Experimental researches in electricity (2nd edition) by Michael Faraday,...

EXPERIMENTAL RESEARCHES

IN

ELECTRICITY.

VOL. III.

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BY

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ALERE FLAMMAM.

PREFACE.

For reasons stated in the First Volume of these Experimental Researches, I have been induced to gather the remaining Series together, and to add to them certain other papers devoted to Electrical and Magnetic Research.

To the prefatory remarks containing these reasons, I would recall the recollection of those who may honour these Researches with any further attention. I have printed the papers in this Volume, as before, with little or no alteration, except that I have placed the fair and just date of each at the top of the pages.

As regards magnecrystallic action, which commences at Paragraph 2454, the reader will see the gradual change and enlargement of view respecting its nature in the course of long investigations at the following places, 2550. 2562. 2576. 2584. &c., 2591. 2639. 2797. 2818. 2836. &c. I would refer readers to the paper by Tyndall and Knoblauch in the Philosophical Magazine, 1850, vol. xxxvii. p. 1, for a very philosophical account of the physical cause of the magnecrystallic action*, and to the paper by Professor W. Thomson on the theory

* Marchand and Scheerer say that bismuth is expanded by pressure and has its structure changed. Gmelin's Handbook, iv. p. 428.

of magnetic induction in crystalline and non-crystalline substances in the Philosophical Magazine, 1851, vol. i. p. 177, as being in all parts in perfect accordance with the various experimental results which I have at different times obtained.

With respect to Paragraphs 2967. 3242, and the intentions there expressed of experimenting with oxygen at low temperatures, I have endeavoured to carry these intentions out; but the extreme difficulty of working on such attenuated matter as gases at low temperatures, without the production of air-currents able to influence the very delicate torsion-balance and apparatus required to measure the result, is so great as to have prevented me as yet from obtaining any results worthy of confidence.

I owe many thanks to the Royal Society and to the Proprietors of the Philosophical Magazine, for the great kindness I have received in the loan of plates, &c., and in other facilities granted to me for the printing of the volume.

As the Index belongs both to the Experimental Researches and to the other papers, its references are of necessity made in two ways; those to the Researches are, as before, to the numbers of the paragraphs, and are easily recognized by the greatness of the numbers: the other references are to the pages, and being always preceded by *p.* or *pp.*, are known by that mark.

MICHAEL FARADAY.

January, 1855.

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EXPERIMENTAL RESEARCHES

IN

ELECTRICITY.

NINETEENTH SERIES[1].

§ 26. *On the magnetization of light and the illumination of magnetic lines of force*[2]. ¶ i. *Action of magnets on light.* ¶ ii. *Action of electric currents on light.* ¶ iii. *General considerations.*

Received November 6,—Read November 20, 1845.

¶ i. *Action of magnets on light.*

2146. I HAVE long held an opinion, almost amounting to conviction, in common I believe with many other lovers of natural knowledge, that the various forms under which the forces of matter are made manifest have one common origin; or, in other words, are so directly related and mutually dependent, that they are convertible, as it were, one into another, and possess equi-

[1] Philosophical Transactions, 1846, p. 1.

[2] The title of this paper has, I understand, led many to a misapprehension of its contents, and I therefore take the liberty of appending this explanatory note. Neither accepting nor rejecting the hypothesis of an æther, or the corpuscular, or any other view that may be entertained of the nature of light; and, as far as I can see, nothing being really known of a ray of light more than of a line of magnetic or electric force, or even of a line of gravitating force, except as it and they are manifest in and by substances; I believe that, in the experiments I describe in the paper, light has been magnetically affected, *i. e.* that that which is magnetic in the forces of matter has been affected, and in turn has affected that which is truly magnetic in the force of light: by the term magnetic I include here either of the peculiar exertions of the power of a magnet, whether it be that which is manifest in the magnetic or the diamagnetic class of bodies. The phrase "illumination of the lines of

valents of power in their action[1]. In modern times the proofs of their convertibility have been accumulated to a very considerable extent, and a commencement made of the determination of their equivalent forces.

2147. This strong persuasion extended to the powers of light, and led, on a former occasion, to many exertions, having for their object the discovery of the direct relation of light and electricity, and their mutual action in bodies subject jointly to their power[2]; but the results were negative and were afterwards confirmed, in that respect, by Wartmann[3].

2148. These ineffectual exertions, and many others which were never published, could not remove my strong persuasion derived from philosophical considerations; and, therefore, I recently resumed the inquiry by experiment in a most strict and searching manner, and have at last succeeded in *magnetizing and electrifying a ray of light, and in illuminating a magnetic line of force.* These results, without entering into the detail of many unproductive experiments, I will describe as briefly and clearly as I can.

2149. But before I proceed to them, I will define the meaning I connect with certain terms which I shall have occasion to use:—thus, by *line of magnetic force,* or *magnetic line of force,* or *magnetic curve,* I mean that exercise of magnetic force which is exerted in the lines usually called magnetic curves, and which equally exist as passing from or to magnetic poles, or forming concentric circles round an electric current. By *line of electric force,* I mean the force exerted in the lines joining two bodies, acting on each other according to the principles of static electric induction (1161, &c.), which may also be either in curved or

magnetic force" has been understood to imply that I had rendered them luminous. This was not within my thought. I intended to express that the line of magnetic force was illuminated as the earth is illuminated by the sun, or the spider's web illuminated by the astronomer's lamp. Employing a ray of light, we can tell, *by the eye,* the direction of the magnetic lines through a body; and by the alteration of the ray and its optical effect on the eye, can see the course of the lines just as we can see the course of a thread of glass, or any other transparent substance, rendered visible by the light: and this is what I meant by *illumination,* as the paper fully explains.—December 15, 1845. M. F.

[1] Experimental Researches, 57, 366, 376, 877, 961, 2071.

[2] Philosophical Transactions, 1834. Experimental Researches, 951-955.

[3] Archives de l'Electricité, ii. pp. 596-600.

straight lines. By a *diamagnetic*, I mean a body through which lnes of magnetic force are passing, and which does not by their action assume the usual magnetic state of iron or loadstone.

2150. A ray of light issuing from an Argand lamp, was polarized in a horizontal plane by reflexion from a surface of glass, and the polarized ray passed through a Nichol's eye-piece revolving on a horizontal axis, so as to be easily examined by the latter. Between the polarizing mirror and the eye-piece two powerful electro-magnetic poles were arranged, being either the poles of a horse-shoe magnet, or the contrary poles of two cylinder magnets; they were separated from each other about 2 inches in the direction of the line of the ray, and so placed, that, if on the same side of the polarized ray, it might pass near them; or if on contrary sides, it might go between them, its direction being always parallel, or nearly so, to the magnetic lines of force (2149.). After that, any transparent substance placed between the two poles, would have passing through it, both the polarized ray and the magnetic lines of force at the same time and in the same direction.

2151. Sixteen years ago I published certain experiments made upon optical glass[1], and described the formation and general characters of one variety of heavy glass, which, from its materials, was called silicated borate of lead. It was this glass which first gave me the discovery of the relation between light and magnetism, and it has power to illustrate it in a degree beyond that of any other body; for the sake of perspicuity I will first describe the phænomena as presented by this substance.

2152. A piece of this glass, about 2 inches square and 0·5 of an inch thick, having flat and polished edges, was placed as a *diamagnetic* (2149.) between the poles (not as yet magnetized by the electric current), so that the polarized ray should pass through its length; the glass acted as air, water, or any other indifferent substance would do; and if the eye-piece were pre-

[1] Philosophical Transactions, 1830, p. 1. I cannot resist the occasion which is thus offered to me of mentioning the name of Mr. Anderson, who came to me as an assistant in the glass experiments, and has remained ever since in the Laboratory of the Royal Institution. He has assisted me in all the researches into which I have entered since that time, and to his care, steadiness, exactitude, and faithfulness in the performamce of all that has been committed to his charge, I am much indebted.—M. F.

viously turned into such a position that the polarized ray was extinguished, or rather the image produced by it rendered invisible, then the introduction of this glass made no alteration in that respect. In this state of circumstances the force of the electro-magnet was developed, by sending an electric current through its coils, and immediately the image of the lamp-flame became visible, and continued so as long as the arrangement continued magnetic. On stopping the electric current, and so causing the magnetic force to cease, the light instantly disappeared; these phænomena could be renewed at pleasure, at any instant of time, and upon any occasion, showing a perfect dependence of cause and effect.

2153. The voltaic current which I used upon this occasion, was that of five pair of Grove's construction, and the electro-magnets were of such power that the poles would singly sustain a weight of from twenty-eight to fifty-six, or more, pounds. A person looking for the phænomenon for the first time would not be able to see it with a weak magnet.

2154. The character of the force thus impressed upon the diamagnetic is that of *rotation*; for when the image of the lamp-flame has thus been rendered visible, revolution of the eye-piece to the right or left, more or less, will cause its extinction; and the further motion of the eye-piece to the one side or other of this position will produce the reappearance of the light, and that with complementary tints, according as this further motion is to the right- or left-hand.

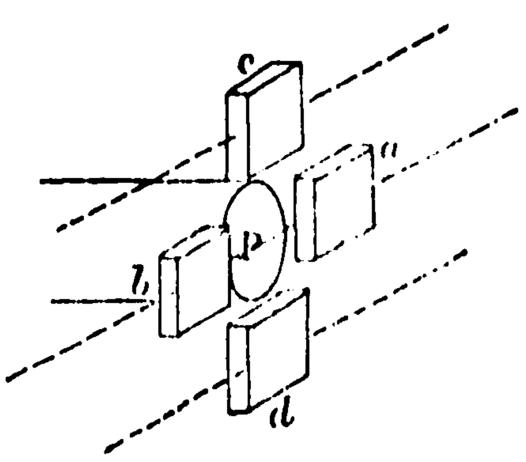
2155. When the pole nearest to the observer was a marked pole, *i. e.* the same as the north end of a magnetic needle, and the further pole was unmarked, the rotation of the ray was right-handed; for the eye-piece had to be turned to the right-hand, or clock fashion, to overtake the ray and restore the image to its first condition. When the poles were reversed, which was instantly done by changing the direction of the electric current, the rotation was changed also and became left-handed, the alteration being to an equal degree in extent as before. The direction was always the same for the same *line of magnetic force* (2149.).

2156. When the diamagnetic was placed in the numerous other positions, which can easily be conceived, about the magnetic poles, results were obtained more or less marked in extent, and very definite in character, but of which the phænomena just described may be considered as the chief example: they will be referred to, as far as is necessary, hereafter.

2157. The same phænomena were produced in the silicated borate of lead (2151.) by the action of a good ordinary steel horse-shoe magnet, no electric current being now used. The results were feeble, but still sufficient to show the perfect identity of action between electro-magnets and common magnets in this their power over light.

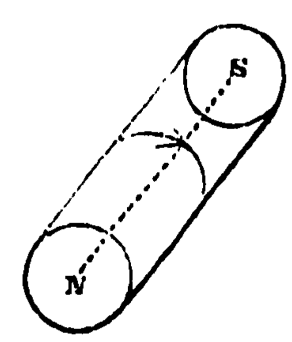
2158. Two magnetic poles were employed end-ways, *i. e.* the cores of the electro-magnets were hollow iron cylinders, and the ray of polarized light passed along their axes and through the diamagnetic placed between them: the effect was the same.

2159. One magnetic pole only was used, that being one end of a powerful cylinder electro-magnet. When the heavy glass was beyond the magnet, being close to it but between the magnet and the polarizing reflector, the rotation was in one direction, dependent on the nature of the pole; when the diamagnetic was on the near side, being close to it but between it and the eye, the rotation for the same pole was in the contrary direction to what it was before; and when the magnetic pole was changed, both these directions were changed with it. When the heavy glass was placed in a corresponding position to the pole, but above or below it, so that the *magnetic curves* were no longer passing through the glass parallel to the ray of polarized light, but rather perpendicular to it, then no effect was produced. These particularities may be understood by reference to fig. 1, where *a* and *b* represent the first positions of the diamagnetic, and *c* and *d* the latter positions, the course of the ray being marked by the dotted line. If also the glass were placed directly at the end of the magnet, then no effect was produced on a ray passing in the direction here described, though it is evident, from what has been already said (2155.), that a ray passing *parallel* to the magnetic lines through the glass so placed, would have been affected by it.



2160. Magnetic lines, then, in passing through silicated borate of lead, and a great number of other substances (2173.), cause these bodies to act upon a polarized ray of light when the lines are parallel to the ray, or in proportion as they are parallel to it: if they are perpendicular to the ray, they have no action upon it. They give the diamagnetic the power of rotating the ray; and the *law* of this action on light is, that if a magnetic line of force be *going from* a north pole, or *coming* from a south pole, along the path of a polarized ray coming to the observer, it will rotate that ray to the right-hand; or, that if such a line of force be coming from a north pole, or going from a south pole, it will rotate such a ray to the left-hand.

2161. If a cork or a cylinder of glass, representing the diamagnetic, be marked at its ends with the letters N and S, to represent the poles of a magnet, the line joining these letters may be considered as a magnetic line of force; and further, if a line be traced round the cylinder with arrow heads on it to represent direction, as in the figure, such a simple model, held up before the eye, will express the whole of the law, and give every position and consequence of direction resulting from it. If a watch be considered as the diamagnetic, the north pole of a magnet being imagined against the face, and a south pole against the back, then the motion of the hand will indicate the direction of rotation which a ray of light undergoes by magnetization.



2162. I will now proceed to the different circumstances which affect, limit, and define the extent and nature of this new power of action on light.

2163. In the first place, the rotation appears to be in proportion to the extent of the diamagnetic through which the ray and the magnetic lines pass. I preserved the strength of the magnet and the interval between its poles constant, and then interposed different pieces of the same heavy glass (2151.) between the poles. The greater the extent of the diamagnetic in the line of the ray, whether in one, two, or three pieces, the greater was the rotation of the ray; and, as far as I could judge by these first experiments, the amount of rotation was exactly proportionate to the extent of diamagnetic through which the ray passed. No addition or diminution of the heavy glass on the *side* of the course of the ray made any difference in the effect of that part through which the ray passed.

2164. The power of rotating the ray of light *increased* with the intensity of the magnetic lines of force. This general effect is very easily ascertained by the use of electro-magnets; and within such range of power as I have employed, it appears to be directly proportionate to the intensity of the magnetic force.

2165. Other bodies, besides the heavy glass, possess the same power of becoming, under the influence of magnetic force, active on light (2173.). When these bodies possess a rotative power of their own, as is the case with oil of turpentine, sugar, tartaric acid, tartrates, &c., the effect of the magnetic force is to add to, or subtract from, their specific force, according as the natural rotation and that induced by the magnetism is right- or left-handed (2231.).

2166. I could not perceive that this power was affected by any degree of motion which I was able to communicate to the diamagnetic, whilst jointly subject to the action of the magnetism and the light.

2167. The interposition of copper, lead, tin, silver, and other ordinary non-magnetic bodies in the course of the magnetic curves, either between the pole and the diamagnetic, or in other positions, produced no effect either in kind or degree upon the phænomena.

2168. Iron frequently affected the results in a very considerable degree; but it always appeared to be, either by altering the direction of the magnetic lines, or disposing within itself of their force. Thus when the two contrary poles were on one side of the polarized ray (2150.), and the heavy glass in its best position between them and in the ray (2152.), the bringing of a large piece of iron near to the glass on the other side of the ray, caused the power of the diamagnetic to fall. This was because certain lines of magnetic force, which at first passed through the glass parallel to the ray, now crossed the glass and the ray; the iron giving two contrary poles opposite the poles of the magnet, and thus determining a new course for a certain portion of the magnetic power, and that across the polarized ray.

2169. Or, if the iron, instead of being applied on the opposite side of the glass, were applied on the same side with the magnet, either near it or in contact with it, then, again, the power of the diamagnetic fell, simply because the power of the magnet was diverted from it into a new direction. These effects depend much of course on the intensity and power of the magnet, and on the size and softness of the iron.

2170. The electro-helices (2190.) without the iron cores were very feeble in power, and indeed hardly sensible in their effect. With the iron cores they were powerful, though no more electricity was then passing through the coils than before (1071.). This shows, in a very simple manner, that the phænomena exhibited by light under these circumstances, is directly connected with the magnetic form of force supplied by the arrangement. Another effect which occurred illustrated the same point. When the contact at the voltaic battery is made, and the current sent round the electro-magnet, the image produced by the rotation of the polarized ray does not rise up to its full lustre immediately, but increases for a couple of seconds, gradually acquiring its greatest intensity; on breaking the contact, it sinks instantly and disappears apparently at once. The gradual rise in brightness is due to the *time* which the iron core of the magnet requires to evolve all that magnetic power which the electric current can develope in it; and as the magnetism rises in intensity, so does its effect on the light increase in power; hence the progressive condition of the rotation.

2171. I cannot as yet find that the heavy glass (2151.), when in this state, *i. e.* with magnetic lines of force passing through it, exhibits any increased degree, or has any specific magneto-inductive action of the recognized kind. I have placed it in large quantities, and in different positions, between magnets and magnetic needles, having at the time very delicate means of appreciating any difference between it and air, but could find none.

2172. Using water, alcohol, mercury, and other fluids contained in very large delicate thermometer-shaped vessels, I could not discover that any difference in volume occurred when the magnetic curves passed through them.

2173. It is time that I should pass to a consideration of this power of magnetism over light as exercised, not only in the silicated borate of lead (2151.), but in many other substances; and here we perceive, in the first place, that if all transparent bodies possess the power of exhibiting the action, they have it in very different degrees, and that up to this time there are some that have not shown it at all.

2174. Next, we may observe, that bodies that are exceedingly different to each other in chemical, physical, and mechanical properties, develope this effect; for solids and liquids, acids, alkalies, oils, water, alcohol, æther, all possess the power.

2175. And lastly, we may observe, that in all of them, though the degree of action may differ, still it is always the same in kind, being a rotative power over the ray of light; and further, the direction of the rotation is, in every case, independent of the nature or state of the substance, and dependent upon the direction of the magnetic line of force, according to the law before laid down (2160.).

2176. Amongst the substances in which this power of action is found, I have already distinguished the *silico-borate of lead* (2151.) as eminently fitted for the purpose of exhibiting the phænomena. I regret that it should be the best, since it is not likely to be in the possession of many, and few will be induced to take the trouble of preparing it. If made, it should be well annealed, for otherwise the pieces will have considerable power of depolarizing light, and then the particular phænomena under consideration are much less strikingly observed. The *borate of lead*, however, is a substance much more fusible, softening at the heat of boiling oil, and therefore far more easily prepared in the form of glass plates and annealed; and it possesses as much magneto-rotative power over light as the silico-borate itself. *Flint-glass* exhibits the property, but in a less degree than the substances above. Crown-glass shows it, but in a still smaller degree.

2177. Whilst employing crystalline bodies as diamagnetics, I generally gave them that position in which they did not affect the polarized ray, and then induced the magnetic curves through them. As a class, they seemed to resist the assumption of the rotating state. *Rock-salt* and *fluor-spar* gave evidence of the power in a slight degree; and I think that a crystal of alum did the same, but its ray length in the transparent part was so small that I could not ascertain the fact decisively. Two specimens of transparent fluor, lent me by Mr. Tennant, gave the effect.

2178. Rock-crystal, 4 inches across, gave no indications of action on the ray, neither did smaller crystals, nor cubes about three-fourths of an inch in the side, which were so cut as to have two of their faces perpendicular to the axis of the crystal (1692, 1693.), though they were examined in every direction.

2179. *Iceland spar* exhibited no signs of effect, either in the form of rhomboids, or of cubes like those just described (1695.).

2180. *Sulphate of baryta, sulphate of lime,* and *carbonate of soda,* were also without action on the light.

2181. A piece of fine clear *ice* gave me no effect. I cannot however say there is none, for the effect of water in the same mass would be very small, and the irregularity of the flattened surface from the fusion of the ice and flow of water, made the observation very difficult.

2182. With some degree of curiosity and hope, I put gold-leaf into the magnetic lines, but could perceive no effect. Considering the extremely small dimensions of the length of the path of the polarized ray in it, any positive result was hardly to be expected.

2183. In experiments with liquids, a very good method of observing the effect, is to enclose them in bottles from 1½ to 3 or 4 inches in diameter, placing these in succession between the magnetic poles (2150.), and bringing the analysing eye-piece so near to the bottle, that, by adjustment of the latter, its cylindrical form may cause a diffuse but useful image of the lamp-flame to be seen through it: the light of this image is easily distinguished from that which passes by irregular refraction through the striæ and deformations of the glass, and the phænomena being looked for in this light are easily seen.

2184. Water, alcohol, and æther, all show the effect; water most, alcohol less, and æther the least. All the fixed oils which I have tried, including almond, castor, olive, poppy, linseed, sperm, elaine from hog's lard, and distilled resin oil, produce it. The essential oils of turpentine, bitter almonds, spike lavender, lavender, jessamine, cloves, and laurel, produce it. Also naphtha of various kinds, melted spermaceti, fused sulphur, chloride of sulphur, chloride of arsenic, and every other liquid substance which I had at hand and could submit in sufficient bulk to experiment.

2185. Of aqueous solutions I tried 150 or more, including the soluble acids, alkalies and salts, with sugar, gum, &c., the list of which would be too long to give here, since the great conclusion was, that the exceeding diversity of substance caused no exception to the general result, for all the bodies showed the property. It is indeed more than probable, that in all these cases the water and not the other substance present was the ruling matter. The same general result was obtained with alcoholic solutions.

2186. Proceeding from liquids to air and gaseous bodies, I have here to state that, as yet, I have not been able to detect the exercise of this power in any one of the substances in this class. I have tried the experiment with bottles 4 inches in diameter, and the following gases: oxygen, nitrogen, hydrogen, nitrous oxide, olefiant gas, sulphurous acid, muriatic acid, carbonic acid, carbonic oxide, ammonia, sulphuretted hydrogen, and bromine vapour, at ordinary temperatures; but they all gave negative results. With air, the trial has been carried, by another form of apparatus, to a much higher degree, but still ineffectually (2212.).

2187. Before dismissing the consideration of the substances which exhibited this power, and in reference to those in which it was superinduced upon bodies possessing, naturally, rotative force (2165. 2231.), I may record, that the following are the substances submitted to experiment: castor oil, resin oil, oil of spike lavender, of laurel, Canada balsam, alcoholic solution of camphor, alcoholic solution of camphor and corrosive sublimate, aqueous solutions of sugar, tartaric acid, tartrate of soda, tartrate of potassa and antimony, tartaric and boracic acid, and sulphate of nickel, which rotated to the right-hand; copaiba balsam, which rotated the ray to the left-hand; and two specimens of camphine or oil of turpentine, in one of which the rotation was to the right-hand, and in the other to the left. In all these cases, as already said (2165.), the superinduced magnetic rotation was according to the general law (2160.), and without reference to the previous power of the body.

2188. Camphor being melted in a tube about an inch in diameter, exhibited high natural rotative force, but I could not discover that the magnetic curves induced additional force in it. It may be, however, that the shortness of the ray length and the quantity of coloured light left, even when the eye-piece was adjusted to the most favourable position for darkening the image produced by the naturally rotated ray, rendered the small magneto-power of the camphor insensible.

¶ ii. *Action of electric currents on light.*

2189. From a consideration of the nature and position of the lines of magnetic and electric force, and the relation of a magnet to a current of electricity, it appeared almost certain that an electric current would give the same result of action on light as a magnet; and, in the helix, would supply a form of apparatus in which great lengths of diamagnetics, and especially of such bodies as appeared to be but little affected between the poles of the magnet, might be submitted to examination and their effect exalted: this expectation was, by experiment, realized.

2190. Helices of copper wire were employed, three of which I will refer to. The first, or *long helix*, was 0·4 of an inch internal diameter; the wire was 0·03 of an inch in diameter, and having gone round the axis from one end of the helix to the other, then returned in the same manner, forming a coil 65 inches long, double in its whole extent, and containing 1240 feet of wire.

2191. The second, or *medium helix*, is 19 inches long, 1·87 inch internal diameter, and 3 inches external diameter. The wire is 0·2 of an inch in diameter, and 80 feet in length, being disposed in the coil as two concentric spirals. The electric current, in passing through it, is not divided, but traverses the whole length of the wire.

2192. The third, or *Woolwich helix*, was made under my instruction for the use of Lieut.-Colonel Sabine's establishment at Woolwich. It is 26·5 inches long, 2·5 inches internal diameter, and 4·75 inches external diameter. The wire is 0·17 of an inch in diameter, and 501 feet in length. It is disposed in the coil in four concentric spirals connected end to end, so that the whole of the electric current employed passes through all the wire.

2193. The long helix (2190.) acted very feebly on a magnetic needle placed at a little distance from it; the medium helix (2191.) acted more powerfully, and the Woolwich helix (2192.) very strongly; the same battery of ten pairs of Grove's plate being employed in all cases.

2194. Solid bodies were easily subjected to the action of these electro-helices, being for that purpose merely cut into the form of bars or prisms with flat and polished ends, and then introduced as cores into the helices. For the purpose of submitting liquid bodies to the same action, tubes of glass were provided, furnished at the ends with caps; the cylindrical part of the cap was brass, and had a tubular aperture for the introduction of the liquids, but the end was a flat glass plate. When the tube was intended to contain aqueous fluids, the plates were attached to the caps, and the caps to the tube by Canada balsam; when the tube had to contain alcohol, æther or essential oils, a thick mixture of powdered gum with a little water was employed as the cement.

2195. The general effect produced by this form of apparatus may be stated as follows:—The tube within the long helix (2190.) was filled with distilled water and placed in the line of the polarized ray, so that by examination through the eye-piece (2150.), the image of the lamp-flame produced by the ray could be seen through it. Then the eye-piece was turned until the image of the flame disappeared, and, afterwards, the current of ten pairs of plates sent through the helix; instantly the image of the flame reappeared, and continued as long as the electric current was passing through the helix; on stopping the current the image disappeared. The light did not rise up gradually, as in the case of electro-magnets (2170.), but instantly. These results could be produced at pleasure. In this experiment we may, I think, justly say that a ray of light is electrified and the electric forces illuminated.

2196. The phænomena may be made more striking, by the adjustment of a lens of long focus between the tube and the polarizing mirror, or one of short focus between the tube and the eye; and where the helix, or the battery, or the substance experimented with, is feeble in power, such means offer assistance in working out the effects: but after a little experience, they are easily dispensed with, and are only useful as accessories in doubtful cases.

2197. In cases where the effect is feeble, it is more easily perceived if the Nichol eye-piece be adjusted, not to the perfect extinction of the ray, but a little short of or beyond that position; so that the image of the flame may be but just visible. Then, on the exertion of the power of the electric current, the light is either increased in intensity, or else diminished, or extinguished, or even re-illuminated on the other side of the dark condition;

and this change is more easily perceived than if the eye began to observe from a state of utter darkness. Such a mode of observing also assists in demonstrating the rotatory character of the action on light; for, if the light be made visible beforehand by the motion of the eye-piece in one direction, and the power of the current be to *increase* that light, an instant only suffices, after stopping the current, to move the eye-piece in the other direction until the light is apparent as at first, and then the power of the current will be to *diminish* it; the tints of the lights being affected also at the same time.

2198. When the current was sent round the helix in one direction, the rotation induced upon the ray of light was one way; and when the current was changed to the contrary direction, the rotation was the other way. In order to express the direction, I will assume, as is usually done, that the current passes from the zinc through the acid to the platinum in the same cell (663. 667. 1627.): if such a current pass under the ray towards the right, upwards on its right side, and over the ray towards the left, it will give left-handed rotation to it; or, if the current pass over the ray to the right, down on the right side, and under it towards the left, it will induce it to rotate to the right-hand.

2199. The LAW, therefore, by which an electric current acts on a ray of light is easily expressed. When an electric current passes round a ray of polarized light in a plane perpendicular to the ray, it causes the ray to revolve on its axis, as long as it is under the influence of the current, in the *same direction* as that in which the current is passing.

2200. The simplicity of this law, and its identity with that given before, as expressing the action of magnetism on light (2160.), is very beautiful. A model is not wanted to assist the memory; but if that already described (2161.) be looked at, the line round it will express at the same time the direction both of the current and the rotation. It will indeed do much more; for if the cylinder be considered as a piece of iron, and not a piece of glass or other diamagnetic, placed between the two poles N and S, then the line round it will represent the direction of the currents, which, according to Ampère's theory, are moving round its particles; or if it be considered as a core of iron (in place of a core of water), having an electric current running round it in the direction of the line, it will also represent such a magnet as would be formed if it were placed between the poles whose marks are affixed to its ends.

2201. I will now notice certain points respecting the degree of this action under different circumstances. By using a tube of water (2194.) as long as the helix, but placing it so that more or less of the tube projected at either end of the helix, I was able, in some degree, to ascertain the effect of length of the diamagnetic, the force of the helix and current remaining the same. The greater the column of water subjected to the action of the helix, the greater was the rotation of the polarized ray; and the amount of rotation seemed to be directly proportionate to the length of fluid round which the electric current passed.

2202. A short tube of water, or a piece of heavy glass, being placed in the axis of the Woolwich helix (2192.), seemed to produce equal effect on the ray of light, whether it were in the middle of the helix or at either end; provided it was always within the helix and in the line of the axis. From this it would appear that every part of the helix has the same effect; and, that by using long helices, substances may be submitted to this kind of examination which could not be placed in sufficient length between the poles of magnets (2150.).

2203. A tube of water as long as the Woolwich helix (2192.), but only 0·4 of an inch in diameter, was placed in the helix parallel to the axis, but sometimes in the axis and sometimes near the side. No apparent difference was produced in these different situations; and I am inclined to believe (without being quite sure) that the action on the ray is the same, wherever the tube is placed, within the helix, in relation to the axis. The same result was obtained when a larger tube of water was looked through, whether the ray passed through the axis of the helix and tube, or near the side.

2204. If bodies be introduced into the helix possessing, naturally, rotating force, then the rotating power given by the electric current is superinduced upon them, exactly as in the cases already described of magnetic action (2165. 2187.).

2205. A helix, 20 inches long and 0·3 of an inch in diameter, was made of uncovered copper wire, 0·05 of an inch in diameter, in close spirals. This was placed in a large tube of water, so that the fluid, both in the inside and at the outside of the helix, could be examined by the polarized ray. When the current was sent *through* the helix, the water within it received rotating power; but no trace of such an action on the light was seen on the outside of the helix, even in the line most close to the uncovered wire.

2206. The water was enclosed in brass and copper tubes, but this alteration caused not the slightest change in the effect.

2207. The water in the brass tube was put into an *iron* tube, much longer than either the Woolwich helix or the brass tube, and quite one-eighth of an inch thick in the side; yet when placed in the Woolwich helix (2192.), the water rotated the ray of light apparently as well as before.

2208. An iron bar, 1 inch square and longer than the helix, was put into the helix, and the small water tube (2203.) upon it. The water exerted as much action on the light as before.

2209. Three iron tubes, each 27 inches long and one-eighth of an inch in thickness in the side, were selected of such diameters as to pass easily one into the other, and the whole into the Woolwich helix (2192.). The smaller one was supplied with glass ends and filled with water; and being placed in the axis of the Woolwich helix, had a certain amount of rotating power over the polarized ray. The second tube was then placed over this, so that there was now a thickness of iron equal to two-eighths of an inch between the water and the helix; the water had *more* power of rotation than before. On placing the third tube of iron over the two former, the power of the water *fell*, but was still very considerable. These results are complicated, being dependent on the new condition which the character of iron gives to its action on the forces. Up to a certain amount, by increasing the development of magnetic forces, the helix and core, *as a whole*, produce increased action on the water; but on the addition of more iron and the disposal of the forces through it, their action is removed in part from the water and the rotation is lessened.

2210. Pieces of heavy glass (2151.), placed in iron tubes in the helices, produced similar effects.

2211. The bodies which were submitted to the action of an electric current in a helix, in the manner already described, were as follows:—Heavy glass (2151. 2176.), water, solution of sulphate of soda, solution of tartaric acid, alcohol, æther, and oil of turpentine; all of which were affected, and acted on light exactly in the manner described in relation to magnetic action (2173.).

2212. I submitted *air* to the influence of these helices carefully and anxiously, but could not discover any trace of action on the polarized ray of light. I put the long helix (2190.) into the other two (2191. 2192.), and combined them all into one consistent series, so as to accumulate power, but could not observe any effect of them on light passing through air.

2213. In the use of helices, it is necessary to be aware of one effect, which might otherwise cause confusion and trouble. At first, the wire of the long helix (2190.) was wound directly upon the thin glass tube which served to contain the fluid. When the electric current passed through the helix it raised the temperature of the metal, and that gradually raised the temperature of the glass and the film of water in contact with it, and so the cylinder of water, warmer at its surface than its axis, acted as a lens, gathering and sending rays of light to the eye, and continuing to act for a time after the current was stopped. By separating the tube of water from the helix, and by other precautions, this source of confusion is easily avoided.

2214. Another point of which the experimenter should be aware, is the difficulty, and almost impossibility, of obtaining a piece of glass which, especially after it is cut, does not depolarize light. When it does depolarize, difference of position makes an immense difference in the appearance. By always referring to the parts that do not depolarize, as the black cross, for instance, and by bringing the eye as near as may be to the glass, this difficulty is more or less overcome.

2215. For the sake of supplying a general indication of the amount of this induced rotating force in two or three bodies, and without any pretence of offering correct numbers, I will give, generally, the result of a few attempts to measure the force, and compare it with the natural power of a specimen of oil of turpentine. A very powerful electro-magnet was employed, with a *constant* distance between its poles of $2\frac{1}{2}$ inches. In this space was placed different substances; the amount of rotation of the eye-piece observed several times and the average taken, as expressing the rotation for the ray length of substance used.

But as the substances were of different dimensions, the ray lengths were, by calculation, corrected to one standard length, upon the assumption that the power was proportionate to this length (2163.). The oil of turpentine was of course observed in its natural state, i. e. without magnetic action. Making water 1, the numbers were as follows:—

Oil of turpentine	11·8
Heavy glass (2151.) . . .	6·0
Flint-glass	2·8
Rock-salt	2·2
Water	1·0
Alcohol	less than water.
Æther	less than alcohol.

2216. In relation to the action of magnetic and electric forces on light, I consider, that to know the conditions under which there is no apparent action, is to add to our knowledge of their mutual relations; and will, therefore, very briefly state how I have lately combined these forces, obtaining no apparent result (955.).

2217. Heavy glass, flint-glass, rock crystal, Iceland spar, oil of turpentine, and air, had a polarized ray passed through them; and, at the same time, lines of electro-static tension (2149.) were, by means of coatings, the Leyden jar, and the electric machine, directed across the bodies, parallel to the polarized ray, and perpendicular to it, both in and across the plane of polarization; but without any visible effect. The tension of a rapidly recurring, induced secondary current, was also directed upon the same bodies and upon water (as an electrolyte), but with the same negative result.

2218. A polarized ray, powerful magnetic lines of force, and the electric lines of force (2149.) just described, were combined in various directions in their action on heavy glass (2151. 2176.), but with no other result than that due to the mutual action of the magnetic lines of light, already described in this paper.

2219. A polarized ray and electric currents were combined in every possible way in electrolytes (951–954). The substances used were distilled water, solution of sugar, dilute sulphuric acid, solution of sulphate of soda, using platinum electrodes; and solution of sulphate of copper, using copper electrodes: the current was sent along the ray, and perpendicular to it in two directions at right angles with each other; the ray was made to rotate, by altering the position of the polarizing mirror, that the plane of polarization might be varied; the current was used as a continuous current, as a rapidly intermitting current, and as a rapidly alternating double current of induction; but in no case was any trace of action perceived.

2220. Lastly, a ray of polarized light, electric currents, and magnetic lines of force, were directed in every possible way through dilute sulphuric acid and solution of sulphate of soda, but still with negative results, except in those positions where the phænomena already described were produced. In one arrangement, the current passed in the direction of radii from a central to a circumferential electrode, the contrary magnetic poles being placed above and below; and the arrangements were so good, that when the electric current was passing, the fluid rapidly rotated; but a polarized ray sent horizontally across this arrangement was not at all affected. Also, when the ray was sent vertically through it, and the eye-piece moved to correspond to the rotation impressed upon the ray in this position by the magnetic curves alone, the superinduction of the passage of the electric current made not the least difference in the effect upon the ray.

¶ iii. *General considerations.*

2221. Thus is established, I think for the first time[1], a true, direct relation and dependence between light and the magnetic and electric forces; and thus a great addition made to the facts

[1] I say, for the first time, because I do not think that the experiments of Morrichini on the production of magnetism by the rays at the violet end of the spectrum prove any such relation. When in Rome with Sir H. Davy in the month of May 1814, I spent several hours at the house of Morrichini, working with his apparatus and under his directions, but could not succeed in magnetising a needle. I have no confidence in the effect as a *direct* result of the action of the sun's rays; but think, that when it has occurred it has been secondary, incidental, and perhaps even accidental; a result that might well happen with a needle that was preserved during the whole experiment in a north and south position.

January 2, 1846.—I should not have written "for the first time" as above, if I had remembered Mr. Christie's experiments and papers on the Influence of the Solar Rays on Magnets, communicated in the Philosophical Transactions for 1826, p. 219, and 1828, p. 379.—M. F.

and considerations which tend to prove that all natural forces are tied together, and have one common origin (2146.). It is, no doubt, difficult in the present state of our knowledge to express our expectation in exact terms; and, though I have said that another of the powers of nature is, in these experiments, directly related to the rest, I ought, perhaps, rather to say that another form of the great power is distinctly and directly related to the other forms; or, that the great power manifested by particular phænomena in particular forms, is here further identified and recognized, by the direct relation of its form of light to its forms of electricity and magnetism.

2222. The relation existing between *polarized* light and magnetism and electricity, is even more interesting than if it had been shown to exist with common light only. It cannot but extend to common light; and, as it belongs to light made, in a certain respect, more precise in its character and properties by polarization, it collates and connects it with these powers, in that duality of character which they possess, and yields an opening, which before was wanting to us, for the appliance of these powers to the investigation of the nature of this and other radiant agencies.

2223. Referring to the conventional distinction before made (2149.), it may be again stated, that it is the magnetic lines of force *only* which are effectual on the rays of light, and they *only* (in appearance) when parallel to the ray of light, or as they tend to parallelism with it. As, in reference to matter not magnetic after the manner of iron, the phænomena of electric induction and electrolyzation show a vast superiority in the energy with which electric forces can act as compared to magnetic forces, so here, in another direction and in the peculiar and correspondent effects which belong to magnetic forces, they are shown, in turn, to possess great superiority, and to have their full equivalent of action on the same kind of matter.

2224. The magnetic forces do not act on the ray of light directly and without the intervention of matter, but through the mediation of the substance in which they and the ray have a simultaneous existence; the substances and the forces giving to and receiving from each other the power of acting on the light. This is shown by the non-action of a vacuum, of air or gases; and it is also further shown by the special degree in which different matters possess the property. That magnetic force acts upon the ray of light always with the same character of manner and in the same direction, independent of the different varieties of substance, or their states of solid or liquid, or their specific rotative force (2232.), shows that the magnetic force and the light have a direct relation: but that substances are necessary, and that these act in different degrees, shows that the magnetism and the light act on each other through the intervention of the matter.

2225. Recognizing or perceiving *matter* only by its powers, and knowing nothing of any imaginary nucleus, abstract from the idea of these powers, the phænomena described in this paper much strengthen my inclination to trust in the views I have on a former occasion advanced in reference to its nature[1].

2226. It cannot be doubted that the magnetic forces act upon and affect the internal constitution of the diamagnetic, just as freely in the dark as when a ray of light is passing through it; though the phænomena produced by light seem, as yet, to present the only means of observing this constitution and the change. Further, any such change as this must belong to opake bodies, such as wood, stone, and metal; for as diamagnetics, there is no distinction between them and those which are transparent. The degree of transparency can at the utmost, in this respect, only make a distinction between the individuals of a class.

2227. If the magnetic forces had made these bodies magnets, we could, by light, have examined a transparent magnet; and that would have been a great help to our investigation of the forces of matter. But it does not make them magnets (2171.), and therefore the molecular condition of these bodies, when in the state described, must be specifically distinct from that of magnetized iron, or other such matter, and must be *a new magnetic condition*; and as the condition is a state of tension (manifested by its instantaneous return to the normal state when the magnetic induction is removed), so the *force* which the matter in this state possesses and its mode of action, must be to us a *new magnetic force* or *mode of action* of matter.

2228. For it is impossible, I think, to observe and see the action of magnetic forces, rising in intensity, upon a piece of heavy glass or a tube of water, without also perceiving that the latter

[1] Vol. ii. p. 284, or Philosophical Magazine, 1844, vol. xxiv. p. 136.

acquire properties which are not only *new* to the substance, but are also in subjection to very definite and precise laws (2160. 2199.), and are equivalent in proportion to the magnetic forces producing them.

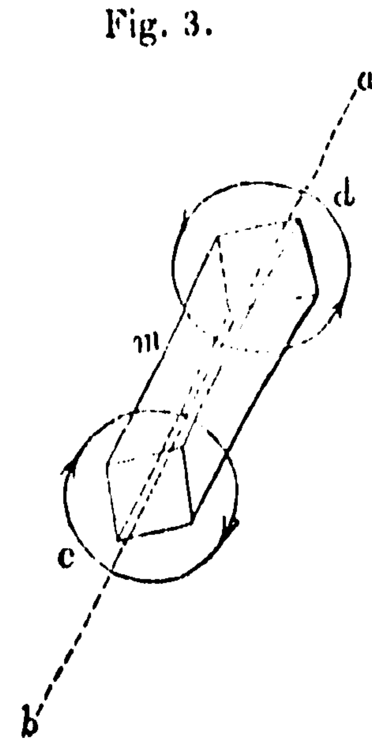
2229. Perhaps this state is a state of *electric tension tending to a current*; as in magnets, according to Ampère's theory, the state is a state of *current*. When a core of iron is put into a helix, everything leads us to believe that currents of electricity are produced within it, which rotate or move in a plane perpendicular to the axis of the helix. If a diamagnetic be placed in the same position, it acquires power to make light rotate in the same plane. The state it has received is a state of tension, but it has not passed on into currents, though the acting force and every other circumstance and condition are the same as those which do produce currents in iron, nickel, cobalt, and such other matters as are fitted to receive them. Hence the idea that there exists in diamagnetics, under such circumstances, a tendency to currents, is consistent with all the phænomena as yet described, and is further strengthened by the fact, that, leaving the loadstone or the electric current, which by inductive action is rendering a piece of iron, nickel, or cobalt magnetic, perfectly unchanged, a mere change of temperature will take from these bodies their extra power, and make them pass into the common class of diamagnetics.

