On some Experiments illustrating an Explanation of Hall's Phenomenon

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a rise of temperature would bring it still nearer. On the other hand, it may be expected that cold would drive it further off the solid, so that any phenomena connected with this layer in liquids might be better developed in descending cold currents than in ascending warm ones.

In conclusion, the writers wish to call attention to a paper read to the Royal Society of Edinburgh, by Mr. John Aitken, on the 21st of January this year, and of which an abstract appeared in 'Nature' of the 31st January last. Judging from the abstract, it would appear that Mr. Aitken, so well known for his previous researches on dust-particles as nuclei, has been travelling over much of the same ground as we have, and that he has arrived on the whole at similar conclusions.

University College, Liverpool.

II. *On some Experiments illustrating an Explanation of Hall's Phenomenon.* By SHELFORD BIDWELL, M.A., LL.B.*

[Plate II.]

IN a recent communication to the Royal Society I attempted to show that the apparent action of a magnet upon an electric current (well known as Hall's Phenomenon) might be completely explained by the operation of mechanical strain and certain Peltier effects.

My experimental acquaintance with the subject began in July 1883, and I have since devoted a considerable amount of attention to it. But the paper referred to contained only a bald and simple statement of the results at which I finally arrived, little or nothing being added by way of comment or explanation. To my mind, saturated as it was with the matter, it appeared that such a statement afforded in itself overwhelming evidence in favour of the views which I had been led to adopt; and I believed that when once set forth, however briefly, they could not fail to meet with immediate acceptance. But Hall's own theory of the phenomenon has for some years been universally recognized as interpreting a well-established law of nature. It seemed to be in complete harmony with the views of Clerk Maxwell as to the existence

* Read March 8, 1884.

Fig. 1.

Fig. 3.

Fig. 2.

Fig. 4.

Fig. 5.

Fig. 6.

of a rotatory coefficient of resistance : it has provided a field for investigation to mathematicians of the highest eminence; and it has derived additional interest and importance from the fact that it was believed to point to an intimate relation between electricity and light. Under the circumstances, it is reasonable that such a theory should not be abandoned without the exercise of very considerable caution.

I propose therefore in this paper to treat the subject in greater detail than at first seemed necessary. Attention will be directed to facts and arguments which in my former paper were left altogether unnoticed ; and those points which, as I have gathered from conversation and correspondence, seem to present the greatest difficulty, will be considered at some length. With regard to the experiments upon which I rely, such of them as are new are of a very simple character, and can easily be repeated by any one possessing a reflecting galvanometer. They are of course of a somewhat delicate nature, and more suitable for the laboratory than for public demonstration ; but I shall be bold enough to attempt the performance of most of them in the presence of the members of the Physical Society.

Mr. Hall's first paper on the subject appeared in Silliman's Journal in 1879, and is reproduced in the 'Philosophical Magazine' for March 1880. After stating that his object was to prove that the current of electricity in a fixed conductor is acted upon by a magnet, he describes the following experiment :—A strip of gold-leaf, forming part of a circuit, was placed between the poles of an electromagnet, cutting the lines of force perpendicularly. The terminals of a galvanometer were applied to opposite edges of the gold-leaf, and moved about until two equipotential points were found. The magnet was then excited, and a galvanometer-deflection immediately occurred. This deflection was too large to be attributed to the direct action of the magnet upon the galvanometer-needle. It was permanent, and so not due to induction. It was reversed when the magnet was reversed. "In short," says Mr. Hall, "the phenomena observed were just such as we should expect to see if the electric current were pressed, but not moved, toward one side of the conductor. In regard to the direction of this pressure, as dependent on the direction of

the current in the gold-leaf and the direction of the lines of magnetic force, the following statement may be made:—If we regard an electric current as a single stream flowing from the carbon pole of the battery through the circuit to the zinc pole, the phenomena indicate that two currents in the same direction tend to repel each other. But if we regard the current as a stream flowing from negative to positive, then the phenomena indicate that two currents in the same direction attract each other.” The meaning of this is, as appears from subsequent papers, that the direction of the transverse current across the gold-leaf is opposite to that in which the gold-leaf itself would move across the lines of force if it were free to do so. If the two points to which the galvanometer is connected are joined by an equipotential line across the gold-leaf, the effect caused by magnetization may be expressed by saying that the equipotential line is rotated in a direction *opposite* to that of the current circulating in the coils of the electromagnet.

