Philosophical Magazine Series 5

Publication details, including instructions for authors and subscription information:
http://www.tandfonline.com/loi/tphm16

XVIII. On the “Rotational Coefficient” in nickel and cobalt

E.H. Hall Ph.D. [a]

[a] Johns Hopkins University , Baltimore

To cite this article: E.H. Hall Ph.D. (1881) XVIII. On the “Rotational Coefficient” in nickel and cobalt , Philosophical Magazine Series 5, 12:74, 157-172, DOI: 10.1080/14786448108627086

To link to this article: http://dx.doi.org/10.1080/14786448108627086

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

SEPTEMBER 1881.

XVIII. *On the "Rotational Coefficient" in Nickel and Cobalt. By* E. H. Hall, *Ph.D., late Assistant in Physics in the Johns Hopkins University, Baltimore*[*].

THIS article may be considered as the continuation of one published in the 'Philosophical Magazine' for November 1880, under the title "On the new Action of Magnetism on a Permanent Electric Current," in which were given the results of some quantitative investigations of a certain phenomenon recently discovered in the Physical Laboratory of the Johns Hopkins University. It will perhaps be remembered that the essential feature of this phenomenon is the setting up, in a conductor bearing an electric current, of an electromotive force at right angles to the primary electromotive force, when the said conductor is subjected to the action of a magnetic force at right angles to the direction of the current.

In the article alluded to, results were given as obtained with gold, silver, tin, platinum, iron, and nickel. The magnitude of the effect observed, relatively to the strength of the primary current, the intensity of the magnetic field, and the dimensions of the conductor, had not been determined with any accuracy in the case of nickel and tin, though it was known to be comparatively large in nickel and small in tin. The other metals ranged themselves, as regards the numerical magnitude of the effect exhibited, in the following order, viz. iron, silver, gold, platinum—the effect observed in iron being

---

[*] Communicated by the Physical Society, having been read at the Meeting on May 28, 1881.

several times greater, and that in platinum several times less, than the effect in gold or silver. The fact of greatest interest, however, was that, if we called the direction of the transverse effect in iron +, that in the diamagnetic metals, *and in nickel and platinum also*, would be —*.

In view of this remarkable disagreement in behaviour between the two strongly magnetic metals iron and nickel, it seemed highly desirable to make a quantitative investigation of the effect in nickel as soon as possible, and extend the examination to the other strongly magnetic metal cobalt. Most of the experiments to be described in this article relate, therefore, to nickel and cobalt. The examination of the latter was a hasty one, and may well be described first.

No thin strips of the metal being at hand, a slice was sawn from a small block of moderately pure cast cobalt and worked into the form of a cross. To the extremity of each arm of this cross was soldered a thin strip of copper 2 or 3 centim. long, for the purpose of making the electrical connexions. The cross of cobalt with the copper strips attached was now fastened with hard cement to a strip of glass and worked down with a file to sufficient thinness. Before placing the cross upon the glass its thickness, and that of the glass also, was measured by the calipers. After cementing the two together, the total thickness was found, and, again, the thickness of the whole after the cross had been filed down. The thickness of the cross in its final condition was thus estimated at ·45 millim., to which value an uncertainty of perhaps 10 or 15 per cent. attaches.

With this apparatus it was found that the direction of the transverse effect in cobalt is +, *i. e.* the same as that in iron. As to the magnitude of the effect, $\frac{M \times V}{E'}$ was found to be 44×10^{10}, placing cobalt between silver and iron. The specimen of cobalt used, however, contained some nickel (how much is not known accurately); and this doubtless counteracted in part the effect of the cobalt. It seems probable, however, that, allowing for all errors, the transverse effect in cobalt is less than that in iron, other things being equal. The magnetic field used was about 9000 (cm.-gr.-sec.), stronger than has yet been used with iron.

---

* These signs are given to avoid tedious repetitions. I have here called the effect in iron + simply because its direction in this metal is that which the conductor itself bearing the current would follow, if free to move across the lines of magnetic force under the action of the ordinary "ponderomotive" force. No significance further than this is at present attached to this choice of signs.

We now return to the consideration of nickel.