2230. The present is, I believe, the first time that the molecular condition of a body, required to produce the circular polarization of light, has been artificially given; and it is therefore very interesting to consider this known state and condition of the body, comparing it with the relatively unknown state of those which possess the power naturally: especially as some of the latter rotate to the right-hand and others to the left; and, as in the cases of quartz and oil of turpentine, the same body chemically speaking, being in the latter instance a liquid with particles free to move, presents different specimens, some rotating one way and some the other.

2231. At first one would be inclined to conclude that the natural state and the state conferred by magnetic and electric forces must be the same, since the effect is the same; but on further consideration it seems very difficult to come to such a conclusion. Oil of turpentine will rotate a ray of light, the power depending upon its particles and not upon the arrangement of the mass. Whichever way a ray of polarized light passes through this fluid, it is rotated in the same manner; and rays passing in every possible direction through it *simultaneously* are all rotated with equal force and according to one common law of direction; *i. e.* either all right-handed or else all to the left. Not so with the rotation superinduced on the *same* oil of turpentine by the magnetic or electric forces: it exists only in one direction, *i. e.* in a plane perpendicular to the magnetic line; and being limited to this plane, it can be changed in direction by a reversal of the direction of the inducing force. The direction of the rotation produced by the natural state is connected invariably with the direction of the ray of light; but the power to produce it appears to be possessed in every direction and at all times by the particles of the fluid: the direction of the rotation produced by the induced condition is connected invariably with the direction of the magnetic line or the electric current, and the condition is possessed by the particles of matter, but strictly limited by the line or the current, changing and disappearing with it.

2232. Let *m*, in fig. 3, represent a glass cell filled with oil of turpentine, possessing naturally the power of producing right-hand rotation, and *a b* a polarized ray of light. If the ray proceed from *a* to *b*, and the eye be placed at *b*, the rotation will be right-handed, or according to the direction expressed by the arrow heads on the circle *c*; if the ray proceed from *b* to *a*, and the eye be placed at *a*, the rotation will still be right-handed *to the observer*, *i. e.* according to the direction indicated on the circle *d*. Let now an electric current pass round the oil of turpentine in the direction indicated on the circle *c*, or magnetic poles be placed so as to produce the same effect (2155.); the particles will acquire a further rotative force (which no motion amongst themselves will disturb), and a ray coming from

Fig. 3.



a to *b* will be seen by an eye placed at *b* to rotate to the right-hand more than before, or in the direction on the circle *c*; but pass a ray from *b* to *a*, and observe with the eye at *a*, and the phænomenon is no longer the same as before; for instead of the new rotation being according to the direction indicated on the circle *d*, it will be in the contrary direction, or to the observer's left-hand (2199.). In fact the induced rotation will be added to the natural rotation as respects a ray passing from *a* to *b*, but it will be subtracted from the natural rotation as regards the ray passing from *b* to *a*. Hence the particles of this fluid which rotate by virtue of their natural force, and those which rotate by virtue of the induced force, cannot be in the same condition.

2233. As respects the power of the oil of turpentine to rotate a ray in whatever direction it is passing through the liquid, it may well be, that though all the particles possess the power of rotating the light, only those whose planes of rotation are more or less perpendicular to the ray affect it; and that it is the resultant or sum of forces in any one direction which is active in producing rotation. But even then a striking difference remains, because the resultant in the same plane is not absolute in direction, but relative to the course of the ray, being in the one case as the circle *c*, and in the other as the circle *d*, fig. 3; whereas the resultant of the magnetic or electric induction is absolute, and not changing with the course of the ray, being always either as expressed by *c* or else as indicated by *d*.

2234. All these differences, however, will doubtless disappear or come into harmony as these investigations are extended; and their very existence opens so many paths, by which we may pursue our inquiries, more and more deeply, into the powers and constitution of matter.

2235. Bodies having rotating power of themselves, do not seem by that to have a greater or a less tendency to assume a further degree of the same force under the influence of magnetic or electric power.

2236. Were it not for these and other differences, we might see an analogy between those bodies, which possess at all times the rotating power, as a specimen of quartz which rotates only in one plane, and also those to which the power is given by the induction of other forces, as a prism of heavy glass in a helix, on the one hand; and, on the other, a natural magnet and a helix through which the current is passing. The natural condition of the magnet and quartz, and the constrained condition of the helix and heavy glass, form the link of the analogy in one direction; whilst the supposition of currents existing in the magnet and helix, and only a tendency or tension to currents existing in the quartz and heavy glass, supplies the link in the transverse direction.

2237. As to those bodies which seem as yet to give no indication of the power over light, and therefore none of the assumption of the new magnetic conditions, these may be divided into two classes, the one including air, gases and vapours, and the other rock crystal, Iceland spar, and certain other crystalline bodies. As regards the latter class, I shall give, in the next series of these researches, proofs drawn from phænomena of an entirely different kind, that they do acquire the new magnetic condition; and these being so disposed of for the moment, I am inclined to believe that even air and gases have the power to assume the peculiar state, and even to affect light, but in a degree so small that as yet it has not been made sensible. Still the gaseous state is such a remarkable condition of matter, that we ought not too hastily to assume that the substances which, in the solid and liquid state, possess properties even general in character, always carry these into their gaseous condition.

2238. Rock-salt, fluor-spar, and, I think, alum, affect the ray of light; the other crystals experimented with did not; these are equiaxed and singly refracting, the others are unequiaxed and doubly refracting. Perhaps these instances, with that of the rotation of quartz, may even now indicate a relation between magnetism, electricity, and the crystallizing forces of matter.

2239. All bodies are affected by helices as by magnets, and according to laws which show that the causes of the action are identical as well as the effects. This result supplies another fine proof in favour of the identity of helices and magnets, according to the views of Ampère.

2240. The theory of static induction which I formerly ventured to set forth (1161, &c.), and which depends upon the action of the contiguous particles of the dielectric intervening between the inductric and the inducteous bodies, led me to expect that the same kind of dependence upon the intervening particles would be found to exist in magnetic action; and I published certain experiments and considerations on this point seven years ago (1709–1736.). I could not then discover any peculiar condition of the intervening substance or diamagnetic; but now that I have been able to make out such a state, which is not only a state of tension (2227.), but dependent entirely upon the magnetic lines which pass through the substance, I am more than ever encouraged to believe that the view then advanced is correct.

2241. Although the magnetic and electric forces *appear* to exert no power on the ordinary or on the depolarized ray of light, we can hardly doubt but that they have some special influence, which probably will soon be made apparent by experiment. Neither can it be supposed otherwise than that the same kind of action should take place on the other forms of radiant agents as heat and chemical force.

2242. This mode of magnetic and electric action, and the phænomena presented by it, will, I hope, greatly assist hereafter in the investigation of the nature of transparent bodies, of light, of magnets, and their action one on another or on magnetic substances. I am at this time engaged in investigating the new magnetic condition, and shall shortly send a further account of it to the Royal Society. What the possible effect of the force may be in the earth as a whole or in magnets, or in relation to the sun, and what may be the best means of causing light to evolve electricity and magnetism, are thoughts continually pressing upon the mind; but it will be better to occupy both time and thought, aided by experiment, in the investigation and development of real truth, than to use them in the invention of suppositions which may or may not be founded on, or consistent with, fact.

Royal Institution,
Oct. 29, 1845.

TWENTIETH SERIES[1].

§ 27. *On new magnetic actions, and on the magnetic condition of all matter*[2]. ¶ i. *Apparatus required.* ¶ ii. *Action of magnets on heavy glass.* ¶ iii. *Action of magnets on other substances acting magnetically on light.* ¶ iv. *Action of magnets on the metals generally.*

Received December 6,—Read December 18, 1845.

2243. The contents of the last series of these Researches were, I think, sufficient to justify the statement, that a new magnetic

[1] Philosophical Transactions, 1846, p. 21.

[2] My friend Mr. Wheatstone has this day called my attention to a paper by M. Becquerel, "On the magnetic actions excited in all bodies by the influence of very energetic magnets," read to the Academy of Sciences on the 27th of September 1827, and published in the Annales de Chimie, xxxvi. p. 337. It relates to the action of the magnet on a magnetic needle, on soft iron, on the deutoxide and tritoxide of iron, on the tritoxide alone, and on a needle of wood. The author observed, and quotes Coulomb as having also observed, that a needle of wood under certain conditions, pointed *across* the magnetic curves; and he also states the striking fact that he had found a needle of wood place itself parallel to the wires of a galvanometer. These effects, however, he refers to a degree of magnetism less than that of the tritoxide of iron, but the same in character, for the bodies take the same position. The polarity of steel and iron is stated to be in the direction of the length of the substance, but that of tritoxide of iron, wood and gum-lac, most frequently in the direction of the width, and always when one magnetic pole is employed. "This difference of effect, which establishes a line of demarcation between these two species of phænomena, is due to this, that the magnetism being very feeble in the tritoxide of iron, wood, &c., we may neglect the reaction of the body on itself, and therefore the direct action of the bar ought to overrule it."

As the paper does not refer the phænomena of wood and gum-lac to an elementary *repulsive* action, nor show that they are common to an immense class of bodies, nor distinguish this class, which I have called diamagnetic, from the magnetic class; and, as it makes all magnetic action of one kind, whereas I show that there are two kinds of such action, as distinct from each other as positive and negative electric action are in their way, so I do not think I need alter a word or the date of that which I have written; but am most glad here to acknowledge M. Becquerel's important facts and labours in reference to this subject.—M. F. Dec. 5, 1845.

condition (*i. e.* one new to our knowledge) had been impressed on matter by subjecting it to the action of magnetic and electric forces (2227.); which new condition was made manifest by the powers of action which the matter had acquired over light. The phænomena now to be described are altogether different in their nature; and they prove, not only a magnetic condition of the substances referred to unknown to us before, but also of many others, including a vast number of opake and metallic b oie, and perhaps all except the magnetic metals and their compounds: and they also, through that condition, present us with the means of undertaking the correlation of magnetic phænomena, and perhaps the construction of a theory of general magnetic action founded on simple fundamental principles.

2244. The whole matter is so new, and the phænomena so varied and general, that I must, with every desire to be brief, describe much which at last will be found to concentrate under simple principles of action. Still, in the present state of our knowledge, such is the only method by which I can make these principles and their results sufficiently manifest.

¶ i. *Apparatus required.*

2245. The effects to be described require magnetic apparatus of great power, and under perfect command. Both these points are obtained by the use of electro-magnets, which can be raised to a degree of force far beyond that of natural or steel magnets; and further, can be suddenly altogether deprived of power, or made energetic to the highest degree, without the slightest alteration of the arrangement, or of any other circumstance belonging to an experiment.

2246. One of the electro-magnets which I use is that already described under the term Woolwich helix (2192.). The soft iron core belonging to it is 28 inches in length and 2·5 inches in diameter. When thrown into action by ten pair of Grove's plates, either end will sustain one or two half-hundred weights hanging to it. The magnet can be placed either in the vertical or the horizontal position. The iron core is a cylinder with flat ends, but I have had a cone of iron made, 2 inches in diameter at the base and 1 inch in height, and this placed at the end of the core, forms a conical termination to it, when required.

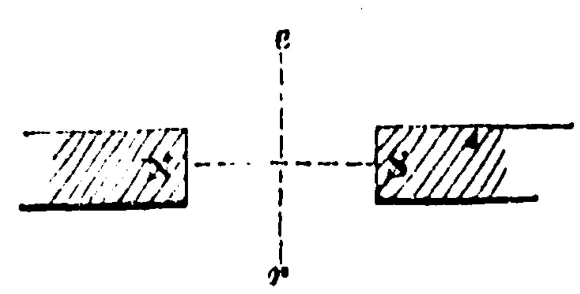
2247. Another magnet which I have had made has the horseshoe form. The bar of iron is 46 inches in length and 3·75 inches in diameter, and is so bent that the extremities forming the poles are 6 inches from each other; 522 feet of copper wire 0·17 of an inch in diameter, and covered with tape, are wound round the two straight parts of the bar, forming two coils on these parts, each 16 inches in length, and composed of three layers of wire: the poles are, of course, 6 inches apart, the ends are planed true, and against these move two short bars of soft iron, 7 inches long and 2½ by 1 inch thick, which can be adjusted by screws, and held at any distance less than 6 inches from each other. The ends of these bars form the opposite poles of contrary name; the magnetic field between them can be made of greater or smaller extent, and the intensity of the lines of magnetic force be proportionately varied.

2248. For the suspension of substances between and near the poles of these magnets, I occasionally used a glass jar, with a plate and sliding wire at the top. Six or eight lengths of cocoon silk being equally stretched, were made into one thread and attached, at the upper end, to the sliding rod, and at the lower end to a stirrup of paper, in which anything to be experimented on could be sustained.

2249. Another very useful mode of suspension was to attach one end of a fine thread, 6 feet long, to an adjustible arm near the ceiling of the room, and terminating at the lower end by a little ring of copper wire; any substance to be suspended could be held in a simple cradle of fine copper wire having 8 or 10 inches of the wire prolonged upward; this being bent into a hook at the superior extremity, gave the means of attachment to the ring. The height of the suspended substance could be varied at pleasure, by bending any part of the wire at the instant into the hook form. A glass cylinder placed between the magnetic poles was quite sufficient to keep the suspended substance free from any motion, due to the agitation of the air.

2250. It is necessary, before entering upon an experimental investigation with such an apparatus, to be aware of the effect of any magnetism which the bodies used may possess; the power of the apparatus to make manifest such magnetism is so great, that it is difficult on that account to find writing-paper fit for the stirrup above mentioned. Before therefore any experiments are instituted, it must be ascertained that the suspending apparatus employed does not point, *i. e.* does not take up a position parallel to the lines joining the magnetic poles, by virtue of the magnetic force. When copper suspensions are employed, a peculiar effect is produced (2309.), but when understood, as it will be hereafter, it does not interfere with the results of experiment. The wire should be fine, not magnetic as iron, and the form of the suspending cradle should not be elongated horizontally, but be round or square as to its general dimensions, in that direction.

2251. The substances to be experimented with should be carefully examined, and rejected if not found free from magnetism. Their state is easily ascertained; for, if magnetic, they will either be attracted to the one or the other pole of the great magnet, or else point between them. No examination by smaller magnets, or by a magnetic needle, is sufficient for this purpose.



2252. I shall have such frequent occasion to refer to two chief directions of position across the magnetic field, that to avoid periphrasis, I will here ask leave to use a term or two, conditionally. One of these directions is that from pole to pole, or along the line of magnetic force; I will call it the axial direction: the other is the direction perpendicular to this, and across the line of magnetic force; and for the time, and as respects the space between the poles, I will call it the equatorial direction. Other terms that I may use, I hope will explain themselves.

¶ ii. *Action of magnets on heavy glass.*

2253. The bar of silicated borate of lead, or heavy glass already described as the substance in which magnetic forces were first made effectually to bear on a ray of light (2152.), and which is 2 inches long, and about 0·5 of an inch wide and thick, was suspended centrally between the magnetic poles (2247.), and left until the effect of torsion was over. The magnet was then thrown into action by making contact at the voltaic battery: immediately the bar moved, turning round its point of suspension, into a position across the magnetic curve or line of force, and after a few vibrations took up its place of rest there. On being displaced by hand from this position, it returned to it, and this occurred many times in succession.

2254. Either end of the bar indifferently went to either side of the axial line. The determining circumstance was simply inclination of the bar one way or the other to the axial line, at the beginning of the experiment. If a particular or marked end of the bar were on one side of the magnetic, or axial line, when the magnet was rendered active that end went further outwards, until the bar had taken up the equatorial position.

2255. Neither did any change in the magnetism of the poles, by change in the direction of the electric current, cause any difference in this respect. The bar went by the shortest course to the equatorial position.

2256. The power which urged the bar into this position was so thoroughly under command, that if the bar were swinging it could easily be hastened in its course into this position, or arrested as it was passing from it, by seasonable contacts at the voltaic battery.

2257. There are two positions of equilibrium for the bar; one stable, the other unstable. When in the direction of the axis or magnetic line of force, the completion of the electric communication causes no change of place; but if it be the least oblique to this position, then the obliquity increases until the bar arrives at the equatorial position; or if the bar be originally in the equatorial position, then the magnetism causes no further changes, but retains it there (2298. 2299. 2384.).

2258. Here then we have a magnetic bar which points east and west, in relation to north and south poles, *i. e.* points perpendicularly to the lines of magnetic force.

2259. If the bar be adjusted so that its point of suspension, being in the axial line, is not equidistant from the poles, but near to one of them, then the magnetism again makes the bar take up a position perpendicular to the magnetic lines of force; either end of the bar being on the one side of the axial line, or the other, at pleasure. But at the same time there is another effect, for at the moment of completing the electric contact, the centre of gravity of the bar recedes from the pole and remains repelled from it as long as the magnet is retained excited. On allowing the magnetism to pass away, the bar returns to the place due to it by its gravity.

2260. Precisely the same effect takes place at the other pole of the magnet. Either of them is able to repel the bar, whatever its position may be, and at the same time the bar is made to assume a position, at right angles, to the line of magnetic force.

2261. If the bar be equidistant from the two poles, and in the axial line, then no repulsive effect is or can be observed.

2262. But preserving the point of suspension in the equatorial line, *i. e.* equidistant from the two poles, and removing it a little on one side or the other of the axial line (2252.), then another effect is brought forth. The bar points as before across the magnetic line of force, but at the same time it recedes from the axial line, increasing its distance from it, and this new position is retained as long as the magnetism continues, and is quitted with its cessation.

2263. Instead of two magnetic poles, a single pole may be used, and that either in a vertical or a horizontal position. The effects are in perfect accordance with those described above; for the bar, when near the pole, is repelled from it in the direction of the line of magnetic force, and at the same time it moves into a position perpendicular to the direction of the magnetic lines passing through it. When the magnet is vertical (2246.) and the bar by its side, this action makes the bar a tangent to the curve of its surface.

2264. To produce these effects, of pointing across the magnetic curves, the form of the heavy glass must be long; a cube, or a fragment approaching roundness in form, will not point, but a long piece will. Two or three rounded pieces or cubes, placed side by side in a paper tray, so as to form an oblong accumulation, will also point.

2265. Portions, however, of any form, are *repelled*: so if two pieces be hung up at once in the axial line, one near each pole, they are repelled by their respective poles, and approach, seeming to attract each other. Or if two pieces be hung up in the equatorial line, one on each side of the axis, then they both recede from the axis, seeming to repel each other.

2266. From the little that has been said, it is evident that the bar presents in its motion a complicated result of the force exerted by the magnetic power over the heavy glass, and that, when cubes or spheres are employed, a much simpler indication of the effect may be obtained. Accordingly, when a cube was thus used with the two poles, the effect was repulsion or recession from either pole, and also recession from the magnetic axis on either side.

2267. So, the indicating particle would move, either along the magnetic curves, or across them; and it would do this either in one direction or the other; the only constant point being, that its tendency was to move from stronger to weaker places of magnetic force.

2268. This appeared much more simply in the case of a single magnetic pole, for then the tendency of the indicating cube or sphere was to move outwards, in the direction of the magnetic lines of force. The appearance was remarkably like a case of weak electric repulsion.

2269. The cause of the pointing of the bar, or any oblong arrangement of the heavy glass, is now evident. It is merely a result of the tendency of the particles to move outwards, or into the positions of weakest magnetic action. The joint exertion of the action of all the particles brings the mass into the position, which, by experiment, is found to belong to it.

2270. When one or two magnetic poles are active at once, the courses described by particles of heavy glass free to move, form a set of lines or curves, which I may have occasion hereafter to refer to; and as I have called air, glass, water, &c. diamagnetic (2149.), so I will distinguish these lines by the term *diamagnetic curves*, both in relation to, and contradistinction from, the lines called magnetic curves.

2271. When the bar of heavy glass is immersed in water, alcohol, or æther, contained in a vessel between the poles, all the preceding effects occur; the bar points and the cube recedes exactly in the same manner as in air.

2272. The effects equally occur in vessels of wood, stone, earth, copper, lead, silver, or any of those substances which belong to the diamagnetic class (2149.).

2273. I have obtained the same equatorial direction and motions of the heavy glass bar as those just described, but in a very feeble degree, by the use of a good common steel horse-shoe magnet (2157.). I have not obtained them by the use of the helices (2191. 2192.) without the iron cores.

2274. Here therefore we have magnetic repulsion without polarity, *i. e.* without reference to a particular pole of the magnet, for either pole will repel the substance, and both poles will repel it at once (2262.). The heavy glass, though subject to magnetic action, cannot be considered as magnetic, in the usual acceptation of that term, or as iron, nickel, cobalt, and their compounds. It presents to us, under these circumstances, a magnetic property new to our knowledge; and though the phænomena are very different in their nature and character to those presented by the action of the heavy glass on light (2152.), still they appear to be dependent on, or connected with, the same condition of the glass as made it then effective, and therefore, with those phænomena, prove the reality of this new condition.

¶ iii. *Action of magnets on other substances acting magnetically on light.*

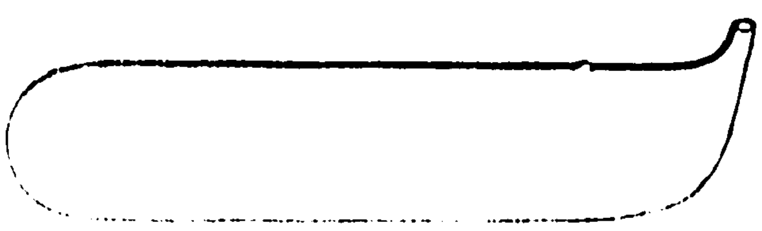
2275. We may now pass from heavy glass to the examination of the other substances, which, when under the power of magnetic or electric forces, are able to affect and rotate a polarized ray (2173.), and may also easily extend the investigation to bodies which, from their irregularity of form, imperfect transparency, or actual opacity, could not be examined by a polarized ray, for here we have no difficulty in the application of the test to all such substances.

2276. The property of being thus repelled and affected by magnetic poles, was soon found not to be peculiar to heavy glass. Borate of lead, flint-glass, and crown-glass set in the same manner equatorially, and were repelled when near to the poles, though not to the same degree as the heavy glass.

2277. Amongst substances which could not be subjected to the examination by light, phosphorus in the form of a cylinder presented the phænomena very well; I think as powerfully as heavy glass, if not more so. A cylinder of sulphur, and a long piece of thick India rubber, neither being magnetic after the ordinary fashion, were well directed and repelled.

2278. Crystalline bodies were equally obedient, whether taken from the single or double refracting class (2237.). Prisms of quartz, calcareous spar, nitre and sulphate of soda, all pointed well, and were repelled.

2279. I then proceeded to subject a great number of bodies, taken from every class, to the magnetic forces, and will, to illustrate the variety in the nature of the substances, give a comparatively short list of crystalline, amorphous, liquid and organic bodies below. When the bodies were fluids, I enclosed them in thin glass tubes. Flint-glass points equatorially, but if the tube be of very thin glass, this effect is found to be small when the tube is experimented with alone; afterwards, when it is filled with liquid and examined, the effect is such that there is no fear of mistaking that due to the glass for that of the fluid. The tubes must not be closed with cork, sealing-wax, or any ordinary substance taken at random, for these are generally magnetic (2285.). I have usually so shaped them in the making, and drawn them off at the neck, as to leave the aperture on one side, so that when filled with liquid they require no closing.



2280. Rock crystal.
Sulphate of lime.
Sulphate of baryta.
Sulphate of soda.
Sulphate of potassa.
Sulphate of magnesia.
Alum.
Muriate of ammonia.
Chloride of lead.
Chloride of sodium.
Nitrate of potassa.
Nitrate of lead.
Carbonate of soda.
Iceland spar.
Acetate of lead.
Tartrate of potash and antimony.
Tartrate of potash and soda.
Tartaric acid.
Citric acid.
Water.
Alcohol.
Æther.
Nitric acid.
Sulphuric acid.
Muriatic acid.
Solutions of various alkaline and earthy salts.
Glass.
Litharge.
White arsenic.
Iodine.
Phosphorus.
Sulphur.
Resin.
Spermaceti.
Caffeine.
Cinchonia.
Margaric acid.
Wax from shell-lac.
Sealing-wax.

Olive oil.	Mutton, dried.
Oil of turpentine.	Beef, fresh.
Jet.	Beef, dried.
Caoutchouc.	Blood, fresh.
Sugar.	Blood, dried.
Starch.	Leather.
Gum-arabic.	Apple.
Wood.	Bread.
Ivory.	

2281. It is curious to see such a list as this of bodies presenting on a sudden this remarkable property, and it is strange to find a piece of wood, or beef, or apple, obedient to or repelled by a magnet. If a man could be suspended, with sufficient delicacy, after the manner of Dufay, and placed in the magnetic field, he would point equatorially; for all the substances of which he is formed, including the blood, possess this property.

2282. The setting equatorially depends upon the form of the body, and the diversity of form presented by the different substances in the list was very great; still the general result, that elongation in one direction was sufficient to make them take up an equatorial position, was established. It was not difficult to perceive that comparatively large masses would point as readily as small ones, because in larger masses more lines of magnetic force would bear in their action on the body, and this was proved to be the case. Neither was it long before it evidently appeared that the form of a plate or a ring was quite as good as that of a cylinder or a prism; and in practice it was found that plates and flat rings of wood, spermaceti, sulphur, &c., if suspended in the right direction, took up the equatorial position very well. If a plate or ring of heavy glass could be floated in water, so as to be free to move in every direction, and were in that condition subject to magnetic forces diminishing in intensity, it would immediately set itself equatorially, and if its centre coincided with the axis of magnetic power, would remain there; but if its centre were out of this line, it would then, perhaps, gradually pass off from this axis in the plane of the equator, and go out from between the poles.

2283. I do not find that division of the substance has any distinct influence on the effects. A piece of Iceland spar was observed, as to the degree of force with which it set equatorially;

it was then broken into six or eight fragments, put into a glass tube and tried again; as well as I could ascertain, the effect was the same. By a second operation, the calcareous spar was reduced into coarse particles; afterwards to a coarse powder, and ultimately to a fine powder: being examined as to the equatorial set each time, I could perceive no difference in the effect, until the very last, when I thought there might be a slight diminution of the tendency; but if so, it was almost insensible. I made the same experiment on silica with the same result, of no diminution of power. In reference to this point I may observe, that starch and other bodies in fine powder exhibited the effect very well.

2284. It would require very nice experiments and great care to ascertain the specific degree of this power of magnetic action possessed by different bodies, and I have made very little progress in that part of the subject. Heavy glass stands above flint-glass, and the latter above plate-glass. Water is beneath all these, and I think alcohol is below water, and æther below alcohol. The borate of lead is I think as high as heavy glass, if not above it, and phosphorus is probably at the head of all the substances just named. I verified the equatorial set of phosphorus between the poles of a common magnet (2273.).

2285. I was much impressed by the fact that blood was not magnetic (2280.), nor any of the specimens tried of red muscular fibre of beef or mutton. This was the more striking, because, as will be seen hereafter, iron is *always* and in almost *all states* magnetic. But in respect to this point it may be observed, that the ordinary magnetic property of matter and this *new property* are in their effects opposed to each other; and that when this property is strong it may overcome a very slight degree of ordinary magnetic force, just as also a certain amount of the magnetic property may oppose and effectually hide the presence of this force (2422.). It is this circumstance which makes it so necessary to be careful in examining the magnetic condition of the bodies in the first instance (2250.). The following list of a few substances which were found slightly magnetic, will illustrate this point:—Paper, sealing-wax, china ink, Berlin porcelain, silkworm-gut, asbestos, fluor-spar, red lead, vermilion, peroxide of lead, sulphate of zinc, tourmaline, plumbago, shell-lac, charcoal. In some of these cases the magnetism was generally diffused through the body, in other cases it was limited to a particular part.

2286. Having arrived at this point, I may observe, that we can now have no difficulty in admitting that the phænomena abundantly establish the existence of a magnetic property in matter, new to our knowledge. Not the least interesting of the consequences that flow from it, is the manner in which it disposes of the assertion which has sometimes been made, that all bodies are magnetic. Those who hold this view, mean that all bodies are magnetic as iron is, and say that they point between the poles. The new facts give not a mere negative to this statement, but something beyond, namely, an affirmative as to the existence of forces in all ordinary bodies, directly the opposite of those existing in magnetic bodies, for whereas those practically produce attraction, these produce repulsion; those set a body in the axial direction, but these make it take up an equatorial position: and the facts, with regard to bodies generally, are exactly the reverse of those which the view quoted indicates.

¶ iv. *Action of magnets on metals generally.*

2287. The metals, as a class, stand amongst bodies having a high and distinct interest in relation both to magnetic and electric forces, and might at first well be expected to present some peculiar phænomena, in relation to the striking property found to be possessed in common by so large a number of substances, so varied in their general characters. As yet no distinction associated with conduction or non-conduction, transparent or opake, solid or liquid, crystalline or amorphous, whole or broken, has presented itself; whether the metals, distinct as they are as a class, would fall into the great generalization, or whether at last a separation would occur, was to me a point of the highest interest.

2288. That the metals, iron, nickel and cobalt, would stand in a distinct class, appeared almost undoubted; and it will be, I think, for the advantage of the inquiry, that I should consider them in a section apart by themselves. Further, if any other metals appeared to be magnetic, as these are, it would be right and expedient to include them in the same class.

2289. My first point, therefore, was to examine the metals for any indication of ordinary magnetism. Such an examination cannot be carried on by magnets anything short in power of those to be used in the further investigation; and in proof of this point I found many specimens of the metals, which appeared to be perfectly free from magnetism when in the presence of a magnetic needle, or a strong horse-shoe magnet (2157.), that yet gave abundant indications when suspended near to one or both poles of the magnets described (2246.).

2290. My test of magnetism was this. If a bar of the metal to be examined, about 2 inches long, was suspended (2249.) in the magnetic field, and being at first oblique to the axial line was upon the supervention of the magnetic forces drawn into the axial position instead of being driven into the equatorial line, or remaining in some oblique direction, then I considered it magnetic. Or, if being near one magnetic pole, it was attracted by the pole, instead of being repelled, then I concluded it was magnetic. It is evident that the test is not strict, because, as before pointed out (2285.), a body may have a slight degree of magnetic force, and yet the power of the new property be so great as to neutralize or surpass it. In the first case, it might seem neither to have the one property nor the other; in the second case, it might appear free from magnetism, and possessing the special property in a *small* degree.

2291. I obtained the following metals, so that when examined as above, they did not appear to be magnetic; and in fact, if magnetic, were so to an amount so small as not to destroy the results of the other force, or to stop the progress of the inquiry.

Antimony.	Lead
Bismuth.	Mercury.
Cadmium.	Silver.
Copper.	Tin.
Gold.	Zinc.

2292. The following metals were, and are as yet to me, magnetic, and therefore companions of iron, nickel and cobalt:—

Platinum.	Titanium.
Palladium.	

2293. Whether all these metals are magnetic, in consequence of the presence of a little iron, nickel, or cobalt in them, or whether any of them are really so of themselves, I do not undertake to decide at present; nor do I mean to say that the metals of the former list are free. I have been much struck by the apparent freedom from iron of almost all the specimens of zinc, copper, antimony and bismuth, which I have examined; and it appears to me very likely that some metals, as arsenic, &c., may have much power in quelling and suppressing the magnetic properties of any portion of iron in them, whilst other metals, as silver or platinum, may have little or no power in this respect.

2294. Resuming the consideration of the influence exerted by the magnetic force over those metals which are not magnetic after the manner of iron (2291.), I may state that there are two sets of effects produced which require to be carefully distinguished. One of these depends upon induced magneto-electric currents, and shall be resumed hereafter (2309.). The other includes effects of the same nature as those produced with heavy glass and many other bodies (2276.).

2295. All the non-magnetic metals are subject to the magnetic power, and produce the same general effects as the large class of bodies already described. The force which they then manifest, they possess in different degrees. Antimony and bismuth show it well, and bismuth appears to be especially fitted for the purpose. It excels heavy glass, or borate of lead, and perhaps phosphorus; and a small bar or cylinder of it about 2 inches long, and from 0·25 to 0·5 of an inch in width, is as well fitted to show the various peculiar phænomena as anything I have yet submitted to examination.

2296. To speak accurately, the bismuth bar which I employed was 2 inches long, 0·33 of an inch wide, and 0·2 of an inch thick. When this bar was suspended in the magnetic field, between the two poles, and subject to the magnetic force, it pointed freely in the equatorial direction, as the heavy glass did (2253.), and if disturbed from that position, returned *freely* to it. This latter point, though perfectly in accordance with the former phænomena, is in such striking contrast with the phænomena presented by copper and some other of the metals (2309.), as to require particular notice here.

2297. The comparative sensibility of bismuth causes several movements to take place under various circumstances, which being complicated in their nature, require careful analysis and explanation. The chief of these, with their causes, I will proceed to point out.

2298. If the cylinder electro-magnet (2246.) be placed vertically so as to present one pole upwards, that pole will exist in the upper end of an iron cylinder, having a flat horizontal face $2\frac{1}{2}$ inches in diameter. A small indicating sphere (2266.) of bismuth, hung over the centre of this face and close to it, does not move by the magnetism. If the ball be carried outwards, half-way, for instance, between the centre and the edge, the magnetism makes it move inwards, or towards the axis (prolonged) of the iron cylinder. If carried still further outwards, it still moves inwards under the influence of the magnetism, and such continues to be the case until it is placed just over the edge of the terminal face of the core, where it has no motion at all (here, by another arrangement of the experiment, it is known to tend in what is at present an upward direction from the core). If carried a little further outwards, the magnetism then makes the bismuth ball tend to go outwards or be repelled, and such continues to be the direction of the force in any further position, or down the side of the end of the core.

2299. In fact, the circular edge formed by the intersection of the end of the core with its sides, is virtually the apex of the magnetic pole, to a body placed like the bismuth ball close to it, and it is because the lines of magnetic force issuing from it diverge as it were, and weaken rapidly in all directions from it, that the ball also tends to pass in all directions either inwards or upwards, or outwards from it, and thus produces the motions described. These same effects do not in fact all occur when the ball, being taken to a greater distance from the iron, is placed in magnetic curves, having generally a simpler direction. In order to remove the effect of the edge, an iron cone was placed on the top of the core, converting the flat end into a cone, and then the indicating ball was urged to move upwards, only when over the apex of the cone, and upwards and outwards, as it was more or less on one side of it, being always repelled from the pole in that direction, which transferred it most rapidly from strong to weaker points of magnetic force.

2300. To return to the vertical flat pole: when a horizontal bar of bismuth was suspended concentrically and close to the pole, it could take up a position in any direction relative to the axis of the pole, having at the same time a tendency to move upwards or be repelled from it. If its point of suspension was a little excentric, the bar gradually turned, until it was parallel to a line joining its point of suspension with the prolonged axis of the pole, and the centre of gravity moved inwards. When its point of suspension was just outside the edge of the flat circular terminating face, and the bar formed a certain angle with a radial line joining the axis of the core and the point of suspension, then the movements of the bar were uncertain and wavering. If the angle with the radial line were less than that above, the bar would move into parallelism with the radius and go inwards: if the angle were greater, the bar would move until perpendicular to the radial line and go outwards. If the centre of the bar were still further out than in the last case, or down by the side of the core, the bar would always place itself perpendicular to the radius and go outwards. All these complications of motion are easily resolved into their simple elementary origin, if reference be had to the character of the circular angle bounding the end of the core; to the direction of the magnetic lines of force issuing from it and the other parts of the pole; to the position of the different parts of the bar in these lines; and the ruling principle that each particle tends to go by the nearest course from *strong* to *weaker* points of magnetic force.

2301. The bismuth points well, and is well repelled (2296.) when immersed in water, alcohol, æther, oil, mercury, &c., and also when enclosed within vessels of earth, glass, copper, lead, &c. (2272.), or when screens of 0·75 or 1 inch in thickness of bismuth, copper, or lead intervene. Even when a bismuth cube (2266.) was placed in an iron vessel 2½ inches in diameter and 0·17 of an inch in thickness, it was well and freely repelled by the magnetic pole.

2302. Whether the bismuth be in one piece or in very fine powder, appears to make no difference in the character or in the degree of its magnetic property (2283.).

2303. I made many experiments with masses and bars of bismuth suspended, or otherwise circumstanced, to ascertain whether two pieces had any mutual action on each other, either of attraction or repulsion, whilst jointly under the influence of the magnetic forces, but I could not find any indication of such mutual action: they appeared to be perfectly indifferent one to another, each tending only to go from stronger to weaker points of magnetic power.

2304. Bismuth, in very fine powder, was sprinkled upon paper, laid over the horizontal circular termination of the vertical pole (2246.). If the paper were tapped, the magnet not being excited, nothing particular occurred; but if the magnetic power were on, then the powder retreated in both directions, inwards and outwards, from a circular line just over the edge of the core, leaving the circle clear, and at the same time showing the tendency of the particles of bismuth in all directions from that line (2299.).

2305. When the pole was terminated by a cone (2246.) and the magnet not in action, paper with bismuth powder sprinkled over it being drawn over the point of the cone, gave no particular result; but when the magnetism was on, such an operation cleared the powder from every point which came over the cone, so that a mark was traced or written out in clear lines running through the powder, and showing every place where the pole had passed.

2306. The bar of bismuth and a bar of antimony was found to set equatorially between the poles of the ordinary horse-shoe magnet.

2307. The following list may serve to give an idea of the apparent order of some metals, as regards their power of producing these new effects, but I cannot be sure that they are perfectly free from the magnetic metals. In addition to that, there are certain other effects produced by the action of magnetism on metals (2309.) which greatly interfere with the results due to the present property.

Bismuth.	Cadmium.
Antimony.	Mercury.
Zinc.	Silver.
Tin.	Copper.

2308. I have a vague impression that the repulsion of bismuth by a magnet has been observed and published several years ago. If so, it will appear that what must then have been considered as a peculiar and isolated effect, was the consequence of a general property, which is now shown to belong to all matter[1].

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[1] M. de la Rive has this day referred me to the Bibliothèque Universelle for 1829, tome xl. p. 82, where it will be found that the experiment spoken of above is due to M. la Baillif of Paris. M. la Baillif showed sixteen years ago that

2309. I now turn to the consideration of some peculiar phænomena which are presented by copper and several of the metals when they are subjected to the action of magnetic forces, and which so tend to mask effects of the kind already described, that if not known to the inquirer they would lead to much confusion and doubt. These I will first describe as to their appearances, and then proceed to consider their origin.

2310. If instead of a bar of bismuth (2296.) a bar of copper of the same size be suspended between the poles (2247.), and magnetic power be developed whilst the bar is in a position oblique to the axial and equatorial lines, the experimenter will perceive the bar to be affected, but this will not be manifest by any tendency of the bar to go to the equatorial line; on the contrary, it will advance towards the axial position as if it were magnetic. It will not, however, continue its course until in that position, but, unlike any effect produced by magnetism, will stop short, and making no vibration beyond or about a given point, will remain there coming at once to a dead rest: and this it will do even though the bar by the effect of torsion or momentum was previously moving with a force that would have caused it to make several gyrations. This effect is in striking contrast with that which occurs when antimony, bismuth, heavy glass, or other such bodies are employed, and it is equally removed from an ordinary magnetic effect.

2311. The position which the bar has taken up it retains with a considerable degree of tenacity, provided the magnetic force be continued. If pushed out of it, it does not return into it, but takes up its new position in the same manner, and holds it with the same stiffness; a push, however, which would make the bar spin round several times if no magnetism were present, will now not move it through more than 20° or 30°. This is not the case with bismuth or heavy glass; they vibrate freely in the magnetic field, and always return to the equatorial position.

both bismuth and antimony repelled the magnetic needle. It is astonishing that such an experiment has remained so long without further results. I rejoice that I am able to insert this reference before the present series of these Researches goes to press. Those who read my papers will see here, as on many other occasions, the results of a memory which becomes continually weaker; I only hope that they will be excused, and that omissions and errors of that nature will be considered as involuntary. M. F. Dec. 30, 1845.

2312. The position taken up by the bar may be any position. The bar is moved a little at the instant of superinducing the magnetism, but allowing and providing for that, it may be finally fixed in any position required. Even when swinging with considerable power by torsion or momentum, it may be caught and retained in any place the experimenter wishes.

2313. There are two positions in which the bar may be placed at the beginning of the experiment, from which the magnetism does not move it, the equatorial and the axial positions. When the bar is nearly midway between these, it is usually most strongly affected by the first action of the magnet, but the position of most effect varies with the form and dimensions of the magnetic poles and of the bar.

2314. If the centre of suspension of the bar be in the axial line, but near to one of the poles, these movements occur well, and are clear and distinct in their direction: if it be in the equatorial line, but on one side of the axial line, they are modified, but in a manner which will easily be understood hereafter.

2315. Having thus stated the effect of the supervention of the magnetic force, let us now remark what occurs at the moment of its cessation; for during its continuance there is no change. If, then, after the magnetism has been sustained for two or three seconds, the electric current be stopped, there is instantly a strong action on the bar, which has the appearance of a revulsion (for the bar returns upon the course which it took for a moment when the electric contact was made), but with such force, that whereas the advance might be perhaps 15° or 20°, the revulsion will cause the bar occasionally to move through two or three revolutions.