A second paper was published in the ‘Philosophical Magazine’ for November 1880, in which were described experiments made with other metals in addition to gold. With silver, nickel, tin, and platinum the direction of the transverse effect was the same as with gold, though it differed in degree in the case of every metal. But with iron the direction of the transverse current was found to be reversed; a fact which Mr. Hall was naturally inclined to attribute to the magnetic properties of that metal, though he was at a loss to account for the difference between its behaviour and that of nickel, which is also strongly magnetic. In iron, therefore, the equipotential lines are rotated in the *same* direction as that of the current circulating in the coils of the electromagnet.

In a third paper, published in the ‘Philosophical Magazine’ for September 1881, the author says that the direction of the transverse electromotive force in the magnetic metal cobalt was the same as in iron. In this paper it is stated that $\frac{E'}{V}$ is probably a constant for any given metal; E' being the transverse electromotive force per centimetre of the width of the strip, and V the direct current divided by the section of the conductor. This quantity $\frac{E'}{V}$ is called, in accordance with a

suggestion of Dr. Hopkinson, the "rotational coefficient" of the metal. The direction of the transverse effect or sign of the rotational coefficient is in iron and metals which behave like iron called +, because its direction is that which the conductor itself bearing the current would follow if free to move across the lines of magnetic force. In gold and similar metals the sign of the rotational coefficient is called -.

A fourth paper appeared in the 'Philosophical Magazine' for May 1883, entitled "Rotational Coefficients of various Metals," in which it is stated that aluminium, copper, and brass behave in the same manner as gold, that zinc behaves like iron, and that with lead no transverse effect whatever is produced.

Collecting Mr. Hall's various results, we find that, of the thirteen different metals which he has tested, three (iron, cobalt, and zinc) are classed as positive; nine (gold, silver, tin, copper, brass, platinum, nickel, aluminium, and magnesium) are negative; and one (lead) shows no signs of rotation.

I began a repetition of Hall's experiments with the object, if possible, of carrying them further, and especially of establishing in a satisfactory manner the existence of a connexion between the phenomena in question and the magnetic rotation of the plane of polarized light. My earlier experiments were made with iron, of which I happened to possess a suitable specimen in the form of foil .019 millim. thick. The magnet used consisted of a bar of soft iron, 38 millim. in diameter, bent into horseshoe shape; if pulled out straight its length would be about 50 centim. It was covered with about 600 turns of wire, 2 millim. in diameter; the pole-pieces used were 30 millim. square, with flat ends, and were generally fixed about 6 millim. apart. The magnet was excited by a battery of either seven Grove's or, more generally, of five 3-quart bichromate cells.

The arrangement of the apparatus is shown in fig. 1 (Plate II.), where PP' is the strip of metal cemented to a plate of glass, and placed above one pole, S, of the electromagnet; M is a section of the yoke of the electromagnet; B is a battery of one or two cells, generating the current through the strip; C a commutator to reverse the direction

of the current ; B' the electromagnet battery ; C' a commutator to reverse the magnetism ; W W' wires from equipotential points in the metallic strip to the galvanometer G ; K a key in the galvanometer circuit.

The method of proceeding was as follows :—A current was passed from B through the strip P P', and sliding-contacts on the opposite edges were adjusted until, when the key K was depressed, no deflection of the galvanometer occurred. The magnet was then excited by the battery B', and on again depressing K the galvanometer was deflected. On reversing the direction of the current either from B or B', by means of one of the commutators, an opposite deflection occurred when K was depressed. The galvanometer used was a reflecting instrument, of .2 ohms resistance; it was placed at a distance of 4 metres from the scale, upon which it reflected the image of a very fine wire. The scale, key, and commutators were all within easy reach of the operator.