The original experiment with this metal had been made with a specimen so irregular, that it had not been possible to determine the magnitude of the transverse effect except in the most general way. The direction had been determined beyond question. The specimen of nickel now employed, and with which the results to be given were obtained, was quite different in appearance and physical condition from the first specimen—though it was obtained in about the same manner, viz. by stripping off a piece of nickel plating from the metal upon which it had been electrolytically deposited. The first specimen was very brittle, the second quite tough. The latter was about ·001 centim. thick. As to its purity hardly any thing is known except what is told by its physical characteristics. It is probably affected by all the impurities of ordinary nickel plating. It contains very likely a little cobalt, and perhaps a trace of iron. I understand, moreover, from Professor Wolcott Gibbs, that nickel plating deposited in the usual manner (*i. e.* from an ammoniacal solution) is much affected in its physical properties by nitrogen in some way retained by the metal. It would have been desirable, of course, in all cases to work with pure metals; but such were not at hand, or easily obtainable in the proper form, and it was not thought best to defer the experiments until pure specimens could be obtained*.

The second specimen of nickel showed an effect of the same sign as the first, and numerically greater than the effect which had been observed in the specimens of iron and cobalt used.

It now became a matter of great interest to determine whether the transverse effect had really any connexion with the magnetic properties of the metals. It was determined therefore to make a series of experiments, keeping the primary current through the metal as nearly as practicable always of the same strength, but varying within wide limits the intensity of the magnetic field. We should in this way ascertain whether the transverse effect was simply proportional to the strength of the magnetic field, or was related to it in some more complicated manner.

---

* This may strike some readers as unwise. It has even been suggested that the difference in behaviour of iron and nickel may be due to impurities in one or the other. This suggestion implies that the transverse effect in these metals is so related to the magnetic properties that, as they resemble each other in one respect, they should also in the other, but at the same time admits that slight impurities, such as would certainly be very far from reversing the magnetic property of either metal, may reverse the transverse effect in the same. This does not seem probable.

By the term "strength of the magnetic field," as just used, is meant the intensity of the field between the poles which obtains when the metal plate is not in the field. This intensity is measured, as described in the article already alluded to, by withdrawing suddenly from the field a small coil of wire and observing the effect upon a galvanometer in circuit with the coil. This gives what is called the magnetic induction in this part of the field. In general, the magnetic induction in any magnetized space would be changed by introducing into that space a body capable of being magnetized by induction. The well-known expression for the magnetic induction within any such body placed in a magnetic field is (Maxwell's 'Treatise,' vol. ii. art. 428)

$$\mathfrak{B}=\mathfrak{H}+4\pi\mathfrak{I}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where $\mathfrak{H}$ is the *magnetic force* within the body (Thomson's 'Polar Definition,' reprint, p. 397), and $\mathfrak{I}$ is the *intensity of magnetization* (Maxwell, art. 384).

Now, in case of uniform magnetization, $\mathfrak{H}$ is equal to the intensity of the field as it would exist if the body magnetized by induction were removed (*i. e.* just what we measure by means of the coil and galvanometer), together with the force exerted by what we may call the magnetism induced on the surface of the magnetized body. This latter force will, of course, depend upon the shape and dimensions of the body. If it is a very thin disk, the reaction of the induced magnetism will, as Maxwell remarks, be equal to $-4\pi\mathfrak{I}$; and in this case, writing $\mathfrak{F}$[*] for the intensity of the magnetic field as above defined, we have

$$\mathfrak{H}=\mathfrak{F}-4\pi\mathfrak{I}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Substituting in (1), we have

$$\mathfrak{B}=\mathfrak{F}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which means that, in a very thin disk magnetized by induction, the magnetic induction is just what it would be in the space occupied by the disk if the disk were removed from the field. Now the strip of nickel which we employ has a width 600 or 800 times its thickness; and it has been assumed that we may, for our present purpose, regard it as such an infinitely thin disk as Maxwell supposes. The error resulting from this assumption may easily be seen to be small. At the centre of the strip of nickel the real value of $\mathfrak{B}$ would be perhaps $\frac{1}{10}$ of one per cent. greater than the value as above determined. At a point 1 millim. from the edge of the strip the error might amount to $\frac{1}{3}$ or $\frac{1}{4}$ of one per cent.; while at $\frac{1}{10}$ millim. from

---

[*] Called M in previous article.

the edge it would perhaps be two or three per cent. The average of the real values of $\mathfrak{B}$, therefore, at points along the line running across the strip from one side connexion to the other, is probably a rather small fraction of one per cent. greater than the value obtained on the assumption that $\mathfrak{B}$ is equal to $\mathfrak{F}$. This error is, to be sure, not constant; but it is nearly so up to $\mathfrak{B}=$ about 5000; and when it begins to change rapidly, it grows smaller.