2316. Heavy glass or bismuth presents no such phænomena as this.

2317. If, whilst the bar is revolving from revulsion the electric current at the magnet be renewed, the bar instantly stops with the former appearances and results (2310.), and then upon removing the magnetic force is affected again, and, of course, now in a contrary direction to the former revulsion.

2318. When the bar is caught by the magnetic force in the axial or equatorial position, there is no revulsion. When inclined to these positions, there is; and the places most powerful in this respect appear to be those most favourable to the first brief advance (2313.). If the bar be in a position at which strong revulsion would occur, and whilst the magnetism is continued be moved by hand into the equatorial or axial position, then on taking off the magnetic force there is no revulsion.

2319. If the continuance of the electric current and consequently of the magnetism be for a moment only, the revulsion is very little, and the shorter the continuance of the magnetic force the less is the revulsion. If the magnetic force be continued for two or three seconds and then interrupted and *instantly* renewed, the bar is loosened and caught again by the power before it sensibly changes its place; and now it may be observed that it does not advance on the *renewal* of the force as it would have done had it been acted on by a first contact in that place (2310.); *i. e.* if the bar be in a certain place inclined to the axial position, the first supervention of the magnetic power causes it to advance towards the axial position; but the bar being in the same place and the magnetic power suspended and *instantly* renewed, the second supervention of force does not move the bar as the first did.

2320. When the copper bar is immersed in water, alcohol, or even mercury, the same effects take place as in the air, but the movements are, of course, not to the same extent.

2321. When plates of copper or bismuth, an inch in thickness, intervene between the poles and the copper bar, the same results occur.

2322. If one magnetic pole only be employed the effects occur near it as well as before, provided that pole have a face large in proportion to the bar, as the end of the iron core (2246.): but if the pole be pointed by the use of the conical termination, or if the bar be opposite the edge of the end of the core, then they become greatly enfeebled or disappear altogether; and only the general fact of repulsion remains (2295.).

2323. The peculiar effects which have just been described are perhaps more strikingly shown if the bar of copper be suspended perpendicularly, and then hung opposite and near to the large face of a single magnetic pole, or the pole being placed vertically, as described (2246. 2263.), anywhere near to its side. The bar, it will be remembered, is 2 inches in length by 0·33 of an inch in width, and 0·2 of an inch in thickness, and as it now will revolve on an axis parallel to its length, the two smaller dimensions are those which are free to move into new positions. In this case the establishment of the magnetic force causes the bar to turn a little in accordance with the effects before described, and the removal of the magnetic force causes a revulsion, which sends the bar spinning round on its axis several times. But at any moment the bar can again be caught and held in a position as before. The tendency on making contact at the battery is to place the longest moving dimension, *i. e.* the width of the bar, parallel to the line joining the centre of action of the magnet and the bar.

2324. The bar, as before (2311.), is extremely sluggish and as if immersed in a dense fluid, as respects rotation on its own axis; but this sluggishness does not affect the bar as a whole, for any pendulum vibration it has, continues unaffected. It is very curious to see the bar, jointly vibrating from its point of suspension (2249.) and rotating on its axis, when first affected by the magnetic force, for instantly the latter motion ceases, but the former goes on with undiminished power.

2325. The same effect of sluggishness occurs with a cube or a globe of copper as with the bar, but the phænomena of the first turn and the revulsion cease (2310. 2315.).

2326. The bars of bismuth and heavy glass present no appearance of this kind. The peculiar phænomena produced by copper are as distinct from the actions of these substances as they are from ordinary magnetic actions.

2327. Endeavouring to explain the cause of these effects, it appears to me that they depend upon the excellent conducting power of copper for electric currents, the *gradual* acquisition and loss of magnetic power by the iron core of the electro-magnet, and the production of those induced currents of magneto-electricity which I described in the First Series of these Experimental Researches (55. 109.).

2328. The obstruction to motion on its own axis, when the bar is subjected to the magnetic forces, belongs equally to the form of a sphere or a cube. It belongs to these bodies, however, only when their axes of rotation are perpendicular or oblique to the lines of magnetic force, and not when they are parallel to it; for the horizontal bar, or the vertical bar, or the cube or sphere, rotate with perfect facility when they are suspended *above* the vertical pole (2246.), the rotation and vibration being then equally free, and the same as the corresponding movements of bismuth or heavy glass. The obstruction is at a maximum when the axis of rotation is perpendicular to the lines of magnetic force, and when the bar or cube, &c. is near to the magnet.

2329. Without going much into the particular circumstances, I may say that the effect is fully explained by the electric currents induced in the copper mass. By reference to the Second Series of these Researches (160.)[1], it will be seen that when a globe, subject to the action of lines of magnetic force, is revolving on an axis perpendicular to these lines, an electric current runs round it in a plane parallel to the axis of rotation and to the magnetic lines, producing consequently a magnetic axis in the globe, at right angles to the magnetic curves of the inducing magnet. The magnetic poles of this axis therefore are in that direction which, in conjunction with the chief magnetic pole, tends to draw the globe back against the direction in which it is revolving. Thus, if a piece of copper be revolving before a north magnetic pole, so that the parts nearest the pole move towards the right-hand, then the right-hand side of that copper will have a south magnetic state, and the left-hand side a north magnetic state; and these states will tend to counteract the motion of the copper towards the right-hand: or if it revolve in the contrary direction, then the right-hand side will have a south magnetic state, and the left-hand side a north magnetic state. Whichever way, therefore, the copper tends to revolve on its own axis, the instant it moves, a power is evolved in such a direction as tends to stop its motion and bring it to rest. Being at rest in reference to this direction of motion, then there is no residual or other effect which tends to disturb it, and it remains still.

2330. If the whole mass be moving parallel to itself, and be small in comparison with the face of the magnetic pole opposite to which it is placed, then, though it pass through the magnetic lines of force, and consequently have a tendency to the formation of magneto-electric currents within it, yet as all parts move with equal velocity and in the same direction through similar magnetic lines of force, the tendency to the formation of a current is the same in every part, and there is no actual production

[1] Philosophical Transactions, 1832, p. 168.

of current, and consequently nothing occurs which can in any way interfere with its freedom of motion. Hence the reason that though the rotation of the bar or cube (2324. 2328.) upon its own axis is stopped, its vibration as a pendulum is not affected.

2331. That neither the one nor the other motion is affected when the bar or cube is over the vertical pole (2328.), is simply because in both cases (with the given dimensions of the pole and the moving metal) the lines of particles through which the induced currents tend to move are parallel throughout the whole mass; and therefore, as there is no part by which the return of the current can be carried on, no current can be formed.

2332. Before proceeding to the explanation of the other phenomena, it will be necessary to point out the fact generally understood and acknowledged, I believe, that *time* is required for the development of magnetism in an iron core by a current of electricity; and also for its fall back again when the current is stopped. One effect of the gradual rise in power was referred to in the last series of these Researches (2170.). This time is probably longer with iron not well annealed than with very good and perfectly annealed iron. The last portions of magnetism which a given current can develope in a certain core of iron, are also apparently acquired more slowly than the first portions; and these portions (or the condition of iron to which they are due) also appear to be lost more slowly than the other portions of the power. If electric contact be made for an instant only, the magnetism developed by the current disappears as instantly on the breaking of the current, as it appeared on its formation; but if contact be continued for three or four seconds, breaking the contact is by no means accompanied by a disappearance of the magnetism with equal rapidity.

2333. In order to trace the peculiar effect of the copper, and its cause, let us consider the condition of the horizontal bar (2310. 2313.) when in the equatorial position, between the two magnetic poles, or before a single pole; the point of suspension being in a line with the axis of the pole and its exciting wire helix. On sending an electric current through the helix, both it and the magnet it produces will conduce to the formation of currents in the copper bar in the contrary direction. This is shown from my former researches (26.), and may be proved, by placing a small or large wire helix-shaped (if it be desired) in the form of the bar, and carrying away the currents produced in it, by wires to a galvanometer at a distance. Such currents, being produced in the copper, only continue whilst the magnetism of the core is rising, and then cease (18. 39.), but *whilst* they continue, they give a virtual magnetic polarity to that face of the copper bar which is opposite to a certain pole, the polarity being the same in kind as the pole it faces. Thus on the side of the bar facing the north pole of the magnet, a north polarity will be developed; and on that side facing the south pole, a south polarity will be generated.

2334. It is easy to see that if the copper during this time were opposite only one pole, or being between two poles, were nearer to one than the other, this effect would cause its repulsion. Still, it cannot account for the whole amount of the repulsion observed alike with copper as with bismuth (2295.), because the currents are of but momentary duration, and the repulsion due to them would cease with them. They do, however, cause a brief repulsive effort, to which is chiefly due the first part of the peculiar effect.

2335. For if the copper bar, instead of being parallel to the face of the magnetic pole, and therefore at right angles to the resultant of magnetic force, be inclined, forming, for instance, an angle of 45° with the face, then the induced currents will move generally in a plane corresponding more or less to that angle, nearly as they do in the examining helix (2333.), if it be inclined in the same manner. This throws the polar axis of the bar of copper on one side, so that the north polarity is not directly opposed to the north pole of the inducing magnet, and hence the action both of this and the other magnetic pole upon the two polarities of the copper will be to send it further round, or to place it edgeways to the poles, or with its breadth parallel to the magnetic resultant passing through it (2323.): the bar therefore receives an impulse, and the angle of it nearest to the magnet appears to be pulled up towards the magnet. This action of course stops the instant the magnetism of the helix core ceases to rise, and then the motion due to this cause ceases, and the copper is simply subject to the action before described (2295.). At the same time that this twist or small portion of a turn round the point of suspension occurs, the centre of gravity of the whole mass is repelled, and thus I believe all the actions up to this condition of things is accounted for.

2336. Then comes the *revulsion* which occurs upon the cessation of the electric current, and the falling of the magnetism in the core. According to the law of magneto-electric induction, the disappearance of the magnetic force will induce brief currents in the copper bar (28.), but in the contrary direction to those induced in the first instance; and therefore the virtual magnetic pole belonging to the copper for the moment, which is nearest the north end of the electro-magnet, will be a south pole; and that which is furthest from the same pole of the magnet will be a north pole. Hence will arise an exertion of force on the bar tending to turn it round its centre of suspension in the contrary direction to that which occurred before, and hence the apparent revulsion; for the angle nearest the magnetic pole will recede from it, the broad face (2323.) or length (2315.) of the bar will come round and face towards the magnet, and an action the reverse in every respect of the first action will take place, except that whereas the motion was then only a few degrees, now it may extend to two or three revolutions.

2337. The cause of this difference is very obvious. In the first instance, the bar of copper was moving under influences powerfully tending to retard and stop it (2329.); in the second case these influences are gone, and the bar revolves freely with a force proportionate to the power exerted by the magnet upon the currents induced by its own action.

2338. Even when the copper is of such form as not to give the oblique resultant of magnetic action from the currents induced in it, when, for instance, it is a cube or a sphere, still the effect of the action described above is evident (2325.). When a plate of copper about three-fourths of an inch in thickness, and weighing two pounds, was sustained upon some loose blocks of wood and placed about 0·1 of an inch from the face of the magnetic pole, it was repelled and held off a certain distance upon the making and continuing of electric contact at the battery; and when the battery current was stopped, it returned towards the pole; but the return was much more powerful than that due to gravity alone (as was ascertained by an experiment), the plate being at that moment actually *attracted*, as well as tending by gravitation towards the magnet, so that it gave a strong tap against it.

2339. Such is, I believe, the explanation of the peculiar phænomena presented by copper in the magnetic field; and the reason why they appear with this metal and not with bismuth or heavy glass, is almost certainly to be found in its high electro-conducting power, which permits the formation of currents in it by inductive forces, that cannot produce the same in a corresponding degree in bismuth, and of course not at all in heavy glass.

2340. Any ordinary magnetism due to metals by virtue of their inherent power, or the presence of small portions of the magnetic metals in them, must oppose the development of the results I have been describing; and hence metals not of absolute purity cannot be compared with each other in this respect. I have, nevertheless, observed the same phænomena in other metals; and as far as regards the sluggishness of rotatory motion, traced it even into bismuth. The following are the metals which have presented the phænomena in a greater or smaller degree:—

Copper.	Mercury.
Silver.	Platinum.
Gold.	Palladium.
Zinc.	Lead.
Cadmium.	Antimony.
Tin.	Bismuth.

2341. The accordance of these phænomena with the beautiful discovery of Arago[1], with the results of the experiments of Herschel and Babbage[2], and with my own former inquiries (81.)[3], is very evident. Whether the effect obtained by Ampère, with his copper cylinder and helix[4], was of this nature, I cannot judge, inasmuch as the circumstances of the experiment and the energy of the apparatus are not sufficiently stated; but it probably may have been.

[1] Annales de Chimie, xxvii. 363; xxviii. 325; xxxii. 213. I am very glad to refer here to the Comptes Rendus of June 9, 1845, where it appears that it was M. Arago who first obtained his peculiar results by the use of electro- as well as common magnets.

[2] Philosophical Transactions, 1825, p. 467.

[3] Ibid. 1832, p. 146.

[4] Bibliothèque Universelle, xxi. p. 48.

2342. As, because of other duties, three or four weeks may elapse before I shall be able to complete the verification of certain experiments and conclusions, I submit at once these results to the attention of the Royal Society, and will shortly embody the account of the action of magnets on magnetic metals, their action on gases and vapours, and the general considerations in another series of these Researches.

Royal Institution,
Nov. 27, 1845.

TWENTY-FIRST SERIES[1].

§ 27. *On new magnetic actions, and on the magnetic condition of all matter—continued.* ¶ v. *Action of magnets on the magnetic metals and their compounds.* ¶ vi. *Action of magnets on air and gases.* ¶ vii. *General considerations.*

Received December 24, 1845,—Read January 8, 1846.

¶ v. *Action of magnets on the magnetic metals and their compounds.*

2343. The magnetic characters of iron, nickel and cobalt, are well known; and also the fact that at certain temperatures they lose their usual property and become, to ordinary test and observation, non-magnetic; then entering into the list of diamagnetic bodies and acting in like manner with them. Closer investigation, however, has shown me that they are still very different to other bodies, and that though inactive when hot, on common magnets or to common tests, they are not so absolutely, but retain a certain amount of magnetic power whatever their temperature; and also that this power is the same in character with that which they ordinarily possess.

2344. A piece of iron wire, about 1 inch long and 0·05 of an inch in diameter, being thoroughly cleaned, was suspended at the middle by a fine platinum wire connected with the suspending thread (2249.) so as to swing between the poles of the electro-magnet. The heat of a spirit-lamp was applied to it, and it soon acquired a temperature which rendered it quite insensible to the presence of a good ordinary magnet, however closely it was approached to the heated iron. The temperature of the iron was then raised considerably higher by adjustment of the flame, and the electro-magnet thrown into action. Immediately the hot iron became magnetic and pointed between the poles. The power was feeble, and in this respect the state of the iron

[1] Philosophical Transactions, 1846, p. 41.

was in striking contrast with that which it had when cold; but in character the force was precisely the same.

2345. The iron was then allowed to fall in temperature slowly so that its assumption of the higher magnetic condition might be observed. The intensity of the force did not appear to increase until the temperature arrived near a certain point, and then as the heat continued to diminish, the iron rapidly, but not instantaneously, acquired its high magnetic power; at which time it could not be kept from the magnet, but flew to it, bending the suspending wire and trembling as it were with magnetic energy as it adhered by one end to the core.

2346. A small bar of nickel was submitted to an experimental examination in the same manner. This metal, as I have shown[1], loses its magnetism as respects ordinary tests at a heat below that of boiling oil, and hence it is very well fitted to show whether the magnetic metals can have their power entirely removed by heat or not; and also whether the disappearance of the whole or greater portion of their power is sudden or gradual. The smallness of the mass to be experimented on assisted much in the determination of the latter point. Upon being heated the nickel soon became indifferent to ordinary magnets; but however high the temperature, still it pointed to and was attracted by the electro-magnet. The power was very feeble, but certain. It was scarcely enough to sustain the weight of the nickel by the magnetic action alone; but was abundantly evident when the metal was supported as described (2344.).

2347. On carefully lowering the temperature of the nickel, it was again found that the transition from one degree of magnetic force to the other was progressive and not instantaneous. With iron it is difficult to preserve all the parts, either in heating or cooling, so nearly at the same temperature as to be sure that it is not the union of hotter and colder portions which gives the appearance of an intermediate degree of magnetism; but with nickel that is not so difficult, for the progression is more gradual, so that when in cooling the power began to increase, the cooling might be continued some time before the full degree of power came on; at any time in that period the temperature might be slightly raised, and though the power would then

[1] Philosophical Magazine, 1836, vol. viii. p. 179, or Experimental Researches, vol. ii. p. 219.

diminish a little, it could yet be retained at a degree stronger than the weakest. In fact it was easy to *keep* the nickel at many of the intermediate degrees of power, and thus to remove all doubt of the progressive assumption of the full degree of force.

2348. I have expressed an opinion, founded on the different temperatures at which the magnetic metals appeared to lose their peculiar power[1], that all the metals would probably have the same character of magnetism if their temperature could be lowered sufficiently. The facts just described appear to me entirely against such an opinion. The metals which are magnetic retain a portion of their power after the great change has been effected, or in what might be called their diamagnetic state; but the other metals, such as bismuth, tin, &c., present no trace of this power, and therefore are not in the condition of the heated iron, nickel, or cobalt; for in fact whilst these point axially and are attracted, the others point equatorially and are repelled. I therefore hope to be allowed to withdraw the view I then put forth.

2349. I next proceeded to examine the peroxides of iron, and in accordance with the observations of M. Becquerel[2] and others found them all, both natural and artificial, possessed of magnetic power at common temperatures. I heated them in tubes but found them still magnetic, suffering *no* diminution of the force by such temperature as I could apply to them.

2350. Different specimens of the oxide of nickel were found to present the same phænomena. They were magnetic both when hot and cold; and that heat should cause no change in this respect is the more striking, because the hot oxide had a temperature given to it far higher than that necessary to produce the great magnetic change in the metal itself (2346.).

2351. The oxide of cobalt also was magnetic, and equally magnetic whether hot or cold. Glass coloured blue by cobalt is magnetic in consequence of the presence of the oxide of that metal, and is so whether hot or cold. In all these cases the degree of power retained was very small compared to that of the pure metal.

[1] Philosophical Magazine, 1836, vol. viii. p. 177; ibid. 1839, vol. xiv. p. 161, or Experimental Researches, vol. ii. pp. 217, 225.

[2] Annales de Chimie, 1827, vol. xxxvi. p. 337. Comptes Rendus, 1845, vol. xx. p. 1708.

2352. Proceeding to the *salts* of iron, I found them magnetic. Clean crystals of the proto-sulphate of iron were attracted and pointed axially very well; so also did the dry salt. As I proceeded I found that every salt and compound containing iron in the basic part was magnetic. To enumerate the different substances subjected to trial would be tedious; the following are selected as illustrations of the variety in kind:—

Protochloride.	Protophosphate.
Perchloride.	Perphosphate.
Iodide.	Nitrate.
Protosulphate.	Carbonate.
Persulphate.	Prussian Blue.

2353. Amongst native compounds—

Bog iron ore.	Yellow sulphuret of iron.
Hæmatite.	Arsenical pyrites.
Chromate of iron.	Copper pyrites, and many others were magnetic.

2354. Green bottle-glass is comparatively very magnetic from the iron it contains, and cannot be used as tubes to hold other substances. Crown glass is magnetic from the same cause. Flint glass is not magnetic, but points equatorially.

2355. Crystals of the yellow ferro-prussiate of potassa were not magnetic, but were repelled and set equatorially; and such was the case also with red ferro-prussiate.

2356. According to my hopes, even the solutions of the ferruginous salts, whether in water or alcohol, were magnetic. A tube filled with a clear solution of proto- or persulphate of iron, or proto- or perchloride, or tincture of muriate of iron, was attracted by the poles, and pointed very well between them in the axial direction.

2357. These solutions supply a very important means of advancing magnetical investigation, for they present us with the power of making a magnet, which is at the same time liquid, transparent, and within certain limits, adjustible to any degree of strength. Hence the power of examining a magnet optically. Hence also, the capability of placing magnetic portions of matter one within another, and so observing dynamic and other phænomena within magnetic media. In fact, not only may these substances be placed as magnets in the magnetic field, but the field generally may be filled with them, and then other bodies and other magnets examined as to their joint or separate actions in it (2361. &c.,).

2358. In reference to the salts of *nickel* and *cobalt,* pure crystals of the sulphate of nickel were found to be well magnetic, and also pure crystals of sulphate of cobalt. Solutions of the sulphate of nickel, the chloride of nickel, and the chloride of cobalt, were also magnetic. That I might be perfectly safe in these conclusions I applied to Mr. Askin of Birmingham, whose power of separating nickel and cobalt from each other and other metals is well known, as also the scale upon which he carries on these operations; and he favoured me with a solution of chloride of nickel and another of chloride of cobalt perfectly pure, both of which proved to be well magnetic between the poles of my magnet.

2359. Heat applied to any of these magnetic solutions did not diminish or affect their power.

2360. These results with the salts of the magnetic metals conjoin with those before quoted, as tending to show that the non-magnetic metals could not by any change of temperature be rendered magnetic (2398.), but as a class are distinct from iron, nickel, and cobalt; for none of the compounds of the non-magnetic metals show, as yet, any indication of ordinary magnetic force, whereas in respect of these three substances all their compounds possess it.

2361. In illustration of the power which the iron and other similar solutions give in the investigation of magnetic phænomena (2357.), as well as in reference to the general conclusions to be drawn from all the facts described in this paper, I will proceed to describe certain anticipated results which were obtained by the employment of these solutions in the magnetic field.

2362. A clear solution of the proto-sulphate of iron was prepared, in which one ounce of the liquid contained seventy-four grains of the hydrated crystals; a second solution was prepared containing one volume of the former and three volumes of water; a third solution was made of one volume of the stronger solution and fifteen volumes of water. These solutions I will distinguish as Nos. 1, 2, and 3; the proportions of crystals of sulphate of iron in them were respectively as 16, 4, and 1 per. cent.

nearly. These numbers may, therefore, be taken as representing (generally only (2423.)) the strength of the magnetic part of the liquids.

2363. Tubes like that before described (2279.) were prepared and filled respectively with these solutions and then hermetically sealed, as little air as possible being left in them. Glasses of the solutions were also prepared, large enough to allow the tubes to move freely in them, and yet of such size and shape as would permit of their being placed between the magnetic poles. In this manner the action of the magnetic forces upon the matter in the tubes could be examined and observed, both when the tubes were in diamagnetic media, as air, water, alcohol, &c., and also in magnetic media, either stronger or weaker in magnetic force, than the substances in the tubes.

2364. When these tubes were suspended in air between the poles, they all pointed axially or magnetically, as was to be expected; and with forces apparently proportionate to the strengths of the solutions. When they were immersed in alcohol or water, they also pointed in the same direction; the strongest solution very well, and also the second, but the weakest solution was feeble in its action, though very distinct in its character (2422.).

2365. When the tubes, immersed in the different ferruginous solutions, were acted upon, the results were very interesting. The tube No. 1 (the strongest magnetically), when in solution No. 1, had no tendency, under the influence of the magnetic power, to any particular position, but remained wherever it was placed. Being placed in solution No. 2, it pointed well axially, and in solution No. 3 it took the same direction, but with still more power.

2366. The tube No. 2, when in the solution No. 1, pointed equatorially, *i. e.* as heavy glass, bismuth, or a diamagnetic body generally, in air. In solution No. 2 it was indifferent, not pointing either way; and in solution No. 3 it pointed axially, or as a magnetic body. The tube No. 3, containing the weakest solution, pointed equatorially in solutions No. 1 and 2, and not at all in solution No. 3.

2367. Several other ferruginous solutions varying in strength were prepared, and, as a general and constant result, it was found that any tube pointed axially if the solution in it was stronger than the surrounding solution, and equatorially if the tube solution was the weaker of the two.

2368. The tubes were now suspended vertically, so that being in the different solutions they could be brought near to one of the magnetic poles, and employed in place of the indicating cube or sphere of bismuth, or heavy glass (2266.). The constant result was, that when the tube contained a stronger solution than that which surrounded it, it was attracted to the pole, but when its solution was the weaker of the two it was repelled. The latter phænomena were as to appearance in every respect the same as those presented in the repulsion of heavy glass, bismuth, or any other diamagnetic body in air.

2369. Having described these phænomena, I will defer their further consideration until I arrive at the last division of this paper, and proceed to certain results more especially belonging to the present part of these Researches.

2370. As the magnetic metals, iron, nickel and cobalt, present in their compounds substances also distinguished by the possession of magnetic properties (2360.), so it appeared very probable that other metals, of whose magnetic character doubts were entertained, because of the possible presence of iron in the specimens experimented with, might in this way have their magnetic character tested; for it seemed likely, from analogy, that every metal well magnetic *per se,* would be magnetic in its compounds; and, judging from the character of the great class of diamagnetic bodies (2275.), that no magnetic compounds would be obtained of a metal not magnetic of itself. Accordingly I proceeded to apply this kind of test to the combinations of many of the metals, and obtained the following results:—

2371. *Titanium.*—Wollaston has described the magnetic effects of crystals of titanium, expressing at the same time a belief that they are due to iron[1]. I took a specimen of the oxide of titanium, which I believe to be perfectly free from iron, and inclosing it in a tube (2279.), subjected it to the action of the electro-magnet (2246. 2247.). It proved to be freely magnetic. Another specimen obtained from Mr. Johnson, and believed by him to be perfectly free from iron, was also magnetic. Hence I conclude that titanium is truly a magnetic metal.

[1] Philosophical Transactions, 1823, p. 400.

2372. *Manganese.*—Berthier, as far as I am aware, first announced that this metal was magnetic at very low temperatures[1]. On submitting specimens of the various oxides, which were considered as pure, to the magnetic force, they were all found to be magnetic, especially the protoxide. So were the following compounds of manganese in the pure, dry, or crystallized state:—chloride, sulphate, ammonio-sulphate, phosphate, carbonate, borate; and also the chloride, nitrate, sulphate, and ammonio-sulphate when in solution. A specimen of the ammonio-sulphate was rendered alkaline by the addition of a little carbonate of ammonia boiled and then carefully crystallized thrice: after that the crystals and solution of the purified salt were perfectly and well magnetic. I have no doubt, therefore, that manganese is a magnetic metal, as Berthier said. If any opinion may be drawn concerning the magnetic force of the metal from the degree of magnetism of the compounds, I should expect that manganese possesses considerable power of this kind when at a sufficiently low temperature[2].

2373. *Cerium.*—I am not aware that cerium has as yet been classed with the magnetic metals. Having made experiments with the hydrated protoxide, the carbonate, and the chloride of this metal, and also with the double sulphate of the oxide and potassa prepared with great care, I found them all magnetic; and those that are soluble are magnetic in the state of solution. Hence, as the compounds are undoubtedly magnetic, there is every reason to believe that cerium also is a magnetic metal (2370.).

2374. *Chromium.*—The magnetic phænomena of chromium compounds are very interesting. Portions of the chromate and the bichromate of potassa were purified by three careful crystallizations each; part of the bichromate was heated in a platinum crucible, until the second equivalent of chromic acid was converted into the crystallized oxide, and this being washed out and dried was found to be well magnetic. So were all the other specimens of oxide of chromium which were examined.

A specimen of Warrington's chromic acid was found to be very feebly magnetic.

[1] Traité des Essais par la Voie Sèche, tome i. p. 532. Philosophical Magazine, 1845, vol. xxvii. p. 2.

[2] Philosophical Magazine, 1845, vol. xxvii. p. 2.

2375. Chromate of lead, when subjected to the magnet, pointed equatorially and was repelled. Such was the case also with crystals of the chromate of potassa. Crystals of the bichromate, however, did not act thus; for if in any way affected they were in the least degree magnetic, showing the influence of the increased proportion of chromic acid. Solutions of either salt pointed well equatorially and were repelled; thus showing the diamagnetic influence of the water present (2422.).

2376. As just stated, a solution of the bichromate contained in a tube, pointed equatorially and was repelled; but if the same solution had a little alcohol added to it, and also some pure muriatic or sulphuric acid, and were then heated for a few minutes to reduce the chromic acid to the state of oxide or chloride, then, on being returned to the tube and subjected to the magnet, it was found strongly magnetic.

2377. I think it has before been said that chromium is a magnetic metal; as these results have been obtained with its pure compounds, there is no longer any doubt on my mind that such is the case.

2378. *Lead.*—The compounds of lead point equatorially and are repelled. The substances tried were the chloride, iodide, sulphuret, nitrate, sulphate, phosphate, carbonate, protoxide fused, and the acetate. A portion of very carefully crystallized nitrate being dissolved was precipitated by pure zinc, and the lead obtained washed with dilute nitric acid, to remove subsalts. Such lead was free from magnetism, and therefore the metal ranks in the diamagnetic class, both directly and by its compounds. Lead usually appears to be magnetic, and it is not very easy to obtain the metal in the pure diamagnetic state.

2379. *Platinum.*—I have, as yet, found no wrought specimens of this metal free from magnetism, not even those prepared by Dr. Wollaston himself, and left with the Royal Society. Specimens of the purest platinum obtained from Mr. Johnson were also found to be slightly magnetic.

2380. Clean platinum foil and cuttings were dissolved in pure nitro-muriatic acid, and the solution evaporated to dryness. Both the solution and the dry chloride pointed equatorially and were repelled by the magnet. A part of the chloride, being dissolved and rendered acid, was precipitated by an acid solution of muriate of ammonia, and the ammonio-chloride of platinum washed and dried: it also, at the magnet, pointed equatorially and was repelled. A portion of this ammonio-chloride, decomposed in a flint-glass tube by heat, gave spongy platinum, which being pressed together into a cake, pointing *axially* and was attracted at the side of the magnetic pole, being magnetic.

2381. At present I believe that platinum is as a metal magnetic, though very slightly so; and that in the compounds, the change of state and the presence of other substances having the diamagnetic character, are sufficient to cover this property and make the whole compound diamagnetic (2422.).

2382. *Palladium.*—All the palladium in the possession of the Royal Society, prepared by Dr. Wollaston, amounting to ten ingots and rolled plates, is magnetic. Specimens of the metal from Mr. Johnson, considered as pure, were also slightly magnetic. The chloride, the ammonio-bichloride, and the cyanuret of palladium, pointed equatorially and were repelled by the magnet. The same cyanuret, reduced by heat either in open platinum vessels or in close glass tubes, gave palladium possessing a feeble degree of magnetic property. Some of Wollaston's palladium was dissolved in pure nitromuriatic acid, and the solution slowly acted upon by pure zinc, free from iron, and not magnetic. Five successive portions of the precipitated metal were collected, and *all* were *magnetic.* Ammonio-bichloride of palladium was prepared from the same solution by pure acid muriate of ammonia, and digested in nitromuriatic acid. The salt itself was repelled, being diamagnetic; but when reduced by heat in glass tubes, or in Berlin capsules, the palladium obtained was magnetic. From the result of all the experiments, I believe the metal to be feebly but truly magnetic.

2383. *Arsenic.*—This metal required very particular examination, and even when carefully sublimed twice or thrice in succession, presented appearances which sometimes made me class it with the magnetic, and at other times with the diamagnetic bodies. On the whole, I incline to believe that it belongs to the latter series of substances, being only in a very small degree removed from the zero or medium point. Pure white arsenic points freely in an equatorial direction, and is repelled by a magnetic pole.

2384. In reference to the pointing of short bars between magnetic poles exposing large flat faces, I ought to observe, that such bars will sometimes point axially and seem to be magnetic when they do not belong to that class, and are repelled by a single pole. The cause of this effect has been already given (2298. 2299.), and is obviated by the use of poles having wedge-shaped or conical terminations.

2385. *Osmium.*—Osmic acid from Mr. Johnson, in fine transparent crystals, was clearly diamagnetic, being repelled. Specimens of the metal and of the protoxide were both slightly magnetic. The protoxide had been obtained by the action of alcohol on a solution of osmic acid which had twice been distilled with water, and the metal was believed to be perfectly free from other substances. Probably, therefore, osmium belongs to the magnetic class.

2386. *Iridium.*—Mr. Johnson supplied me with several preparations of iridium. The oxide, chloride, and ammonio-chloride were magnetic; and so was a sample of the metal. One specimen of the metal, which seemed to be very pure, was scarcely at all magnetic; and on the whole, I incline to believe that iridium does not stand in the magnetic class.

2387. *Rhodium.*—A well-fused specimen of this metal, prepared by Dr. Wollaston, was magnetic; but crystals of the chloride and the sodio-chloride of rhodium prepared by the same philosopher, and others also from Mr. Johnson, were not magnetic, but pointed well equatorially. I conclude, therefore, that the metal is probably not magnetic, or if magnetic, is but little removed from the zero point.

2388. *Uranium.*—Peroxide of this metal was obtained not magnetic; protoxide very slightly magnetic: I have set the metal for the present in the diamagnetic class.

2389. *Tungsten.*—The oxide of this metal, and also the acid, were submitted to examination, and found to point well equatorially. The acid was distinctly repelled by a single magnetic pole; the oxide appeared nearly neutral. Hence I have, for the present, considered tungsten as a diamagnetic metal.

2390. *Silver* is not magnetic (2291.), nor its compounds.

2391. *Antimony* is not magnetic (2291.), nor its compounds.

2392. *Bismuth* is not magnetic (2291.), nor its compounds.

Having tried many of the compounds of each of these three metals, I thought it well to record the accordance existing between them and their metallic bases (2370.).

2393. *Sodium.*—A fine large globule, equal to half a cubic inch in size, was well repelled, and is therefore diamagnetic.

2394. *Magnesium.*—None of the compounds or salts of this base are magnetic.

2395.	Calcium.	Sodium.
	Strontium.	Potassium.
	Barium.	Ammonia.

None of the compounds or salts of these substances are magnetic.

2396. From the characters, therefore, of the compounds, as well as from direct evidence in respect of some of the metals, it would appear that, besides iron, nickel, and cobalt, the following are also magnetic; namely, titanium, manganese, cerium, chromium, palladium, platinum. It is, however, very probable that there may be metals possessing distinct magnetic power, yet in so slight a degree, as, like platinum and palladium, not to exhibit in their compounds any sensible trace of it. Such may be the case with tungsten, uranium, rhodium, &c.

2397. I have heated several of the diamagnetic metals, even up to their fusing-points, but have not been able to observe any change, either in the character or degree of their magnetic relations.

2398. Perhaps the cooling of some of the metals, whose compounds, like those of iron, nickel and cobalt, are magnetic, might develope in them a much higher degree of force, than any which they have as yet been known to possess. Manganese, chromium, cerium, titanium, are metals of much interest in this point of view. Osmium, iridium, rhodium and uranium, ought to be subjected with them to the same trial.

2399. The following is an attempt to arrange some of the metals in order, as respects their relation to magnetic force. The 0° or medium point is supposed to be the condition of a metal or substance indifferent to the magnetic force as respects attraction or repulsion in air or space. The further substances are placed from this point, the more distinctive are they as regards their attraction or repulsion by the magnet. Nevertheless this order may, very probably, be found inaccurate by more careful observation.

Magnetic.	Diamagnetic.
	Bismuth.
	Antimony.
	Zinc.
	Tin.
	Cadmium.
	Sodium.
Iron.	Mercury.
Nickel.	Lead.
Cobalt.	Silver.
Manganese.	Copper.
Chromium.	Gold.
Cerium.	Arsenic.
Titanium.	Uranium.
Palladium.	Rhodium.
Platinum.	Iridium.
Osmium.	Tungsten.

0°

¶ vi. *Action of magnets on air and gases.*

2400. It was impossible to advance in an experimental investigation of the kind now described, without having the mind impressed with various theoretical views of the mode of action of the bodies producing the phænomena. In the passing consideration of these views, the apparently middle condition which *air* held between magnetic and diamagnetic substances was of the utmost interest, and led to many experiments upon its probable influence, which I will now proceed briefly to describe.

2401. A thin flint-glass tube, in which common air was hermetically enclosed, was placed between the magnetic poles (2249.) surrounded by air, and the effect of the magnetic force observed upon it. There was a very feeble tendency of the tube to an equatorial position, due to the substance of the tube in which the air was enclosed.

2402. The air was then withdrawn from around the tube more or less, and at last up to the highest amount which a good air-pump would effect; but whatever the degree of rarefaction, the tube of air still seemed to be affected exactly in the same manner as if surrounded by air of its own density.

2403. I then surrounded the air-tube with hydrogen and carbonic acid in succession; but in both these, and in each of them at different degrees of rarefaction, the tube of air remained as indifferent as before.

2404. Hence there appears to be no sensible distinction between dense or rare air; or, as far as these experiments go, between one gas or vapour and another.

2405. As it did not seem at all unlikely that the equatorial and axial set of bodies, or their repulsions and attractions, might depend upon converse actions of the media by which they were surrounded (2361.), so I proceeded to examine what would occur with diamagnetic substances, when the air or gas which surrounded them was changed in its density or nature, or what would happen to air itself when surrounded by these substances.

2406. The air tube (2401.) was suspended horizontally in water (being retained below the surface by a cube of bismuth attached to it, just beneath the point of suspension, which therefore could have no power of giving it direction); it was then subjected to the magnetic forces, and imediately pointed well in an axial direction, or as a magnet would have done. Being brought near to one pole, it moved, on the supervention of the magnetic force, appearing as if *attracted* after the manner of a magnetic body; and this continued as long as the magnetic force was sustained in action.

2407. The air-tube was in like manner subjected to the action of the magnetic force, when surrounded by alcohol, and also by oil of turpentine, with precisely the same results as in water. In all these cases the action of air in the fluids was precisely the same as the action of a magnetic body in air. The air-tube was subjected to the action of the magnet even when under the surface of mercury, and here also it pointed axially.

2408. In order to extend the experimental relations of air and gases, I proceeded to place substances of the diamagnetic class in them. Thus the bar of heavy glass (2253.) was suspended in a jar of air, and then the air about it more or less rarefied, but as before, in the case of the air-tube (2402.), alterations of this kind produced no effect. Whether the bar were in air at the ordinary pressure, or as rare as the pump could render it, it still pointed equatorially, and apparently always with the same degree of force.

2409. The bar of bismuth (2296.) was suspended in the jar and the same alteration in the density of the air made as before; but this caused no difference in the action of the bismuth, either in kind or degree. Carbonic acid and hydrogen gases were then introduced in succession into the jar, and these also were employed in different degrees of rarefaction, but the results were the same; no change took place in the action on the bismuth.

2410. A bismuth cube was suspended in air and gases at ordinary pressure, and also rarefied as much as could be, and under these circumstances it was brought near the magnetic pole and its repulsion observed; its action was in all these cases precisely the same as in the atmosphere.

2411. The perpendicular copper bar (2323.) was suspended near the magnetic pole *in vacuo*, but its set, sluggish movements and revulsion were just the same as before in air (2324.).

2412. The following preparations in tubes (2401.), namely, a vacuum, air, hydrogen, carbonic acid gas, sulphurous acid gas, and vapour of æther, were surrounded by water, and then subjected to the magnetic force; they all pointed axially, and, as far as I could perceive, with equal force. Being placed in alcohol, the same effect occurred.

2413. The same preparations being surrounded by air, or by carbonic acid gas, all set equatorially.

2414. The axial position of the tubes in the liquid (2412.) depends, doubtless, upon the relation of the contents of the tube to the surrounding medium; for as far as the matter of the tube is concerned, it alone would have tended to give the equatorial position. In the following succeeding experiments (2413.), where the tubes of gases were in surrounding gases, the equatorial position is due to this effect of the glass of the tube; and that it should produce its constant feeble effect, undisturbed by all the variations of the gases and vapours, is a proof how like and how indifferent these are one to the other.

2415. I suspended a tube of liquid sulphurous acid in gaseous sulphurous acid; when under the magnetic influence, the liquid pointed well equatorially. I surrounded liquid nitrous acid by gaseous nitrous acid; the liquid pointed well equatorially. I placed liquid æther in the vapour of æther; the former pointed equatorially. Upon suspending the tube of vapour of æther in liquid æther, the vapour pointed axially.

2416. In every kind of trial, therefore, and in every form of experiment, the gases and vapours still occupy a medium position between the magnetic and the diamagnetic classes. Further, whatever the chemical or other properties of the substances, however different in their specific gravity, or however varied in their own degree of rarefaction, they all become alike in their magnetic relation, and apparently equivalent to a perfect vacuum. Bodies which are very marked as diamagnetic substances, immediately lose all traces of this character when they become vaporous (2415.). It would be exceedingly interesting to know whether a body from the magnetic class, as chloride of iron, would undergo the same change.

¶ vii. *General considerations.*

2417. Such are the facts which, in addition to those presented by the phænomena of light, establish a magnetic action or condition of matter new to our knowledge. Under this action, an elongated portion of such matter usually (2253. 2384.) places itself at right angles to the lines of magnetic force; this result may be resolved into the simpler one of repulsion of the matter by either magnetic pole. The set of the elongated portion, or the repulsion of the whole mass, continues as long as the magnetic force is sustained, and ceases with its cessation.

2418. By the exertion of this new condition of force, the body moved may pass either *along* the magnetic lines or *across* them; and it may move along or across them in either or any direction. So that two portions of matter, simultaneously subject to this power, may be made to approach each other as if they were mutually attracted, or recede as if mutually repelled. All the phænomena resolve themselves into this, that a portion of such matter, when under magnetic action, tends to move from stronger to weaker places or points of force. When the substance is surrounded by lines of magnetic force of equal power on all sides, it does not tend to move, and is then in marked contradistinction with a linear current of electricity under the same circumstances.