With this comparatively small apparatus there was at first some difficulty in producing a sensible Hall effect, and the greatest care had to be exercised to avoid disturbing influences. In addition to the possible sources of error mentioned by Mr. Hall, I was much troubled by one which he does not seem to have noticed, and which may probably account for the uncertain nature of some of his results. It was found that, though the galvanometer-deflection indicating the Hall effect was invariably reversed when the direction of the magnetizing current was reversed, it sometimes appeared to be altogether independent of the direction of the current passing through the strip. It occurred to me to excite the magnet without passing any current at all through the strip ; and I found that when the key K was depressed a steady deflection of the galvanometer occurred, which was reversed on reversing the magnetizing current. That this effect was not due to the direct action of the magnet on the galvanometer-needle was proved by the fact that, when the key was raised, the deflection immediately ceased. It appeared at first sight as if the magnet had the power of generating a current in the strip of metal. But this, of course, was impossible if the magnetic force remained constant. I concluded therefore that it was an effect of induction caused by weakening of the magnet

in consequence of the running-down of the battery, or perhaps of the resistance of the wires being increased by the heating-effect of the current; but it was difficult to see how this could induce a current in the strip which was symmetrically situated with regard to the lines of force. Finally, I found that the effect was produced, not in the strip at all, but in the wire which passed from the strip between the arms of the electro-magnet to the galvanometer. When this wire was led out on one side of the yoke, the deflection produced was opposite to that which occurred when the wire was led out on the other side; and if the wire was turned half a dozen times round the magnet, the resultant galvanometer-deflection was so great that the spot of light disappeared from the scale. This was ultimately remedied by forming the wire in question into a loop embracing the yoke of the magnet, as shown at LL' (fig. 1). From this loop an independent wire W' , attached to a sliding binding-screw D , was led to the galvanometer. It was found that the position of D could be so adjusted that, K being steadily depressed, the magnetism of the electro-magnet might be interrupted, or even reversed, without causing any material deflection of the spot of light even at the instant of the change. When so adjusted it was, of course, impossible that the comparatively small changes in the magnetic force, caused either by polarization of the battery or by the heating of the wires, could have any effect upon the galvanometer. The Hall effect now came out with great distinctness, the deflections to the right or left of the zero-point of the scale, caused by exciting the magnet, being generally as much as 15 or 20 centim.*

A number of experiments were now made with pieces of iron cut into various shapes, and occasionally with fine copper wires arranged in different forms, and the conviction gradually forced itself upon me that the effect in question was not due to any such action of the magnet upon the current as that which Mr. Hall supposed.

* That the magnetizing current used by Hall was not absolutely constant is evidenced by a statement in his paper in the 'Philosophical Magazine' for September 1881, that, the readings of a tangent-galvanometer, placed in the circuit for a special purpose, "slowly decreased with the running-down of the current."

The possibility of a thermoelectric effect suggested itself at an early stage, the heat being derived from the current flowing through the strip, and the changes in direction being possibly produced by some effect of magnetization upon the thermoelectric properties of the metal. Such effects, I thought, could only occur at the points where the wires were attached which went to the galvanometer. Disks of bismuth and antimony were therefore inserted at the contacts of the wires and the strip, and their relative disposition varied in order that the thermoelectric effect, if any existed, might be exaggerated. But it was found that, whether the disks were of bismuth or of antimony, or of the two metals in various combinations, the extent of the galvanometer-deflections was unaffected, except in such a manner as could be perfectly accounted for by ordinary thermoelectric action. The idea of mechanical strain then occurred to me; and I may mention that this occurred to Mr. Hall, but only to be rejected by him as utterly inadequate to account for the phenomenon. I attached a string by means of sealing-wax across the middle of a thin sheet of iron, which was cemented to glass and connected with the battery and the galvanometer as usual. On pulling the string in a transverse direction I found unmistakable indications of a galvanometer-deflection in the same direction as if the sheet of iron had been acted upon, not by a mechanical pull, but by the electromagnetic force. The effect was small, but I have since greatly increased it by the following device:—A strip of thin iron was cemented between two thin slips of deal about 20 centim. long, forming a kind of sandwich. The sandwich was attached to a board by means of four screws, the distance between the middle screws being about 7 centim. The two ends of the iron were connected with a battery, and the middle points of its opposite edges with the galvanometer. On pressing with the finger the middle of one edge of the sandwich in a direction perpendicular to its length and in the plane of the metal, though no appreciable distortion was produced, the galvanometer was immediately deflected, the direction of the current from the strip to the galvanometer being always the same as the direction of the force. A similar sandwich was prepared in which platinum-foil was used instead of iron, and with this a transverse force produced

opposite deflections to those which occurred in the case of iron.