Its influence upon the curve given further on must be very small. We assume therefore, as stated above, that by determining the strength of the magnetic field by means of the coil and galvanometer before the nickel is placed in the field, we ascertain with sufficient accuracy the value of the magnetic induction in the nickel strip itself when placed in the magnetic field. The advantage of determining this quantity is of course very great; for though we are probably unable to say what is the exact physical nature of magnetic induction, we do attach to the quantity represented by that term a very definite and important mathematical significance.

It was designed, therefore, to investigate the law of the variation of the transverse effect with the variation of the magnetic induction. Nickel was the best metal to experiment upon, for the following reasons: the strip of this metal at hand was very thin; the transverse effect appears to be essentially more powerful in nickel than in iron or cobalt; the magnetic permeability of nickel changes more rapidly than that of iron or cobalt with high magnetizing-powers.

As it was desired to determine simply what function of the magnetization the transverse effect would prove to be, the primary current through the nickel strip has been kept approximately constant, the greatest variation from the mean being probably not many per cent., as will be shown further on. Within these limits it has been assumed that the transverse effect may be considered a linear function of the direct current.

It should be here stated that this latter relation has not yet been proved to hold rigidly even in a non-magnetic conductor like gold; and the matter must some time be investigated, though there seems to be no reason to think that the assumption, as above limited, can prove to have involved any considerable error.

The intensity of the magnetic field, and so the magnetic induction in the nickel plate, has been varied from about 1600 to about 10,000 in absolute (cm.-grm.-sec.) measure.

In the course of this investigation I have become indebted to nearly every one connected with the Physical Laboratory of the Johns Hopkins University, but particularly to Mr. S.

H. Freeman, Fellow in Physics, and Mr. H. R. Goodnow, Special Student in Physics, who for a while carried on the experiments together. Mr. Freeman especially worked with me for a long time; and several suggestions of his in regard to the arrangement of apparatus and the method of experimenting were adopted with great advantage to the work.

In my last article on this subject the results of measurements were given in the form $\frac{M\times V}{E'}$, where M was the strength of the magnetic field[*], V was the direct current divided by the section of the conductor, and E′ was the transverse electromotive force per centimetre of the width of the strip. In that article were given certain reasons for thinking the above quantity more likely to be a constant for any given metal than the quantity $\frac{M\times E}{E'}$, where E is the electromotive force per centimetre of the *length* of the metal strip. Recent developments, to be spoken of further on, raise the question whether the ratio $\frac{M\times E}{E'}$ will not after all prove to be the more fundamental and invariable quantity; but as E is rather difficult to determine with accuracy, and as in any given strip of metal V is likely to remain under ordinary conditions of temperature &c. very nearly proportional to E, the use of the former quantity will be retained for this article at least. The values of M [$\mathfrak{J}$] will be given separately, however; and, for convenience in plotting the results, the quantity $\frac{E'}{V}$ will be used instead of $\frac{V}{E'}$. The values of $\mathfrak{J}$ will, in plotting, be laid off as abscissas, and the values of $\frac{E'}{V}$ be taken as ordinates. This method of plotting gives a simple curve in the present case, and puts the results of the experiments in form to be compared with those of previous investigations of some of the magnetic properties of nickel. It is this quantity $\frac{E'}{V}$ which, after Maxwell, in accordance with the suggestion of Mr. Hopkinson[†], is now called the "rotational coefficient" of nickel.

---

[*] Called $\mathfrak{J}$ in this article.

[†] Phil. Mag. Dec. 1880, p. 430. Prof. Rowland has (Phil. Mag. April 1881, p. 254) remarked upon Mr. Hopkinson's note. Maxwell did not know any such effect to exist. In fact he expressly stated that it probably did not exist; yet, seeing the possibility of it, he let fall the phrase which seems now best fitted to define this newly discovered property of the metals.

Mr. Hopkinson has suggested "rotational coefficient of resistance;" and possibly some quantity might be found which would demand that title. At first sight $\frac{E'}{V}$, which is an electromotive force divided by a quantity proportional to a current, would seem to be of the nature of a resistance; but it is to be noticed that the electromotive force E′ is not the cause, but the effect, of the current implied in V.