2419. This condition and effect is new, not *only* as it respects the exertion of power by a magnet over bodies previously supposed to be indifferent to its influence, but is *new* as a magnetic action, presenting us with a second mode in which the magnetic power can exert its influence. These two modes are in the same general antithetical relation to each other as positive and negative in electricity, or as northness and southness in polarity, or as the lines of electric and magnetic force in magneto-electricity; and the diamagnetic phænomena are the more important, because they extend largely, and in a new direction, that character of duality which the magnetic force already, in a certain degree, was known to possess.

2420. All matter appears to be subject to the magnetic force as universally as it is to the gravitating, the electric and the chemical or cohesive forces; for that which is not affected by it in the manner of ordinary magnetic action, is affected in the manner I have now described; the matter possessing for the time the solid or fluid state. Hence substances appear to arrange themselves into two great divisions; the magnetic, and that which I have called the diamagnetic classes; and between these classes the contrast is so great and direct, though varying in degree, that where a substance from the one class will be attracted, a body from the other will be repelled; and where a bar of the one will assume a certain position, a bar of the other will acquire a position at right angles to it.

2421. As yet I have not found a single solid or fluid body, not being a mixture, that is perfectly neutral in relation to the two lists; *i. e.* that is neither attracted nor repelled in air. It would probably be important to the consideration of magnetic action, to know if there were any natural simple substance possessing this condition in the solid or fluid state. Of compound or mixed bodies there may be many; and as it may be important to the advancement of experimental investigation, I will describe the principles on which such a substance was prepared when required for use as a circumambient medium.

2422. It is manifest that the properties of magnetic and diamagnetic bodies are in opposition as respects their dynamic effects; and, therefore, that by a due mixture of bodies from each class, a substance having any intermediate degree of the property of either may be obtained. Protosulphate of iron belongs to the magnetic, and water to the diamagnetic class; and using these substances, I found it easy to make a solution which was neither attracted nor repelled, nor pointed when in air. Such a solution pointed axially when surrounded by water.

If made somewhat weaker in respect of the iron, it would point axially in water but equatorially in air; and it could be made to pass more and more into the magnetic or the diamagnetic class by the addition of more sulphate of iron or more water.

2423. Thus a *fluid* medium was obtained, which, practically, as far as I could perceive, had every magnetic character and effect of a gas, and even of a vacuum; and as we possess both magnetic and diamagnetic glass (2354.), it is evidently possible to prepare a *solid* substance possessing the same neutral magnetic character.

2424. The endeavour to form a general list of substances in the present imperfect state of our knowledge would be very premature: the one below is given therefore only for the purpose of conveying an idea of the singular association under which bodies come in relation to magnetic force, and for the purpose of general reference hereafter:—

Iron.
Nickel.
Cobalt.
Manganese.
Palladium.
Crown-glass.
Platinum.
Osmium.
0° Air and vacuum.
Arsenic.
Æther.
Alcohol.
Gold.
Water.
Mercury.
Flint-glass.
Tin.
Heavy glass.
Antimony.
Phosphorus.
Bismuth.

2425. It is very interesting to observe that metals are the substances which stand at the extremities of the list, being of all bodies those which are most powerfully opposed to each other in their magnetic condition. It is also a very remarkable circumstance, that these differences and departures from the medium condition, are in the metals at the two extremes, iron and bismuth, associated with a small conducting power for electricity. At the same time the *contrast* between these metals, as to their fibrous and granular state, their malleable and brittle character, will press upon the mind whilst contemplating the possible condition of their molecules when subjected to magnetic force.

2426. In reference to the metals, as well as the diamagnetics not of that class (2286.), it is satisfactory to have such an answer to the opinion that all bodies are magnetic as iron, as does not consist in a mere negation of that which is affirmed, but in proofs that they are in a different and opposed state, and are able to counteract a very considerable degree of magnetic force (2448.).

2427. As already stated, the magnetic force is so strikingly distinct in its action upon bodies of the magnetic and the diamagnetic class, that when it causes the attraction of the one it produces the repulsion of the other; and this we cannot help referring, in some way, to an action upon the molecules or the mass of the substances acted upon, by which they are thrown into different conditions and affected accordingly. In that point of view it is very striking to compare the results with those which are presented to us by a polarized ray, especially as then a remarkable difference comes into view; for if transparent bodies be taken from the two classes, as for instance, heavy glass or water from the diamagnetic, and a piece of green glass or a solution of green vitriol from the magnetic class, then a given line of magnetic force will cause the repulsion of one and the attraction of the other; but this same line of force, which thus affects the particles so differently, affects the polarized ray when passing through them precisely in the *same* manner in both cases; for the two bodies cause its rotation in the *same* direction (2160. 2199. 2224.).

2428. This consideration becomes even more important when we connect it with the diamagnetic and the optical properties of bodies which rotate a polarized ray. Thus the iron solution and a piece of quartz, having the power to rotate a ray, point by the influence of the *same* line of magnetic force, the one axially and the other equatorially; but the rotation which is impressed on a ray of light by these two bodies, as far as they are under the influence of the same magnetic force, is the *same* for both. Further, this rotation is quite independent of, and quite unlike that of the quartz in a most important point; for the quartz by itself can only rotate the ray in one direction, but under the influence of the magnetic force it can rotate it both to the right and left, according to the course of the ray (2231. 2232.). Or, if two pieces of quartz (or two tubes of oil of turpentine) be taken which can rotate the ray *different* ways, the further rotative force manifested by them when under the dominion of the magnetism is always the *same* way; and the direction of that way may be made either to the right or left in either crystal of quartz. All this time the *contrast* between the quartz as a diamagnetic, and the solution of iron as a magnetic body remains undisturbed. Certain considerations regarding the character of a ray, arising from these contrasts, press strongly on my mind, which, when I have had time to submit them to further experiment, I hope to present to the Society.

2429. Theoretically, an explanation of the movements of the diamagnetic bodies, and all the dynamic phænomona consequent upon the actions of magnets on them, might be offered in the supposition that magnetic induction caused in them a contrary state to that which it produced in magnetic matter; *i. e.* that if a particle of each kind of matter were placed in the magnetic field both would become magnetic, and each would have its axis parallel to the resultant of magnetic force passing through it; but the particle of magnetic matter would have its north and south poles opposite, or facing towards the contrary poles of the inducing magnet, whereas with the diamagnetic particles the reverse would be the case; and hence would result approximation in the one substance, recession in the other.

2430. Upon Ampère's theory, this view would be equivalent to the supposition, that as currents are induced in iron and magnetics parallel to those existing in the inducing magnet or battery wire; so in bismuth, heavy glass and diamagnetic bodies, the currents induced are in the contrary direction. This would make the currents in diamagnetics the same in direction as those which are induced in diamagnetic conductors at the *commencement* of the inducing current; and those in magnetic bodies the same as those produced at the *cessation* of the same inducing current. No difficulty would occur as respects non-conducting magnetic and diamagnetic substances, because the hypothetical currents are supposed to exist not in the mass, but round the particles of the matter.

2431. As far as experiment yet bears upon such a notion, we may observe, that the known inductive effects upon masses of magnetic and diamagnetic metals *are the same*. If a straight rod of iron be carried across magnetic lines of force, or if it, or a helix of iron rods or wire, be held near a magnet, as the power in it rises electric currents are induced, which move through the bars or helix in certain determinate directions (38. 114., &c.). If a bar or a helix of bismuth be employed under the same circumstances the currents are again induced, and precisely in the same direction as in the iron, so that here no difference occurs in the direction of the induced current, and not very much in its force, nothing like so much indeed as between the current induced in either of these metals and a metal taken from near the neutral point (2399.). Still there is this difference remaining between the conditions of the experiment and the hypothetical case; that in the former the induction is manifested by currents in the masses, whilst in the latter, *i. e.* in the special magnetic and diamagnetic effects, the currents, if they exist, are probably about the particles of the matter.

2432. The magnetic relation of aëriform bodies is exceedingly remarkable. That oxygen or nitrogen gas should stand in a position intermediate between the magnetic and diamagnetic classes; that it should occupy the place which *no* solid or liquid element can take; that it should show no change in its relations by rarefaction to any possible degree, or even when the space it occupies passes into a vacuum; that it should be the same magnetically with any other gas or vapour; that it should not take its place at one end but in the very middle of the great series of bodies; and that all gases or vapours should be alike, from the rarest state of hydrogen to the densest state of carbonic acid, sulphurous acid, or æther vapour, are points so striking, as to persuade one at once that air must have a great and perhaps an active part to play in the physical and terrestrial arrangement of magnetic forces.

2433. At one time I looked to air and gases as the bodies which, allowing attenuation of their substance without addition, would permit of the observation of corresponding variations in their magnetic properties; but now all such power by rarefaction appears to be taken away; and though it is easy to prepare a liquid medium which shall act with other bodies as air does (2422.), still it is not truly in the same relation to them; neither does it allow of dilution, for to add water or any such substance is to add to the diamagnetic power of the liquid; and if it were possible to convert it into vapour and so dilute it by heat, it would pass into the class of gases and be magnetically undistinguishable from the rest.

2434. It is also very remarkable to observe the apparent disappearance of magnetic condition and effect when bodies assume the vaporous or gaseous state, comparing it at the same time with the similar relation to light; for as yet no gas or vapour has been made to show any magnetic influence over the polarized ray, even by the use of powers far more than enough to manifest such action freely in liquid and solid bodies.

2435. Whether the negative results obtained by the use of gases and vapours depend upon the smaller quantity of matter in a given volume, or whether they are direct consequences of the altered physical condition of the substance, is a point of very great importance to the theory of magnetism. I have imagined, in elucidation of the subject, an experiment with one of M. Cagniard de la Tour's æther tubes, but expect to find great difficulty in carrying it into execution, chiefly on account of the strength, and therefore the mass of the tube necessary to resist the expansion of the imprisoned heated æther.

2436. The remarkable condition of air and its relation to bodies taken from the magnetic and the diamagnetic classes, causes it to point equatorially in the former and axially in the latter. Or, if the experiment presents its results under the form of attraction and repulsion, the air moves as if repelled in a magnetic medium and attracted in a medium from the diamagnetic class. Hence it seems as if the air were magnetic when compared with diamagnetic bodies, and of the latter class when compared to magnetic bodies.

2437. This result I have considered as explained by the assumption that bismuth and its congeners are absolutely repelled by the magnetic poles, and would, if there were nothing else concerned in the phænomenon than the magnet and the bismuth, be equally repelled. So also with the iron and its similars, the attraction has been assumed as a direct result of the mutual action of them and the magnets; further, these actions have been admitted as sufficient to account for the pointing of the air both axially and equatorially, as also for its apparent attraction and repulsion; the effect in these cases being considered as due to the travelling of the air to those positions which the magnetic or diamagnetic bodies tended to leave.

2438. The effects with air are, however, in these results precisely the same as those which were obtained with the solutions of iron of various strength (2365.), where *all* the bodies belonged to the magnetic class, and where the effect was evidently due to the greater or smaller degree of magnetic power possessed by the solutions. A weak solution in a stronger pointed equatorially and was repelled like a diamagnetic, not because it did not tend by attraction to an axial position, but because it tended to that position with less force than the matter around it; so the question will enter the mind, whether the diamagnetics, when in air, are repelled and tend to the equatorial position for any other reason, than that the air is more magnetic than they are, and tends to occupy the axial space. It is easy to perceive that if all bodies were magnetic in different degrees, forming one great series from end to end, with air in the middle of the series, the effects would take place as they do actually occur. Any body from the middle part of the series would point equatorially in the bodies above it and axially in those beneath it; for the matter which, like bismuth, goes from a strong to a weak point of action, may do so only because that substance, which is already at the place of weak action, tends to come to the place where the action is strong; just as in electrical induction the bodies best fitted to carry on the force are drawn into the shortest line of action. And so air in water, or even under mercury, is, or appears to be, drawn towards the magnetic pole.

2439. But if this were the true view, and air had such power amongst other bodies as to stand in the midst of them, then one would be led to expect that rarefaction of the air would affect its place, rendering it, perhaps, more diamagnetic, or at all events altering its situation in the list. If such were the case, bodies that set equatorially in it in one state of density, would, as it varied, change their position, and at last set axially: but this they do not do; and whether the rarefied air be compared with the magnetic or the diamagnetic class, or even with dense air, it keeps its place.

2440. Such a view also would make mere space magnetic, and precisely to the same degree as air and gases. Now though it may very well be, that space, air and gases, have the same general relation to magnetic force, it seems to me a great additional assumption to suppose that they are all absolutely magnetic, and in the midst of a series of bodies, rather than to suppose that they are in a normal or zero state. For the present, therefore, I incline to the former view, and consequently to the opinion that diamagnetics have a specific action antithetically distinct from ordinary magnetic action, and have thus presented us with a magnetic property new to our knowledge.

2441. The amount of this power in diamagnetic substances seems to be very small, when estimated by its dynamic effect, but the motion which it can generate is perhaps not the most striking measure of its force; and it is probable that when its nature is more intimately known to us, other effects produced by it and other indicators and measurers of its powers, than those so imperfectly made known in this paper, will come to our knowledge; and perhaps even new classes of phænomena will serve to make it manifest and indicate its operation. It is very striking to observe the feeble condition of a helix when alone, and the astonishing force which, in giving and receiving, it manifests by association with a piece of soft iron. So also here we may hope for some analogous development of this element of power, so new as yet to our experience. It cannot for a moment be supposed, that, being given to natural bodies, it is either superfluous or insufficient, or unnecessary. It doubtless has its appointed office, and that one which relates to the whole mass of the globe; and it is probably because of its relation to the whole earth, that its amount is necessarily so small (so to speak) in the portions of matter which we handle and subject to experiment. And small as it is, how vastly greater is this force, even in dynamic results, than the mighty power of gravitation, for instance, which binds the whole universe together, when manifested by masses of matter of equal magnitude!

2442. With a full conviction that the uses of this power in nature will be developed hereafter, and that they will prove, as all other natural results of force do, not merely important but essential, I will venture a few hasty observations.

2443. Matter cannot thus be affected by the magnetic forces without being itself concerned in the phænomenon, and exerting in turn a due amount of influence upon the magnetic force. It requires mere observation to be satisfied that when a magnet is acting upon a piece of soft iron, the iron itself, by the condition which its particles assume, carries on the force to distant points, giving it direction and concentration in a manner most striking. So also here the condition which the particles of intervening diamagnetics acquire, may be the very condition which carries on and causes the transfer of force through them. In former papers (1161. &c.)[1] I proposed a theory of electrical induction founded on the action of contiguous particles with which I am now even more content than at the time of its proposition: and I then ventured to suggest that probably the lateral action of electrical currents which is equivalent to electro-dynamic or *magnetic* action, was also conveyed onwards in a similar manner (1663. 1710. 1729. 1735.). At that time I could discover no peculiar condition of the intervening or diamagnetic matter; but now that we are able to distinguish such an action, so *like* in its nature in bodies so *unlike* in theirs, and by that so like in character to the manner in which the magnetic force pervades all kinds of bodies, being at the same time as universal in its presence as it is in its action; now that diamagnetics are shown not to be indifferent bodies, I feel still more confidence in repeating the same suggestion, and asking whether it may not be by the action of the contiguous or next succeeding particles that the magnetic force is carried onwards, and whether the peculiar condition acquired by diamagnetics when subject to magnetic action, is not that condition by which such propagation of the force is affected?

2444. Whichever view we take of solid and liquid substances, whether as forming two lists, or one great magnetic class (2424. 2437.), it will not, as far as I can perceive, affect the question. They are all subject to the influence of the magnetic lines of

[1] Philosophical Transactions, 1838, Part I.

force passing through them, and the virtual difference in property and character between any two substances taken from different places in the list (2424.) will be the same; for it is the differential relation of the two which governs their mutual effects.

2445. It is that group which includes air, gases, vapours, and even a vacuum which presents any difficulty to the mind; but here there is such a wonderful change in the physical constitution of the bodies, and such high powers in some respects are retained by them, whilst others seem to vanish, that we might almost expect some peculiar condition to be assumed in regard to a power so universal as the magnetic force. Electric induction being an action through distance, is varied enough amongst solid and liquid bodies; but, when it comes to be exerted in air or gases, where it most manifestly exists, it is alike in amount in all (1292.); neither does it vary in degree in air however rare or dense it may be (1284.). Now magnetic action may be considered as a mere function of electric force, and if it should be found to correspond with the latter in this particular relation to air, gases, &c., it would not excite in my mind any surprise.

2446. In reference to the manner in which it is possible for electric force, either static or dynamic, to be transferred from particle to particle when they are at a distance from each other, or across a vacuum, I have nothing to add to what I have said before (1614, &c.). The supposition that such can take place, can present nothing startling to the mind of those who have endeavoured to comprehend the radiation and the conduction of heat under one principle of action.

2447. When we consider the magnetic condition of the earth as a whole, without reference to its possible relation to the sun, and reflect upon the enormous amount of diamagnetic matters which, to our knowledge, forms its crust; and when we remember that magnetic curves of a certain amount of force and universal in their presence, are passing through these matters and keeping them constantly in that state of tension, and therefore of action, which I hope successfully to have developed, we cannot doubt but that some great purpose of utility to the system, and us its inhabitants, is thereby fulfilled, which now we shall have the pleasure of searching out.

2448. Of the substances which compose the crust of the earth, by far the greater portion belongs to the diamagnetic class; and though ferruginous and other magnetic matters, being more energetic in their action, are consequently more striking in their phænomena, we should be hasty in assuming that therefore they overrule entirely the effect of the former bodies. As regards the ocean, lakes, rivers, and the atmosphere, they will exert their peculiar effect almost uninfluenced by any magnetic matter in them; and as respects the rocks and mountains, their diamagnetic influence is perhaps greater than might be anticipated. I mentioned that by adjusting water and a salt of iron together, I obtained a solution inactive in air (2422.); that is, by a due association of the forces of a body from each class, water and a salt of iron, the magnetic force of the latter was entirely counteracted by the diamagnetic force of the former, and the mixture was neither attracted nor repelled. To produce this effect, it required that more than 48·6 grains of crystallized protosulphate of iron should be added to 10 cubic inches of water (for these proportions gave a solution which still set equatorially), a quantity so large, that I was greatly astonished on observing the power of the water to overcome it. It is not therefore at all unlikely that many of the masses which form the crust of this our globe may have an excess of diamagnetic power and act accordingly.

2449. Though the general disposition of the magnetic curves which permeate and surround our globe resemble those of a very short magnet, and therefore give lines of force rapidly diverging in their general form, yet the magnitude of the system prevents us from observing any diminution of their power within small limits; so that probably any attempt on the surface of the earth to observe the tendency of matter to pass from stronger to weaker places of action would fail. Theoretically, however, and at first sight, I think a pound of bismuth or of water, estimated at the equator, where the magnetic needle does not dip, ought to weigh less when taken into latitudes where the dip is considerable; whilst a pound of iron, nickel or cobalt, ought, under the same change of circumstances, to weigh more. If such should really prove to be the case, then a ball of iron and another of bismuth, attached to the ends of a delicate balance beam, should cause that beam to take different inclinations on different parts of the surface of the earth; and it does not seem quite impossible that an instrument to measure one of the conditions of terrestrial magnetic force might be constructed on such a principle.

2450. If one might speculate upon the effect of the whole system of curves upon very large masses, and these masses were in plates or rings, then they would, according to analogy with the magnetic field, place themselves equatorially. If Saturn were a magnet as the earth is, and his ring composed of diamagnetic substances, the tendency of the magnetic forces would be to place it in the position which it actually has.

2451. It is a curious sight to see a piece of wood, or of beef, or an apple, or a bottle of water repelled by a magnet, or taking the leaf of a tree and hanging it up between the poles, to observe it take an equatorial position. Whether any similar effects occur in nature among the myriads of forms which, upon all parts of its surface, are surrounded by air, and are subject to the action of lines of magnetic force, is a question which can only be answered by future observation.

2452. Of the interior of the earth we know nothing, but there are many reasons for believing that it is of a high temperature. On this supposition I have recently remarked, that at a certain distance from the surface downwards, magnetic substances must be entirely destitute, either of the power of retaining magnetism, or becoming magnetic by induction from currents in the crust or otherwise[1]. This is evidently an error; that the iron, &c. can retain no magnetic condition of itself, is very probably true, but that the magnetic metals and all their compounds retain a certain power of becoming magnetic by induction, whatever their temperature, has now been proved (2344, &c.). The deep magnetic contents of the earth, therefore, though they probably do not constitute of themselves a central magnet, are just in the condition to act as a very weak iron core to the currents around them, or other inducing actions, and very likely are highly important in this respect. What the effect of the diamagnetic part may be under the influence of such inductive forces, we are not prepared to state; but as far as I have been able to observe, such bodies have not their power diminished by heat (2397.).

2453. If the sun have anything to do with the magnetism of the globe, then it is probable that part of its effect is due to the

[1] Philosophical Magazine, 1845, vol. xxvii. p. 3.

action of the light that comes to us from it; and in that expectation the air seems most strikingly placed round our sphere, investing it with a transparent diamagnetic, which therefore, is permeable to his rays, and at the same time moving with great velocity across them. Such conditions seem to suggest the possibility of magnetism being there generated; but I shall do better to refrain from giving expression to these vague thoughts (though they will press in upon the mind), and first submitting them to rigid investigation by experiment, if they prove worthy, then present them hereafter to the Royal Society.

Royal Institution,
Dec. 22, 1845.

Feb. 2, 1846.—I add the following notes and references to these Researches.

Brugmans first observed the repulsion of bismuth by a magnet in 1778. *Antonii Brugmans Magnetismus seu de affinitatibus magneticis observationes magneticæ.* Lugd. Batav. 1778, § 41.

M. le Baillif on the Repulsion of a Magnet by Bismuth and Antimony, Bulletin Universel, 1827, vol. vii. p. 371; vol. viii. pp. 87, 91, 94.

Saigey on the Magnetism of certain natural combinations of Iron, and on the mutual repulsions of Bodies in general. Ibid. 1828, vol. ix. pp. 89, 167, 239.

Seebeck on the Magnetic Polarity of different Metals, Alloys and Oxides. Ibid. 1828, vol. ix. p. 175.

TWENTY-SECOND SERIES[1].

§ 28. *On the Crystalline polarity of bismuth (and other bodies), and on its relation to the magnetic form of force.* ¶ i. *Crystalline polarity of bismuth.* ¶ ii. *Crystalline polarity of antimony.* ¶ iii. *Crystalline polarity of arsenic.*

Received October 4,—Read December 7, 1848.

2454. MANY results obtained by subjecting bismuth to the action of the magnet have at various times embarrassed me, and I have either been contented with an imperfect explanation, or have left them for a future examination: that examination I have now taken up, and it has led to the discovery of the following results. I cannot, however, better enter upon the subject than by a brief description of the anomalies which occurred, and which may be obtained at pleasure.

2455. If a small open glass tube have a bulb formed in its middle part and some clean good bismuth be placed in the bulb and melted by a spirit-lamp, it is easy afterward, by turning the metal into the tubular part of the arrangement, to cast it into long cylinders: these are very clean, and when broken are seen to be crystallized, usually giving cleavage planes, which run across the metal. I prepare them from 0·05 to 0·1 of an inch in diameter, and, if the glass be thin, usually break both it and the bismuth together, and then keep the little cylinders in their vitreous cases.

2456. Taking some of these cylinders at random and suspending them horizontally between the poles of the electro-magnet (2247.), they presented the following phænomena. The first pointed axially; the second, equatorially; the third, equatorial in one position, and obliquely equatorial if turned round on its axis 50° or 60°; the fourth, equatorially and axially under the same treatment; and all of them, if suspended perpendicularly, pointed well, vibrating about a final fixed position which

[1] Philosophical Transactions 1849, p. 1. The Bakerian Lecture.

seemed to have no reference to the form of the cylinders. In all these cases the bismuth was strongly diamagnetic (2295, &c.), being repelled by a single magnetic pole, or passing off on either side from the axial line between two poles. A similar piece of finely-grained or granular bismuth was, under the same circumstances and at the same time, affected in a perfectly regular manner, taking up the equatorial position (2253.), as a body simply diamagnetic ought to do. The cause of these variations was finally traced to the regularly crystalline condition of the metallic cylinders.

¶ i. *Crystalline polarity of bismuth.*

2457. Some bismuth was crystallized in the usual manner by melting it in a clean iron ladle, allowing it partly to congeal, and then pouring away the internal fluid portion. Pieces so obtained were then broken up by copper hammers and tools, and groups of the crystals separated, each group or piece consisting only of those crystals which were symmetrically arranged, and therefore likely to act in one direction. If any part of the fragments had been in contact with the iron ladle, it was cleared away by rubbing on sandstone and sand-paper. Pieces weighing from 18 grains to 100 grains were thus easily obtained.

2458. The electro-magnet employed in the first instance was that already described (2247,), having moveable terminations which supplied either conical, round, or flatfaced poles. That the suspension of the bismuth might be readily effected and unobjectionable as to magnetic influence, the following arrangement was generally adopted. A single fibre of cocoon silk, from 12 to 24 inches in length, was attached to a fit support above, and made fast below to the end of a piece of fine, straight, well-cleaned copper wire, about 2 inches in length; the lower end of this wire was twisted up into a little head, and then furnished with a pellat of cement, made by melting together a portion of pure white wax, with about one-fourth its weight of Canada balsam. The cement was soft enough to adhere by pressure to any dry substance, and sufficiently hard to sustain weights up to 300 grains, or even more. When prepared, the suspender was subjected by itself to the action of the magnet, to ascertain that it was free from any tendency to point, or be affected; without which precaution no confidence could be reposed in the results of the experiments.

2459. A piece of selected bismuth (2457.), weighing 25 grains was hung up between the poles of the magnet, and moved with great freedom. The constituent cubes were associated in the usual manner, being attached to each other chiefly in the line joining two opposite solid angles; and this line was in the greatest length of the piece. The instant that the magnetic force was on, the bismuth vibrated strongly about a given line, in which, at last, it settled; and if moved out of that position, it returned, when at liberty, into it; pointing with considerable force, and having its greatest length *axial.*

2460. Another piece was then selected, having a flatter form, which when subjected to the magnetic power, pointed with the same facility and force, but its greatest length was equatorial: still the line according to which the cubes tended to associate diametrally, was, as before, in the *axial* direction. Other pieces were then taken of different forms, or shaped into various forms by rubbing them down on stone, but they all pointed well; and took up a final position, which had no reference to the shape, but was manifestly dependent on the crystalline condition of the substance.

2461. In all these cases the bismuth was diamagnetic, and strongly repelled by either magnetic pole, or from the axial line. It was affected only whilst the magnetic force was present. It set in a given constant position perfectly determinate; and, if moved, always returned to it, unless the extent of motion was above 90°, and then the piece moved further round and took up a new position diametrically opposed to the former, which it then retained with equal force, and in the same manner. This phenomenon is general in all the results I have to refer to, and I will express it by the word diametral:—diametral set or position.

2462. The effect occurs with a single magnetic pole, and it is then striking to observe a long piece of a substance, so diamagnetic as bismuth, repelled, and yet at the same moment set round with force, axially or end on, as a piece of magnetic substance would do.

2463. Whether the magnetic poles employed (2458.) are pointed, round, or flatfaced, still the effect on the bismuth is the same: nevertheless, the form of the poles has an important influence of a subordinate kind; and some forms are much more fitted for these investigations than others. When pointed poles are employed, the lines of magnetic force (2149.) rapidly diverge, and the force itself diminishes in intensity to the middle distance from each pole. But when flat-faced poles are used, though the lines of power are curved and vary in intensity at and towards the edges of the flat faces, yet there is a space at the middle of the magnetic field where they may be considered as parallel to the magnetic axes, and of equal force throughout. If the flat faces of the poles be square or circular, and their distance apart about one-third of their diameter, this space of uniform power is of considerable extent. In my experience the central or axial portion of the magnetic field is sensibly weaker than the circumjacent parts; but, then, there is a small screw-hole in the middle of each pole face, for the attachment of other forms of termination.

2464. Now the law of action of bismuth, as a *diamagnetic* body, is, that it tends to go from stronger to weaker places of magnetic force (2267. 2418.); but as a *magnecrystallic* body it is subject to no effect of the kind; and is as powerfully affected by lines of equal force as by any other. So a piece of amorphous bismuth, suspended in a magnetic field of uniform power, seems to have lost its diamagnetic force altogether, and tends to acquire no motion but what is due to torsion of the suspending fibre, or currents of air: but a piece of regularly crystallized bismuth is, in the same situation, very powerfully affected by virtue of its magnecrystallic condition.

2465. Hence the great value of a magnetic field of uniform force; and, if, hereafter, in the extension of these investigations to bodies having only a small degree of crystalline power, a perfectly uniform field should be required, it could easily be given by making the form of the pole face somewhat convex, and rounded at the edges more or less. The required shape could be ascertained by calculation, or perhaps better in practice, by the use of a little test cylinder of bismuth in the granular or amorphous-state, or of phosphorus.

2466. In addition to these observations, it may be remarked, that small crystals, or masses of crystals, and such as approach in their general shape to that of a cube or a sphere, are better than large or elongated pieces; inasmuch, as if there be irregularities in the force of a magnetic field, such pieces are less likely to be affected by them.

2467. When the crystal of bismuth is in a magnetic field of equal strength, it is equally affected whether it be in the middle of the field or close up to one or the other magnetic pole; *i. e.* the number of vibrations in equal times appears to be equal. Much care, however, is required in estimating it by such means, because, from the occurrence of two positions of unstable equilibrium in the equatorial direction, the vibrations in large arcs are much slower than those in small arcs; and it is difficult in different cases to adjust them to the same extent of vibration.

2468. Whether the bismuth be in a field of intense magnetic force or one of feeble powers; whether the magnetic poles are close up to the piece, or are opened out until they are five or six inches or even a foot asunder; whether the bismuth be in the line of maximum force, or raised above, or lowered beneath it; whether the electric current be strong or weak, and the magnetic force, therefore, more or less in that respect; if the bismuth be affected at all it is always affected in the same manner.

2469. The results are, altogether, very different from those produced by diamagnetic action (2418.). They are equally distinct from those discovered and described by Plücker, in his beautiful researches into the relation of the optic axis to magnetic action; for there the force is equatorial, whereas here it is axial. So they appear to present to us a new force, or a new form of force, in the molecules of matter, which, for convenience sake, I will conventionally designate by a new word, as the *magnecrystallic* force.

2470. The direction of this force is, in relation to the magnetic field, *axial* and *not equatorial*: this is proved by several considerations. Thus, when a piece of regularly crystallized bismuth was suspended in the magnetic field, it pointed; keeping it in this position, the point of suspension was removed 90° in the equatorial plane (2252.), so that when again freely suspended, the line through the crystal, which was before horizontal in the equatorial plane, was now vertical; the piece again pointed, and generally with more force than before. The line passing through the crystal, coincident with the magnetic axis, may now be taken as a line of force; and if the process of a quarter revolution in the equatorial plane be repeated, however often, the crystal still continues to point with the assumed line of force in the magnetic axis, and with a maximum degree of power. But now, if the point of suspension be removed 90° in the plane of the axis, *i. e.* to the end of the assumed line of force, so, that when the crystal is again freely suspended this line is vertical; then, the crystal presents its peculiar effect at a *minimum*, being almost or entirely devoid of pointing power, and exhibits in relation to the magnet, only the ordinary diamagnetic force (2418.).

2471. Now if the power had been equatorial and polar, its maximum effect would not have been produced by a change of the point of suspension through 90° in the equatorial plane, but by the same change in the axial plane, and any similar change after that in the axial plane, would not have disturbed the maximum force; whereas a single change of 90° in the equatorial plane, would have brought the line of force vertical (as in Plücker's case of Iceland spar), and reduced the results to a minimum or zero.

2472. The directing force, therefore, and the set of the crystal are in the *axial* direction. This force is, doubtless, resident in the particles of the crystal. It is such, that the crystal can set with equal readiness and permanence in two diametral positions; and that between these there are two positions of equatorial equilibrium, which are, of course, unstable in their nature. Either end of the mass or of its molecules, is, to all intents and purposes, both in these phænomena, and in the ordinary results of crystalization, like the other end; and in many cases, therefore, the words *axial* and *axiality* would seem more expressive than the words *polar* and *polarity*. In presenting the ideas to my own mind, I have found the meaning belonging to the former words the more useful.

2473. On placing the *metal* in other positions, and therefore in a constrained condition, no alteration of the state or power of the bismuth, either in force or direction, is produced by the power of the magnet, however strong its enforcement or long its continuance.

2474. It is difficult readily to describe the position of this force in relation to the crystal, though most easy to ascertain it experimentally. The form of the bismuth crystals is said to be that of a cube, and of its primitive particle a regular octohedron. To me the crystals do not seem to be cubes, but either rhomboids or rhombic prisms, approaching very nearly to cubes. My measurements were very imperfect and the crystals not regular; but as an average of several observations, the planes were inclined to each other at angles of $91\frac{1}{2}°$ and $88\frac{1}{2}°$; and the boundary lines of a plane at $87\frac{1}{2}°$ and $92\frac{1}{2}°$. Whatever be the true form, it is manifest, upon inspection, that the aggregating force tends to produce crystals having more or less of the rhomboidal shape and rhombic planes; and that these crystals run together in symmetric groups, generally in the direction of their longest diameters. Now the line of *magnecrystallic* force almost always coincides with this direction where the latter is apparent.

2475. The *cleavage* of bismuth crystals removes the solid angles and replaces them by planes; so that there are four directions producing the octohedron. These cleavages are not (in my experience) made with equal facility, nor do they produce planes equally bright and perfect. Two, and more frequently one, of these planes is more perfect than the others; and this, the most perfect plane, is that which is produced at the most acute solid angle (2474.); and is generally easily recognized. When a bismuth crystal presents many planes of cleavage and is suspended in the magnetic field, one of these planes faces towards one of the magnetic poles, and its corresponding plane, if it be there, towards the other; so that the line of magnecrystallic force is perpendicular to this plane: and this plane corresponds to the one which I have already described as being, generally, the most perfect, and replacing the acute angle of the crystal.

2476. A single crystal of bismuth was selected and cut out from the mass by copper tools, and the places where it had adhered were rubbed down on sand-paper, so as to give the fragment a cube-like form with six planes; four of these planes were natural. One of the solid angles, expected to be that terminating or in the direction of the line of magnecrystallic force, was removed, so as to expose a small cleavage plane, which was bright and perfect, as also was expected. When suspended in the magnetic field with this plane vertical, the crystal instantly pointed with considerable force, and with the plane towards either one or the other magnetic pole; so that the magnecrystallic axis appeared now to be horizontal and acting with its greatest power. When this axial line was made vertical, and the plane therefore horizontal, the position being carefully adjusted, the crystal did not point at all. Being now suspended in succession at all the angles and faces of the cube, it always pointed with more or less force; but always so that a line drawn perpendicularly through the indicating cleavage plane (representing therefore the line of force) was in the same vertical plane as that including the magnetic axis: and, finally, when the bright cleavage plane was horizontal and the line of directive force therefore vertical, inclining it a little in a given direction would make any given part of the crystal point to the magnetic poles.

2477. A *group* of bismuth crystals, the apex of which was terminated by a single small cleavage facet, was found to give the same results.

2478. Occasionally groups of crystals (2457.) occurred which did not seem capable of being placed in some one position in which they lost all directive power, but seemed to retain a minimum degree of force. It is very unlikely, however, that all the groups should be perfectly symmetric in the arrangement of their parts. It is more surprising that they should be so distinct in their action as they are. In reference to bismuth, and many other bodies, it is probable that magnetic force will give a more important indication in relation to the essential and real crystalline structure of the mass than its form can do.

2479. I have already stated that the *magnecrystallic* force does not manifest itself by attraction or repulsion, or, at least, does not cause approach or recession, but gives *position* only. The *law* of action appears to be, that, *the line or axis* of MAGNECRYSTALLIC *force* (being the resultant of the action of all the molecules), *tends to place itself parallel, or as a tangent, to the magnetic curve or line of magnetic force, passing through the place where the crystal is situated.*

2480. I now broke up masses of bismuth which had been melted and solidified in the ordinary way, and, selecting those fragments which appeared to be most regularly crystallized, submitted them to experiment. It was almost impossible to take a small piece which did not obey the magnet and point more or less readily. By selecting the thin plates with perfect cleavage planes, I readily obtained specimens which corresponded in all respects with the crystals; but thicker plates or angular pieces often proved complicated in the results, though apparently simple and regular as to form. Occasionally, the cleavage plane, which I have beforehand taken for that perpendicular to the line of force (2475.), has proved not to be the plane supposed; but, after observing experimentally the direction of the magnecrystallic power, I have always either found, or else obtained by cleavage, a plane corresponding to it, possessing the appearance and character before described (2475.). Bismuth plates from the one-twentieth to the one-tenth of an inch in thickness, and bounded by parallel and similar planes, when broken up, often proved, upon ocular examination, to be compounded and irregular.

2481. When a well-selected plate of bismuth (mine are about 0·3 of an inch in length and breadth, and 0·05, more or less, in thickness) is hung up by the edge in the magnetic field, it vibrates and points, presenting its faces to the magnetic poles, and setting diametrally (2461.). By whatever part of the edge it is suspended, the same results follow. But if it be suspended horizontally, the cleavage planes of the fragment and of the magnetic axis being parallel to the plane of motion of the plate, then it is perfectly indifferent; for then the line of magnecrystallic force is perpendicular to the line of magnetic force in every position that it can take.

2482. But if the plate be inclined only a very small quantity from this position, it points, and that with more force as the planes become more nearly vertical (2475.); and the phænomena before described with a crystal (2476.), can here be obtained with a fragment from a mass, and any part of the edge of the plate made to point axially, by elevating or depressing it above or below the horizontal plane.

2483. If a number of these crystalline plates be selected at the magnet, they may afterwards be built up together, with a little good cement (2458.), into a mass which has perfectly regular magnecrystallic action; and in that respect resembles the crystals before spoken of (2459. 2468. 2476.). In this manner, also, the *diamagnetic* effect of the bismuth may be neutralized; for it is easy to build up a prism whose breadth and thickness are equal, and this being hung with the length vertical, points well and without any interference of diamagnetic action.

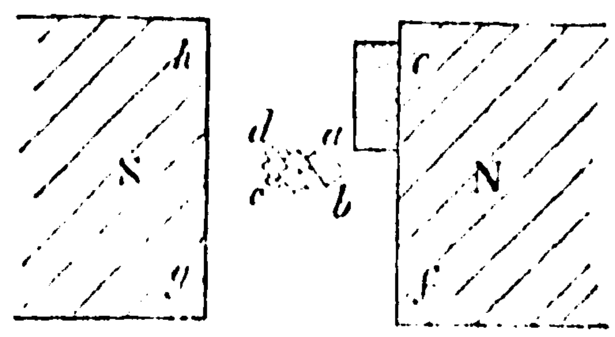
2484. By placing three equal plates at right angles to each other, a system is obtained, which has lost all power of pointing under the influence of the magnet, the force being, in every direction, neutralized. This represents the case of finely crystallized or amorphous bismuth. The same result (having the same nature) may be obtained by taking a selected uniform mass of crystals (2457.), melting it in a glass tube and resolidifying it: unless the crystallization is large and distinct, which rarely happens, the piece obtained is apparently without magnecrystallic force. A like result is also obtained by breaking up the crystal and putting the small fragments or powder into a tube, and submitting the whole to the force of the magnet.

2485. These experiments on bismuth are not difficult of repetition; for, except those which require the sudden production or cessation of the magnetic force, the whole may be repeated with an ordinary horse-shoe magnet. A magnet, with which I have wrought considerably, consists of seven bars placed side by side, and being fixed in a box with the poles upwards, presents two magnet cheeks, an inch and a quarter apart, between which is the magnetic field, having the lines of force in a horizontal direction. The poles of the magnet should be covered, each with paper, to prevent communication of particles of iron or rust. The best place for the piece of bismuth is, of course, between the poles; not level, however, with their tops, but from 0·4 to 1·0 inch lower down (2463.), that the effect of flat-faced poles may be obtained. If it be desired to strengthen the lines of magnetic force, this may be done by introducing a piece of iron between the poles of the magnet, and so, by virtually causing them to approach, lessen the width of the magnetic field between them.

2486. The magnet I used would sustain 30 lbs. at the keeper; but employing small pieces of bismuth, I have easily obtained the effects with magnets weighing themselves not more than 7 ounces, and able to sustain only 22 ounces; so that the experiments are within the reach of every one.

2487. Whilst the crystal of bismuth is in the magnetic field, it is affected very distinctly, and even strongly, by the near approximation of soft iron or magnets, and after the following manner. Let fig. 1 represent in plan the position of the two chief magnetic poles, and of a piece of crystallized bismuth between them, which, by its magnecrystallic condition, points axially. Then, if a piece of soft iron be applied against the cheek of the pole, as at *e*, and also near to the bismuth, as at *a*, it will affect the latter and cause its approach to the iron. If the iron be applied in a similar manner at *f*, *g*, or *h*, it will have a like result in causing motion of the bismuth; and the parts marked *b*, *c*, and *d*, will in turn approach it, seeming to be attracted. If the soft iron do not touch the magnetic pole, but be held between it and the bismuth so as to represent generally the same positions, the same effects, but in a weaker degree, are produced.

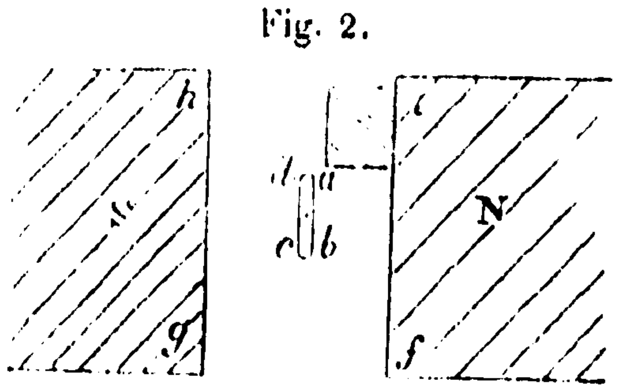
Fig. 1.