There is, of course, no doubt whatever, as to the real existence of a mechanical stress in a conductor through which a current is passing, and which is at right angles to the lines of magnetic force. Its effect may be rendered evident to the eye by a simple experiment, to which I attach no importance whatever except as affording a rather pretty illustration of a perfectly well-known law. A shallow glass cell is fitted with iron electrodes at its two ends, and is filled with mercury to a depth of 3 or 4 millim. When this cell is placed between the poles of an electromagnet and a current is passed through it, the mercury along the transverse middle line is drawn towards one side of the cell. Here it is heaped up, and, yielding to the force of gravitation, it runs along the two ends of the cell in the opposite direction, once more to be urged forwards across the lines of force. The result of a continuation of this process is the formation of two little whirlpools of mercury which rotate in opposite directions in the two halves of the cell. The same effect may be produced, but with less violence, if a solution of sulphate of zinc is used with zinc electrodes.

Having completely satisfied myself that mechanical strain was concerned in the production of Hall's phenomenon, it remained to ascertain the *modus operandi*. Thermoelectric action again occurred to me, and I remembered that Sir William Thomson had discovered that thermoelectric effects occurred in stretched and unstretched wires of the same material when brought into contact with each other. On referring to Thomson's paper, which was published in the 'Philosophical Transactions' for 1856, I found it stated that if a stretched copper wire is connected with an unstretched copper wire and the junction heated, a thermoelectric current will flow from the stretched to the unstretched wire through the hot junction; while, if the wires are of iron, the direction of the current is from unstretched to stretched. There could be little doubt that effects of the same nature would occur in sheets of these metals; and I proceeded to consider the precise manner in which a sheet of metal conveying a current would be strained when placed between the poles of an electromagnet.

My first idea was that, since the magnetic force was strongest in the middle of the field, the lower half of the metallic sheet would be, on the whole, transversely compressed and the upper half extended. But I immediately saw that strains of this nature would not explain Hall's phenomenon, unless indeed it turned out to be the case that lateral compression and extension produced opposite thermoelectric effects to those caused by longitudinal compression and extension. The effect of lateral compression in an iron wire was noticed by Thomson in the paper referred to. He states that when an iron wire is laterally compressed, a current passes from uncompressed to compressed through the hot junction. He supposes therefore that since a lateral traction would probably produce the reverse effect of a lateral pressure, it would give rise to currents from stretched to free—the reverse of longitudinal traction. In making this experiment, Thomson used many turns of wire wound round a block of wood (to multiply the effect) and squeezed the whole in a hydraulic press. I attempted to repeat the experiment by a simple method and using only one wire; but no amount of pressure that I was able to produce resulted in any indication whatever of a current. I have since endeavoured to ascertain whether any appreciable current could be produced in a sheet of metal by lateral traction. The arrangement used is shown in Pl. II. fig. 2:—A B is a sheet of metallic foil; the shaded part D E was held between two fixed blocks of wood, and the shaded part C F between two blocks of wood which were attached to a lever. A heated glass rod was applied along C D, and on drawing the lever upwards in the direction of the arrow, a current immediately passed between the unstrained and the strained portions of the metal through the hot junction. Two metals only have been thus treated—zinc and iron; and the direction of the current was in both cases the same as if the traction had been longitudinal instead of lateral. In zinc the effect was very strong, in iron it was feeble, but not, I think, so feeble as to be doubtful.

On further consideration I came to the conclusion that no appreciable strains of the kind above referred to would be produced by the transverse force in the magnetic field. This force would be sensibly uniform across the whole of the middle portion of the strip, for the pole-pieces were flat and

within a very short distance of each other, and the strip did not extend across their whole width. Considering the analogy of a beam of wood, rigidly fixed at both ends, and acted upon in the middle by a force perpendicular to its length, I imagined that the transverse force would tend to make the metallic strip assume a distorted form of the same character as that indicated in fig. 3. As thus distorted, the strip may be mapped out into six districts, in three of which the metal is, on the whole, subjected to longitudinal traction, while in the other three it undergoes compression. This, of course, supposes that the metal, though cemented to glass, is capable of yielding to some extent to the force acting upon it. And since neither the metal itself, nor the glass, nor the cement which connects them, is absolutely rigid, this must necessarily be the case, though probably in a very small degree. The ends C, D of the strip, which are in a weaker part of the magnetic field, or even removed from its influence altogether, will not be acted upon in the same manner; and if the strip is sufficiently long they will remain absolutely at right angles to the middle line, as shown in the figure. Two points of contrary flexure will therefore exist on each of the edges; and the necessary consequence of this will be alternate regions of compression and extension, as indicated in the figure. It seems to me that, however thin the metal may be, so long as it is not altogether without cohesion and elasticity, the action of a transverse force across the middle will affect it in some such manner as that pointed out, to some degree which may be very small.