In the experiments which I have described in previous papers, no account was taken of the temperature of the conductor experimented upon. When these experiments upon nickel, however, had been going on for a long time, it began to be suspected that the temperature of the room, and so of the nickel plate, did exercise a very considerable influence upon the magnitude of the transverse effect as expressed by the ratio $\frac{E'}{V}$.

A few hasty experiments with considerable ranges of temperature in the room indicated very decidedly that the temperature was a factor to be considered, and that the higher the temperature the greater the value of E′, other things being equal. The magnitude of this influence can hardly be determined from results thus far reached. It may prove that the transverse electromotive force E′ is no more increased by a rise of temperature than the direct electromotive force E* is; and in this case it would appear, as intimated above, that the ratio $\frac{E'}{E}$ is the one to be investigated rather than $\frac{E'}{V}$.

Future investigation must determine this matter; and meanwhile it has been sought to avoid evil consequences by regulating, as well as practicable, the temperature of the nickel plate. Sometimes an experiment had to be made at a rather high temperature for instance; and an attempt would then be made to balance this by making another with about the same strength of magnetic field but at a low temperature, or *vice versâ*. There was, however, even now no attempt to determine the actual temperature of the nickel; but a thermometer was hung up with its bulb close to the plate, and as nearly as practicable always in the same position with respect to the latter, and both plate and thermometer were protected from sudden changes of temperature. As the nickel was of course heated by the current, its temperature must have been always considerably higher than that indicated by the thermometer.

---

* Apparently E′ is in nickel affected by temperature more than E in most metals would be; but the rate of increase of the resistance of nickel with rise of temperature seems not to be known.

Moreover this difference must have varied somewhat with the strength of the direct current; so that the temperature read can be assumed to give only a very rough indication of the *changes* in temperature of the nickel.

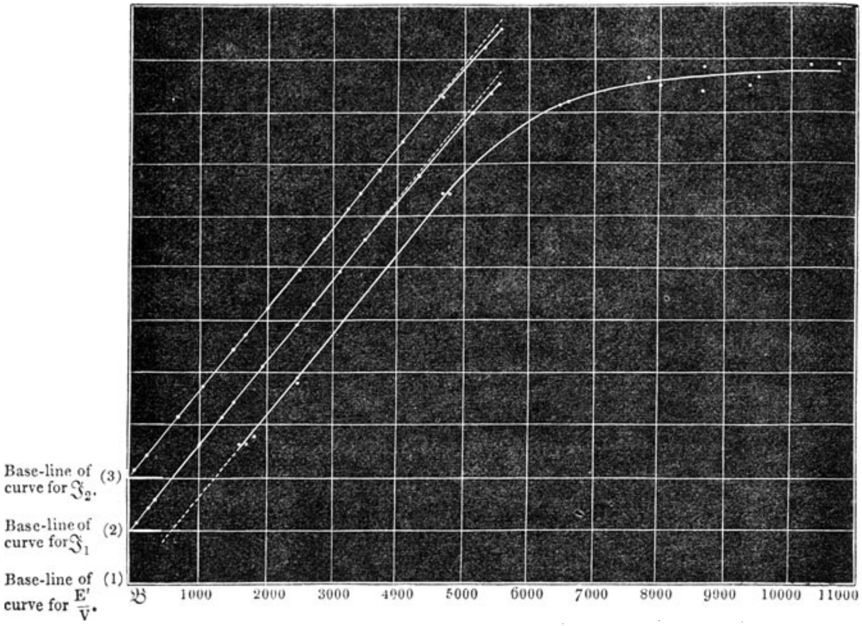
None of the numerical results of measurements made with nickel before the disturbing influence of temperature was discovered are here published. In some of the results afterwards obtained, however, the effects of variations of temperature can apparently be detected, as will be pointed out hereafter.

The general method of experiment has been already sufficiently described in previous papers. There will now be given in tabular form the most important data involved in this examination of nickel, and the values of $\frac{E'}{V}$ obtained. The absolute strength of the primary current through the nickel strip in any case is not given, as, by the method of experiment, both the constant of the galvanometer used to measure this current and the horizontal intensity of the earth's magnetism at this galvanometer (this intensity being assumed to be constant during any one determination of $\frac{E'}{V}$) are eliminated from the formula for $\frac{E'}{V}$. There will be given, however, the tangents of the angles of deflection of the galvanometer-needle, in order to show about what were the limits of variation of the primary current. It may be well to state that this current was what one Bunsen cell would send through—say, six or eight ohms. It will be seen that there are variations of about 6 per cent. in $\tan \alpha$; and the actual variations in the primary current may possibly have been considerably greater than this; for on March 11th, 12th, and 14th the galvanometer stood in a different room from that in which it was placed for the previous observations, and the horizontal intensity of the earth's magnetism was probably somewhat different in the two places. I have, however, as stated above, assumed that within the limits of these variations the value of E′ is a linear function of the direct current. It is evident that no large error can result from this assumption.