2488. Though these motions seem to indicate an effect of attraction, I do not believe them to be due to any such cause, but simply to the influence of the law of action (2479.) before expressed. The previously uniform condition of the magnetic field is destroyed by the presence of the iron; lines of magnetic force, of greater intensity than the others, proceed from the angle *a* of the iron in the position represented, or from the corresponding angles in the other positions (the shape of the pole now approximating more or less to the conical or pointed form), and therefore the crystal of bismuth moves round on the axis of suspension, that it may place the line of magnecrystallic force parallel or as a tangent to the resultant of the magnetic forces which pass through its mass.

2489. When in place of the group of crystals a crystalline plate of bismuth (2481.) is employed, the appearances produced under similar circumstances, are those of *repulsion;* for if fig. 2 be allowed to represent this state of things, the piece of iron applied at *e* causes the plate to recede

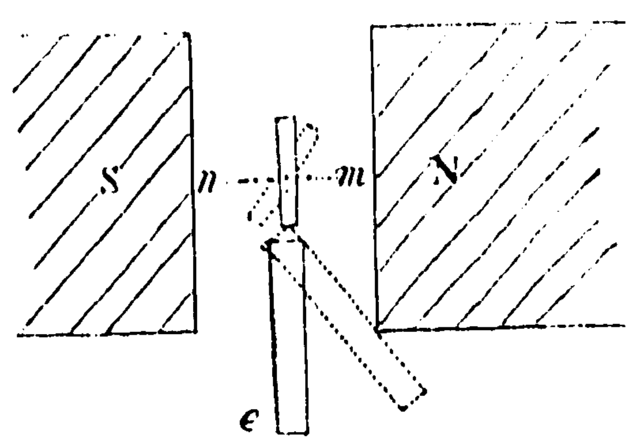
Fig. 2.



from it at *a*, or if applied at *f*, *g*, or *h*, it causes recession of the bismuth from it at the points *b*, *c*, and *d*. Now though these effects look like repulsion, they are, as I conclude, nothing more than the consequences of the endeavour which the bismuth makes under the law before expressed (2479.), to place the magnecrystallic line of force parallel to, or as a tangent to the resultant of magnetic force passing through the bismuth.

2490. A piece of iron wire about $1\frac{1}{2}$ inch long, and 0·1 or 0·2 of an inch thick, being held in the equatorial plane to the edge of the plate (fig. 3), did not alter its position; but if the end *e* were inclined to either pole, the plate began to move, and moved most when the iron touched the pole as in the figure. When it approached or touched the N pole, the inclination of the crystal plate of bismuth was as indicated by the dotted figure. When it touched the S, the inclination was the contrary way. If the end *e* were kept in contact with the N pole, and the other end of the soft iron rod placed in the position *m*, the bismuth was not affected; but if then this subsidiary pole were moved the one way or the other towards the edge of the plate, the latter turned as the pole moved, always tending to keep its face towards it, and evidently by the tendency of the magnecrystallic axis to place itself parallel to the resultant of magnetic force passing through the bismuth. The same results were obtained with the crystal (2487.) under similar circumstances, and corresponding results were obtained when the soft iron rod was applied between the S cheek of the magnet and the bismuth. The like effects were also obtained with plates of arsenic and antimony.

Fig. 3.



2491. When a magnet is used instead of soft iron, corresponding effects are produced; only it must be remembered, that if the chief magnet be very powerful, it may often neutralize, and even change, the magnetism of the small approximated magnet; and this can happen with the latter (as to external influence), whilst in the magnetic field, even though when withdrawn it may appear to remain unaltered.

2492. Thus, when the plate of bismuth was suspended between the cheeks of the horse-shoe magnet (2485.), fig. 2, and the north pole of a small magnet (the blade of a pocket-knife) was placed at *a* or *b*, it caused recession of the part of the bismuth near it, and precisely for the same reasons as those that existed when the soft iron was there. When the extra pole was placed at *c* or *d*, the action was more feeble than in the former case, and consisted in an approximation of that part of the bismuth to the pole. As this position of the subordinate pole would terminate and neutralize certain of the lines of magnetic force proceeding from the south pole of the horse-shoe magnet, so the resultant of the lines of force passing through the bismuth would be changed in direction, being rendered oblique to their former course, and precisely in the manner represented by the motion of the bismuth, in its tendency to place its line of force parallel with them in their new position.

2493. An approximated south pole caused motions in the contrary direction.

2494. When the subordinate pole was applied to the edge of the plate, the little magnet being in the equatorial position (fig. 3), then instead of being neutral, as the iron was, it caused the plate to move in a tangential direction, either to the right or the left, according as it was either a south or a north pole, just indeed as the iron did when, by inclining it, the approximated end became a pole (2490.). This effect was shown in a still more striking degree by using the crystal of bismuth (2487.), because, from its form and position the magnetic curves most affected by the extra pole were more included in the bismuth than when the plate was used.

2495. Innumerable variations of these motions may be caused, and appearances of attraction or repulsion, or tangential action be obtained at pleasure by the use of crystals having the magnecrystallic axis corresponding with their length, or plates where it accords with their thickness; and either permanent or temporary subsidiary magnetic poles. By making the moveable pole travel slowly round the bismuth from the neutral point *m* to the other neutral point *n*, fig. 3, a summary of the whole can be obtained, and it is found that they all resolve themselves into the general law before expressed (2479.); the magnecrystallic axis and the resultant of magnetic force passing through the bismuth, tending to become parallel.

2496. Hence a small crystal or plate of bismuth (or arsenic

(2532.)) may become a very useful and important indicator of the direction of the lines of force in a magnetic field, for at the same time that it takes up a position showing their course, it does not by its own action tend sensibly to disturb them.

2497. Many of these motions are similar to, and have relation with, those described by Plücker, Reich, and others, as obtained by the action of iron and magnets on bismuth, in its simple diamagnetic condition. These results are by them and others considered as indicating that the bismuth, as I had originally supposed (2429, &c.), has really, in its diamagnetic state, a magnetic condition the reverse of that of iron. I am not acquainted with all of them, or with the reasoning thereon (being in the German language); but such as I am aware of, and have reobtained, seem to me to be simple results of the law I formerly laid down (2267. 2418.), namely, that diamagnetic bodies tend to proceed from stronger to weaker places of magnetic force; and give no additional or other proof of the assumed reverse polarity of bismuth than the former cases of action which I had given, coming under that law.

2498. Supposing that the intervening or surrounding matter might, in some manner, affect the magnecrystallic action of bismuth and other bodies, I fixed the magnetic poles at a given distance (about two inches) asunder, suspended a crystal of bismuth in the middle of the magnetic field, and observed its vibrations and set. Then, without any other change, I introduced screens of bismuth, being blocks about two inches square and 0·75 of an inch in thickness, between the poles and the crystal, but I could not perceive that any change in the phænomena was produced by their presence.

2499. The bismuth crystal (2459.) was suspended in water between the magnetic poles of the horse-shoe magnet. It set well in accordance with the general law (2479.), and it took five revolutions of the torsion index at the upper end of the suspending silk filament to displace it, and cause it to turn into the diametral position. This is, as well as I could observe the results, the same amount of torsion force required to effect its displacement when the crystal was placed in the same position, but surrounded with air only.

2500. The same bismuth was then suspended in a saturated solution of protosulphate of iron (adapted as a magnetic medium), it set as before with apparently no change of any kind; and when the torsion force was put on, it still required five turns of the index, as before, to cause the displacement of the crystal, and its passage into the diametral position.

2501. Whether therefore crystals of bismuth be immersed in air, or water, or solution of sulphate of iron, or placed between thick masses of bismuth, if they be subject to the same magnetic force, the magnecrystallic force exerted by them is the same both in nature, direction and amount.

2502. It seemed possible and probable that magnetic force might affect the crystallization of bismuth, if not of other bodies. For, as the force affects the mass of a crystal by that power which its particles possess, and which they give to the crystal as a whole by their polar (or axial (2472.)) and symmetric condition; and, as the final position of the crystalline mass in the magnetic field may be considered as that of the least constraint, so it was likely enough that, if the bismuth in a fluid state were placed under the influence of the magnetism, the individual particles would tend to assume one and the same axial condition, and the crystalline arrangement and direction of the mass upon its solidification, be in some degree determined and under government.

2503. Some bismuth, therefore, was fused in a glass tube and held in a fixed position in the strong magnetic field until it had become solid; then, being removed from the glass, it was suspended so that it might assume the same position under the influence of the magnet; but no signs of *magnecrystallic* force were evident. It was not expected that the whole would become regularly crystallized, but that a *difference* between one direction and another might appear. Nothing of the kind however occurred, whatever the direction in which the piece was suspended; and when it was broken open, the crystallization within was found to be small, confused, and in all directions. Perhaps if longer time were allowed, and a permanent magnet used, a better result might be obtained. I had built many hopes upon the process, in reference to the crystalline condition of gold, silver, platina, and the metals generally, and also in respect of other bodies.

2504. I cannot find that crystals of bismuth acquire any power, either temporary or permanent, which they can *bring away* from the magnetic field. I held crystals in different positions in the field of intense action of a powerful electro-magnet, having conical terminations very near to each other; and, after some time, removed them and applied them instantly to a very delicate astatic magnetic needle; but I could not perceive that they had the least extra effect upon it, because of such treatment.

2505. As a crystal of bismuth is subject to, and obeys the influence of, the lines of magnetic force (2479.), so it follows that it ought to obey even the earth's action, and point, though with a very feeble degree of power. I have suspended a good crystal by a long filament of cocoon silk, and sheltered it as well as I could from currents of air by concentric glass tubes, and I think have observed indications of a set or pointing. The crystal was so hung that the magnecrystallic axis made the same angle with the horizontal plane (about 70°) as the magnetic dip, and the indication was, that the axis and the dip tended to coincide: but the experiments require careful repetition.

2506. A more important point, as to the nature of the polar or axial forces of bismuth, is to know whether two crystals, or uniformly crystallized masses of bismuth, can mutually affect each other; and if so, what the nature of these affections are? what is the relation of the equatorial and terminal parts? and what, the direction of the forces? I have made many experiments, in relation to this subject, both in and out of the magnetic field, but obtained only negative results. I employed however small masses of bismuth, and it is my purpose to repeat and extend them at a more convenient season with larger masses, built up, if necessary, in the manner already described (2483.).

2507. I need hardly say that a crystal of bismuth ought to point in a helix or ring of wire carrying an electric current, and so that its magnecrystallic axis should be parallel to the axis of the ring or helix. This I find experimentally to be the case.

¶ ii. *Crystalline Polarity of Antimony.*

2508. *Antimony* is a *magnecrystallic* body. Some crystalline masses, procured in the manner before described (2457.), were broken up with copper tools, and some excellent groups of crystals were obtained, weighing from ten to twenty grains each, in which all the constituent crystals appeared to be uniformly placed. The individual crystals were very good on the whole, and much more frequently complete and full at the faces than those of bismuth. They were very bright, having a steel-gray or silvery appearance, and to the eye appeared more surely as cubes than bismuth, though here and there distinctly rhomboidal faces presented themselves. Planes of cleavage can be made to replace the solid angles; and, as with bismuth, there is one plane generally brighter and more perfect than the others.

2509. In the first place, it was ascertained that all these crystals were diamagnetic and strongly so.

2510. In the next it was ascertained, as with bismuth, that all of them exhibited the magnecrystallic phænomena with considerable power, showing the existence of a line of force (2470.); which, when placed vertically, left the crystal free to move in any direction (2476.); but when placed horizontally, caused the crystal to point, and in so doing took up its own position parallel to the resultant of magnetic force passing through the crystal (2479.). This line proceeded, as in bismuth, from one of the solid angles to the opposite one, and was perpendicular to the bright cleavage plane just spoken of (2508.).

2511. So, generally, the action of the magnet upon these crystals was the same as upon the crystals of bismuth; but there are some points of variation which require to be more distinctly stated and distinguished.

2512. In the first place, when the magnecrystallic axis was horizontal, and a certain crystal used, upon the evolution of the magnetic force, the crystal went up to its position slowly, and pointed, as with a dead set. If the crystal were moved from this position on either side, it returned to it at once: there was no vibration. Other crystals did the same imperfectly; and others again made one or perhaps two vibrations, but all appeared as if they were moving in a thick fluid, and were, in that respect, utterly unlike bismuth, in the freedom and mobility with which it vibrated (2459.).

2513. In the next place, when the crystals were so suspended as to have the magnecrystallic axis vertical, there was no pointing nor any other signs of magnecrystallic force; but other appearances presented themselves. For, if the crystalline mass was revolving when the magnetic force was excited, it suddenly stopped, and was caught in a position which might, as was found by experience, be any position; but if the greatest length was out of the axial or equatorial position, the arrest was followed by a *revulsive* motion on the discontinuance of the electric current (2315.). This revulsive motion was never great, but was most when the length of the mass formed about an angle of 45° with the axis of the magnetic field.

2514. On further examination it appeared that this arresting and revulsive effect was precisely the same in kind as that observed on a former occasion with copper and other metals (2309.), and due to the same cause, namely, the production of circular electric currents in the metal under the inductive force of the magnet. Now, the reason appeared why, in the former case, the crystals of antimony did not oscillate (2512.); and why, also, they went up to their position of rest with a dead set; for the currents produced by the motion are just those which tend to stop the motion (2329.)*; and though the magnecrystallic force was sufficient to make the crystal move and point, yet the very motion so produced generated the current which reacted upon the tendency to motion, and so caused the mass to advance towards its position of rest as if it moved in a thick fluid.

2515. Having this additional knowledge respecting the arrest and revulsion of the antimony, (effects dependent upon its superior conducting power, in this compact crystalline state, as compared with bismuth,) one has no difficulty in identifying the magnecrystallic force of this metal with that of the former, and establishing the correspondence of the results in all essential characters and particulars. In most of the pieces of crystals of

* Any one who wishes to form a sufficient idea of the arresting powers of these induced currents, should take a lump of solid copper, approaching to the cubical or globular form, weighing from a quarter to half a pound; should suspend it by a long thread, give it a rapid rotation, and then introduce it, spinning, into the magnetic field of the electro-magnet; he will find its motion to be instantly stopped; and if he further tries to spin it, whilst in the field, will find it impossible to do so.

antimony the force seemed less than in bismuth, but the fact may not really be so, for the inductive current action just described tends to hide the magnecrystallic phænomena.

2516. Different pieces of antimony also seem to differ from each other in their setting force, and also in their tendency to exhibit revulsive effects; but these differences are either only apparent, or may easily be explained. The arresting and revulsive action depends much upon the continuity of the mass, so that one large piece shows it much better than several small pieces, and these again better than a powdered substance. Even the revulsive action of copper may be entirely destroyed by reducing the single lump to filings. It is easy to perceive, therefore, that of two groups of antimony crystals, each symmetrically disposed within itself, the one may have larger crystals well connected together, as regards the induction of currents through the whole mass, and the other smaller crystals less favourably united. These would present very different appearances, as regards the arrest of motion and succeeding revulsive action; and further, on that very account, would differ in their readiness to present the magnecrystallic phænomena, though they might possess precisely equal degrees of that force.

2517. On proceeding to experiment with plates of antimony, further illustrations of the effects resulting from the causes just described were obtained, with abundant accompanying evidence of the existence of the magnecrystallic condition in the metal. The plates were selected from broken masses, as with bismuth (2480.). Some were soon found which acted simply, instantly, and well; their large surfaces were bright cleavage planes. When suspended by any part of the edge, these planes faced towards the magnetic poles; and the plate oscillated on each side of its final position, gradually acquiring its state of rest.

2518. When these plates were suspended with their planes horizontal, they had no power of pointing in the magnetic field. When they were inclined, the points which were most depressed below and raised above the horizontal plane, were those which took up their places nearest the magnetic poles (2482.).

2519. When several plates were arranged together into a consistent bundle (2483.), the *diamagnetic* effect was removed, and the magnecrystallic oscillation and pointing became very ready a characteristic.

2520. Thus it is evident that, in all these cases, there was a line of magnecrystallic force perpendicular to the planes of the plates, and perfectly consistent in its position and action with the force before found in the solid crystals of antimony.

2521. But another plate of antimony was now selected, which had every appearance of being able to present all the phænomena of the former plates; and yet, when hung up by its edge, it showed no signs of magnecrystallic results; for it first advanced a little (2310.), then was arrested and kept in its place, and if standing between the axial and equatorial positions, was revulsed when the battery current was interrupted, exhibiting effects equal to those of copper (2315.). Many other plates were tried with precisely the same result.

2522. When this plate (2521.) was placed in the field of intense power between two *conical* magnetic poles, it exhibited the same phænomena; but notwithstanding the arresting action, it moved slowly until it stood in the equatorial position; a result which was probably due to the exertion of both *magnecrystallic* and *diamagnetic* force. When the plate was suspended with its planes horizontal, the arresting and revulsive actions were gone; for the induced currents which before caused them could not now exist in the necessary vertical plane; further, it had no setting power, which showed that there was no axis of magnecrystallic force in the length or breadth of the plates.

2523. Other plates were then found able to produce mixed effects, and those in different degrees. Thus, some, like the first, vibrated freely, pointed well, and presented no indication of the arrest and revulsive phænomena. Others vibrated sluggishly, set well, and showed a tendency to be arrested. Others pointed well, going up to their place with a dead set, but moving as if in a fluid; or, if the magnetic force were taken off before the piece had settled, it was revulsed feebly: and others were caught at once, did not set (within the time of my observation), and were strongly revulsed.

2524. Finally, a careful investigation, carried on by means both of the horse-shoe (2485.) and the great electro-magnet (2247.), made the cause of these differences in the effects apparent.

2525. It may be observed, in the first place, that sometimes a plate of antimony being selected (2517.), having planes very bright and perfect in their appearance, and, therefore, giving reason to think that it may point well in the magnetic field, when submitted to the horse-shoe magnet does not do so; but points obliquely, feebly, and perhaps in two undiametral positions. This is, I have no doubt, because the crystallization is complicated and confused. Such a plate, if it be sufficiently broad and long (*i. e.* not less than a quarter or one-third of an inch), when submitted to the electro-magnet, will show the arresting (2310.) and revulsive (2315.) action well.

2526. In the next place, we have to remember that, for the development of the induced currents which cause the arresting and revulsive action, the plate must have certain sufficient dimensions in a vertical plane (2329.). The currents occur in the mass and not round the separate particles (2329.), and the resultant of the magnetic lines of force passing through the substance, is the axis round which these currents are produced. Hence the reason why the effect does not occur with plates suspended in the horizontal position, which yet produce it well in the vertical position; a result which a disc half an inch in diameter of thin foil or plate, being copper, silver, gold, tin, or almost any malleable metal, will show; though the best conductors are the fittest for the purpose. Now this condition is of no consequence in respect of *magnecrystallic* action, and a narrow plate has as much force as a broad one, having the same mass. The first plate that I happened to select (2517.) was well crystallized, thick and narrow; hence it was favourable for magnecrystallic action, unfavourable to the arresting and revulsive action, and showed no signs, comparatively, of the latter

2527. When a broad and well-crystallized plate is obtained, then both sets of effects appear: thus, if the plate is revolving when the magnetic force is brought into action, it quickens its velocity for an instant, then is stopped; and if the magnetic force is at once taken off, it is revulsed, exactly as a piece of copper would be (2315.). But if the magnetic force be continued, it will then be perceived that the stop is only apparent; for the plate moves, though with a greatly reduced velocity, and continues to move until it has taken up its magnecrystallic position. It moves as if in a thick fluid. Hence the magnecrystallic force is there and produces its full effect; and the reason why the appearances have changed is, that the very motion which the force tends to give, and does give to the mass, causes those magneto-electric currents (2329.) which by their mutual action with the magnet tends to stop the motion; and therefore its slowness and the final dead set (2512. 2523.).

2528. A magnet which is weaker (as the horse-shoe instrument described (2485.)) produces the currents by induction in a much weaker degree, and yet manifests the magnecrystallic power well; hence it is more favourable, under certain circumstances, for such investigations; as it helps to distinguish the one effect from the other.

2529. It will readily be seen that plates, whether of the same metal or of different metals, cannot, even roughly, be compared with each other as to magnecrystallic force by their vibrations; for under the influence of these induced currents, plates of the same magnecrystallic force oscillate in very different manners. I took a plate, and by cement (2458.) attached selected paper to its faces, and then observed how it acted in the magnetic field; it set slowly, and it showed the arresting and revulsive effects (2521.). I then pressed it in a mortar, so as to break it up into many parts, which still kept their place; and now it set more freely and quickly, and showed very little of the revulsive action.

2530. Though the indication by vibration is thus uncertain, the torsion force still remains to us, I believe, a very accurate indication of the strength of the set (2500.), and, therefore, of the degree of the magnecrystallic force; and though the suspending silk fibre may give way a little, a glass thread, according to Ritchie's suggestion, would answer perfectly.

2531. Antimony must be a good conductor of electricity in the direction of the plates of the crystals, or it would not give, so freely, these indications of revulsive action. The groups of crystals of antimony (2508.) showed the effect in such a degree, as to make me think that the constituent cubes possessed the power nearly equally in all directions. A piece of finely crystallized or granular antimony does not, however, show it in the same proportion; from which it would seem as if an effect equivalent in some degree to that of division occurs, either at the meeting of two incongruous crystals, or between the contiguous plates of the crystals, and affects the conducting power in these directions.

¶ iii. *Crystalline Polarity of Arsenic.*

2532. A mass of the metal arsenic exhibiting crystalline structure (2480.), was broken up, and several plates selected from the fragments, having good cleavage plane surfaces, about 0·3 of an inch in length, 0·1 inch in width, and 0·03 in thickness. These, when suspended opposite one conical pole, proved to be perfectly diamagnetic; and when before it or between two poles strongly *magnecrystallic.* I have a pair of flat-faced poles with screw-holes in the centre of the faces, and these so much weaken the intensity of the lines of magnetic force about the middle of the field, when the faces are within half an inch of each other, that a cylinder of granular bismuth 0·3 in length sets *axially*, or from pole to pole (2384.). But with the plates of arsenic between the same poles there was no tendency of this kind; so much was the magnecrystallic force predominant over the diamagnetic force of the substance.

2533. When the plates of arsenic were suspended with their planes horizontal, then they did not point at all between the flat-faced poles. Any inclination of the planes to the horizontal line produced pointing, with more or less force as the planes approached more or less to the vertical position, exactly in the manner already described in relation to bismuth and antimony (2482. 2518.).

2534. Thus, arsenic with bismuth and antimony are found to possess the magnecrystallic force or condition.

Royal Institution,
September 23, 1848.

TWENTY-SECOND SERIES (*continued*)[1].

§ 28. *On the crystalline polarity of bismuth and other bodies, and on its relation to the magnetic and electric form of force* (*continued*). ¶ iv. *Crystalline condition of various bodies.* ¶ v. *Nature of the magnecrystallic force, and general observations.*

Received October 31,—Read December 7, 1848.

¶ iv. *Crystalline condition of various bodies.*

2535. *Zinc.*—Plates of zinc broken out of crystallized masses gave irregular indications, and, being magnetic from the impurity in them, the effects might be due entirely to that circumstance. Pure zinc was thrown down electro-chemically on platina from solutions of the chloride and the sulphate. The former occurred in ramifying dendritic associations of small crystal; the latter in a compact close form. Both were free from magnetic action and freely diamagnetic, but neither showed any trace of the magnecrystallic action.

2536. *Titanium*[2].—Some good crystals of titanium obtained from the bottom of an iron furnace, were cleansed by the alternate action of acids and fluxes until as clear from iron as I could procure them. They were bright, well-formed and magnetic (2371), and contained iron, I think, diffused through their whole mass, for nitromuriatic acid, by long boiling, continually removed titanium and iron from them. These crystals had a certain magnetic property which I am inclined to refer to their crystalline condition. When between the poles of the electro-magnet, they set; and when the electric current was discontinued, they still set between the poles of the enfeebled magnet as they did before. If left to itself, a crystal always took the *same* position, showing that it was constantly rendered magnetic

[1] Philosophical Transactions, 1849, p. 19.

[2] For these and many other crystals I am indebted to the kindness of Sir Henry T. De la Beche and Mr. Tennant.

in the same direction. But if a crystal was placed and kept in another position between the magnetic poles whilst the electric current was on, and afterwards the current suspended, and then the crystal set free, it pointed between the poles of the enfeebled magnet in this new direction; showing that the magnetism was in a different direction in the body of the crystal to that which it had before. If now the magnet were reinvigorated by the electric current, the crystal instantly spun round and took a magnetic state in the first or original direction. The crystals could in fact become magnetized in any direction, but there was one direction in which they could be magnetized with a facility and force greater than in any other. From the appearances I am inclined to refer this to the crystalline condition, but it may be due to an irregular diffusion of iron in the masses of titanium. The crystals were too small for me to make out the point clearly.

2537. *Copper.*—I selected some good crystals of native copper, and, having carefully separated them from the mass, examined them in respect of their magnecrystallic force. At the horse-shoe magnet (2486.) they gave no signs of such power, whatever the direction in which they were suspended, but stood in any position; and any degree of torsion, however small, applied at the upper extremity of the suspending filament, was obeyed at once, and to the full extent, by the crystal beneath. When subjected to the electro-magnet, the phænomena of arrest and revulsion were produced (2513. 2310.), as was to be expected. If after the arrest the magnetic force were continued, there was no slow advance of the crystal up to a distinct pointing position (2512.); it stood perfectly still in any position. So there is no evidence of magnecrystallic action in this case.

2538. *Tin.*—I selected from block and grain tin some pieces which appeared, by their external forms and the surface produced under the action of acids, to have a regular crystalline structure internally; and, cutting off portions, carefully submitted them to the power of the magnets, but there was no appearance of any magnecrystallic phænomena. Indications of the arresting and revulsive actions were presented, and also of diamagnetic force, but nothing else. I also examined some crystals of tin obtained by electro-chemical deposition. They were pure and diamagnetic: they were arrested and revulsed, but they showed no signs of magnecrystallic action.

2539. *Lead.*—Lead was crystallized by fusion, partial solidification, and pouring off (2457.), and some very fair crystals, having the general form of octohedra, were obtained. Observed at the magnets, these were arrested and revulsed feebly, but presented no magnecrystallic phænomena. Some fine crystalline plates of lead obtained electro-chemically from the decomposition of the acetate by zinc, were submitted to the magnet: they were pure, diamagnetic, and were arrested and revulsed, but presented no appearance of magnecrystallic action.

2540. *Gold.*—Three fine large crystals of gold were examined. They were diamagnetic, and easily arrested (2310. 2340.); the revulsion did not take place, because of their octohedral or orbicular form. They presented no magnecrystallic indications.

2541. *Tellurium.*—Two fractured pieces of this substance, presenting large and parallel planes of cleavage, were examined: both pointed, and the greatest length was across the axial line between flat-faced poles (2463.). I think the effects were in part, if not altogether, due to the magnecrystallic state of the substance; but I do not think the evidence was quite conclusive.

2542. *Iridium and Osmium alloy.*—The native grains of iridium and osmium are often flat, presenting two planes looking like crystal planes, which are parallel to each other even when the grains are thick. Some of the largest and most crystalline were selected, and, after ignition with flux and digestion in nitromuriatic acid, were examined at the magnet. Some were more magnetic than others, being attracted; others were very little magnetic: the latter were selected and examined more carefully. These all pointed with great readiness and force, comparatively speaking; for they were not above one-fifteenth of an inch long, and yet they set freely when the magnetic poles were 3 or 4 inches apart. The faces of the crystalline particles were *always* towards the poles, and their length consequently not *in* but *across* the axial line; and this was true whether the distance between the poles was small or great, or whether flat-faced or conical poles were used. I believe they were magnecrystallic.

2543. *Fusible metal.* — Crystals of fusible metal (2457.) pointed, but the crystals, which were apparently quadrangular plates or prisms, were not good, and the evidence not clear and distinct.

2544. *Wires.*—I thought it possible that thin wires, which by the action of acids exhibited fibrous arrangements, might have their particles in a state approaching to the crystalline condition, and therefore submitted bundles of platinum, copper, and tin wire to the action of the magnet; but no indications of magnecrystallic action appeared.

2545. I submitted several metallic compounds to the power of the magnet, applied so as to develope any indication of the magnecrystallic phænomena. Galena, native cinnabar, oxide of tin, sulphuret of tin, native red oxide of copper, Brookite or oxide of titanium, iron pyrites, and also diamond, fluor spar, rock-salt and boracite, being all well crystallized and diamagnetic, presented no evidence of the magnecrystallic force. Native and well-crystallized sulphuret of copper, sulphuret of zinc, cobalt glance and leucites were magnetic. Arsenical iron, specular iron and magnetic oxide of iron were still more so. I could not in any of them distinguish any magnetic results due to crystallization.

2546. On examining magnetic salts, several of them presented very striking magnecrystallic phænomena. Thus, with *sulphate of iron,* the first crystal which I employed was suspended with the magnecrystallic axis vertical, and it presented no particular appearances; only the longest horizontal direction went into the magnetic axis pointing feebly. But on turning the piece 90° (2470.), instantly it pointed with much force, and the greatest length went equatorially. The crystal was compounded of superposed flat crystals or plates, and the magnecrystallic axis went directly across these; it was easy therefore, after one or two experiments, to tell beforehand how the crystal should be suspended, and how it would point. Whether the crystals were long, or oblique, or irregular, still the magnecrystallic force predominated and determined the position of the crystal, and this happened whether pointed or flat poles were used, and whether they were near together or far asunder. The magnecrystallic axis is perpendicular, or nearly so, to two of the sides of the rhomboidal prism. I have some small prismatic crystals of which the length is nearly three times the width of the prism; but when both the length and the magnecrystallic axis are horizontal, no power of the magnet, or shape, or position of the poles, will cause the length to take the axial direction, for that is constantly retained by the magnecrystallic axis, so greatly does it predominate in power over the mere magnetic force of the crystal. Yet this latter is so great as at times to pull the suspending fibre asunder when the crystal is above the poles (2615.).

2547. *Sulphate of nickel.*—When a crystal of sulphate of nickel was suspended in the magnetic field, its length set axially. This might be due, either to mere magnetic force, or partly to magnecrystallic force. Therefore I cut a cube out of the crystal, two faces of which were perpendicular to the length of the original prism. This cube pointed well in the magnetic field, and the line coincident with the axis of the prism was that which pointed axially, and represented the magnecrystallic axis. Even when the cube was reduced in this direction and converted into a square plate whose thickness coincided with the magnecrystallic axis, it pointed as well as before, though the shortest dimensions of the piece were now axial.

2548. The *persulphate of ammonia and iron*, and the *sulphate of manganese*, did not give any indication of magnecrystallic phænomena; the *sulphate of ammonia and manganese* I think did, but the crystals were not good. The sulphate of potassa and nickel is magnecrystallic. All three salts were magnetic.

2549. Thus it seems that other bodies besides bismuth, antimony and arsenic, present magnecrystallic effects. Amongst these are the alloy of iridium and osmium, probably tellurium and titanium, and certainly the sulphates of iron and nickel. Before leaving this part of the subject, I may remark that this property has probably led me into error at times on a former occasion (2290.). A mistake with arsenic (2383.) might very easily arise from this cause.

¶ v. *On the nature of the magnecrystallic force, and general observations*[1].

2550. The magnecrystallic force appears to be very clearly distinguished from either the magnetic or diamagnetic forces, in that it causes neither approach nor recession; consisting not in attraction or repulsion, but in its giving a certain determinate position to the mass under its influence, so that a given line in relation to the mass is brought by it into a given relation with the direction of the external magnetic power.

[1] See onwards, 2836, &c.

2551. I thought it right very carefully to examine and prove the conclusion, that there was no connexion of the force with either attractive or repulsive influences. For this purpose I constructed a torsion-balance, with a bifilar suspension of cocoon silk, consisting of two bundles of seven filaments each, 4 inches long and one-twelfth of an inch apart; and suspended a crystal of bismuth (2457.) from one end of the lever, so that it might be fixed and retained in any position. This balance was protected by a glass case, outside of which the conical terminal of one pole of the great electro-magnet (2247.) was adjusted, so as to be horizontal, at right angles to the lever of the torsion-balance, and in such a position that the bismuth crystal was in the prolongation of the axis of the pole, and about half an inch from its extremity when all was at rest. The other pole, 4 inches off, was left large so that the lines of magnetic force should diverge, as it were, and rapidly diminish in strength from the end of the conical pole. The object was to observe the degree of repulsion exerted by the magnet on the bismuth, as a diamagnetic body, either by the distance to which it was repelled, or by the torsion required to bring it back to its first position; and to do this with the bismuth, having its magnecrystallic axis at one time axial or parallel to the lines of magnetic force, at another equatorial, observing whether any difference was produced.

2552. The crystal was therefore placed with its magnecrystallic axis first parallel to the lines of magnetic force, and then turned four times in succession 90° in a horizontal plane, so as to observe it under all positions of the magnecrystallic axis; but in no case could any difference in the amount of the repulsion be observed. In other experiments the axis was placed oblique, but still with the same result. If there be therefore any difference it must be exceedingly small[1].

2553. A corresponding experiment was made, hanging the crystal as a pendulum by a bifilar suspension of cocoon silk 30 feet in length, with the same result.

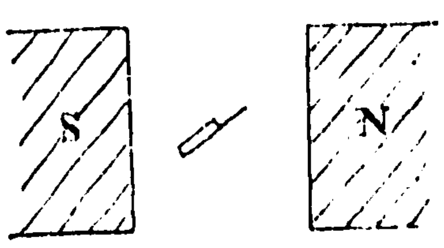
2554. Another very striking series of proofs that the effect is not due to attraction or repulsion, was obtained in the following manner. A skein of fifteen filaments of cocoon silk, about 14 inches long, was made fast above, and then a weight of an

[1] See now 2839, &c.

ounce or more hung to the lower end; the middle of this skein was about the middle of the magnetic field of the electro-magnet, and the square weight below rested against the side of a block of wood, so as to give a steady, silken vertical axis, without swing or revolution. A small strip of card, about half an inch long, and the tenth of an inch broad, was fastened across the middle of this axis by cement; and then a small prismatic crystal of sulphate of iron about 0·3 of an inch long, and 0·1 in thickness, was attached to the card, so that the length, and also the magnecrystallic axis, were in the horizontal plane; all the length was on one side of the silken axis, so that as the crystal swung round, the length was radius to the circle described, and the magnecrystallic axis parallel to the tangent.

2555. This crystal took a position of rest due to the torsion force of the suspending skein of silk; and the position could be made any one that was desired, by turning the weight below. The torsion force was such, that, when the crystal was made to vibrate on its silken axis, forty complete (or to and fro) vibrations were performed in a minute.

Fig. 4.



2556. When the crystal was made to stand between the flat-faced poles (2463.) obliquely, as in fig. 4, the moment the magnet was excited it moved, tending to stand with its length equatorial or its magnecrystallic axis parallel to the lines of magnetic force. When the N pole was removed, and the experiment repeated, the same effect took place, but not as strongly as before; and when, finally, the pole S was brought as near to the crystal as it could be, without touching it, the same result occurred, and with more strength than in the last case.

2557. In the two latter experiments, therefore, the crystal of sulphate of iron, though a magnetic body and strongly attracted by such a magnet as that used, actually *receded* from the pole of the magnet under the influence of the magnecrystallic condition.

2558. If the pole S be removed and that marked N be retained for action on the crystal, then the latter approaches the pole, urged by both the magnetic and magnecrystallic forces; but if the crystal be revolved 90° to the left, or 180° to the right, round the silken axis, so as to come into the contrary or opposite position, then this pole repels or rather causes the removal to a distance of the crystal, just as the former did. The experiment requires care, and I find that conical poles are not good; but with attention I could obtain the results with the utmost readiness.

2559. The sulphate of iron was then replaced by a crystalline plate (2480.) of bismuth, placed as before on one side of the silk suspender, and with its magnecrystallic axis horizontal. Making the position the same as that which the crystal had in relation to the N pole in the former experiment (2556.), so that to place its axis parallel to the lines of magnetic force it must approach this magnetic pole, and then throwing the magnet into an active state, the bismuth moved accordingly, and did approach the pole, against its diamagnetic tendency, but under the influence of the magnecrystallic force. The effect was small but distinct.

2560. Anticipating, for a short time, the result of the reasoning to be given further on (2607.), I will describe a corresponding effect obtained with the red ferro-prussiate of potassa. A crystal of this salt had its acute linear angles ground away, so as to convert it into a plate with faces parallel to the plane of the optic axis, and was then made to replace the plate of bismuth. Being in the position before represented (2556.), and the magnet rendered active, it moved, placing the plane of the optic axes equatorially, as Plücker describes. When the pole N was removed and S brought up to the crystal, the same motion occurred, the crystal *retreating from* the pole; and when S pole was removed and N brought towards the crystal, it moved as before, the whole body now *approaching* towards the pole. On inclining the crystal the other way, i. e. making its place on the other side of the equatorial line, the S pole caused it to *approach* and the N pole to *recede*. So that the same pole seemed able either to attract or repel the same side of the crystal; and either pole could be made to show this apparent attractive and repulsive force.

2561. Hence a proof that neither attraction nor repulsion causes the set, or governs the final position of the body, or of any of the bodies whose movements are due to the same cause (2607.).

2562. This force then is distinct in its character and effects from the magnetic and diamagnetic forms of force. On the other hand, it has a most manifest relation to the crystalline structure of the bismuth and other bodies; and therefore to the molecules, and to the power by which these molecules are able to build up the crystalline masses. It appears to me impossible to conceive of the results in any other way than by a mutual re-action of the magnetic force, and the force of the particles of the crystal on each other: and this leads the mind to another conclusion, namely, that as far as they can act on each other they partake of a like nature; and brings, I think, fresh help for the solution of that great problem in the philosophy of molecular forces, which assumes that they all have one common origin (2146.).

2563. Whether we consider a crystal or a particle of bismuth, its polarity has a very extraordinary character, as compared with the polarity of a particle in the ordinary magnetic state, or when compared with any other of the dual conditions of physical force; for the opposite poles have *like* characters; as is shown first of all by the diametral pointing of the masses (2461.), and also by the physical characters and relations of crystals generally. As the molecules lie in the mass of a crystal, therefore, they can in no way represent, or be represented by, the condition of a parcel of iron filings between the poles of a magnet, or the particles of iron in the keeper when in its place; for these have poles of *different* names and quality adhering together, and so giving a sort of structure; whereas, in the crystal, the molecules have poles of like nature towards each other, for, so to say, all the poles are alike.

2564. As made manifest by the phænomena, the magnecrystallic force is a force acting at a distance; for the crystal is moved by the magnet at a distance (2556. 2574.), and the crystal also can move the magnet at a distance. To produce the latter result, I converted a steel bodkin, about 3 inches long, into a magnet; and then suspended it perpendicularly by a single cocoon filament 4 inches long, from a small horizontal rod, which again was suspended by its centre and another length of cocoon filament, from a fixed point of support. In this manner the bodkin was free to move on its own axis, and could also describe a circle about 1½ inch in diameter; and the latter motion was not hindered by any tendency of the needle to point under the earth's influence, because it could take any position in the circle and yet remain parallel to itself.

2565. A support perfectly free from magnetic action was constructed of glass rod and copper wire, which passing through the bottom of the stand, and being in the prolongation of the upper axis of motion, was concentric with the circle which the little magnet could describe; its height was such that it could sustain a crystal or any other substance level with the pole at the lower end of the needle, and in the centre of the small circle in which the latter could revolve around it. By moving the lower end of the support, the upper end also could be made to approach to or recede from the magnet. The whole was covered with a glass shade, and when left to become of uniform temperature, and at rest, the needle magnet was found to take up a *constant* position under the torsion force of the suspending filaments. Further, any rotation of the glass and copper wire support did not produce a final change in the position of the magnet; for though the motion of the air would carry the magnet away, it returned, ultimately, to the same spot. When removed from this spot, the torsion force of the silk suspension made the system oscillate; the time of a half oscillation, or a passage in one direction, was about three minutes, and of a whole oscillation therefore six minutes.

2566. When a crystal bismuth was fixed on the support with the magnecrystallic axis in a horizontal direction, it could be placed near the lower pole of the magnet in any position, and being then left for two or three hours, or until by repeated examination the magnetic pole was found to be stationary, the place of the latter could be examined and the degree and direction in which it was affected by the bismuth ascertained. Extreme precaution was required in these observations, and all steel or iron things, as spectacles, knives, keys, &c., had to be dismissed from the observer before he entered the place of experiment; and glass candlesticks were used. The effect produced was but small, but the result was, that if the direction of the magnecrystallic axis made an angle of 10°, 20°, or 30° with the line from the magnetic pole to the middle of the bismuth crystal, then the pole followed it, tending to bring the two lines into parallelism; and this it did whichever end of the magnecrystallic axis was towards the pole, or whichever side it was inclined to. By moving the bismuth at successive times, the deviation of the magnetic pole could be carried up to 60°.

2567. The crystal of bismuth therefore is able to react upon and affect the magnet at a distance.

2568. But though it thus take up the character of a force acting at a distance, still it is due to that power of the particles which makes them cohere in regular order, and gives the mass its crystalline aggregation; which we call at other times the attraction of aggregation, and so often speak of as acting at *insensible* distances.

2569. For the further explication of the nature of this force, I proceeded to examine the effect of heat on crystals of bismuth when in the magnetic field. The crystals were suspended either by platina or fine copper wire, and heated, sometimes by a small spirit-lamp flame applied directly, sometimes in an oil-bath placed between the magnetic poles; and though the upward currents of air and fluid were strong in these cases, they were far too weak to overcome the set caused by magnecrystallic action, and helped rather to show when that action was weakened or ceased.