I have prepared a rough model to illustrate my idea of the nature of the distortion which the plate undergoes. A number of narrow strips of deal are attached at short intervals to a rectangular sheet of indiarubber: they are equal in length, and parallel to the shorter sides of the rectangle. The two strips at the ends of the sheet are nailed firmly to a board; otherwise the indiarubber is free. If the middle strip of wood is drawn or pressed in a direction transverse to the rectangle, the indiarubber is distorted and assumes the form of fig. 3; and the regions which are there marked as stretched and compressed appear very clearly, the strips being drawn apart in the former, and crowded together in the latter. The same kind of distortion would be produced, though to a

smaller extent, if the ends, instead of being rigidly fixed, were able to yield, more or less, to pressure or tension. But unless they were each attached to a single point, and were otherwise perfectly free (in which case the sheet would be longitudinally divided into two equal regions of compression and extension), some such distribution of strain as that indicated in fig. 3 must necessarily occur*.

Suppose now that a current is passing through the plate from C to D, and that A and B are two points on the opposite edges, which, when the metal is unstrained, are at the same potential. Then the ratio of the resistances between the points C and A, and the points A and D, is equal to that between the points C, B and the points B, D. And so far as mechanical strain alone is concerned, this equality will not be disturbed by placing the plate in a magnetic field, the strain produced being symmetrically distributed on both sides of the middle line. At all events, no strain could occur which would in itself affect the resistance of gold and iron in opposite ways, for the resistance of both is increased by extension, and (presumably) diminished by compression. But the currents from C to A and from B to D pass from regions which are compressed to regions which are stretched, while the opposite is the case with currents passing from C to B and from A to D. And here the thermoelectric effects come into play.

It has already been mentioned that a thermoelectric current will pass from a stretched copper wire to an unstretched copper wire in contact with it, if the junction is heated. From this it might be inferred, that a current would flow through the heated junction from an unstretched or free copper wire to a longitudinally compressed copper wire. And I have proved by actual experiment that this is the case. *A fortiori*, therefore, a current would pass through the heated junction from a stretched copper wire to a compressed copper wire. For similar reasons the current would, if the wires were of iron, flow in the opposite direction, from the compressed wire to the stretched wire, across the hot junction. And the

* It should be remarked that no account whatever is taken in the model of lateral compression and extension, which, as already stated, are probably very small in the case of the metallic sheets used in Hall's experiments.

same effects, so far as regards currents between stretched and unstretched portions, occur, as I have proved experimentally, if strips of foil are used instead of wires. If, therefore, a battery-current is passed from a stretched portion of a wire or foil to a compressed portion, heat will (according to the laws of the Peltier effect) be absorbed at the junction if the metal is copper, and will be developed at the junction if the metal is iron. In passing from compressed to stretched regions, the converse effects will occur.

Let us now imagine the metal plate to be divided into four equal regions A, B, C, D, as shown in fig. 4. Let a current pass through the plate from E to F, and let a force (produced electromagnetically or otherwise) be applied in the direction H G. First, suppose that the plate is of copper, then the current travelling from E to the line O G passes from a compressed to a stretched portion of the metal; heat will therefore be developed in the region A. Between the line O G and the point F, the current passes from a stretched to a compressed portion; heat will therefore be absorbed in the region B. For like reasons, heat will be absorbed in C and developed in D. The temperature of the copper plate will therefore not be uniform, the portions A and D being on the whole hotter than the portions B and C. But the resistance of a metal increases with its temperature. The resistance of A and D will therefore be greater, and the resistance of B and C smaller, than before the plate was strained. If therefore G H were originally an equipotential line, it is clear that it will be so no longer. An equipotential line through the point O will now be inclined to G H in the direction K L, as shown in the figure.

Supposing the plate to be of iron instead of copper, the Peltier effects will be reversed, and the regions which in the former case were hot will now be cold, and *vice versâ*. The distribution of resistance will be changed in a corresponding manner, and the equipotential line will be rotated in the opposite direction.