It will be seen from the table that the experiments began with the smallest values of the magnetizing force and went on by stages to the highest. This is the proper course to follow in order to avoid at any stage of the magnetizing force the effects of a previous stronger magnetizing force. It must, however, be stated that, before the series of experiments whose results are here published was begun, the nickel had already been several times subjected to a magnetizing force of about 7500, *i. e.* four or five times as great as the forces with which this series begins. The question, of course, arises whether there may not have been induced by this means a permanent magnetism sufficient to affect the results of subsequent experiments. In order to settle this question as far as possible, a small piece of nickel film, of the same quality as the strip in use, was first subjected to the action of a field of about 7000 or 8000. It was then placed in a field of perhaps 1500 or 1600, whose direction was such as to tend to reverse any permanent magnetization which might have been induced in the film by the previous field. It was found that now in the second field the nickel became magnetized, temporarily at least, in the direction of that field. No attempt, I believe, was made in any case to detect the permanent magnetization. In this trial the small piece of nickel film was magnetized, not in the direction of its thickness, but in a lateral or longitudinal direction; so that we do not here have an exact parallel to the case of the strip; but it seems probable that magnetization in the direction of the shortest dimension would be much more easily disturbed than that in a longitudinal direction. Moreover, just before the series of experiments was begun whose results are here published, quite a long series was made with magnetizing forces about equal to those with which the published series begins; and this treatment would have tended, no doubt, to obliterate any traces of permanent magnetism due to the action of previous higher forces, even if this permanent magnetism had been much greater than we have any reason to suppose it was. On the whole, therefore, the probability of any considerable error from this source seems to be very small.

Date.	Temperature.	Tan α.	$\mathfrak{H}$ or $\mathfrak{B}$.	$\frac{E'}{V} \times 10^{10}$.
Feb. 24, 1881	18°·5	·330	1667	209·3
25, ,, ...	22·0	·332	1655	211·1
26, ,, ...	21·5	·335	1664	208·1
26, ,, ...	16·0	·336	1735	213·2
28, ,, ...	19·5	·333	2512	314·3
28, ,, ...	20·0	·333	2512	307·0
Mar. 1, ,, ...	20·0	·330	4734	596·1
2, ,, ...	19·5	·327	4775	596·4
5, ,, ...	19·0	·338	6540	735·5
7, ,, ...	20·0	·339	6415	726·7
7, ,, ...	20·5	·340	7996	761·0
10, ,, ...	21·5	·324	7791	771·0
11, ,, ...	21·0	·342 } Galv. in new position.	8712	783·5
11, ,, ...	18·5	·343	8644	755·1
12, ,, ...	20·0	·338	9561	772·4
12, ,, ...	18·0	·338	9708	759·8
14, ,, ...	21·0	·326	10720	793·3
14, ,, ...	21·0	·323	10290	785·6

Laying off the values of $\mathfrak{B}$ on the base-line, and taking the values of $\frac{\mathrm{E}'}{\mathrm{V}}$, plotted on a convenient scale, as ordinates, we have curve (1). It will be seen that this curve is nearly straight for a considerable distance, and that if this portion were extended backward it would pass very near the origin. Between the points corresponding to $\mathfrak{B}=5000$ and $\mathfrak{B}=8000$ the line tends strongly to the right, and thenceforward it continues as if asymptotic to some horizontal line not very far above.



The points marking the highest values of $\frac{\mathrm{E}'}{\mathrm{V}}$ do not fall so well in line as one might wish; but by looking at the table it will be seen that there were considerable variations of temperature accompanying these observations; and to these variations the irregularities can perhaps be in some part attributed.

We see now at once from the diagram that $\frac{\mathrm{E}'}{\mathrm{V}}$ is not proportional to $\mathfrak{B}$, the magnetic induction in the nickel. Can we find any magnetic quantity to which it is more simply related?

If we turn to the observations of Prof. Rowland on nickel*, we find that they can, as he says, be plotted in several ways.