2570. When the temperature was gradually raised in the air the bismuth crystal continued to point, until of a sudden it became indifferent in that respect, and turned in any direction under the influence of the rising currents of air. Instantly removing the lamp flame the bismuth revolved slowly and regularly, as if there were no tendency to take up one position more than another, or no remains of magnecrystallic action; but in a few seconds, as the temperature fell, it resumed its power of pointing; and, apparently, in an instant and with full force, and the pointing was precisely in the same direction as at first. On examining the crystal carefully, its external shape and its cleavage showed that, as a crystal, it was unchanged; but the appearance of a minute globule of bismuth, which had exuded upon the surface in one place, showed that the temperature had been close upon the point of fusion.

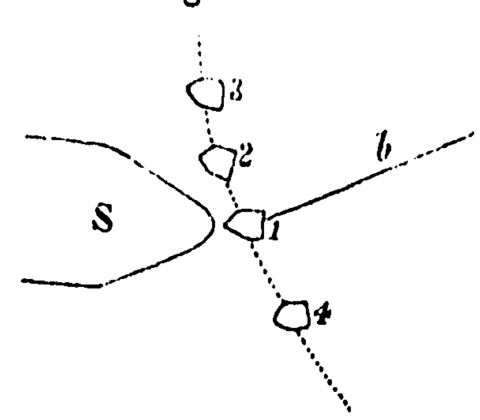
2571. The same result occurred in the oil-bath, except that as removing the lamp from the oil-bath did not immediately stop the addition of heat to the bismuth, so more of the latter was melted; and about one-fourth of the metal appeared as a drop hanging at the lower part. Still the whole mass lost its power at the high temperature, and the power was regained in the same direction, but in a less degree on cooling. The diminished force was accounted for on breaking up the crystal; for the parts which had been liquefied were now crystallized irregularly, and therefore, though active at the beginning of the experiment, were neutral at the end.

2572. As heat has this effect, the expectation entertained (2502.) of crystallizing bismuth regularly *in the magnetic field* is of course unfounded; for the metal must acquire the solid state, and be lowered through several degrees probably, before it can exhibit the magnecrystallic phænomena. If heat has the same effect on all bodies prior to their liquefaction, then, of course, such a process can be applied to none of them.

2573. A crystallized piece of antimony was subjected to the same experiment, and it also lost its magnecrystallic power below a dull red heat, and just as it was softening so as to take the impression of the copper loop in which it was hung. On being cooled it did not resume its former state, but then became ordinarily magnetic and pointed. This I conclude arose from iron affected by the flame and heat of the spirit-lamp; for, as the heat was high enough to burn off part of the antimony and make it rise in fumes of oxide of antimony, so this might set a certain portion of iron free which the carbon and hydrogen of the flame would leave in a very magnetic state (2608.).

2574. In further elucidation of the mutual action of the bismuth and the magnet, the bismuth was suspended, as already described (2551.), on the bifilar balance, but so turned, that its magnecrystallic axis, being horizontal, was not parallel or perpendicular to the arm of the lever, but a little inclined, as in the figure (5.), where 1 represents the crystal of bismuth attached to the balance arm *b*, the axis of which is so placed that the crystal can swing through the various positions 1, 2, 3, 4; S is the pole of the magnet separated only by the glass of the shade. It is manifest, that in position 1 the magnecrystallic axes and the lines of magnetic force are parallel to each other; whereas in the positions 2, 3, 4, they are oblique. When the apparatus was so arranged that the crystal of bismuth rested at 1, the superinduction of the full

Fig. 5.



magnetic force sent it towards 4; a result of diamagnetic action. When however the bismuth had its place of rest at 2, the development of the magnetic force did not make it pass towards 3, in accordance with the former result, but towards 1, which it usually attained and often passed, going a little towards 4. In this case the magnecrystallic and the diamagnetic forces were opposed to each other, and the former gained the advantage up to position 1.

2575. But though the crystal of bismuth in these cases moves across the lines of force in the magnetic field, it cannot be expected to do so in a field where the lines are parallel and of equal force, as between flat-faced poles; the crystal being restrained so as to move only parallel to itself; for under such circumstances the forces are equal in both directions and on both sides of the mass, and the only tendency the crystal has, in relation to its magnecrystallic condition, is to turn round a vertical axis until it is in its natural position in the magnetic field.

2576. A most important question next arises in relation to the magnecrystallic force, namely, whether it is an original force inherent in the crystal of bismuth, &c., or whether it is induced under the magnetic and electric influences. When a piece of soft iron is held in the vicinity of a magnet it acquires new powers and properties; some persons assume this to depend upon the development by induction of a new force in the iron and its particles, like in nature to that in the inducing magnet: by others it is considered that the force originally existed in the particles of the iron, and that the inductive action consisted only in the arrangement of all the elementary forces in one general direction. Applying this to the crystal of bismuth, we cannot make use of the latter supposition in the same manner; for all the particles are arranged beforehand, and it is that very arrangement of them and their forces which gives the bismuth its power. If the particles of a substance be in the heterogeneous condition possessed by those of the iron in its unmagnetic state, then the magnetic force may develope the magnetic, and also the diamagnetic condition, which probably is a condition of induction; but it does not appear at once, that it can develope a state of the kind now under consideration.

2577. That the particles hold their own to a great extent in all the results is manifest, by the consideration that they have an inherent power or force, the crystalline force, which is so unchangeable that no treatment to which they can be subjected can alter it; that it is this very force which, placing the particles in a regular position in the mass, enables them to act jointly on the magnet or the electric current, and affect or be affected by them; and that if the particles are not so arranged, but are in all directions in the mass, then the sum of their forces externally is nothing, and no inductive exertion of the magnet or current can develope the slightest trace of the phænomena.

2578. And that particles even before crystallization can act in some degree at a distance, by virtue of their crystallizing force, is, I think, shown by the following fact. A jar containing about a quart of solution of sulphate of soda, of such strength as to crystallize when cold by the touch of a crystal of the salt or an extraneous body, was left, accidentally, for a week or more unattended to and undisturbed. The solution remained fluid; but on the jar being touched, crystallization took place throughout the whole mass at once, producing clear, distinct, transparent plates, which were an inch or more in length, up to half an inch in breadth; and very thin, perhaps about the one-fiftieth or one-sixtieth of an inch. These were all horizontal, and of course parallel to each other; and I think, if I remember rightly, had their length in the same direction; and they were alike in character, and, apparently, in quantity in every part of the jar. They almost held the fluid in its place when the jar was tilted; and when the liquid was poured off presented a beautiful and uniform assemblage of crystals. The result persuaded me, at the time, that though the influence of a particle in solution and about to crystallize, must be immediately and essentially upon its neighbours, yet that it could exert an influence beyond these, without which influence, the whole mass of solution could hardly have been brought into such a uniform crystallizing state. Whether the horizontality of the plates can have any relation to the almost vertical lines of magnetic force, which from the earth's magnetism was pervading the solution during the whole time of its rest, is more than I will venture to say.

2579. The following are considerations which bear upon this great question (2576.) of an original or an induced state.

2580. In the first place, the bismuth carries off no power or particular state from the magnetic field, able to make it affect a magnet (2504.); so that if the condition acquired by the crystal be an induced condition, it is probably a transient one, and continues only whilst under induction. The fact, therefore, though negative in its evidence, agrees, as far as it tells, with that supposition.

2581. In the next place, if the effect were wholly due, as far as the crystal is concerned, to an original power inherent in the mass, we might expect to find the earth's magnetism, or any weak magnet, affecting the crystal. It is true that a weak magnetic force ought to *induce* any given condition in a crystal of bismuth just as well as a stronger, only proportionally. But if the given condition were inherent in the crystal, and did not change in its amount by the degree of magnetic force to which it was subjected, then a weak magnetic force ought to act more decidedly on the bismuth than it would do if the condition were induced in the bismuth, and only in proportion to its own force. Whatever the value of the argument, I was induced to repeat the experiment of the earth's influence (2505.) very carefully, and by sheltering the suspended crystals in small flasks or jars contained within the larger covering jar, and making the experiment in an underground place of uniform and constant temperature, I was able to exclude every effect of currents of air, so that the crystals obeyed the slightest degree of torsion given to the suspending fibre by the index above. Under these circumstances I could obtain no indications of pointing by the earth's action, either with crystals of bismuth or of sulphate of iron. Perhaps at the equator, where the lines of force are horizontal, they might be rendered sensible.

2582. In the third place, assuming that there is an original force in the crystals and their molecules, it might be expected that they would show some direct influence upon each other, independent of the magnetic force, and if so the best possible argument would be thus obtained that the force which is rendered manifest in the magnetic field was inherent in them. But on placing a large crystal with its magnecrystallic axis horizontal under a smaller and suspended one, or side by side with it, I could procure no signs of mutual action; even when the approximated parts of the crystals were ground or dissolved away, so as to let the two masses come as near as possible to each other, having large surfaces at the smallest possible distance. Extreme care is required in such experiments (2581.), or else many results are produced which seem to show a mutual affection of the bodies.

2583. Neither could I find any trace of mutual action between crystals of bismuth, or of sulphate of iron, when they were both in the *magnetic field*, the one being freely suspended and the other brought in various positions near to it.

2584. From the absence therefore or extreme weakness of any power in the crystals to affect each other, and also from the action of heat which can take away the power of the crystal before it has lost its mere crystalline condition (2570.), I am induced to believe that the force manifested in the crystal when in the magnetic field, which appears by external actions, and causes the motion of the mass, is chiefly and almost entirely *induced*, in a manner, subject indeed to the crystalline force, and finally additive to it; but at the same time exalting the force and the effects to a degree which they could not have approached without the induction.

2585. In that case the word magnetocrystallic ought probably to be applied to this force, as it is generated or developed under the influence of the magnet. The word magnecrystallic I used purposely to indicate that which I believed belonged to the crystal itself, and I shall still speak of the magnecrystallic axis, &c. in that sense.

2586. This force appears to me to be very strange and striking in its character. It is not polar, for there is no attraction or repulsion. Then what is the nature of the mechanical force which turns the crystal round (2460.), or makes it affect a magnet (2564.)? It is not like a turning helix of wire acted on by the lines of magnetic force; for there, there is a current of electricity required, and the ring has polarity all the time and is powerfully attracted or repelled*.

* Perhaps these points may find their explication hereafter in the action of contiguous particles (1663. 1710. 1729. 1735. 2443.).

2587. If we suppose for a moment that the axial position is that in which the crystal is unaffected, and that it is in the oblique position that the magnecrystallic axial direction is affected and rendered polar, giving two tensions pulling the crystal round, then there ought to be attractions at these times, and an obliquely presented crystal ought to be attracted by a single pole, or the nearest of two poles; but no action of this kind appears.

2588. Or we might suppose that the crystal is a little more apt for magnetic induction, or a little less apt for diamagnetic induction, in the direction of the magnecrystallic axis than in other directions. But, if so, it should surely show polar attractions in the case of the magnetic bodies, as sulphate of iron (2557. 2583.); and in the case of diamagnetic bodies, as bismuth, a difference in the degree of repulsion when presented with the magnecrystallic axis parallel and perpendicular to the lines of magnetic force (2552.); which it does not do.

2589. I do not remember heretofore such a case of force as the present one, where a body is brought into position only, without attraction or repulsion.

2590. If the power be induced, it must be like, generally, to its inducing predominants; and these are, at present, the magnetic and electric forces. If induced, subject to the crystalline force (2577.), it must show an intimate relation between it and them. How hopeful we may be, therefore, that the results will help to throw open the doors which may lead us to a full knowledge of these powers (2146.), and the combined manner in which they dwell in the particles of matter, and exert their influence in producing the wonderful phænomena which they present!

2591. I cannot resist throwing forth another view of these phænomena which may possibly be the true one. The lines of magnetic force may perhaps be assumed as in some degree resembling the rays of light, heat, &c.; and may find difficulty in passing through bodies, and so be affected by them, as light is affected. They may, for instance, when a crystalline body is interposed, pass more freely, or with less disturbance, through it in the direction of the magnecrystallic axis than in other directions. In that case, the position which the crystal takes in the magnetic field with its magnecrystallic axis parallel to the lines of magnetic force, may be the position of no, or of least resistance; and therefore the position of rest and stable equilibrium. All the diametral effects would agree with this view. Then, just as the optic axis is to a ray of polarized light, namely, the direction in which it is not affected, so would the magnecrystallic axis be to the lines of magnetic force. If such were the case, then, also, as the phænomena are developed in crystalline bodies, we might hope for the discovery of a series of effects dependent upon retardation and influence in direction, parallel to the beautiful phænomena presented by light with similar bodies. In making this supposition, I do not forget the points of inertia and momentum; but such an idea as I can form of inertia does not exclude the above view as altogether irrational. I remember too, that, when a magnetic pole and a wire carrying an electric current are fastened together, so that one cannot turn without the other, if the one be made axis the other will revolve round and carry the first with it; and also, that if a magnet be floated in mercury and a current sent down it, the magnet will revolve by the powers which are *within* its mass. With my imperfect mathematical knowledge, there seems as much difficulty in these motions as in the one I am supposing, and therefore I venture to put forth the idea[1]. The hope of a polarized bundle of magnetic forces is enough of itself to make one work earnestly with such an object, though only in imagination, before us; and I may well say that no man, if he take industry, impartiality and caution with him in his investigations of science, ever works experimentally in vain.

2592. I have already referred, in the former paper (2469.), to Plücker's beautiful discovery and results in reference to the repulsion of the optic axis[2] of certain crystals by the magnet, and have distinguished them from my own obtained with bismuth, antimony and arsenic, which are not cases of either repulsion or attraction; believing then, with Plücker, that the force there manifested is an optic axis force, exerted in the equatorial direction; and therefore existing in a direction at right angles to that which produces the magnecrystallic phænomena.

[1] See note (2639.).

[2] On the Repulsion of the Optic Axes of Crystals by the Poles of a Magnet, Poggendorff's Annalen, vol. lxxii., October 1847, or Taylor's Scientific Memoirs, vol. v. p. 353.

2593. But the relations of *both* to crystalline structure, and therefore to the force which confers that condition, are most evident. Other considerations as to position, set, and turning, also show that the two forces, so to say, have a very different relation to each other to that which exists between them and the magnetic or diamagnetic force. As, therefore, this strong likeness on the one hand, and distinct separation on the other is clearly indicated, I will endeavour to compare the two sets of effects, with the view of ascertaining whether the force exerted in producing them is not identical.

2594. I had the advantage of verifying Plücker's results under his own personal tuition in respect of tourmaline, staurolite, red ferro-prussiate of potassa, and Iceland spar. Since then, and in reference to the present inquiry, I have carefully examined calcareous spar, as being that one of the bodies which was at the same time free from magnetic action, and so simple in its crystalline relations as to possess but one optic axis.

2595. When a small rhomboid, about 0·3 of an inch in its greatest dimension, is suspended, with its optic axis horizontal, between the pointed poles (2458.) of the electro-magnet, approximated as closely as they can be to allow free motion, the rhomboid sets in the equatorial direction, and the optic axis coincides with the magnetic axis; but, if the poles be separated to the distance of half, or three-quarters of an inch, the rhomboid turned through 90°, and set with the optic axis in the equatorial direction, and the greatest length axial. In the first instance the diamagnetic force overcame the optic axis force; in the second the optic axis force was the stronger of the two.

2596. To remove the diamagnetic effect I used flat poles (2463.), and then the little rhomboid always set in, or vibrated about, that position in which its optic axis was equatorial.

2597. I also took three cubes of calcareous spar (1695.), in which the optic axes were perpendicular to two of the faces, of the respective dimensions of 0·3, 0·5, and 0·8 of an inch in the side, and placed these in succession in the magnetic field, between either flat or pointed poles. In all cases, the optic axis, if horizontal, passed into the equatorial position; or, if vertical, left the cubes indifferent as to direction. It was easy by the method of two positions (2470.) to find the line of force, which, being vertical, left the mass unaffected by the magnet; or being horizontal, went into the equatorial position; and then examining the cube by polarized light, it was found that this line coincided with the optic axis.

2598. Even the horse shoe magnet (2485.) is sufficiently strong to produce these effects.

2599. I tried two similar cubes of rock-crystal (1692.), but could perceive no traces of any phænomena having either magneoptic, or magnecrystallic, or any other relation to the crystalline structure of the masses.

2600. But though it is thus very certain that there is a line in a crystal of calcareous spar coinciding with the optic axis, which line seems to represent the resultant of the forces which make the crystal take up a given position in the magnetic field; and, though it is equally certain that this line takes up its position in the equatorial direction ; yet, considered as a line of force, *i. e.* as representing the direction of the force which places the crystal in that position, it seems to me to have something anomalous in its character. For, that a directing and determining *line* of force should have, as its full effect, the result of going into a *plane* (the equatorial), in which it can take up any one of an infinite number of positions indifferently, leaves an imperfect idea on my mind ; and a thought, that there is some other effect or residual phænomena to be recognized and accounted for.

2601. On further consideration, it appears that a simple combination of the magnecrystalline condition, as it exists in bismuth, will supply us with a perfect representation of the state of calcareous spar ; for, by placing two equal pieces of bismuth with their magnecrystallic axes perpendicular to each other (2484.), we have a system of forces which seems to possess, as a resultant, a line setting in the equatorial direction. When that line is vertical, the system is, as regards position, indifferent ; but when horizontal, the system so stands, that the line is in the equatorial plane. Still, the real force is not in the equatorial direction, but axial ; and the system is moved by what may be considered a *plane of axial force* (resulting from the union of the two axes at right angles to each other), rather than by a *line of equatorial force*.

2602. Doubtless, the rhomboid or cube (2597.) of calcareous spar is not a compound crystal, like the system of bismuth crystals just referred to (2601.) ; but its molecules may possess a compound disposition of their forces, and may have two or more axes of power, which at the same time that they cause the crystalline structure, may exert such force in relation to the magnet, as to give results in the same manner, and of the same kind, as those of the double crystal of bismuth (2601.). Indeed, that there should be but one axis of crystalline force, either in the particle of Iceland spar, or in those of bismuth, does not seem to me to be any way consistent with the cleavage of the substances in three or more directions.

2603. The optic axis in a piece of calcareous spar, is simply the line in which, if a polarized, or ordinary ray of light moves, it is the least affected. It may be a line which, as a resultant of the molecular forces, is that of the least intensity; and, certainly, as regards ordinary and mechanical means of observing cohesion, a piece of calcareous spar is sensibly, and much harder on the faces and parts which are parallel to the optic axis, than on those perpendicular to it. An ordinary file or a piece of sandstone shows this. So that the plane equatorial to the optic axis, as it represents directions in which the force causing crystallization is greater in degree than in the direction of the optic axis, may also be that in which the resultant of its magnecrystallic force is exerted.

2604. I am bound to state, as in some degree in contrast with such considerations, that, with bismuth, antimony and arsenic, the cleavage is very facile perpendicular to the magnecrystallic axis (2475. 2510. 2532.). But we must remember that the cleavage (and therefore the cohesive) force is not the only thing to be considered, for in calcareous spar it does not coincide with either the axial or the equatorial direction of the substance in the magnetic field: we must endeavour to look beyond this to the polar (or axial) condition of the particles of the masses, for the full understanding and true relation of all these points.

2605. I am bound, also, to admit that, if we consider calcareous spar as giving the simple system of force, we may, by the juxtaposition of two crystals with their optic axes at right angles to each other, produce a compound mass, which will truly represent the bismuth in the direction of the force; *i. e.* it will, in the magnetic field, point with apparently one line of force only, and that in the axial direction, whilst it may be really moved by a system of forces lying in the equatorial plane. I will not at present pretend to say that this is not the state of things; but I think, however, that the metals, bismuth, antimony and arsenic, present us with the simplest as they do the strongest cases of magnecrystallic force; and whether that be so or not, I am still of opinion that the phænomena discovered by Plücker and those of which I have given an account in these two papers, have one common origin and cause.

2606. I went through all the experiments and reasonings with Plücker's crystals (as the carbonate of lime, tourmaline and red ferro-prussiate of potassa), in reference to the question of original or induced power (2576.), as before, and came to the same conclusion as in the former case (2584.).

2607. I could not find that crystals of red ferro-prussiate of potassa or tourmaline were affected by the earth's magnetism (2581.), or that they had the power of affecting each other (2582.). Neither could I find that Plücker's effect with calcareous spar, or red ferro-prussiate of potassa, was either an attractive or repulsive effect, but one connected with position only (2550. 2560.). All which circumstances tend to convince me that the force active in his experiments, and that in my results with bismuth, &c., is the same[1].

2608. A small rhomboid of Iceland spar was raised to the highest temperature in the magnetic field which a spirit-lamp could give (2570.); it was at least equal to the full red heat of copper, but it pointed as well then as before. A short thick tourmaline was heated to the same degree, and it also pointed equally well. As it cooled, however, it became highly magnetic, and seemed to be entirely useless for experiments at low temperatures; but on digesting it for a few seconds in nitromuriatic acid, a little iron was dissolved from the surface, after which it pointed as well, and in accordance with Plücker's law, as before. A little peroxide upon the surface had been reduced by the flame and heat to protoxide, and caused the magnetic appearances.

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[1] The optic axis is the direction of least optic force; and by Plücker's experiments, coincides with what I consider in my results as the direction of minimum magnecrystallic force. It is more than probable that, wherever the two sets of effects (whether really or only nominally different) can be recognized in the same body, the directions of maximum effect, and also those of minimum effect, will be found to coincide.—November 23, 1848.

2609. There is a general and, as it appears to me, important relation between Plücker's magneto-optical results and those I formerly obtained with heavy glass and other bodies (2152, &c.). When any of these bodies are subject to strong induction under the influence of the magnetic or electric forces, they acquire a peculiar state, in which they can influence a polarized ray of light. The effect is a rotation of the ray, if it be passed through the substance parallel to the lines of magnetic force, or in other words, in the axial direction; but if it be passed in the equatorial direction, no effect is produced. The equatorial plane, therefore, is that plane in which the condition of the molecular forces is the least disturbed as respects their influence on light. So also in Plücker's results, the optic axis, or the optic axes, if there be two, go into that plane under the same magnetic influence, they also being the lines in which there is the least, or no action on polarized light.

2610. If a piece of heavy glass, or a portion of water, could be brought beforehand into this constrained condition, and then placed in the magnetic field, I think there can be no doubt that it would move, if allowed to do so, and place itself naturally, so that the plane of no action on light should be equatorial, just as Plücker shows that a crystal of calcareous spar or tourmaline does in his experiments. And, as in his case, the magnetic or diamagnetic character of the bodies, makes no difference in the general result; so in my experiments, the optical effect is produced in the same direction, and subject to the same laws, with both classes of substances (2185. 2187.).

2611. But though thus generally alike in this great and leading point, there is still a vast difference in the disposition of the forces in the heavy glass and the crystal; and there is a still greater difference in this, that the heavy glass takes up its state only for a time by constraint and under induction, whilst the crystal possesses it freely, naturally and permanently. In both cases, however, whether natural or induced, it is a state of the particles; and comparing the effect on light of the glass under constraint with that of the crystal at liberty, it indicates a power in the magnet of inducing something like that condition in the particles of matter which is necessary for crystallization; and that even in the particles of fluids (2184.).

2612. If there be any weight in these considerations, and if the forces manifested in the crystals of bismuth and Iceland spar be the same (2607.), then there is further reason for believing that, in the case of bismuth and the other metals named, there is, when they are subjected to the power of the magnet, both an induced condition of force (2584.), and also a pre-existing force (2577.). The latter may be distinguished as the crystalline force, and is shown, first, by such bodies exhibiting optic axes and lines of force when not under induction; by the symmetric condition of the whole mass, produced under circumstances of ordinary occurrence; and by the fixity of the line of magnecrystallic force in the bodies shown experimentally to possess it.

2613. Though I have spoken of the magnecrystallic axis as a given line or direction, yet I would not wish to be understood as supposing that the force decreases, or state changes, in an equal ratio all round from it. It is more probable that the variation is different in degree in different directions, dependent on the powers which give difference of form to the crystals. The knowledge of the disposition of the force can be ascertained minutely hereafter, by the use of good crystals, an unchangeable ordinary magnet (2485. 2528.), or a regulated electro-magnet, flat-faced poles (2463.), and torsion (2500. 2530.).

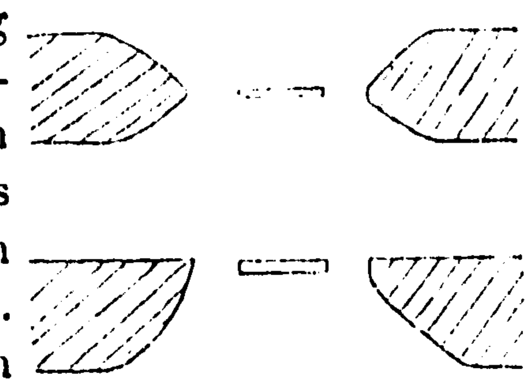
2614. I cannot conclude this series of researches without remarking how rapidly the knowledge of molecular forces grows upon us, and how strikingly every investigation tends to develope more and more their importance, and their extreme attraction as an object of study. A few years ago magnetism was to us an occult power, affecting only a few bodies; now it is found to influence all bodies, and to possess the most intimate relations with electricity, heat, chemical action, light, crystallization, and, through it, with the forces concerned in cohesion; and we may, in the present state of things, well feel urged to continue in our labours, encouraged by the hope of bringing it into a bond of union with gravity itself.

Royal Institution,
October 20, 1848.

¶ vi. NOTE.—*On the position of a crystal of sulphate of iron in the magnetic field.*

Received December 7, 1848.—Read December 7, 1848.

Fig. 6.



2615. Though effects of the following nature are general, yet I think it convenient to state that I obtained them chiefly by the use of magnetic poles (2247.), the form of which is given in the plan and side-view annexed (fig. 6.). The crystals submitted to their action were suspended by cocoon silk, so as to be level with the upper surface of the poles.

2616. A prismatic crystal of protosulphate of iron was selected, which was nearly 0·9 of an inch in length, 0·1 in breadth, and 0·05 in thickness; by examination the magnecrystallic axis was found to coincide with the thickness, and therefore to be perpendicular, or nearly so (2546.), to the plate. Being suspended as above described, and the magnet (2247.) excited by ten pair of Grove's plates, the crystal stood transverse, or with its *magnecrystallic axis* parallel to the axis of magnetic force, when the distance between the poles was 2·25 inches or more; but when the distance was about 2 inches or less, then it stood with its length axial, or nearly so, and its magnecrystallic axis therefore transverse to the lines of magnetic force. In the intermediate distances between 2 and 2·25 inches, the prism assumed an oblique position (2634.), more or less inclined to the axial line, and so passing gradually from the one position to the other. This intermediate distance I will for the present call *n* (neutral) distance.

2617. If the poles be 2 inches apart and the crystal be gradually lowered, it passes through the same intermediate oblique positions into the transverse position; or if the crystal be raised, the same transitions occur; at any less distance the changes are the same, but later. They occur more rapidly when the crystal is raised than when it is lowered; but this is only because of the unsymmetric disposition and intensity of the lines of magnetic force around the magnetic axis, due to the horse-shoe form of the magnet and shape of the poles. If two cylinder magnets with equal conical terminations were employed, there is no doubt that for equal amounts of elevation or depression, corresponding changes would take place in the position of the crystal.

2618. These changes however are not due to mere diminution of the magnetic force by distance, but to differences in the *forms* or *direction* of the resultants of force. This is shown by the fact that, if the crystal be left in its first position, and so pointing with the length axially, no diminution of the force of the magnet alters the position; thus, whether one or ten pair of plates be used to excite the magnet, the *n* distance (2616.) remains unchanged; and even descending to the use of an ordinary horse-shoe magnet, I have found the same result.

2619. Variation in the length of the prismatic crystal has an important influence over the result. As the crystal is shorter, the distance *n* diminishes, all the other phænomena remaining the same. A crystal 0·7 of an inch long, but thicker than the last, had for its maximum *n* distance 1·7 inch. A still shorter crystal had for its maximum *n* distance 1·1 inch. In all these cases variation of the force of the magnet caused no sensible change.

2620. Variation in that dimension of the crystal coincident with the magnecrystallic axis affected the *n* distance: thus, increase in the length of the magnecrystallic axis diminished the distance, and diminution of it in that direction increased the distance. This was shown in two ways; first, by placing a second prismatic crystal by the side of the former in a symmetric position (2636.), which reduced the *n* distance to between 1·75 and 2 inches; and next, by employing two crystals in succession of the same length but different thicknesses. The thicker one had the smaller *n* distance.

2621. Variation in the depth of the crystal, *i. e.* its vertical dimension, did not produce any sensible effect on the *n* distance: nor by theory should it do so, until the extension upwards or downwards brings the upper or lower parts into the condition of raised or depressed portions (2617.).

2622. Variation in the form of the poles affects the *n* distance. As they are more acute, the distance increases; and as they are more obtuse up to flat-faced poles (2463.), the distance diminishes.

2623. With the shorter crystals, or with obtuse poles, it is often necessary to diminish the power of the magnet, or else the crystal is liable to be drawn to the one or other pole. This, however, may be avoided by employing a vertical axis which is confined below as well as above (2554.); and then the difference in *strength* of the magnet is shown to be indifferent to the results, or very nearly so.

2624. These effects may probably be due to the essential difference which exists between the ordinary magnetic and the magnecrystallic action, in that the first is *polar*, and the second only *axial* (2472.) in character. If a piece of magnetic matter, iron for instance, be in the magnetic field, it immediately becomes polar (*i.e.* has terminations of different qualities). If many iron particles be there, they all become polar; and if they be free to move, arrange themselves in the direction of the axial line, being joined to each other by contrary poles; and by that the polarity of the extreme particles is increased. Now this does not appear to be at all the case with particles under the influence of the magnecrystallic force; the force seems to be altogether *axial*, and hence probably the difference above, and in many other results.

2625. Thus, if four or more little cubes of iron be suspended in a magnetic field of equal force (2465.), they will become polar; if also four similar cubes of crystallized bismuth be similarly circumstanced, they will be affected and point. If the iron cubes be arranged together in the direction of the equatorial line, they will form an aggregate in a position of unstable equilibrium, and will immediately, as a whole, turn and point with the length axially; whereas the bismuth cubes by such approximation will suffer no sensible change.

2626. The *extreme* (and the other) associated cubes of the elongated iron arrangement now have a polar force above that which they had before; and the whole group serves, as it were, as a conductor for the lines of magnetic power; for many of them concentrate upon the iron, and the intensity of power is much stronger between the ends of the iron arrangement and the magnetic poles, than it is in other parts of the magnetic field. Such is not the case with the bismuth cubes; for however they be arranged, the intensity of force in the magnetic field is, as far as experiments have yet gone, unaffected by them; and the intensity of the molecules of the crystals appears to remain the same. Hence the iron stands lengthways between the poles; the bismuth crystals, on the contrary, whether arranged side by side, as respects the magnecrystallic axis, so as to stand as to length equatorially; or end to end, so as to stand axially, are perfectly indifferent in that respect, vibrating and setting equally both ways.

2627. A given piece of iron when introduced into a field of equal magnetic force, and brought towards the pole, adheres to it and disturbs the intensity of the field, producing a pointed form of pole in one part with diverging lines of force: a crystal of bismuth vibrates with sensibly equal force in every part of the field (2467.), and does not disturb the distribution of the power.

2628. Considering all these actions and conditions, it appears to me that the occurrence of the n distance with a body which is at the same time magnetic and magnecrystallic, may be traced to that which causes them and their differences, namely, the *polarity* belonging to the magnetic condition, and the *axiality* belonging to the magnecrystallic condition. Thus, suppose an uniform magnetic field three inches from pole to pole, and a bar of magnetic matter an inch long, suspended in the middle of it; by virtue of the polarity it acquires, it will point axially, and carry on, or conduct, with its mass, the magnetic force, so much better than it was conducted in the same space before, that the lines of force between the ends of this bar and the magnetic poles, will be concentrated and made more intense than anywhere else in the magnetic field. If the poles be made to approach towards the bar, this effect will increase, and the bar will conduct more and more of the magnetic force, and point with proportionate intensity. It is not merely that the magnetic field becomes more intense by the approximation of the poles, but the proportion of force carried on by the bar becomes greater as compared to that conveyed onwards by an equal space in the magnetic field at its side.

2629. But if a similar bar of magnecrystallic substance be placed in the magnetic field, its power does not rise in the same manner, or in the same great proportion, by approximation of the poles. There can be no doubt that such approximation increases the intensity of the lines of force, and therefore increases the intensity of the magneto-crystallic state; but this state does not appear to be due to polarity, and the bar does not convey more power through it than is conveyed onwards elsewhere through an equal space in the magnetic field. Hence its directive force does not increase in the same rapid degree as the directive force of the magnetic bar just referred to.

2630. If then we take a bar which, like a prism of sulphate of iron, is magnetic, and also magnecrystallic, having the magnecrystallic axis perpendicular to its length, such a bar, properly suspended, ought to have an *n* distance of the poles, within which the forces ought to be nearly in equilibrium; whilst at a greater distance of the poles, the magnecrystallic force ought to predominate; and at a lesser distance, the magnetic force ought to have the advantage; simply, because the magnetic force, in consequence of the true polarity of the molecules, grows up more rapidly and diminishes more rapidly than the magneto-crystallic force.

2631. This view, also, is consistent with the fact that variation of the force of the magnet does not affect the *n* distance (2618. 2619.); for, whether the force be doubled or quadrupled, both the magnetic and magneto-crystallic forces are at the same time doubled or quadrupled; and their proportion therefore remains the same.

2632. The raising or lowering of the crystal above or below the line of maximum magnetic force is manifestly equivalent in principle to the separation of the magnetic poles; and therefore should produce corresponding effects: and that is the case (2617.). Besides that, when the crystal is raised above the level of the poles, such resultants of magnetic force as pass through it, are no longer parallel to its length, but more or less curved, so that they probably cannot act with the same amount of power in throwing the whole crystal into a consistent polarized magnetic condition, as if they were parallel to it: whereas, as respects the induction of the magneto-crystallic condition, each of the particles appears to be affected independently of the others; and, therefore, any loss of an effect dependent upon joint action would not be felt here.

2633. M. Plücker told me, when in England in August last, that the repulsive force on the optic axis diminishes and increases less rapidly than the magnetic force, by change of distance; but is not altered in its proportion to the magnetic force by employing a stronger or weaker magnet. This is manifestly the same effect as that I have been describing; and makes me still more thoroughly persuaded that his results and mine are due to one and the same cause (2605. 2607.).

2634. I have said that, within the *n* distance, the crystal of sulphate of iron pointed more or less obliquely (2616.); I will now state more particularly what the circumstances are. If the distance *n* be so adjusted, that the prismatic crystal, which is at the time between the magnetic poles, shall make an angle of 30° (or any quantity) with the axial line; then it will be found that there is another stable position, namely, the diametral position (2461.), in which it can stand; but that the obliquity is always on the same side of the axial line; and that the crystal will not stand with the like obliquity of 30° on the opposite side of the magnetic axis.

2635. If the crystal be turned 180° round a vertical axis, or end for end, then the inclination, and the direction in which it occurs, remain unchanged; in fact, it is simply giving the crystal the diametral position. But if the crystal be revolved 180° round a horizontal axis; either that coinciding with its length, which represents its maximum magnetic direction; or that corresponding with its breadth, and therefore with the magnecrystallic axis; then the inclination is the same in amount as before, but it is on the *other* side of the axial line.

2636. This is the case with all the prismatic crystals of sulphate of iron which I have tried. The effect is very determinate; and, as would be expected, when two crystals correspond in the direction of the inclination, they also correspond in the position of their form and direction of the various planes.

2637. All these variations of position indicate an oblique resultant of setting force, derived from the joint action of the magnetic and magnecrystallic forces; and would be explained by the supposition, that the magnecrystallic axis or line of maximum magnecrystallic force, was not perpendicular to the chief planes of the crystal (or those terminating it), but a little inclined in the direction of the length.

2638. Whether this be the case, or whether the maximum line of magnetic force may not, even, be a little inclined to the length of the prism; still, the *n* distance supplies an excellent experimental opportunity of examining this inclination, however small its quantity may be; because of the facility with which the influence of either the one or the other may be made predominant in any required degree.

Royal Institution,
December 5, 1848.

2639. *Note.* (2591.) Another supposition may be thrown out for consideration. I have already said that the assumption of a mere axial condition (2587. 2591.) would account for the set without attraction or repulsion. Now if we suppose it possible that the molecules should become polar in relation to the north and south poles of the magnet, but with *no mutual relation amongst themselves,* then the bismuth or other crystal might set as if induced with mere axial power: but it seems to me very improbable that polarities of a given particle in a crystal should be subject to the influence of the polarities of the distant magnet poles, and not also to the *like polarities* of the contiguous particles.—January 24, 1849.

TWENTY-THIRD SERIES[1].

§ 29. *On the polar or other condition of diamagnetic bodies.*

Received January 1,—Read March 7 and 14, 1850.

2640. Four years ago I suggested that all the phænomena presented by diamagnetic bodies, when subjected to the forces in the magnetic field, might be accounted for by assuming that they then possessed a polarity the same in kind as, but the reverse in direction of, that acquired by iron, nickel and ordinary magnetic bodies under the same circumstances (2429. 2430.). This view was received so favourably by Plücker, Reich and others, and above all by W. Weber[2], that I had great hopes it would be confirmed; and though certain experiments of my own (2497.) did not increase that hope, still my desire and expectation were in that direction.

2641. Whether bismuth, copper, phosphorus, &c., when in the magnetic field, are polar or not, is however an exceedingly important question; and very essential and great differences, in the mode of action of these bodies under the one view or the other, must be conceived to exist. I found that in every endeavour to proceed by induction of experiment from that which is known in this department of science to the unknown, so much uncertainty, hesitation and discomfort arose from the unsettled state of my mind on this point, that I determined, if possible, to arrive at some experimental proof either one way or the other. This was the more needful, because of the conclusion in the affirmative to which Weber had come in his very philosophical paper; and so important do I think it for the progress of science, that, in those imperfectly developed regions of knowledge, which form its boundaries, our conclusions and deductions should not go far beyond, or at all events not aside

[1] Philosophical Transactions, 1850, p. 171.

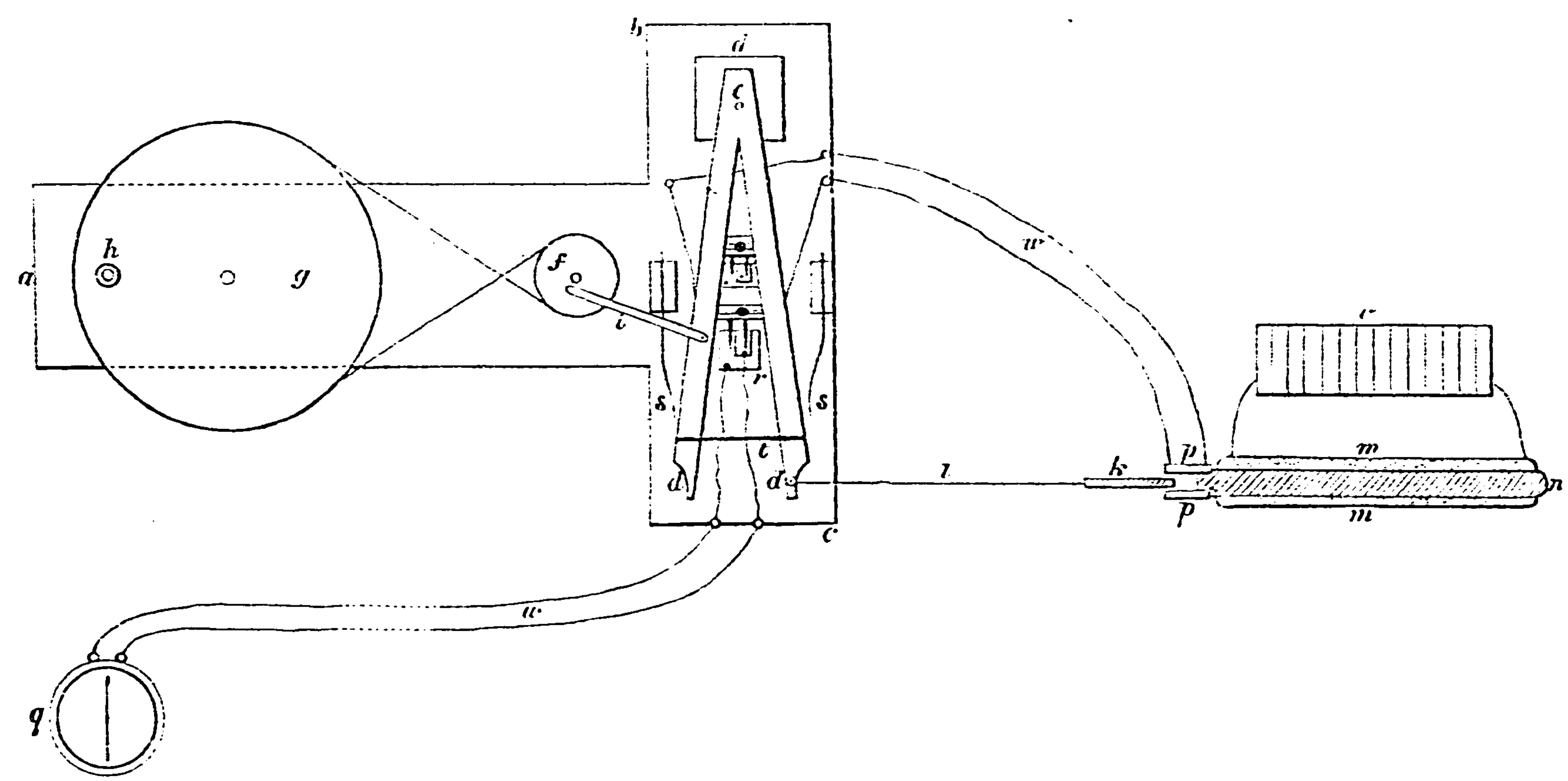
[2] Poggendorff's Annalen, January 7, 1848, or Taylor's Scientific Memoirs, v. p. 477.

from the results of experiment (except as suppositions), that I do not hesitate to lay my present labours, though they arrive at a negative result, before the Royal Society.