The peculiar thermoelectric properties of copper and iron, discovered by Sir William Thomson, are thus seen to be sufficient to account for Hall's phenomenon in the case of those metals. If the explanation which I have offered be the

correct one, it is clear that Hall's "positive" metals, cobalt and zinc, should, when tested by Thomson's method, exhibit the same thermoelectric effects as iron; that his "negative" metals should behave thermoelectrically in the same manner as copper; and that lead might be expected to be thermoelectrically unaffected by strain. It became exceedingly interesting to ascertain if this was the case, and I therefore proceeded to repeat Thomson's experiment upon all the metals mentioned by Hall. In most cases at least two or three different specimens of the metal were used, and, except in the case of cobalt, the following simple method of operating was adopted.

A short piece of the wire or strip of the foil to be examined was held at the ends by two fixed brass clamps, about 5 or 6 centim. apart, which were connected with the terminals of a low resistance reflecting-galvanometer. A point near the middle of the metal was then gripped by a pair of pliers which had been previously heated, and one half of the wire was drawn taut, leaving the other half slack. A galvanometer-deflection immediately occurred, which ceased when the tension was discontinued. The process was then repeated, the pull being made in the opposite direction: this resulted in an opposite deflection of the galvanometer.

Upon first gripping the metals, especially the harder ones, it frequently happens that before beginning to pull a thermoelectric current is set up between different parts owing to local differences in physical condition, and these thermocurrents are sometimes so strong as to completely mask the effect produced by stretching. Their effect may be practically eliminated by giving a series of short pulls at regular intervals, corresponding with the periodic swing of the galvanometer. The results can thus be observed with perfect accuracy, but the method is not well suited for use before an audience on account of the difficulty of rendering evident the correspondence between the pulls and the consequent galvanometer-deflections. The hand and eye should, in fact, work together*.

* The experiment was, however, successfully exhibited at the meeting. All Hall's metals were used except cobalt, which could not be obtained in the form of wire, and the only doubtful case was that of tin, the wire

The directions of the currents through the hot junctions are given in the following table, which is taken from my former paper, with the addition of the results for wires of tin and lead:—

S = Stretched. U = Unstretched.

Metals.	Forms used.	Direction of current.	Hall's effect.
Copper	Wire and foil; pure.....	S to U	—
Iron	Wire and sheet; annealed	U to S	+
Brass	Wire, commercial	S to U	—
Zinc	Wire and foil	U to S	+
Nickel	Wire	S to U	—
Platinum	Wire and foil	S to U	—
Gold	Foil, purity 99·9 per cent.	S to U	—
	<i>Wire, commercially pure</i>	<i>U to S</i>	
	Jewellers' 18 ct. wire and sheet ..	S to U	
	Jewellers' 15 ct. sheet	S to U	
Silver	Wire and foil	S to U	—
Aluminium ..	<i>Wire and foil, pure</i>	<i>U to S</i>	—?
Cobalt	Rod; 8 mm. diameter.....	U to S	+
Magnesium ..	Ribbon	S to U	—
Tin.....	Foil and wire	S to U	—
Lead	Foil (assay) and wire	No current	Nil.

It will be seen that, in every case excepting that of aluminium and one out of five specimens of gold, there is perfect correspondence between the direction of the thermoelectric current and the sign of Hall's effect. With regard to the aluminium, a piece of the foil was mounted upon glass, and Hall's experiment performed with it. As was anticipated, the sign of the "rotational coefficient" was found to be + like that of iron, zinc, and cobalt. It is probable, therefore, that the specimen of aluminium with which Mr. Hall worked differed in some respect from that used by myself. The anomalous specimen of gold being in the form of wire, could not be submitted to the same test: it probably contains some disturbing impurity.

With regard to cobalt, since it could not be obtained in the

being ruptured before any marked deflection was produced. It should be noticed that the temperature of the hot junction should not exceed about 100° C., otherwise, at least in the case of iron and nickel, different results may be obtained.

form either of wire or of sheet, it was necessary for the observation of the thermoelectric effect to treat it in a somewhat different manner from that previously described. A rod of the metal was held firmly by one end in a vice, and both ends were connected by wires with the terminals of the galvanometer. The middle of the rod was gripped with gas-tongs which had been made hot in a Bunsen flame, and stress was produced by twisting. It was found that, while the stress continued, a current passed from the strained to the free portion of the rod through the hot part. The direction of the current was the same whether the twist was clockwise or counterclockwise, and it ceased the moment the stress was discontinued. For the sake of comparison, a copper rod was treated in a similar manner, and the resulting current was in the reverse direction. For the purpose of this experiment, torsion is clearly equivalent to traction, both tending to draw asunder the molecules of the metal.