---

* Phil. Mag. Aug. 1873 and Nov. 1874.

In order to compare them with the observations above given, we need to plot them in some manner that will lay off the values of $\mathfrak{B}$ (M in Prof. Rowland's first paper) on the base-line. We may then take as ordinates the values of the magnetic permeability, as Rowland has done in his first paper (plate iii.), or the values of κ, Neumann's coefficient of induced magnetization, or the values of $\mathfrak{H}$, the "magnetic force"* within the nickel, which would be a reversal of one method used by Rowland in his first paper (plate ii.); or, finally, we may use the values of $\mathfrak{I}$, "the intensity of magnetization according to the German theory," as Rowland calls it in his second article.

Having plotted these various curves we may compare them with (1) above, in order to determine whether our quantity $\frac{E'}{V}$ corresponds most nearly to μ, to κ, to $\mathfrak{H}$, or to $\mathfrak{I}$.

The curve for $\mu\left[\frac{\mathfrak{B}}{\mathfrak{H}}\right]$ will, long before $\mathfrak{B}$ has reached the higher values used in the curve for $\frac{E'}{V}$, have reached a maximum and returned nearly to the base-line. The curve for κ $\left[\frac{\mathfrak{I}}{\mathfrak{H}}\right]$ will be very similar to that for μ. We do not, then, find suggested a close connexion between μ or κ and the quantity we are studying.

The curve for $\mathfrak{H}$ bends *upward*, and is therefore quite dissimilar to that for $\frac{E'}{V}$.

The values of $\mathfrak{I}\left[\mathfrak{B}\frac{\kappa}{\mu}\right]$ obtained from two of Rowland's series†, made either with different specimens of nickel or with one specimen under quite varied conditions, give the curves (2) and (3). A separate base-line is taken for each of the three curves; and the ordinates of (2) and (3) have been plotted on different scales, in order to make the general inclination of those curves agree with that of (1). The values of $\mathfrak{B}$, however (and this is the essential particular in the plotting), are given on the same scale for all three curves. The important facts about the lines (2) and (3) are that they are sensibly straight for a long distance, that they appear to come nearly straight from the origin, and that they begin to bend perceptibly toward the horizontal when $\mathfrak{B}$ becomes 4000 or 5000. Although these lines are carried only a short distance beyond this region,

---

* Thomson's 'Polar Definition,' reprint, p. 397; and Maxwell's Treatise, art. 398.

† Phil. Mag. Aug. 1873, p. 153, and Nov. 1874, p. 327.

we can yet be sure that the bend is not due to faulty observations; for to make an error of 1 per cent. in the value of $\mathfrak{J}$ at this point would require an error of very many per cent., say 20 or 30, in the value of μ as determined by Prof. Rowland. From the manner and rate at which μ was changing at the points where his experiments ceased, it seems almost certain that these lines would continue to bend, and for a time to bend rapidly. Indeed the curve in which Prof. Rowland has continued μ beyond the range of his experiments would indicate that the curves (2) and (3), if continued a short distance further, would turn downward and approach the base-line. This, however, would mean that the magnetization $\mathfrak{J}$ actually *decreases* after a certain point with *increase* of the magnetizing-force. The possibility of this is spoken of by Rowland*; but there seems to be no experimental evidence of such an effect; and if it does not exist, it appears altogether probable that the lines (2) and (3) would become asymptotic to horizontal lines lying considerably higher than any points reached by the curves as here given.

We can therefore say that, so far as actual experiments have gone, there seems to be much tending to prove a very simple and intimate relation in nickel between the transverse effect and the "magnetization according to the German theory."

It would, of course, be desirable to test for some more minute agreement than has yet been traced between the curves for $\frac{E'}{V}$ and $\mathfrak{J}$; but such a testing would probably be difficult to make. An exact agreement could not be expected; for it would probably be almost impossible to obtain exactly the same quality and condition of metal in the very different shapes required for experiments on $\mathfrak{J}$ and experiments on $\frac{E'}{V}$.

There are, however, certain minute characteristics which would belong to all curves for $\mathfrak{J}$. Thus (2) and (3) should not be straight at any point. They are lines of double curvature, the steepest part of each being not far from $\mathfrak{B}=2000$. The curvature in this region, however, is very slight; and to detect a corresponding curvature in the line for $\frac{E'}{V}$, if such exists, would be a matter of considerable difficulty, though not, perhaps, impossible.