2642. It appeared to me that many of the results which had been supposed to indicate a polar condition, were only consequences of the law that diamagnetic bodies tend to go from stronger to weaker places of action (2418.); others again appeared to have their origin in induced currents (26. 2338.); and further consideration seemed to indicate that the differences between these modes of action and that of a real polarity, whether magnetic or diamagnetic, might serve as a foundation on which to base a mode of investigation, and also to construct an apparatus that might give useful conclusions and results in respect of this inquiry. For, if the polarity exists it must be in the particles and for the time permanent, and therefore distinguishable from the momentary polarity of the mass due to induced temporary currents; and it must also be distinguishable from ordinary magnetic polarity by its contrary direction.

2643. A straight wooden lever, 2 feet in length, was fixed by an axis at one end, and by means of a crank and wheel made to vibrate in a horizontal plane, so that its free extremity passed to and fro through about 2 inches. Cylinders or cores of metal or other substances, 5½ inches long and three quarters of an inch diameter, were fixed in succession to the end of a brass rod 2 feet long, which itself was attached at the other end to the moving extremity of the lever, so that the cylinders could be moved to and fro in the direction of their length through the space of 2 inches. A large cylinder electro-magnet was also prepared (2191.), the iron core of which was 21 inches long, and 1·7 inch in diameter; but one end of this core was made smaller for the length of 1 inch, being in that part only 1 inch in diameter.

2644. On to this reduced part was fixed a hollow helix consisting of 516 feet of fine covered copper wire: it was 3 inches long, 2 inches external diameter, and 1 inch internal diameter: when in its place, 1 inch of the central space was occupied by the reduced end of the electro-magnet core which carried it; and the magnet and helix were both placed concentric with the metal cylinder above mentioned, and at such a distance that the latter, in its motion, would move within the helix in the di-



a, b, c, frame board; *d, d, d* wooden lever, of which *e* is the axis, *f* the crank-wheel, and *g* the great wheel with its handle *h*; *i* the bar connecting the crank-wheel and lever; *k* a cylinder or core of metal to be submitted to experiment; *l* the rod connecting it with the lever; *m* the helix of the electro-magnet; *n* the iron core, and *o* the exciting battery; *p* the experimental helix; *q* the galvanometer, 20 feet from the electro-magnet; *r* the commutator; *w, w* connecting wires; *s, s* springs of brass or copper; *t* a copper rod connecting the two arms of the lever, to give strength. The plan is to a scale of one fifteenth: the part at the electro-magnet and experimental helix is in section; the further description is contained in paragraphs 2643, 2644, 2645 and 2648.

rection of its axis, approaching to and receding from the electro-magnet in rapid or slow succession. The least and greatest distances of the moving cylinder from the magnet during the journey were one-eighth of an inch and 2·2 inches. The object of course was to observe any influence upon the experimental helix of fine wire which the metal cylinders might exert, either whilst moving to or from the magnet, or at different distances from it[1].

2645. The extremities of the experimental helix wire were connected with a very delicate galvanometer, placed 18 or 20 feet from the machine, so as to be unaffected directly by the electro-magnet; but a commutator was interposed between them. This commutator was moved by the wooden lever (2643.), and as the electric currents which would arrive at it from the experimental helix, in a complete cycle of motion or to and fro action of the metal cylinder (2643.), would consist of two contrary portions, so the office of this commutator was, sometimes to take up these portions in succession and send them on in one consistent current to the galvanometer, and at other times to oppose them and to neutralize their result; and therefore it was made adjustible, so as to change at any period of the time or part of the motion.

2646. With such an arrangement as this, it is known that, however powerful the magnet, and however delicate the other parts of the apparatus, no effect will be produced at the galvanometer as long as the magnet does not change in force, or in its action upon neighbouring bodies, or in its distance from, or relation to, the experimental helix; but the introduction of a piece of iron into the helix, or anything else that can influence or be influenced by the magnet, can, or ought to, show a corresponding influence upon the helix and galvanometer. My apparatus I should imagine, indeed, to be almost the same in principle and practice as that of M. Weber (2640.), except that it gives me contrary results.

2647. But to obtain correct conclusions, it is most essential that extreme precaution should be taken in relation to many

[1] It is very probable that if the metals were made into cylinders shorter, but of larger diameter than those described above, and used with a corresponding wider helix, better results than those I have obtained would be acquired.

points which at first may seem unimportant. All parts of the apparatus should have perfect steadiness, and be fixed almost with the care due to an astronomical instrument; for any motion of any portion of it is, from the construction, sure to synchronize with the motion of the commutator; and portions of effect, inconceivably small, are then gathered up and made manifest as a whole at the galvanometer; and thus, without care, errors might be taken for real and correct results. Therefore, in my arrangements, the machine (2643, &c.), the magnet and helix, and the galvanometer stood upon separate tables, and these again upon a stone floor laid upon the earth; and the table carrying the machine was carefully strutted to neighbouring stone-work.

2648. Again, the apparatus should itself be perfectly firm and without shake in its motion, and yet easy and free. No iron should be employed in any of the moving parts. I have springs to receive and convert a portion of the momentum of the whole at the end of the to and fro journey; but it is essential that these should be of hammered brass or copper.

2649. It is absolutely necessary that the cylinder or core in its motion should not in the least degree disturb or shake the experimental helix and the magnet. Such a shake may easily take place and yet (without much experience) not be perceived. It is important to have the cores of such bodies as bismuth, phosphorus, copper, &c., as large as may be, but I have not found it safe to have less than one-eighth of an inch of space between them and the interior of the experimental helix. In order to float, as it were, the core in the air, it is convenient to suspend it in the bight or turn of a fine copper wire passing once round it, the ends of which rise up, and are made fast to two fixed points at equal heights but wide apart, so that the wire has a V form. This suspension keeps the core parallel to itself in every part of its motion.

2650. The magnet, when excited, is urged by an electric current from five pairs of Grove's plates, and is then very powerful. When the battery is not connected with it, it still remains a magnet of feeble power, and when thus employed may be referred to as in the *residual state*. If employed in the residual state, its power may for the time be considered constant, and the experimental helix may at any moment be connected with the galvanometer without any current appearing there. But if the magnet be employed in the excited state, certain important precautions are necessary; for upon connecting the magnet with the battery and then connecting the experimental helix with the galvanometer, a current will appear at the latter, which will, in certain cases, continue for a minute or more, and which has the appearance of being derived at once from that of the battery. It is not so produced, however, but is due to the *time* occupied by the iron core in attaining its maximum magnetic condition (2170. 2332.), during the whole of which it continues to act upon the experimental helix, producing a current in it. This time varies with several circumstances, and in the same electro-magnet varies especially with the period during which the magnet has been out of use. When first employed, after two or three days' rest, it will amount to eighty or ninety seconds, or more. On breaking battery contact and immediately renewing it, the effect will be repeated, but occupy only twenty or thirty seconds. On a third intermission and renewal of the current, it will appear for a still shorter period; and when the magnet has been used at short intervals for some time, it seems capable of receiving its maximum power almost at once. In every experiment it is necessary to wait until the effect is shown by the galvanometer to be over; otherwise the last remains of such an effect might be mistaken for a result of polarity, or some peculiar action of the bismuth or other body under investigation.

2651. The galvanometer employed was made by Ruhmkorff and was very sensible. The needles were strengthened in their action and rendered so nearly equal, that a single vibration to the right or to the left occupied from sixteen to twenty seconds. When experimenting with such bodies as bismuth or phosphorus, the place of the needle was observed through a lens. The perfect communication in all parts of the circuit was continually ascertained by a feeble thermo-electric pair, warmed by the fingers. This was done also for every position of the commutator, where the film of oxide formed on any part by two or three days' rest was quite sufficient to intercept a feeble current.

2652. In order to bring the phænomena afforded by magnetic and diamagnetic bodies into direct relation, I have not so much noted the currents produced in the experimental helix, as the effects obtained at the galvanometer. It is to be understood, that the standard of deviation, as to direction, has always been that produced by an iron wire moving in the same direction at the experimental helix, and with the same condition of the commutator and connecting wires, as the piece of bismuth or other body whose effects were to be observed and compared.

2653. A thin glass tube, of the given size (2643.), 5½ by ¾ inches, was filled with a saturated solution of protosulphate of iron, and employed as the experimental core: the velocity given to the machine at this and all average times of experiment was such as to cause five or six approaches and withdrawals of the core in one second; yet the solution produced no sensible indication at the galvanometer. A piece of magnetic glass tube (2354.), and a core of foolscap paper, magnetic between the poles of the electro-magnet, were equally inefficient. A tube filled with small crystals of protosulphate of iron caused the needle to move about 2°, and cores formed out of single large crystals, or symmetric groups of crystals of sulphate of iron, produced the same effect. Red oxide of iron (colcothar) produced the least possible effect. Iron scales and metallic iron (the latter as a thin wire) produced large effects.

2654. Whenever the needle moved, it was consistent in its direction with the effect of a magnetic body; but in many cases with known magnetic bodies, the motion was little or none. This proves that such an arrangement is by no means so good a test of magnetic polarity as the use of a simple or an astatic needle. This deficiency of power in that respect does not interfere with its ability to search into the nature of the phænomena that appear in the experiments of Weber, Reich and others.

2655. Other metals than iron were now employed and with perfect success. If they were magnetic, as nickel and cobalt, the deflection was in the same direction as for iron. When the metals were diamagnetic, the deflection was in the contrary direction; and for some of the metals, as copper, silver and gold, it amounted to 60° or 70°, which was permanently sustained as long as the machine continued to work. But the deflection was not the greatest for the most diamagnetic substances, as bismuth or antimony, or phosphorus; on the contrary, I have not been able to assure myself, up to this time, that these three bodies can produce any effect. Thus far the effect has been proportionate to the *conducting power* of the substance for electricity. Gold, silver and copper have produced large deflections, lead and tin less. Platina very little. Bismuth and antimony none.

2656. Hence there was every reason to believe that the effects were produced by the currents induced in the mass of the moving metals, and not by any polarity of their particles. I proceeded therefore to test this idea by different conditions of the cores and the apparatus.

2657. In the first place, if produced by induced currents, the great proportion of these would exist in the part of the core near to the dominant magnet, and but little in the more distant parts; whereas in a substance like iron, the polarity which the whole assumes makes length a more important element. I therefore shortened the core of copper from 5½ inches (2643.) to 2 inches, and found the effect not sensibly diminished; even when 1 inch long it was little less than before. On the contrary, when a fine iron wire, 5½ inches in length, was used as core, its effects were strong; when the length was reduced to 2 inches, they were greatly diminished; and again, with a length of 1 inch, still further greatly reduced. It is not difficult to construct a core of copper, with a fine iron wire in its axis, so that when above a certain length it should produce the effects of iron, and beneath that length the effects of copper.

2658. In the next place, if the effect were produced by induced currents in the mass (2642.), division of the mass would stop these currents and so alter the effect; whereas if produced by a true diamagnetic polarity, division of the mass would not affect the polarity seriously, or in its essential nature (2430.). Some copper filings were therefore digested for a few days in dilute sulphuric acid to remove any adhering iron, then well-washed and dried, and afterwards warmed and stirred in the air, until it was seen by the orange colour that a very thin film of oxide had formed upon them: they were finally introduced into a glass tube (2653.) and employed as a core. It produced no effect whatever, but was now as inactive as bismuth.

2659. The copper may however be divided so as either to interfere with the assumed currents or not, at pleasure. Fine copper wire was cut up into lengths of $5\frac{1}{2}$ inches, and as many of these associated together as would form a compact cylinder three-quarters of an inch in diameter (2643.); it produced no effect at the galvanometer. Another copper core was prepared by associating together many *discs* of thin copper plate, three-quarters of an inch in diameter, and this affected the galvanometer, holding its needle 25° or 30° from zero.

2660. I made a solid helix cylinder, three-quarters of an inch in diameter and 2 inches long, of covered copper wire, one-sixteenth of an inch thick, and employed this as the experimental core. When the two ends of its wire were unconnected, there was no effect upon the experimental helix, and consequently none at the galvanometer; but when the ends were soldered together, the needle was well affected. In the first condition, the currents, which tended to be formed in the mass of moving metal, could not exist because the metal circuit was interrupted; in the second they could, because the circuit was not interrupted; and such division as remained did not interfere to prevent the currents.

2661. The same results were obtained with other metals. A core cylinder of gold, made of half-sovereigns, was very powerful in its effect on the galvanometer. A cylinder of silver, made of sixpenny pieces, was very effectual; but a cylinder made of precipitated silver, pressed into a glass tube as closely as possible, gave no indications of action whatever. The same results were obtained with disc cylinders of tin and lead, the effects being proportionate to the condition of tin and lead as bad conductors (2655.).

2662. When iron was divided, the effects were exactly the reverse in kind. It was necessary to use a much coarser galvanometer and apparatus for the purpose; but that being done, the employment of a solid iron core, and of another of the same size or weight formed of lengths of fine iron wire (2659.), showed that the division had occasioned no inferiority in the latter. The excellent experimental researches of Dove[1] on the electricity of induction, will show that this ought to be the case.

2663. Hence the result of division in the diamagnetic metals is altogether of a nature to confirm the conclusion, that the effects

[1] Taylor's Scientific Memoirs, v. p. 129. I do not see a date to the paper.

produced by them are due to induced currents moving through their masses, and not to any polarity correspondent in its general nature (though opposed in its direction) to that of iron.

2664. In the third place (2656.), another and very important distinction in the actions of a diamagnetic metal may be experimentally established according as they may be due either to a true polarity, or merely to the presence of temporary induced currents; and as for the consideration of this point diamagnetic and magnetic polarity are the same, the point may best be considered, at present, in relation to iron.

2665. If a core of any kind be advanced towards the dominant magnet and withdrawn from it by a motion of uniform velocity, then a complete journey, or *to* and *from* action, might be divided into four parts; the *to*, the *stop* after it; the *from*, and the *stop* succeeding that. If a core of iron make this journey, its end towards the dominant magnet becomes a pole, rising in force until at the nearest distance, and falling in force until at the greatest distance. Both this effect and its *progression* inwards and outwards, cause currents to be induced in the surrounding helix, and these currents are in one direction as the core advances, and in the contrary direction as it recedes. In reality, however, the iron does not travel with a constant velocity; for, because of the communication of motion from a revolving crank at the machine (2643.), it, in the *to* part of the journey, gradually rises from a state of rest to a maximum velocity, which is half-way, and then as gradually sinks to rest again near the magnet:—and the *from* part of the journey undergoes the same variations. Now as the maximum effect upon the surrounding experimental helix depends upon the velocity conjointly with the intensity of the magnetic force in the end of the core, it is evident that it will not occur with the maximum velocity, which is in the middle of the *to* or *from* motion; nor at the *stop* nearest to the dominant magnet, where the core end has greatest magnetic force, but somewhere between the two. Nevertheless, during the *whole* of the advance, the core will cause a current in the experimental helix in one direction, and during the whole of the recession it will cause a current in the other direction.

2666. If diamagnetic bodies, under the influence of the dominant magnet, assume also a polar state, the difference between them and iron being only that the poles of like names or forces are changed in place (2429. 2430.), then the same kind of action as that described for iron would occur with them; the only difference being, that the two currents produced would be in the reverse direction to those produced by iron.

2667. If a commutator, therefore, were to be arranged to gather up these currents, either in the one case or the other, and send them on to the galvanometer in one consistent current, it should change at the moments of the two *stops* (2665.), and then would perform such duty perfectly. If, on the other hand, the commutator should change at the times of maximum velocity or maximum intensity, or at two other times equidistant either from the one *stop* or from the other, then the parts of the opposite currents intercepted between the changes would exactly neutralize each other, and no final current would be sent on to the galvanometer.

2668. Now the action of the iron is, by experiment, of this nature. If an iron wire be simply introduced or taken out of the experimental helix with different conditions of the commutator, the results are exactly those which have been stated. If the machine be worked with an iron wire core, the commutator changing at the stops (2665.), then the current gathered up and sent on to the galvanometer is a maximum; if the commutator change at the moments of maximum velocity, or at any other pair of moments equidistant from the one stop or the other, then the current at the commutator is a minimum, or 0.

2669. There are two or three precautions which are necessary to the production of a pure result of this kind. In the first place, the iron ought to be soft and not previously in a magnetic state. In the next, an effect of the following kind has to be guarded against. If the iron core be away from the dominant magnet at the beginning of an experiment, then, on working the machine, the galvanometer will be seen to move in one direction for a few moments, and afterwards, notwithstanding the continued action of the machine, will return and gradually take up its place at 0°. If the iron core be at its shortest distance from the dominant magnet at the beginning of the experiment, then the galvanometer needle will move in the contrary direction to that which it took before, but will again settle at 0°. These effects are due to the circumstance, that, when the iron is away from the dominant magnet, it is not in so strong a magnetic state, and when at the nearest to it is in a stronger state, than the *mean* or *average state*, which it acquires during the continuance of an experiment; and that in rising or falling to this average state, it produces two currents in contrary directions, which are made manifest in the experiments described. These existing only for the first moments, do, in their effects at the galvanometer, then appear, producing a vibration which gradually passes away.

2670. One other precaution I ought to specify. Unless the commutator changes accurately at the given points of the journey, a little effect is gathered up at each change, and may give a permanent deflection of the needle in one direction or the other. The tongues of my commutator, being at right angles to the direction of motion and somewhat flexible, dragged a little in the *to* and *from* parts of the journey: in doing this they approximated, though only in a small degree, to that which is the best condition of the commutator for gathering up (and not opposing) the currents; and a deflection to the right or left appeared (2677.). Upon discovering the cause and stiffening the tongues so as to prevent their flexure, the effect disappeared, and the iron was perfectly inactive.

2671. Such therefore are the results with an iron core, and such would be the effects with a copper or bismuth core if they acted by a diamagnetic polarity. Let us now consider what the consequences would be if a copper or bismuth core were to act by currents, induced for the time, in its moving mass, and of the nature of those suspected (2642.). If the copper cylinder moved with uniform velocity (2665.), then currents would exist in it, parallel to its circumference, during the whole time of its motion; and these would be at their maximum force just before and just after the *to* or inner stop, for then the copper would be in the most intense parts of the magnetic field. The rising current of the copper core for the *in* portion of the journey would produce a current in one direction in the experimental helix, the stopping of the copper and consequent falling of its current would produce in the experimental helix a current contrary to the former; the first instant of motion *outwards* in the core would produce a maximum current in it contrary to its former current, and producing in the experimental helix its inductive result, being a current the same as the last there produced; and then, as the core retreated, its current would fall, and in so doing and by its final stop, would produce a fourth current in the experimental helix, in the same direction as the first.

2672. The four currents produced in the experimental helix alternate by twos, *i. e.* those produced by the falling of the first current in the core and the rising of the second and contrary current, are in one direction. They occur at the instant before and after the stop at the magnet, *i. e.* from the moment of maximum current (in the core) before, to the moment of maximum current after, the stop; and if that stop is momentary, they exist only for that moment, and should during that brief time be gathered up by the commutator. Those produced in the experimental helix during the falling of the second current in the core and the rising of a third current (identical with the first) in the return of the core to the magnet, are also the same in direction, and continue from the beginning of the retreat to the end of the advance (or from maximum to maximum) of the core currents, *i. e.* for almost the whole of the core journey; and these, by its change at the maximum moments, the commutator should take up and send on to the galvanometer.

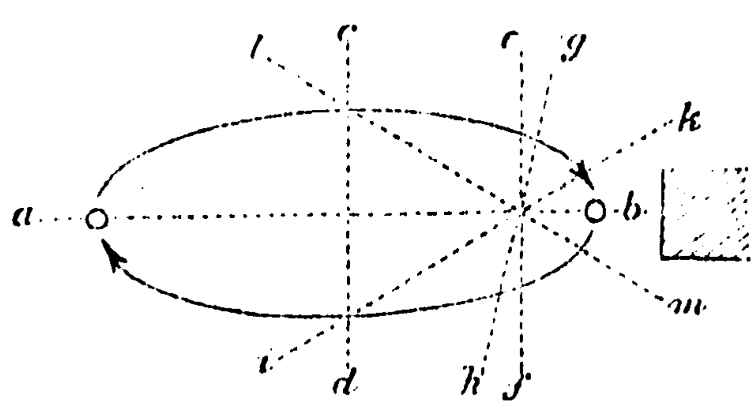
2673. The motion however of the core is not uniform in velocity, and so, sudden in its change of direction, but, as before said (2665.), is at a maximum as respects velocity in the middle of its approach to and retreat from the dominant magnet; and hence a very important advantage. For its stop may be said to commence immediately after the occurrence of the maximum velocity; and if the lines of magnetic force were equal in position and power there to what they are nearer to the magnet, the contrary currents in the experimental helix would commence at those points of the journey; but, as the core is entering into a more intense part of the field, the current in it still rises though the velocity diminishes, and the consequence is, that the maximum current in it neither occurs at the place of greatest velocity, nor of greatest force, but at a point between the two. This is true both as regards the approach and the recession of the core, the two maxima of the currents occurring at points equidistant from the place of rest near the dominant magnet.

2674. It is therefore at these two points that the commutator should change, if adjusted to produce the greatest effect at the galvanometer by the currents excited in the experimental helix, through the influence of, or in connexion with, currents of induction produced in the core; and experiment fully justifies this conclusion. If the length of the journey from the stop out to the stop in, which is 2 inches (2643. 2644.), be divided into 100 parts, and the dominant magnet be supposed to be on the right-hand, then such an expression as the following, 50|50, may represent the place where the commutator changes, which in this illustration would be midway in the to and from motion, or at the places of greatest velocity.

2675. Upon trial of various adjustments of the commutat I have found that from 77|23 to 88|12, gave the best result with a copper core. On the whole, and after many experiments, I conclude that with the given strength of electro-magnet, distance of the experimental core when at the nearest from the magnet, length of the whole journey, and average velocity of the machine, 86|14 may represent the points where the induced currents in the core are at a maximum and where the commutator ought to change.

2676. From what has been said before (2667.), it will be seen that both in theory and experiment these are the points in which the effect of any polarity, magnetic or diamagnetic, would be absolutely nothing. Hence the power of submitting by this machine metals and other bodies to experiment, and of eliminating the effects of magnetic polarity, of diamagnetic polarity, and of inductive action, the one from the others: for either by the commutator or by the direction of the polarity, they can be separated; and further, they can also be combined in various ways for the purpose of elucidating their joint and separate action.

2677. For let the arrows in the diagram represent the to and from journey, and the intersections of the lines *a*, *b* or *c*, *d*, &c. the periods in the journey when the commutator changes (in



which case *c*, *d* will correspond to 50|50, and *e*, *f* to 86|14), then *a*, *b* will represent the condition of the commutator for the maximum effect of iron or any other polar body. If the line *a, b* be gradually revolved until parallel to *c, d,* it will in every position indicate points of commutator change, which will give the iron effect at the galvanometer by a deflection of the needle always in the same direction; it is only when the ends *a* and *b* have passed the points *c* and *d,* either above or below, that the direction of the deflection will change for iron. But the line *a, b* indicates those points for the commutator with which no effect will be produced on the galvanometer by the induction of *currents* in the mass of the core. If the line be inclined in one direction, as *i, k,* then these currents will produce a deflection at the galvanometer on one side; if it be inclined in the other direction, as *l, m,* then the deflection will be on the other side. Therefore the effects of these induced currents may be either combined with, or opposed to, the effects of a polarity, whether it be magnetic or diamagnetic.

2678. All the metals before mentioned (2655.), namely, gold, silver, copper, tin, lead, platina, antimony and bismuth, were submitted to the power of the electro-magnet under the best adjustment (2675.) of the commutator. The effects were stronger than before, being now at a maximum, but in the same order; as regarded antimony and bismuth, they were very small, amounting to not more than half a degree, and may very probably have been due to a remainder of irregular action in some part of the apparatus. All the experiments with the divided cores (2658, &c.) were repeated with the same results as before. Phosphorus, sulphur and gutta percha did not, either in this or in the former state of the commutator, give any indication of effect at the galvanometer.

2679. As an illustration of the manner in which this position of the commutator caused a separation of the effects of copper and iron, I had prepared a copper cylinder core 2 inches in length having an iron wire in its axis, and this being employed in the apparatus gave the pure effect of the copper with its induced currents. Yet this core, as a whole, was highly magnetic to an ordinary test-needle; and when the two changes of the commutator were not equidistant from the one stop or the other (2670. 2677.), the iron effect came out powerfully, overruling the former and producing very strong contrary deflections at the needle. The platinum core which I have used is an imperfect cylinder, 2 inches long and 0·62 of an inch thick: it points magnetically between the poles of a horseshoe electro-magnet (2381.), making a vibration in less than a second, but with the above condition of the commutator (2675.) gives 4° of deflection due to the induced currents, the magnetic effect being annulled or thrown out.

2680. Some of the combined effects produced by oblique position of the commutator points were worked out in confirmation of the former conclusions (2677.). When the commutator was so adjusted as to combine any polar power which the bismuth, as a diamagnetic body, might possess, with any conducting power which would permit the formation of currents by induction in its mass (2676.), still the effects were so minute and uncertain as to oblige me to say that, experimentally, it is without either polar or inductive action.

2681. There is another distinction which may usefully be established between the effects of a true sustainable polarity, either magnetic or diamagnetic, and those of the transient induced currents dependent upon *time*. If we consider the resistance in the circuit, which includes the experimental helix and the galvanometer coil, as nothing, then a magnetic pole of constant strength passed a certain distance into the helix, would produce the same amount of current electricity in it, whether the pole were moved into its place by a quick or a slow motion. Or if the iron core be used (2668.) the same result is produced, provided, in any alternating action, the core is left long enough at the extremities of its journey to acquire, either in its quick or slow alternation, the same state. This I found to be the fact when no commutator nor dominant magnet was used; a single insertion of a weak magnetic pole gave the same deflection, whether introduced quickly or slowly; and when the residual dominant magnet, an iron wire core, and the commutator in its position *a*, *b* (2677.) were used, four journeys to and from produced the *same* effect at the galvanometer when the velocities were as 1 : 5 or even as 1 : 10.

2682. When a copper, silver, or gold core is employed in place of the iron, the effect is very different. There is no reason to doubt, that, as regards the core itself, the same amount of electricity is thrown into the form of induced circulating currents within it, by a journey to or from, whether that journey is performed quickly or slowly: the above experiment (2681.) in fact confirms such a conclusion. But the effect which is produced upon the experimental helix is not proportionate to the whole amount of these currents, but to the maximum intensities to which they rise. When the core moves slowly, this intensity is small; when it moves rapidly, it is great, and necessarily so, for the same current of electricity has to travel in the two differing periods of time occupied by the journeys. Hence the quickly moving core should produce a far higher effect on the experimental helix than the slowly moving core; and this also I found to be the fact.

2683. The short copper core was adjusted to the apparatus, and the machine worked with its average velocity until forty journeys to and from had been completed; the galvanometer needle passed 39° west. Then the machine was worked with a greater rapidity, also for forty journeys, and the needle passed through 80° or more west; finally, being worked at a slow rate for the same number of journeys, the needle went through only 21° west. The extreme velocities in this experiment were probably as 1 : 6; the time in the longest case was considerably less than that of one vibration of the needle (2651.), so that I believe all the force in the slowest case was collected. The needle is very little influenced by the swing or momentum of its parts, because of the deadening effect of the copper plate beneath it, and, except to return to zero, moves very little after the motion of the apparatus ceases. A silver core produced the same results.

2684. These effects of induced currents have a relation to the phænomena of revulsion which I formerly described (2310. 2315. 2338.), being the same in their exciting cause and principles of action, and so the two sets of phænomena confirm and illustrate each other. That the revulsive phænomena are produced by induced currents, has been shown before (2327. 2329. 2336. 2339.); the only difference is, that with them the induced currents were produced by exalting the force of a magnet placed at a fixed distance from the affected metal; whilst in the present phænomena, the force of the magnet does not change, but its distance from the piece of metal does.

2685. So also the same circumstances which affect the phænomena here affect the revulsive phænomena. A plate of metal will, as a whole, be well-revulsed; but if it be divided across the course of the induced currents it is not then affected (2529.). A ring helix of copper wire, if the extremities be unconnected, will not exhibit the phænomena, but if they be connected then it presents them (2660.).

2686. On the whole, the revulsive phænomena are a far better test and indication of these currents than the present effects; especially if advantage be taken of the division of the mass into plates, so as to be analogous, or rather superior, in their action to the disc cylinder cores (2659. 2661.). Platinum, palladium and lead in leaf or foil, if cut or folded into squares half an inch in the side, and then packed regularly together, will show the phænomena of revulsion very well; and that according to the direction of the leaves, and not of the external form. Gold, silver, tin and copper have the revulsive effects thus greatly exalted. Antimony, as I have already shown, exhibits the effect well (2514. 2519.). Both it and bismuth can be made to give evidence of the induced currents produced in them when they are used in thin plates, either single or associated, although, to avoid the influence of the diamagnetic force, a little attention is required to the moments of making and breaking contact between the voltaic battery and the electro-magnet.

2687. Copper, when thus divided into plates, had its revulsive phænomena raised to a degree that I had not before observed. A piece of copper foil was annealed and tarnished by heat, and then folded up into a small square block, half an inch in the side and a quarter of an inch thick, containing seventy-two folds of the metal. This block was suspended by a silk film as before (2248.), and whilst at an angle of 30° or thereabouts with the equatorial line (2252.), the electro-magnet was excited; it immediately advanced or turned until the angle was about 45° or 50°, and then stood still. Upon the interruption of the electric current at the magnet the revulsion came on very strongly, and the block turned back again, passed the equatorial line, and proceeded on until it formed an angle of 50° or 60° on the other side; but instead of continuing to revolve in that direction as before (2315.), it then returned on its course, again passed the equatorial line, and almost reached the axial position before it stood still. In fact, as a mass, it vibrated to and fro about the equatorial line.

2688. This however is a simple result of the principles of action formerly developed (2329. 2336.). The revulsion is due to the production of induced currents in the suspended mass during the falling of the magnetism of the electro-magnet; and the effect of the action is to bring the axis of these induced currents parallel to the axis of force in the magnetic field. Consequently, if the time of the fall of magnetic force, and therefore of the currents dependent thereon, be greater than the time occupied by the revulsion of the copper block as far as the equatorial line, any further motion of it by momentum will be counteracted by a contrary force; and if this force be strong enough the block will return. The conducting power of the copper and its division into laminæ, tend to set up these currents very readily and with extra power; and the very power which they possess tends to make the time of a vibration so short, that two or even three vibrations can occur before the force of the electro-magnet has ceased to fall any further. The effect of *time*, both in the rising and falling of power, has been referred to on many former occasions (2170. 2650.), and is very beautifully seen here.

2689. Returning to the subject of the assumed polarity of bismuth, I may and ought to refer to an experiment made by Reich, and described by Weber[1], which, if I understand the instruction aright, is as follows: a strong horseshoe magnet is laid upon a table in such a position that the line joining its two poles is perpendicular to the magnetic meridian and to be considered as prolonged on one side; in that line, and near the magnet, is to be placed a small powerful magnetic needle, suspended by cocoon silk, and on the other side of it, the pole of a bar magnet, in such a position and so near, as exactly to counteract the effect of the horseshoe magnet, and leave the needle to point exactly as if both magnets were away. Then a mass of bismuth being placed between the poles of the horseshoe magnet is said to react upon the small magnet needle, causing its deflection in a particular direction, and this is supposed to indicate the polarity of the bismuth under the circumstances, as it has no such action when the magnets are away. A piece of

[1] Taylor's Scientific Memoirs, v. p. 480.

iron in place of the bismuth produces the contrary deflection of the needle.

2690. I have repeated this experiment most anxiously and carefully, but have never obtained the slightest trace of action with the bismuth. I have obtained action with the iron; but in those cases the action was far less than if the iron were applied outside between the horseshoe magnet and the needle, or to the needle alone, the magnets being entirely away. On using a garnet, or a weak magnetic substance of any kind, I cannot find that the arrangement is at all comparable for readiness of indication or delicacy, with the use of a common or an astatic needle, and therefore I do not understand how it could become a test of the polarity of bismuth when these fail to show it. Still I may have made some mistake; but neither by close reference to the description, nor to the principles of polar action, can I discover where.

2691. There is an experiment which Plücker described to me, and which at first seems to indicate strongly the polarity of bismuth. If a bar of bismuth (or phosphorus) be suspended horizontally between the poles of the electro-magnet, it will go to the equatorial position with a certain force, passing, as I have said, from stronger to weaker places of action (2267.). If a bar of iron of the same size be fixed in the equatorial position a little below the plain in which the diamagnetic bar is moving, the latter will proceed to the equatorial position with much greater force than before, and this is considered as due to the circumstance, that, on the side where the iron has N polarity, the diamagnetic body has S polarity, and that on the other side the S polarity of the iron and the N polarity of the bismuth also coincide.

2692. It is however very evident that the lines of magnetic force have been altered sufficiently in their intensity of direction, by the presence of the iron, to account fully for the increased effect. For, consider the bar as just leaving the axial position and going to the equatorial position; at the moment of starting its extremities are in places of stronger magnetic force than before, for it cannot be doubted for a moment that the iron bar determines more force from pole to pole of the electro-magnet than if it were away. On the other hand, when it has attained the equatorial position, the extremities are under a much weaker magnetic force than they were subject to in the *same places* before; for the iron bar determines downwards upon itself much of that force, which, when it is not there, exists in the plane occupied by the bismuth. Hence, in passing through 90°, the diamagnetic is urged by a much greater difference of intensity of force when the iron is present than when it is away; and hence, probably, the whole additional result. The effect is like many others which I have referred to in magnecrystallic action (2487-2497.), and does not, I think, add anything to the experimental proof of diamagnetic polarity.

2693. Finally, I am obliged to say that I can find no experimental evidence to support the hypothetical view of diamagnetic polarity (2640.), either in my own experiments, or in the repetition of those of Weber, Reich, or others. I do not say that such a polarity does not exist; and I should think it possible that Weber, by far more delicate apparatus than mine, had obtained a trace of it, were it not that then also he would have certainly met with the far more powerful effects produced by copper, gold, silver, and the better conducting diamagnetics. If bismuth should be found to give any effect, it must be checked and distinguished by reference to the position of the commutator, division of the mass by pulverization, influence of time, &c. It appears to me also, that, as the magnetic polarity conferred by iron or nickel in very small quantity, and in unfavourable states, is far more readily indicated by its effect on an astatic needle, or by pointing between the poles of a strong horseshoe magnet, than by any such arrangement as mine or Weber's or Reich's, so diamagnetic polarity would be much more easily distinguished in the same way, and that no indication of that polarity has as yet reached to the force and value of those already given by Brugmann and myself.

2694. So, at present, the actions represented or typified by iron, by copper and by bismuth, remain distinct; and their relations are only in part made known to us. It cannot be doubted that a larger and simpler law of action than any we are yet acquainted with, will hereafter be discovered, which shall include all these actions at once; and the beauty of Weber's suggestion in this respect was the chief inducement to me to endeavour to establish it.

2695. Though from the considerations above expressed (2693.)

I had little hopes of any useful results, yet I thought it right to submit certain magnecrystallic cores to the action of the apparatus. One core was a large group of symmetrically disposed crystals of bismuth (2457.); another a very large crystal of red ferroprussiate of potassa; a third a crystal of calcareous spar; and a fourth and fifth large crystals of protosulphate of iron. These were formed into cylinders of which the first and fourth had the magnecrystallic axes (2479.) parallel to the axis of the cylinder, and the second, third and fifth, had the equatorial direction of force (2594. 2595. 2546.) parallel to the axis of the cylinder. None of them gave any effect at the galvanometer, except the fourth and fifth, and these were alike in their results, and were dependent for them on their ordinary magnetic property.

2696. Some of the expressions I have used may seem to imply, that, when employing the copper and other cores, I imagine that currents are first induced in them by the dominant magnet, and that these induce the currents which are observed in the experimental helix. Whether the cores act directly on the experimental helix or indirectly through their influence on the dominant magnet, is a very interesting question, and I have found it difficult to select expressions, though I wished to do so, which should not in some degree prejudge that question. It seems to me probable, that the cores act indirectly on the helix, and that their immediate action is altogether directed towards the dominant magnet, which, whether they consist of magnetic or diamagnetic metals, raises them into power either permanently or transiently, and has their power for that time directed towards it. Before the core moves to approach the magnet, the magnet and experimental helix are in close relation; and the latter is situated in the intense field of magnetic force which belongs to the pole of the former. If the core be iron, as it approaches the magnet it causes a strong convergence and concentration of the lines of magnetic force upon itself; and these, as they so converge, passing through the helix and across its convolutions, are competent to produce the currents in it which are obtained (2653. 2668.). As the iron retreats these lines of force diverge, and again crossing the line of the wire in the helix in a contrary direction to their former course, produce a contrary current. It does not seem necessary, in viewing the action of the iron core, to suppose any direct action of it on the helix, or any other action than this which it exerts upon the lines of force of the magnet. In such a case its action upon the helix would be indirect.

2697. Then, by all parity of reasoning, when a copper core enters the helix its action upon it should be indirect also. For the currents which are produced in it are caused by the direct influence of the magnet, and must react equivalently upon it. This they do, and because of their direction and known action, they will cause the lines of force of the magnet to diverge. As the core diminishes in its velocity of motion, or comes to rest, the currents in it will cease, and then the lines of force will converge; and this divergence and convergence, or passage in two directions across the wire of the experimental helix, is sufficient to produce the two currents which are obtained in the advance of the core towards the dominant magnet (2671. 2673.). A corresponding effect in the contrary direction is produced by the retreat of the core.

2698. On the idea that the actions of the core were not of this kind, but more directly upon the helix, I interposed substances between the core and the helix during the times of the experiment. A thick copper cylinder 2·2 inches long, 0·7 of an inch external diameter, and 0·1 of an inch internal diameter, and consequently 0·3 of an inch thick in the sides, was placed in the experimental helix, and an iron wire core (2668.) used in the apparatus. Still, whatever the form of the experiment, the kind and amount of effect produced were the same as if the copper were away, and either glass or air in its place. When the dominant magnet was removed and the wire core made a magnet, the same results were produced.

2699. Another copper lining, being a cylinder 2·5 inches long, 1 inch in external diameter, and one-eighth of an inch in thickness, was placed in the experimental helix, and cores of silver and copper five-eighths of an inch in thickness, employed as before, with the best condition of the commutator (2675.): the effects, with and without the copper, or with and without the glass, were absolutely the same (2698.).

2700. There can be no doubt that the copper linings, when in place, were full of currents at the time of action, and that when away no such currents would exist in the air or glass replacing them. There is also full reason to admit, that the divergence and convergence of the magnetic lines of force supposed above (2697.) would satisfactorily account for such currents in them, supposing the indirect action of the cores were assumed. If that supposition be rejected, then it seems to me that the whole of the bodies present, the magnet, the helix, the core, the copper lining, or the air or glass which replaces it, must all be in a state of tension, each part acting on every other part, being in what I have occasionally elsewhere imagined as the electro-tonic state (1729.).

2701. The advance of the copper makes the lines of magnetic force diverge, or, so to say, drives them before it (2697.). No doubt there is reaction upon the advancing copper, and the production of currents in it in such a direction as makes them competent, if continued, to continue the divergence. But it does not seem logical to say, that the currents which the lines of force cause in the copper, are the cause of the divergence of the lines of force. It seems to me, rather, that the lines of force are, so to say, diverged, or bent outward by the advancing copper (or by a connected wire moving across lines of force in any other form of the experiments), and that the reaction of the lines of force upon the forces in the particles of the copper cause them to be resolved into a current, by which the resistance is discharged and removed, and the line of force returns to its place. I attach no other meaning to the words *line of force* than that which I have given on a former occasion (2149.).

Royal Institution,
14 *Dec.* 1849.

TWENTY-FOURTH SERIES[1].

§ 30. *On the possible relation of Gravity to Electricity.*

Received August 1,—Read November 28, 1850.

2702. The long and constant persuasion that all the forces of nature are mutually dependent, having one common origin, or rather being different manifestations of one fundamental power (2146.), has made me often think upon the possibility of establishing, by experiment, a connexion between gravity and electricity, and so introducing the former into the group, the chain of which, including also magnetism, chemical force and heat, binds so many and such varied exhibitions of force together by common relations. Though the researches I have made with this object in view have produced only negative results, yet I think a short statement of the matter, as it has presented itself to my mind, and of the result of the experiments, which offering at first much to encourage, were only reduced to their true value by most careful searchings after sources of error, may be useful, both as a general statement of the problem, and as awakening the minds of others to its consideration.

2703. In searching for some principle on which an experimental inquiry after the identification or relation of the two forces could be founded, it seemed that if such a relation existed, there must be something in gravity which would correspond to the dual or antithetical nature of the forms of force in electricity and magnetism. To my mind it appeared possible that the ceding to the force or the approach of gravitating bodies on the one hand, and the effectual reversion of the force or separation of the bodies on the other, might present the points of correspondence; quiescence (as to motion) being the neutral condition. The final unchangeability of gravity did not seem affected by such an assumption; for the acting bodies when at rest would ever have the same relation to each other, and it would only be

[1] Philosophical Transactions, 1851, p. 1. The Bakerian Lecture.

at the times of motion to and fro that any results related to electricity could be expected. Such results, if possible, could only be exceedingly small; but, *if possible*, i. e. if true, no terms could exaggerate the value of the relation they would establish.