Some months ago, when experimenting with pieces of iron cut into different shapes, I tried the effect of making a longitudinal slit along the middle of the plate. The result was, that the Hall effect was considerably weakened even though a stronger current was used than before the slit was cut. In the light of subsequently acquired knowledge, I have varied the experiment thus :—A plate of iron was prepared as usual, but the galvanometer-wires, instead of being attached in the ordinary manner, were connected to points nearer to the middle of the plate than to the edges. When the magnet was excited, the ordinary Hall effect was found to occur, but it was somewhat weakened. Two longitudinal slits were then cut along the middle of the plate, a connecting piece about 4 millim. wide being left at the centre. A current from two bichromate cells was passed through the plate, and the positions of the galvanometer connexions were carefully adjusted, until, on depressing the key K (see fig. 5), not the smallest movement of the spot of light occurred. The electromagnet was then excited, the key was depressed, and the spot of light was deflected. The magnet-current was reversed, the key was again depressed, and there was again a deflection of the spot of light. And the directions of the deflections were in both cases *opposite* to those which occurred before the slits

were cut in the plate. The amount of deflection thus produced was about 35 scale-divisions*.

Now it is quite clear that, if Hall's effect were due to the direct action of the magnet upon the current, it ought to occur in each of the two halves of the divided plate. The transverse current in the lower half would (supposing its direction to be upwards in the figure) pass from the point B to the metallic bridge at the centre, and the transverse current in the upper half would pass from the centre to the point A. Upon no conceivable hypothesis could the mere division of the plate have the effect of actually reversing any direct action which the magnet might exert upon the current.

The strain theory, however, explains the matter perfectly. Supposing a south pole to be beneath the plate, and the current through it to be flowing from left to right, then, before the cuts were made, the point A would be in a stretched district, and the point B in a compressed district (see fig. 3); and the resulting Peltier effects would cause the transverse current to flow in the direction A G B, fig. 5. But in consequence of the longitudinal cuts, the distribution of stress is altered. Instead of the whole plate, each half of it may now be mapped out into six areas which are differently strained, and the point A will now be in a compressed district, and B in a stretched district. The Peltier effects will therefore be reversed, and the transverse current will flow in the direction B G A. The existence of the central bridge does not prevent the new strain, because there is no shearing at points midway

* The experiment as described is an exceedingly troublesome one, and no attempt was made to exhibit it at the meeting. The difficulty arises from the fact that, in order to show the effect with certainty, a very perfect balance must be obtained, and this alone generally occupies a great deal of time. But even when two exactly equipotential points have been found, it often happens that the mere reversal of the polarity of the magnet disturbs the adjustment of the galvanometer-contacts to such an extent that the effect cannot be observed: more time must therefore be consumed in readjusting the contacts before a successful experiment can be made. Believing this inconvenience to be due to small displacements resulting from the attractive action of the magnet upon the iron, I have since tried the experiment with gold-foil. This turns out to be a much more suitable metal for the purpose, rendering it possible to observe the effect with ease and certainty.

between the ends of the plate; and as regards transverse distortion, the central bridge merely forms a link between the upper and lower portions of the plate, which link shares their movement in the upward direction without impeding it. Thus the reversal of the Hall effect is easily and completely accounted for.

The 'Philosophical Magazine' for last January contains a translation of a paper by Prof. Righi, on Hall's Phenomenon. He uses three electrodes instead of four. The current enters or leaves by one, and leaves or enters by the other two. The two partial currents pass through a "Wiedemann's" (presumably a differential) galvanometer, and zero is obtained by the use of resistance-coils. On exciting the electromagnet a galvanometer-deflection occurs, indicating a current in the opposite direction to that named by Hall.

