.

In practical applications of the polarimeter we have almost always to determine, not so much a particular plane of polarisation as the *rotation* of this plane, due to electromagnetic action, to the substitution of syrop for water, etc., and it appears that the measurement of this angle must be affected with a possible error, double of the error possible in the determination of a single plane. M. BECQUEREL, indeed, in his interesting memoir upon the rotation in bisulphide of carbon under the terrestrial magnetic force,† describes a procedure by which, as he considers, the error may be reduced. By the introduction of a half-wave plate, adjusted so that its principal section coincides nearly with the plane of first polarisation, the angle of rotation is, as it were, reflected by the former plane, and the difference of readings taken with and without the plate is the double of the real angle of rotation. If ϵ be the greatest angular error possible in determining a single plane, M. BECQUEREL shows that the error in setting the plate cannot exceed ϵ, from which he argues that the whole error possible in determining the double angle of rotation is only 3ϵ, or $\frac{3}{2}\epsilon$ upon the single angle. It appears, however, that the error of adjustment of the half-wave plate enters

* [August, 1885.—I find that the sensitiveness of the eye to small differences of brightness is subject to very rapid fatigue. Even a few seconds' gazing is often enough to obliterate a distinction quite apparent at first, and appreciable again after a little repose. This defect is a great obstacle to the further improvement of photometric methods.]

† Ann. d. Chimie, t. 201, p. 323, 1882.

doubly into the result, so that the whole error possible in determining the double angle of rotation rises to 4ϵ, and the use of the half-wave plate gives no advantage.

One other point may be considered in conclusion. In determinations of rotation by magnetic force, the effect to be measured may be multiplied (as FARADAY showed), by causing the light to be reflected backwards and forwards at the ends of the tube. Against this augmentation of the angle of rotation we must set the loss in the section of the beam, and the waste of light in reflection and by absorption. Putting out of sight for the moment the alteration in the section of the beam, we may easily determine the most advantageous number of passages as dependent upon magnitude of rotation and intensity of light. If r be the factor by which the original intensity must be multiplied, in order to express the intensity after a single passage and reflection, r^n will express the intensity after n such passages and reflections. The accuracy of the determination will thus be proportional to $nr^{\frac{1}{2}n}$, which is a maximum when $r=e^{-2/n}$. The values of r corresponding to n equal to 1, 3, 5, 7, . . . , are ·135, ·514, ·670, ·752, . . . , so that 3 or 5 passages will usually give the best result.

The argument in favour of a moderate use only of the principle of reflection is strengthened when we take into account the diminution in the section of the beam. The already contracted aperture is seen at a greater distance (proportional to n), so that the apparent magnitude of the field of view is rapidly narrowed. Under these circumstances the comparisons cannot be made with the usual accuracy. If we have recourse to a telescope we can indeed restore the apparent magnitude, but (usually) only at expense of the illumination, since the aperture of the telescope is limited. If the available aperture do not exceed $\frac{1}{4}$ inch, *any* degree of magnification involves a loss of brightness. The importance of these considerations depends upon the length and diameter of the tube; but the tendency of the discussion is to show that more than five passages can rarely be desirable, and that in many cases three passages ought to be preferred to five. If there is any exception it will be when powerful white light (as from the sun) is available, or when it is possible by use of a larger number of passages to bring the whole rotation up to 90° or 180°, in which cases, as has already been noticed, the angle may be determined with peculiar advantage.

POSTSCRIPT.

(October, 1885.)

An important paper* has recently been communicated to the French Academy† by M. BECQUEREL, in which he abandons his former result (§ 4), obtained with the aid of terrestrial magnetic force, in favour of a number agreeing more nearly with that given by GORDON and myself. In the new experiments a long column of CS_2 was employed, encompassed by a spiral conveying a current, the effect of which is shown to depend upon the magnitude of the current and upon the number of turns, in approximate

* Ann. d. Chim., Oct., 1885.

† C. R., June 2, 1885.

independence of other circumstances. M. BECQUEREL speaks of this method as new, but it is in reality that employed by GORDON in 1877.* Most of the complication in GORDON'S memoir relates to the determination of the current, and especially to the circumstance that the number of turns in the spiral was not ascertained (as it should have been) during construction, *but subsequently by electrical processes.* When the number of turns and the current are known, there is no difference between the procedure of GORDON and BECQUEREL and that of the present memoir.

There is a pretty close resemblance between M. BECQUEREL'S recent work and mine. In both a soda flame is used as the source of light, and in both the number of windings on the helices is ascertained during construction. In the current determinations, M. BECQUEREL used a galvanometer as an intermediate standard, while I employed for the same purpose a CLARK'S cell, the ultimate standard being a silver voltameter (and in my case a current-weighing apparatus). Inasmuch as M. BECQUEREL uses the same number as that which I obtained for the electro-chemical equivalent of silver, there should be no difference between us in the estimation of currents.

In M. BECQUEREL'S experiments the temperature of the CS_2 was usually about 0° C., and he reduces his results to that standard temperature. He regards BICHAT'S formula as confirmed by his observations. According to this my result for 18° would become

$$\cdot 04302';$$

whereas M. BECQUEREL obtains

$$\cdot 04341',$$

nearly 1 per cent. higher. I am at a loss to understand the cause of this discrepancy. M. BECQUEREL estimates that his result should be correct to $\frac{1}{800}$, about the same degree of accuracy which I also had hoped to have attained. So far as I can judge, I should consider that in respect of current measurement the advantage lay with me, but that on the optical side M. BECQUEREL'S arrangements were probably superior.

M. BECQUEREL repeats his proposal† to found upon his value of the constant a method for current measurement. I had considered this question at (I believe) an earlier date; and the less sanguine view expressed in the following paragraph seems to be justified by the discrepancies between the results of various observers at various times as to the value of the constant in bisulphide of carbon :—

"Another method, available with the strong currents which are now common, depends upon FARADAY'S discovery of the rotation of the plane of polarisation by magnetic force. GORDON found 15°‡ as the rotation due to the reversal of a current

* See his equation (24), p. 15.

† C. R., t. xcviii., p. 1253; 1884.

‡ Jan., 1884. In a note recently communicated to the Royal Society (Proceedings, Nov. 15, 1883), Mr. GORDON points out that, owing to an error in reduction, the number given by him for the value of VERDET'S constant is twice as great as it should be. The rotations above mentioned must therefore be halved, a correction which diminishes materially the prospect of constructing a useful instrument upon this principle.

of 4 ampères circulating about 1000 times round a column of bisulphide of carbon. With heavy glass, which is more convenient in ordinary use, the rotation is somewhat greater. With a coil of 100 windings we should obtain 15° with a current of 40 ampères; and this rotation may easily be tripled by causing the light to traverse the column three times, or what is desirable with so strong a current, the thickness of the wire may be increased and the number of windings reduced. With the best optical arrangements the rotation can be determined to one or two minutes, but in an instrument intended for practical use such a degree of delicacy is not available. One difficulty arises from the depolarising properties of most specimens of heavy glass. Arrangements are in progress for a redetermination of the rotation in bisulphide of carbon."*

* From the Proceedings of the Cambridge Philosophical Society for Nov. 26, 1883. See also 'Nature,' Dec. 13, 1883.