Having gone thus far with nickel, we might, were it not for the anomaly presented by the sign of the rotational coefficient

---

* Phil. Mag. Nov. 1874, p. 322.

in iron and cobalt, make a prediction by analogy as to what would prove to be the character of the curves for $\frac{E'}{V}$ in these metals. We should say that they would be sensibly straight for a much longer distance than the curve for nickel, and that in fact it might be difficult to carry the magnetization far enough to detect any marked departure from a straight course. So great a difference in behaviour as is indicated by a reversal of the sense of the transverse effect, however, makes any such predictions hazardous.

This difference of sign in the rotational coefficients of the magnetic metals is so anomalous and so important a fact, that one returns again and again to its consideration. Quite recently the determination of this sign for all three metals has been made anew. I have now tested, in all, four plates of iron (three of them having been cut from the same sheet, but the fourth being of a different thickness and probably of a somewhat different character), two plates of nickel (certainly very different from each other in condition), and one specimen of cobalt. With all these the record is perfectly consistent. Nevertheless it would be desirable to examine more specimens, and those differing widely in character. Different experimenters have observed many peculiar effects in iron under the influence of magnetism and the electric current, magnetism and mechanical strain, or the combined influence, which in a certain form we have here, of all three; and these effects appear to differ greatly, and sometimes to be of different signs, in soft iron and hard iron or steel. Thomson has found* that, under conditions of the above character, soft iron and nickel are, in certain apparently very important particulars, opposed in behaviour. I have looked in vain through all the facts of this kind with which I am acquainted for any plausible explanation of the fundamental phenomenon of the transverse action, nor can it be said that any clue has been found to the cause of the diversity observed. Nevertheless the opposition which Thomson has found in the behaviour of soft iron and nickel, under conditions of magnetism and mechanical strain, furnishes an analogy which should not be lost sight of. Thomson has moreover noticed that the effect which he was studying in soft iron became reversed in this metal at a very moderate value of the magnetizing force. It might be well to test the direction of the transverse effect also with very small intensities of the magnetic field.

An extended examination of the effect in iron and cobalt, similar to that which has been made in the case of nickel,

---

* Phil. Trans. May 1878.

should be undertaken as soon as practicable. It will require very great intensities of the magnetic field and a very large battery* to carry these metals through a range of magnetization corresponding to that through which nickel has been examined. It seems doubtful whether the magneto-electric machine can be here employed, as the current which it produces may not be sufficiently uniform to be used with advantage.

The examination of the non-magnetic metals also should be continued as fast as circumstances will permit, with the object of determining the sign and, when practicable, approximately the magnitude of the rotational coefficient in every case.

In my article of last November I stated that, in accordance

---

* In the experiments here detailed, the largest battery used has consisted of 48 large Bunsen cells arranged 8 in series. The resistance of each cell was probably something more than an ohm; the resistance of the electromagnet is, I think, rather less than an ohm. The resistance of the connexions was considerable, however; and the battery probably gave about its best effect.

After this powerful battery had been applied to the electromagnet, a rather singular effect was observed on returning to the use of weak currents. In making observations in the usual way to determine the strength of the field produced by these weak currents, it was found that the impulses given to the galvanometer-needle were very capricious. These observations may be arranged under two heads, + and −, according to the direction of the current through the electromagnet, this current being usually reversed after each withdrawal of the little coil from between the poles. The observations being arranged in this way, it would be found that there were occasional sudden changes of many per cent. in the readings in the same column. Of course the most obvious explanation of the phenomenon was that some connexion was loose, either in the circuit of the galvanometer and the test-coil or in that of the electromagnet. That the fault was not in the former circuit was made probable by the fact that by means of the earth-inductor, which was in the same circuit, quite uniform deflections of the galvanometer-needle were produced. To test for a fault in the magnet circuit, a tangent-galvanometer was introduced into it and its deflections observed during the series of observations on the strength of the field.