This also can be explained by strain and Peltier action. Let us suppose, as before, that the plate is of iron, and that the south pole is beneath it. Let a current enter the plate at A, fig. 6, and leave it at the two points D and E by the wires leading to the differential galvanometer G. The plate is divided into six strained regions exactly as in Hall's arrangement, those which are compressed being marked C, and those which are stretched S. But the neighbourhood of the upper electrode D will, in this case, be *heated*, because the current is passing from stretched to compressed: the resistance of the region will therefore be increased. Similarly the resistance of the metal in the neighbourhood of the lower electrode F will be diminished. Thus a greater quantity of electricity will pass by E than by D, and the galvanometer-deflection will be in the opposite direction to that which would have occurred if the connexions had been made in the middle of the plate, in the usual manner. Righi's experiment therefore confirms my explanation of the Hall effect.

The theory which I have ventured to submit seems to me capable of bearing any test which could reasonably be applied to it. Not only does it completely account for the phenomena described by Hall, but it explains anomalies and enables one to predict results. The chief point of difficulty lies in the strain. In conversation on the subject, I have found that, while it is readily admitted that a strain such as is described

might well be produced in a piece of metal a millimetre in thickness, it is by no means so easy to conceive the possibility of a similar strain in a sheet the thickness of which does not exceed a hundredth or a thousandth of a millimetre. But let it be imagined that the thick piece of metal is divided into a hundred or a thousand independent layers, each of which is acted upon by a force equal to a hundredth or a thousandth part of that which acted upon the whole. Then it becomes evident that each individual stratum, including the bottom one, will be affected in precisely the same manner as the sum of them which constitutes the thick plate. Of course, if the film of metal is so exceedingly thin that its physical properties of cohesion and elasticity are impaired, we should expect the effects of extension and compression to be diminished. This is, in fact, the case with thin gold-leaf, not so much on account of its mere thinness as of the inequalities and irregularities necessarily existing in it; and accordingly we find it noted by Mr. Hall (Phil. Mag. November 1880, p. 312) that the transverse effect in gold-leaf appears to be *relatively* much smaller than the effect in strips of sensible thickness*. *Cæteris paribus*, the galvanometer-deflection is, of course, greater with a thin sheet than with a thick one, because the difference of potential at any two points in a thick sheet must always be comparatively small.

If it is objected that, since the plate is firmly cemented to glass, the distortion actually produced must be very small, it may be replied that the consequent effect is also very small, and that a distortion far too insignificant to be visible to the eye would be sufficient to produce it. The warmth communicated to the plate by lightly touching opposite corners of it with the fingers is capable of causing galvanometer-deflections many times as great as those produced by Peltier effects requiring three or four amperes of current for their production.

Lastly, it may be urged that the force available for producing the required strain is a very small one. This is

* Mr. Hall thinks that the apparent difference may be accounted for by the existence of a multitude of small holes in the thin leaf, by impurities in the gold, and by the difficulty of securing good contact with the electrodes.

undoubtedly true. But, apart from the fact that a very small force would be sufficient to cause a sensible strain in a very thin sheet of metal, it must be remembered that the force with which we are dealing acts in an exceedingly advantageous manner. It does not, like an external push or pull, act upon a single point or group of points in the sheet, but it exerts a perfectly independent action upon every individual molecule of the metal which is within the magnetic field, most powerfully in the middle, gradually falling off towards the ends. It acts, in short, in the most effective manner possible for the production of the strain.

Though it may not impossibly turn out, on further investigation, that the details of the action differ in some respects from those which I have suggested, I think no reasonable doubt can remain that Hall's phenomenon is simply a consequence of mechanical action combined with Peltier effects. And if it should be thought that I have, in this paper, brought forward a superfluity of argument in support of a proposition which, when once stated, appears almost self-evident, I can only say that, under the circumstances, and having regard to the great importance of the subject, it seemed desirable that it should receive the most thorough and exhaustive treatment possible.

III. *On the Adjustment of Resistance-Coils.* By Professor SILVANUS P. THOMPSON, *B.A., D.Sc., Univ. Coll. Bristol*.*

1. ALTHOUGH the existing methods for the exact measurement of the electric resistance of a wire are simple, rapid, and reliable to a very high degree, there is not, so far as I am aware, any simple, rapid, and reliable method yet known of adjusting resistance-coils to their exact value, when some standard coil of known resistance is used for comparison. It is, of course, much more important to know what the exact value of a coil is than to have that coil adjusted to one ohm or one hundred ohms, if such adjustment involves some uncertainty in the percentage of its precise value. But I do not see why our standards should not only be accurately

* Read February 23, 1884.