The readings of the tangent-galvanometer decreased slowly with the running-down of the current; but the changes were quite regular, and not at all of a character to account for the irregularity of the other observations. The most plausible explanation I could finally propose was, that this irregularity in the strength of the magnetic field was due to a sort of uncertain struggle between the action of the present weak magnetizing current, and the magnetization previously induced by the strong currents in the poles of the electromagnet, which are not, I believe, of very soft iron, and are probably capable of considerable permanent magnetization. I do not by any means feel able to assert, from my rather hasty observations, that there can be no other explanation. I have, however, simply thought the matter of sufficient importance to justify me in recording what seemed to be the fact.

with Prof. Rowland's suggestion, I had tested the Kerr effect with one specimen of nickel, and found it to be of the same sign as the effect which Kerr had observed with iron. In order to prevent mistakes, the experiment was repeated with iron, or rather, I suppose, with steel, the result being the same which Kerr had obtained. The surface of nickel first used in this way was the coating upon one of the plates of Prof. Rowland's absolute electrometer, the metal beneath being brass. Two other specimens of nickel have since been tried. One was a coating deposited electrolytically directly upon the iron pole of the electromagnet, the other was a nickel film fastened with soft cement to a plate of glass. Probably none of these specimens was pure; but (and this is a matter of more importance) the third was of precisely the same character and origin as the specimen in which the transverse effect was studied. The Kerr effect is of the same sign in all three plates of nickel, *i. e.* of the same sign as the effect in iron.

One specimen of cobalt has also been tested for this effect. A block of cast cobalt, quite similar to that from which was cut the cross mentioned in the first part of this article, was sawn in two, and one of the fresh surfaces was made quite smooth with a file and then polished with emery. It is not difficult to get a sufficiently good surface. An hour's work might prepare it.

With sunlight and a tolerably strong magnetic field, say 4000 (cm.-grm.-sec.), the rotation produced by cobalt was detected, and found to be of the same sign as that observed with nickel and iron.

The fact that nickel behaves like the other magnetic metals in optical effect, but differently from them in the transverse electrical effect, is on its face undoubtedly an argument against the theory which refers the two effects to the same cause. In order if possible to examine the optical effect in a somewhat different manner, an attempt has been made to detect an action of magnetized nickel upon polarized light transmitted directly through it. For this purpose a thin piece of glass was coated over a part of its surface with nickel by Wright's process*, the action being stopped before the nickel film became thick enough to be opaque. It was found, however, that the glass alone, although only about $\frac{1}{6}$ millim. thick, perceptibly rotated the plane of polarization of the light sent through it when subjected to the very strong magnetic field employed. The action produced by the nickel and glass together was of the same sign as that produced by the glass alone; and as the magnitude of the effect could not in either case be measured

---

* Amer. Journ. of Science, Jan. 1877, p. 49, and Sept. 1877, p. 169.

with any accuracy, the experiment was quite negative in result. I now, however, took a glass tube, fused the end, and blew out the bubble till it burst. A piece of the exceedingly thin film thus obtained was subjected to the action of the magnet, and most strenuous endeavours were made to detect its action upon the beam of polarized light. This action must have been exceedingly slight, though there is some evidence, which it is not necessary to give, that it was detected. The glass, however, was coated as the first piece had been, and again with its coating subjected to the action of the magnet. The trial was, for certain reasons, rather unsatisfactory; and although no rotation of the plane of polarization was now detected, I do not think this fact can be taken as evidence that the effect of the nickel had counterbalanced the effect of the glass. Both these experiments on direct transmission have been, we may say, quite negative; but these details are given as marking out a line of research which will probably be some time resumed.

An endeavour has also been made to detect a possible rotational effect due to reflection from silver when strongly magnetized*. For this purpose two strips of silver upon glass were used; and these strips were fastened one upon each pole of the magnet, the silvered surfaces being turned toward each other and as nearly parallel as practicable. The poles being brought near together and the light being let in between the silvered surfaces at a large incidence, it was possible to obtain twenty or thirty successive reflections before the beam emerged toward the analyzing Nicol. Certain difficulties were introduced by this arrangement; but in spite of these I think that, if the action of silver had been one tenth as strong as that of iron, the effect would have been detected. No such effect was observed.

XIX. *On the Results obtained from a Modification of Bunsen's Calorimeter.* *By Prof.* B. Stewart *and* W. Stroud†.

A DESCRIPTION of this instrument was brought before this Society on June 26, 1880, and afterwards appeared in the Proceedings of the Physical Society, vol. iv. p. 52, and Philosophical Magazine, vol. x. p. 171. The results obtained at that time were not very good, owing

---

* In this experiment I had the very efficient assistance of Mr. Arthur W. Wheeler, Fellow in Physics, whose untimely death is so deeply lamented.

† Communicated by the Physical Society, having been read at the meeting on June 26, 1881.