2704. The thought on which the experiments were founded was, that, as two bodies moved towards each other by the force of gravity, currents of electricity might be developed either in them or in the surrounding matter in one direction; and that as they were by extra force moved from each other against the power of gravitation, the opposite currents might be produced. Also, that these currents would have relation to the line of approach and recession, and not to space generally, so that two bodies approaching would have currents in the opposite direction as to space generally, but the same as to the direction of their motion along the line joining them. It will be unnecessary to go further into the suppositions which arose concerning these points, or regarding the effect of forced motions either coinciding with, or across the direction of the earth's gravitation, and many other matters, than to say that, as the effect looked for was exceedingly small, so no hope was entertained of any result except by means of the gravitation of the earth. The earth was therefore made to be the one body, and the indicating mass of matter to be experimented with the other.

2705. First of all, a body, which was to be allowed to fall, was surrounded by a helix, and then its effect in falling sought for. Now a body may either fall with a helix or through a helix. Covered copper wire, to the amount of 350 feet in length, was made into a hollow cylindrical helix, about 4 inches long, its internal diameter being 1 inch and its external diameter 2 inches. It was attached to a line running upon an easy pulley, so that it could be raised 36 feet, and then allowed to fall with an accelerated velocity on to a very soft cushion, its axis remaining vertical the whole time. Long covered wires were made fast to its two extremities, and these being twisted round each other, were attached to a very delicate galvanometer, placed about 50 feet aside from the line of fall, and on a level midway with its course. The accuracy of the connexion and the direction of the set of the needle, were then both ascertained by the introduction of a feeble thermo-electric combination into the current. Such a helix, either in rising or falling, can produce no deviation at the galvanometer by any current due to the magnetism of the earth; for as it remains parallel to itself during the fall, so the lines of equal magnetic force, which being parallel to the dip, are intersected by the wire convolutions of the descending helix, are cut with an equal velocity on both sides of the helix, and consequently no effect of magneto-electric induction is produced. Neither in rising nor in falling did this helix present any trace of action at the galvanometer; whether the connexion with the galvanometer was continued the whole time, or whether it was cut off just before the diminution or cessation of motion either way, or whether the rising and the falling were made to occur isochronously with the times of vibration of the galvanometer needle. So, though no effect of gravity appeared in the helix itself, still no source of error appeared to arise in this mode of using it.

2706. A solid cylinder of copper, three-fourths of an inch in diameter and 7 inches in length, was now introduced into the helix and carefully fastened in it, being bound round with a cloth so as not to move, and this compound arrangement was allowed to fall as before (2705.). It gave very minute but remarkably regular indications of a current at the galvanometer; and the probability of these being related to gravity appeared the greater, when it was found that on raising the helix or core, similar indications of contrary currents appeared. It was some time before I was able to refer these currents to their true cause, but at last I traced them to the action of a part of the connecting wires proceeding from the helix to the galvanometer. The two wires had been regularly twisted together, but the effect of many falls had opened a part near the middle distance into a sort of loop, so that the wires, instead of being tightly twisted together like the strands of a rope, were separate for 3 feet, as if the strands were open. In falling, this loop opened out more or less, but always in the same manner; and the consequence was that the part of it representing the transverse opening, which was furthest from the galvanometer, travelled over a larger space than the corresponding part nearest the galvanometer. Now had they travelled through equal spaces, the effect of the magnetic lines of force of the earth upon them would have been equal, and no effect at the galvanometer would have been produced; as it was, currents in opposite directions, but of unequal

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amounts of force, tended to be produced, and a current equal to the difference actually appeared. Such a case is described in my earliest researches on terrestrial magno-electro induction (171.). It is evident that the current should appear in the reverse direction, as the helix and wires are raised in the air, and thus arose the reverse effect described above. Therefore no positive or favourable evidence was supplied in favour of the original assumption by this use of a copper core in the helix.

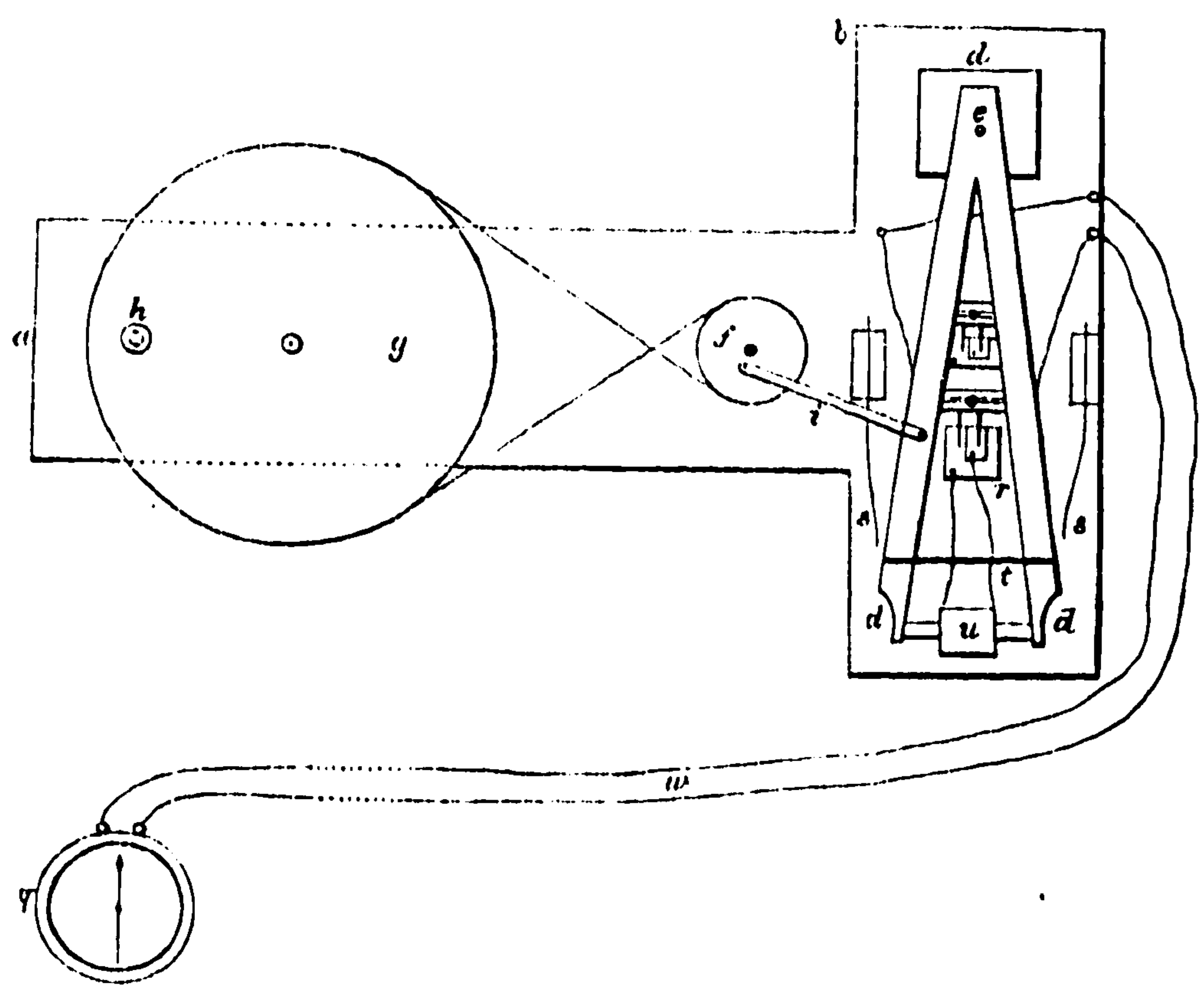
2707. The copper was selected as a heavy body and an excellent conductor of electricity. On its dismissal, a bismuth cylinder of equal size was employed to replace it as a substance eminently diamagnetic, and a bad conductor amongst metals. Uncertain evidence arose; but by close attention, first to one point and then to another, all the indications disappeared, and then the rising or falling of the bismuth produced no effect on the galvanometer.

2708. An *iron* cylinder was also employed as a magnetic metal, but when made perfectly secure, so as to prevent any motion relative to the helix, it was equally indifferent with the copper and bismuth (2706. 2707.).

2709. Cylinders of glass and shell-lac were employed as non-conducting substances, but without effect.

2710. In other experiments the helix was *fixed*, and the different substances in the form of cylinders, three-fourths of an inch in diameter and 24 inches long, were dropped through it, or else raised through it with an accelerated velocity; but in neither case was any effect produced. Rods of copper, bismuth, glass, shell-lac and sulphur were employed. Occasionally these rods were made to rotate rapidly before and during their fall; and many other conditions were devised and carried into effect, but always with negative results, when sources of error were avoided or accounted for.

2711. On further consideration of the original assumption, namely, a relation between the forces, and of the effects that might be looked for consequent upon a condition of tension in and around the particles of the body, which, as we know, are at the same moment the residence of both gravitating and electric forces, and are subject to the gravitation of the earth, it seemed probable that the stopping of the up and down motion (2703. 2704.) in the line of gravity would produce contrary effects to the coming on of the motion, and that, whether the stopping was sudden or gradual; also that a motion downward quicker than that which gravity could communicate, would give more effect than the gravity result by itself, and that a corresponding increase in the velocity upwards would be proportionally effectual. In such case a machine which could give a rapid alternating up and down motion, might be very useful in producing many minute units of inductive action in a small space and moderate time; for then, by proper commutators, the accelerated and retarded parts of each half-vibration could be separated and recombined into one consistent current, and this current could be sent through the galvanometer during the time its needle was swinging in one direction, and afterwards reversed for the time of a swing in the other direction; and so on alternately until the effect had become sensible, if any were produced by the assumed cause.



2712. The machine which I had made for this purpose is that described in the last Series of these Researches (2643.), the electro-magnet, the experimental core and the rod which carried them being removed:—*a*, *b*, *c* frame-board; *d*,*d*,*d* wooden lever, of which *e* is the axis; *f* the crank-wheel, and *g* the great wheel with its handle *h*; *i* the bar connecting the crank-wheel and lever; *q* the galvanometer; *r* the commutator; *w*, connecting wires; *s*, *s* springs of brass or copper; *t* a copper rod connecting the two arms of the lever to give strength; *u* the hollow helix fixed, or moveable at pleasure. The plan is to a scale of one-fifteenth. Being on a moveable frame, it could be placed in any position. The cylinder of metal or other substance to be submitted to its action, was $5\frac{1}{2}$ inches long and three-fourths of an inch in diameter, and was firmly held between the ends *d*, *d* of the lever arms. The extent of the alternating motion was 3 inches. A hollow cylindrical helix *u*, $2\frac{1}{2}$ inches in length, and of such internal diameter that the cylinders could complete their rapid journeys to and fro within it without any danger of striking against its sides, was constructed, containing 516 feet of covered copper wire; this cylinder could be either fixed immoveably or attached firmly to the cylinder under experiment so as to move with it. The wires from this helix passed to the commutators and from them to the galvanometer. Part of the momentum of this machine was taken up by springs *s*, *s* (2648.), and converted into the contrary motion; but so much remained undisposed of thus, that great care was required in fixing and strutting to render the action of the whole very steady, or else derangement quickly occurred at the cylinder and helix, and electro-currents were frequently produced.

2713. The employment of cylinders of iron, copper and other substances in this machine, was competent to produce electro-currents in various ways. Thus, iron might produce magneto-electric currents consequent upon its polar condition under the influence of the earth; these it would be easy to detect and separate by the use of adjusted magnets, which should neutralize or reverse the lines of magnetic force passing through the iron. Currents like those induced in copper cylinders and good conductors (2663. 2684.), might be produced by the earth's action; but as the lines of gravitating force and of terrestrial magnetic force are inclined to each other, these might be separated by position; and it appeared that there was no source of error that might not by care be eliminated. I will not occupy time by describing how this long lesson of care was learned, but pass at once to the chief results.

2714. The copper cylinder (2712.) was placed in the machine, and the helix fixed immoveably around it, the whole being in such a position that the cylinder should be vertical, and move up and down parallel to the line of gravitating force within the helix. However rapidly the machine was worked, or whatever the position of the commutator, there was no result at the galvanometer. Cylinders of bismuth, glass, sulphur, gutta percha, &c., were also employed, but with the same negative conclusion.

2715. Then the helix was taken from its fixed support and fastened on to the copper cylinder so as to move with it, and now very regular and comparatively large effects were produced. After a while, however, these were traced to causes other than gravity, and of the following kind. The helix was fixed at one end of a lever, at a point 22 inches from its axis, and being 2 inches in diameter its wires on one side were only 21 inches, and on the other side 23 inches from this axis. Hence, in vibrating these parts travelled with velocities and through spaces which are as 21 : 23. When therefore their paths were *across* the lines of magnetic force of the earth, electro-currents tended to form in these different parts proportionate in amount or strength to these numbers; and the differences of these currents being continually gathered up by the commutators, were made sensible at the galvanometer. This was rendered manifest by placing the machine so, that though the plane of vibration was still vertical, the place of the helix was just under the centre of motion, and the central line of the helix therefore, instead of being vertical, was horizontal. Now the convolutions of the helix cut the lines of magnetic force in the most favourable manner; and the consequence was that the commutators were not required, for a single motion of the helix in one direction was sufficient to show at the galvanometer the magneto-electric currents induced. If, on the contrary, the plane of motion was made horizontal, then no current was produced by any amount of motion; for though the helix was as horizontal as, and not sensibly more so than before, yet the parts of the convolutions which intersected the magnetic lines of force (being the upper and the lower parts) now moved with exactly equal velocity, and no differential result was produced.

2716. The former small result (2715.) was therefore probably dependent upon an effect of this kind; and this was confirmed by placing the machine in such a position that the axis of the moving copper cylinder and helix should in its medium position be parallel to the line of the dip, and then no effect was produced. Other bodies in the same position were equally unable to produce any effect.

2717. Here end my trials for the present. The results are negative. They do not shake my strong feeling of the existence of a relation between gravity and electricity, though they give no proof that such a relation exists.

Royal Institution,
July 19, 1850.

TWENTY-FIFTH SERIES[1].

§ 31. *On the magnetic and diamagnetic condition of bodies.* ¶ i. *Non-expansion of gaseous bodies by magnetic force.* ¶ ii. *Differential magnetic action.* ¶ iii. *Magnetic characters of Oxygen, Nitrogen and Space.*

Received August 15,—Read November 28, 1850.

¶ i. *Non-expansion of gaseous bodies by magnetic force.*

2718. THERE can be no doubt that the magnetic force, the diamagnetic force, and the magneoptic or magnecrystallic force, will, when thoroughly understood, be found to unite or exist under one form of power, and be essentially the same. Hence the great interest which exists in the development of any one of these modes of action; for differing so greatly as they do in very peculiar points, it is hardly possible that any one of them should be advanced in its illustration or comprehension, without a corresponding advance in the knowledge of the others. Stimulated by such a feeling, I have been engaged with Plücker, Weber, Reich and others, in endeavouring to make out, with some degree of precision, the mode of action of diamagnetic as well as magnecrystallic bodies; and the recent investigation (2640, &c.) and endeavour to confirm the idea of polarity in bismuth and diamagnetic bodies, the reverse of that in a magnet or in iron bodies, was one of the results of that conviction and desire.

2719. Having failed however to establish the existence of such an antipolarity, and having shown, as I think, that the phænomena which were supposed to be due to it are in fact dependent upon other conditions and causes, I was induced, in the search after something precise as to the nature of diamagnetic bodies, to examine another idea which had arisen in consequence of the development of magnetic and diamagnetic phænomena amongst gaseous substances: this thought, with some of the results which have grown out of it during its experimental examination, I purpose making the subject of the present paper.

[1] Philosophical Transactions, 1851. p. 7.

2720. Bancalari first showed that flame was diamagnetic[1]. The effect, as I proved, was due chiefly to the heated state of gaseous portions of the flame[2]; but besides that, it appeared that at common temperatures diamagnetic phænomena could be exhibited by gases; and also that in their production the gases differed very much one from another[3]; so that, taking common air, for instance, as a standard, nitrogen, and many other gases, were strongly diamagnetic in relation to it, whilst oxygen took on the appearance of a magnetic body; for *they* were repelled from, while it was attracted to, the place of maximum force in the magnetic field.

2721. Recalling the general law given respecting the action of magnetic and diamagnetic bodies (2267. 2418.), namely, that the former tended to go from weaker to stronger places, and the latter from stronger to weaker places of magnetic power, and applying it to such bodies as the gases, which are at the same time both highly elastic and easily changed in bulk by the superaddition of very small degrees of force, it would seem to follow, that if the particles of a diamagnetic gas tended to go from strong to weak places of action, in consequence of the direct and immediate effect of the magnetic power on them, then such a gas should tend to become enlarged or expanded in the magnetic field. For, the amount of power by which the particles would tend to recede from the axis of the magnetic field, would be added to the expansive force by which they before resisted the pressure of the atmosphere; that pressure would therefore be in part sustained by the new force, and expansion would of necessity be the result. On the other hand, if a gas were magnetic (as for instance oxygen), then the force cast upon the particles, by such a direct and immediate action of the magnetic power upon them, would urge them *towards* the axis of the magnetic field, and so coinciding with, and being superadded to the pressure of the atmosphere, would tend to cause contraction and diminution of bulk.

2722. If such supposititious cases were to prove true, we should then be able to arrive at the knowledge of the real zero-point (2416. 2432. 2440.)[4], not amongst gases only, but amongst

[1] Philosophical Magazine, 1847, vol. xxxi. pp. 401, 421.
[2] Ibid. pp. 404, 406. [3] Ibid. p. 409. [4] Ibid. p. 420.

all bodies, and should be able to tell whether such a gas as oxygen were a magnetic or a diamagnetic body, and also able to range individual gases and other substances in their proper places. And though I had originally endeavoured to ascertain whether there was any change in the bulk of air in the magnetic field, and found none, still Plücker's statement that he had obtained such an effect[1], and the great enlargement of knowledge respecting the gases which since then we have acquired relating to their diamagnetic relations, and especially of the great difference which exists between them, encouraged me to proceed.

2723. I first endeavoured to determine whether there was any affection of the layer of air (or other gas) immediately in contact with the magnetic pole, which, either by the consequent expansion or contraction of that layer, could render it able to affect the course of a ray of light and thus make manifest the changes occurring within. A metal screen, with a pin-hole in it, was set up before the flame of a bright lamp in a dark room, and thus an artificial star or small definite luminous object was formed. Forty-six feet from it was placed the great horseshoe magnet (2247.), ready to be excited by twenty pairs of Grove's plates; the poles were in a line, so that the ray from the lamp passed for 4 inches close to the surface of the first pole, then through 6 inches of air, and then, for 4 inches, close to the surface of the second pole. A very fine refracting telescope, belonging to Sir James South, having an aperture of 3 inches and 46 inches focal length, received the ray. The telescope was furnished with a perfect micrometer, so that the smallest change in the place of the luminous image could be observed on the threads. The axis of the telescope was just above the level of the magnetic poles. Not the smallest change in either the character or place of the luminous image could be observed, either on the making or the breaking of the contact between the voltaic battery and the magnetic wire.

2724. As the chief part of the light which came to the telescope consisted of rays which passed at some distance above the magnetic poles, these were cut off by a screen, which rising only one-eighth of an inch above the level of the poles, allowed no ray

[1] Annales de Chimie, 1850, vol. xxix. p. 134.

to pass that was not within that distance. The intensity of the light was of course diminished, and the image was distorted by inflection; still its place was well marked by the micrometer. Not the slightest change in that or any other character occurred in the supervention or the withdrawal of the magnetic force.

2725. The terminals of the magnetic poles were then varied, so that the ray sometimes passed parallel and close to a long right-angled edge, or parallel to and between two right-angled edges, a little above or below them, or over the line joining two hemispherical poles, placed close together (and also in many other ways), but in no case did the magnetic action produce any effect upon the course of the ray.

2726. In another form of the experiment the telescope was dismissed, and a simple card, with a pin-hole $\frac{1}{50}$th or $\frac{1}{100}$th of an inch in diameter, employed in its place. The image of the star of light could be seen through the pin-hole in the dark room, and yet every ray tending to its formation passed within $\frac{1}{50}$th of an inch of the surface of the magnetic pole; still no effect due to the magnetic force could be observed.

2727. By another arrangement of the polar terminations, analogous to one I had formerly employed when experimenting on the diamagnetic relations of the gases[1], I was able to surround them with other gaseous substances than air, and subject the ray for 2 inches of its course to these gases whilst under the influence of the magnet. Though the glass of the enclosing vessel disturbed the image of the object, *i. e.* the point of light, yet it was easy to perceive that no additional effect occurred when the magnetism was superinduced.

2728. Oxygen, nitrogen, hydrogen and coal-gas were thus employed; but whether any one of these, or whether air itself was submitted to examination, when in contact with the active pole of a very powerful magnet, it did not appear to be either expanded or condensed to such a degree as to cause any sensible change in its refractive force.

2729. In order to compare the expected result with the real result due to change of volume, I took a bar of iron 7 inches long, and placed it so that the ray from the luminous object in passing to the eye should proceed by the side of the bar at not more than $\frac{1}{50}$th of an inch from it, and then raised the tempera-

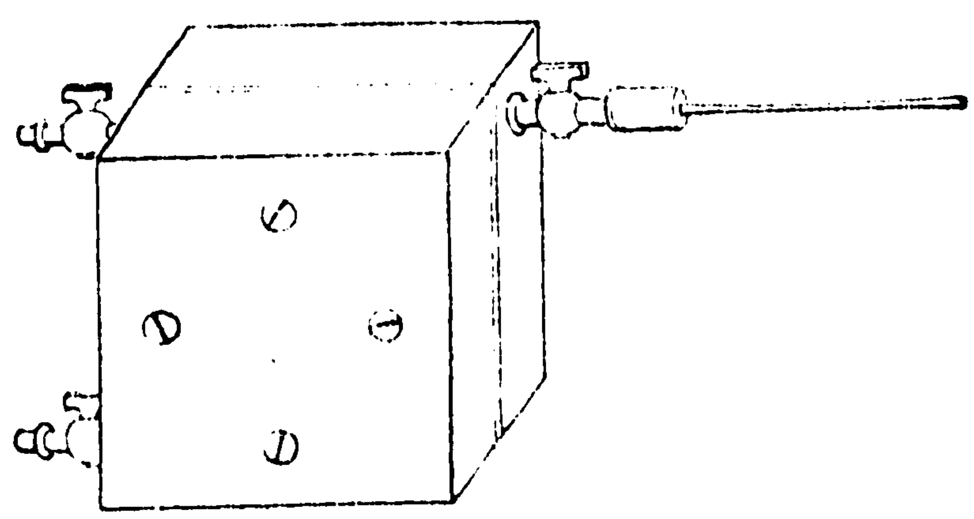
[1] Philosophical Magazine, 1847, vol. xxxi. pp. 414, 415.

ture of the bar gradually, until by expanding the air in contact with it, the course of the ray of light was sensibly affected; to do this it required to be exalted many degrees. When the air of the place was at 60° and the iron raised to 100° Fahr., the effect was not distinct. Hence it seemed, that observation of the expected change of volume of the air would be rendered far more sensible by some arrangement, measuring that change directly, than by such means as those referred to above, dependent on refractive force; for it is certain that the change of volume, in a very small quantity of air, raised from 60° to 100°, would be very evident by the former method. On the other hand, it was just possible that if the air or gas was affected by the magnet, it might only be in that film immediately contiguous to the pole; and also that great differences in the degree of change might exist along the *edge* of a solid angle, and along the *sides* of the planes forming that angle. Hence the assumed necessity for examining those parts by a ray of light; and every precaution was taken, by inclining the course of the ray a little more or less to the sides or edges of the poles, and by making the sides or edges very slightly convex, to include every variation of the experiment, that might help to make any magnetic or diamagnetic effect, whether special or local, or general, manifest; but without effect.

2730. I proceeded, as these attempts had failed, to endeavour to determine and compare the *volume* of air subjected to the magnetic force, before and after its subjection; and there seemed to be the greater hope of obtaining some results in this way, provided any such change was a consequence of the action of magnetic power, because air and gases, at a considerable distance from the surface of the magnet, are known to be strongly affected diamagnetically, and because Plücker had already said he had obtained such change of volume (2722.).

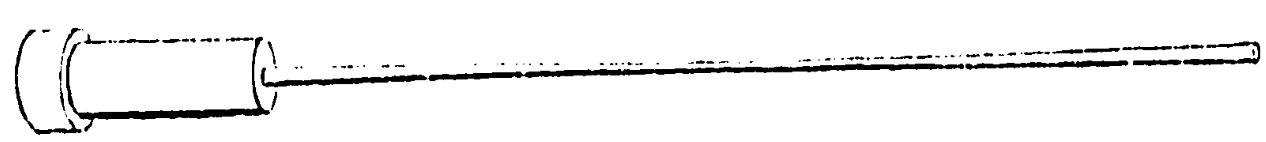
2731. The first instrument constructed for this purpose was of the following kind. Two blocks of soft iron, each 1 inch thick and 3 inches square, having filed and flattened surfaces, were prepared; and also a sheet of copper, $\frac{1}{50}$th of an inch in thickness and 3 inches square, having its middle part cut away to within 0·3 of an inch of the edge all round. This plate or frame was then placed between the iron blocks, and the whole held together very tightly by copper screws, so as to make an air-chamber $\frac{1}{50}$th of an inch wide and 2·4 inches square, having the faces of the blocks, which were to become the magnetic poles, for its sides. Three apertures and corresponding passages gave access to the interior of this chamber; small stopcocks were attached to each. By two of these, any gas, after it had been properly dried, could be sent into the chamber, or swept out of it, by any other entering gas; and to the third was attached a gauge (2732.) for the purpose of indicating and measuring any change of volume which might occur. The edges of the central copper plate and the heads of the countersunk screws, were touched with white hard varnish, and the chamber thus rendered perfectly tight, under every condition to which it had to be subjected (fig. 1).

Fig. 1.



2732. The gauges were formed of small capillary tubes from 1·5 to 3 inches in length, the diameter in the middle of their length being less than one-half of that at either termination.

Fig. 2.



These were fixed at one end into a small socket, which screwed on to the third, or gauge-cock mentioned above (2731.). A minute portion of spirit, coloured by cochineal, being put into the external end of this gauge, from a slip of wood or glass, immediately advanced to the middle or narrowest part, forming, as it always should do, a single portion of fluid. By shutting the cock, this little cylinder could be easily retained in its place undisturbed during the filling of the air-chamber with gas, and the adjustment of its pressure to equality with that of the atmosphere. On shutting the other cocks and opening the gauge-cock, the gauge was then ready to show any change of volume which the supervention of the magnetic force might cause; but to give it the highest degree of sensibility, it was necessary previously to make the liquid cylinder travel right and left of its place of rest, that the tube might be moistened on each side of the indicating fluid; an effect easily obtained by inclining the chamber to and fro, the gravity of the fluid making it pass one way or the other. But this and many other necessary precautions as to position, temperature, &c., can only be learned from experience.

2733. When this box was in its place, it stood between the poles of the great electro-magnet, with the plane of the gas-chamber in the equatorial position; then square blocks of soft iron, resting on the magnet poles, were made to abut and bear against the sides of the box, so that in fact the inner faces of the air-chamber were the virtual magnetic poles, and being 3 inches square were only $\frac{1}{60}$th of an inch apart. Hence, whatever air or gas was within the chamber, would be subjected to a very powerful magnetic action, and could have very small changes in its bulk measured; but it is perhaps necessary to observe, that it would be contained in a field having everywhere lines of equal magnetic power (2463. 2465.).

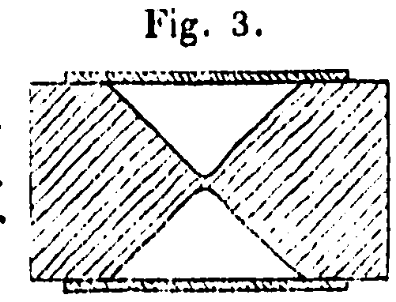
2734. *Air* was introduced into the box, and when all was properly arranged, the place of the indicating fluid was observed by a microscope. Then the magnet was rendered powerfully active, and there appeared a very slight motion of the fluid, as if the air were a little expanded; on taking off the magnetic force the fluid returned to its first place. The same effect recurred again and again. The amount of this change was very small, and there was reason to refer it to the pressure exercised by the magnet, when in action, upon the sides of the iron box; for afterwards, when the box was placed in a vice and squeezed, the same motion in the fluid occurred; and further, when the square blocks of soft iron (2733.) were kept apart by an under block of wood, so as not absolutely to touch and press the box, the effect was reduced to almost nothing.

2735. Oxygen, nitrogen, carbonic acid and nitrous oxide gases, were then introduced successively into the iron box, and with exactly the same result as with air. No difference appeared between oxygen and the other gases, greatly as they differ in magnetic and diamagnetic force and relations. Hydrogen and coal-gas were also subjected to experiment; but when these gases were in the box there was a gradual recession of the indicating fluid, due, as I found, to the absorption of the gases, probably either by the varnish or cement or cork used at the gauge, or at the joints of the box. The delicacy of the gauge was thus made manifest; but when the effect was taken into account, it was found that these gases were equally unaffected in bulk as the other gases by the magnetic influence.

2736. The diameter of the gauge, at the place where the fluid was placed, was rather less than $\frac{1}{100}$th of an inch. An amount of motion equal to $\frac{1}{100}$th of an inch was easily discerned. Comparing these numbers with the capacity of the gas-chamber, it would appear that if the gas in the latter had expanded or contracted to the extent of $\frac{1}{100,000}$th part, the result would have been visible; or any *difference* approaching to this amount, between oxygen and nitrogen or the other gases, would have become sensible, *but no such effects or differences appeared.*

2737. As the establishment of either the *occurrence* or the *absence* of change of volume in gases, when under the magnetic influence, appeared to me to be of great and almost equal importance, I was led to consider whether, in the experiment just described, the circumstance of the gases having been subjected to the magnetic power in a field of equal force (2733.) might not have interfered with the production of the effect sought for; for such a field is that where the diamagnetic phænomena, of solid and liquid bodies, occur in the most unfavourable manner, and where indeed they almost entirely disappear. I therefore constructed another apparatus so that this condition was removed, and in which, if the particles of the diamagnetic gas, by any unknown disposition of the powers in action, tended only to pass from strong to weaker places of force, and being thus incapable of enlargement in the axial direction, would only show that effect equatorially, the opportunity for their doing so should be present.

Fig. 3.



2738. A cylinder of soft iron had the central parts removed in a lathe, until it had assumed the form of an hour-glass, or that represented in fig. 3, which is to a scale of one-third. When placed between the poles of the magnet instead of the former box, it was expected that the continuation of the iron throughout would prevent any diminution of its length, from the pressure of the poles (2734.), and that the diamagnetic phænomena would be abundantly produced in the parts from whence the iron had been removed. The latter was found to be the fact, for flame, smoke, bismuth and other diamagnetic matter, when placed there, passed equatorially very freely.

2739. A copper tube, 2·5 inches long, made of metal 0·1 of an inch thick, was fitted to the iron, so that when in its place it should occupy the position represented (fig. 3), and could easily be made perfectly gas-tight by a little soft cement. In this way it formed an annular air-chamber round the iron, which, when measured, was found to have a capacity of rather more than 2 cubic inches, and included the most intense part of the magnetic field. Three stopcocks were fitted into this copper jacket, by two of which gas was passed into and out of the chamber, and the third was appropriated to the pressure-gauge as before. Whilst naked, this apparatus could not be used, because of its ever-varying temperature, and the consequent disturbance and ejectment of the fluid in the gauge; but when clothed in three thicknesses of flannel its temperature was perfectly steady; and by the further use of wooden keys to turn the cocks the apparatus became unexceptionable.

2740. Before proceeding to employ this apparatus with different gases, and in order to obtain some idea of what might be expected by comparing one gas with another, I made a preliminary experiment, dependent on the relative specific gravities of air and hydrogen, of the following nature. It is easy to diffuse a trace of ammonia through the air of a jar, by putting a little paper wetted with a strong solution into it[1]; and it is equally easy to send a jet of hydrogen, containing the smallest portion of muriatic acid gas, by a horizontal tube into the ammoniated air. When this is done, the course of the light hydrogen in the heavy air is rendered very distinctly visible; and it is seen, on leaving the horizontal tube, to turn at once upwards and to ascend rapidly, becoming wire-drawn in its course, in consequence of its small specific gravity compared to air.

2741. Two hemispherical iron pole terminations, associated with the great magnet, were then placed in contact with each

[1] Philosophical Magazine, 1847, vol. xxxi. p. 415.

other, so that they might be surrounded either by air or oxygen[1], and the jet of hydrogen, delivering at the rate of 6 cubic inches per minute, was placed exactly beneath the axial line, in the centre of the magnetic field. When there was no magnetic force employed the hydrogen rose vertically, breaking against the points where the hemispherical poles touched; but when the magnetic power was on, the stream of hydrogen divided into two parts, moving right and left, and ascended in two streams at a distance from the point of contact. Now this division took place at a certain distance *below* the axial line; and at that point, notwithstanding the ascensive power of hydrogen in air or oxygen, it was constrained to go horizontally by the apparently repulsive power of the magnetic force, and did not in its further course approach nearer to the axial line, but formed a curve concentric with it, or nearly so, so that the compound streams of gas assumed exactly the shape of a tuning-fork.

2742. When air occupied the magnetic field, the division of the stream of hydrogen was 0·3 or 0·32 of an inch below the axial line. When oxygen was about the poles, then the division of the hydrogen took place as far off as 0·55 of an inch below the axial line. Hence at these distances the power which tended to make the hydrogen pass from the axial line, equatorially in the direction of the radius, was equal to the difference of the specific gravity of hydrogen compared with that of air and oxygen respectively. At lesser distances the power would be much greater; and indeed, if in any experiment the hydrogen was delivered nearer to the axial line, it was blown downwards and away with much force. Calculating with these data, and still assuming that the diamagnetic gases receded from the axial line, in consequence of the direct action of the magnet and that only, causing them to pass from stronger to weaker places of action, I found, as I thought, reason to believe that the more diamagnetic gases, occupying the space within the copper box (2739.), might probably be expanded at least $\frac{1}{60,000}$th part of their volume by the magnetic force. Now the gauges that I employed were sensible when the fluid in them moved the $\frac{1}{100}$th of an inch (2736.), yet that space is only the $\frac{1}{2,500,000}$th part of the capacity of the chamber, and therefore such an expansion as that above

[1] Philosophical Magazine, 1847, vol. xxxi. pp. 413, 414.

would have made it move through 0·4 of an inch; a quantity abundantly sufficient to render the result sensible if the fundamental assumption were correct.

2743. *Air* was first submitted to the power of the great horseshoe magnet, urged by twenty pairs of Grove's plates in this apparatus (2739.). The fluid moved very slightly outwards, as if a little expansion occurred on putting on the magnetic force, and returned when the force was taken off. This small effect was found afterwards to be due to compression, occasioned by the tendency of the magnetic poles to approximate (2734.).

2744. *Oxygen* presented exactly the same appearances as common air and to the same amount, so that no effect, due to magnetic or diamagnetic action, was here evident, but only that of the compression observed in the case of air (2743.).

2745. *Nitrogen* gave exactly the same results as oxygen and air. Now nitrogen is probably more diamagnetic than hydrogen, and should therefore have given a striking contrast with oxygen, if any positive results were to be obtained.

2746. *Carbonic acid* and nitrous oxide gases yielded the same negative results, and, as I believe, when the apparatus was in an unexceptionable condition.

2747. There is at the Pharmaceutical Society an excellent electro-magnet, of the horseshoe form, similar in arrangement to our own (2247.), but far more powerful, and this through Mr. Redwood I was favoured with the use of, for the repetition of the foregoing experiments at the house of the Society. The iron, which is very soft and good in quality, is a square bar, 5 inches in thickness, and the medium line is 50 inches in length. It has 1500 feet of copper wire, 0·175 of an inch in thickness, coiled round it, and arranged (when I used it) in one continuous length. The moveable terminal pieces for the poles are massive in proportion to the magnet. Eighty pairs of Grove's plates were used to excite this magnet, and as it was found, by preliminary trials, that these were most powerful when arranged as four twenties, with their similar ends connected, they were so used, constituting a battery of twenty pairs of plates, in which each platinum plate was 4×9 inches in the immersed part, and therefore presented 72 square inches of surface towards the active zinc.

2748. On repeating the former experiments (2743.) the effect of pressure was again evident, and it was manifest that the magnet itself, though 5 inches in thickness, was a little bent by the mutual attraction of its poles. The effect was very small, because of the unity of the iron core passing through the centre of the experimental gas-chamber (2738.). It was the only effect indicated by the gauge, and was the same for all the gases; and when allowance was made for it, nothing remained to indicate any change in volume of the gas itself.

2749. *Air, oxygen, nitrogen, carbonic acid* and *nitrous oxide* were submitted, in varying order, to the effect of this very powerful magnet, but not the slightest trace of change of bulk in any of them appeared.

2750. I think that the experiments are in every respect sufficient to decide that these gases, whether they are considered as magnetic or diamagnetic bodies, or whether they include bodies of both classes (for oxygen is in striking contrast to the rest), are not affected in volume by the magnetic force, whether in fields of equal power (2737.), or in places where the power is rapidly diminishing. I think this decision very important in relation to the true nature of the magnetic force, either as existing in, or acting upon the particles of bodies; and as in the magnetic field the force exhibits itself, not as a central but as an axial power, so the further distinction of the phænomena, into such as are related to the axial direction (2733.), and such as are related to or include the equatorial direction (2737.), is not unimportant, for they show that the particles do not tend to separate either parallel to the lines of magnetic power, or in a direction perpendicular to these lines. Without the experiments, the mind might have considered it very possible that one of these modes of expansion might have occurred and not the other.

2751. No doubt it is true, that even yet changes in volume in these directions may occur, provided the change in one direction is expansion and in the other contraction, and that these are in amount equal to each other. It was partly in reference to such possible changes (which may be considered as molecular), that the experiments with the ray of light were made (2723. 2729.), and also that in these and other experiments instituted for the purpose, a polarized ray was employed as the examiner; but the results were always negative, when by repetition and care sources of error were removed.

2752. The great differences in the degree of diamagnetic susceptibility and condition which the gases employed in the foregoing experiments possess or can assume, are such as to make one ready to suppose, that if they show no tendency in any case to change in volume under the action of the magnet, so neither would any other gas or vapour do so, but that all the individuals belonging to this great class of bodies would be alike in that respect. In connexion with this conclusion I may state, that I have on former occasions, and more lately, endeavoured to ascertain, by the use of very delicate apparatus and powerful electro-magnets, whether any change was produced in the volume of such fluids as water, alcohol and solution of sulphate of iron, but could observe no effect of the kind, and I do not believe in its existence. Still more recently, and in reference to the class of solid bodies, I have submitted iron as a magnetic metal, and bismuth as a diamagnetic body, to the same examination; the metals were employed both in the state of solid cylinders and of filings or fragments. The cylinders were put into glass tubes and the particles into glass bottles; gauges, like those described (2732.), were applied to them, and that part of the containing vessel which was not filled with metal, was occupied, in one set of experiments, by air, and in another by alcohol, yet in no case could the least change in the volume of the iron or bismuth be observed, however powerful the magnetic force to which they were submitted.

2753. One other result of a repulsive force seemed possible even in cases when, according to a former supposition (2751.), the tendency to expand equatorially might be compensated by an equal amount of tendency to contract in the axial direction, namely, that of the production of currents outwards or equatorially, *i. e.* in lines perpendicular to the magnetic axis, where pointed poles or the hour-glass core, already described, were used, and of other currents setting in towards that line along the inclined surfaces of the polar terminations; in some degree like those occurring so powerfully, and traced so readily when flame or hot air is observed in air, or when a stream of one gas is observed in another gas[1].

[1] Philosophical Magazine, 1847, vol. xxxi. pp. 402, 404, 409.

2754. When however the gas occupying the whole of the magnetic field was uniform in nature and alike in temperature, not the slightest trace of such currents as these could be observed. It is not easy to devise unexceptionable tests of such motions, because visible bodies introduced into such a magnetic field to test the movements of the air there, are themselves diamagnetic; and if they form a little isolated cloud, are moved together and away as a diamagnetic body would be; but when the whole field was occupied pretty equally by very light particles of dust or lycopodium, and the magnet in powerful action, no signs of currents in the air were visible. Further, when a faint stream of diffuse cold smoke from a taper spark[1] was allowed to fall or rise a little on one side of the axial line, it was determined outwards and equatorially; but though it went outwards with the most force when equidistant from the two conical poles, or their representative parts in the double iron core (2738), still when it was made to pass near to one side, it continued to go outwards and equatorially, even when, from its close vicinity to the iron surface, it had as it were to move over it; showing that the tendency of the smoke was outwards in *every part* of the magnetic field occupied by air or gas, and that therefore its motion was due to the action of the magnet on it as a diamagnetic, and not to currents of the air, which, if existing, would be inwards in one place or direction, and outwards in another.

2755. When magnetic or diamagnetic fluids were subject to the magnetic force upon a plate of mica over the poles, according to the ingenious arrangement of Plücker, they quickly assumed the different forms correspondent to their nature, but after that there was no further motion or current in them. The cases are no doubt different to those where the whole of the magnetic field is occupied by the same medium; still, as far as it goes, it helps to confirm the conclusion that no currents are formed. On putting the same liquids between the poles in glass cells, no magnetic currents could be observed in them, though fine particles were introduced into the fluids, for the purpose of making such changes of place visible, if they occurred.

2756. So there is no evidence, either by the action on a ray of light (2727. 2729.), or by any expansion or contraction (2750.),

[1] Philosophical Magazine, 1847, vol. xxxi. p. 403.

or by the production of any currents (2754.), that the magnet exerts any direct power of attraction or repulsion on the particles of the different gases tried, or that they move in the magnetic field, as they are known to do, by any such immediate attraction or repulsion.

¶ ii. *Differential magnetic action.*

2757. Then what is the cause of the diamagnetic change of place? The effect is evidently a differential result, depending upon the *differences* of the two portions or masses of matter occupying the magnetic field, as the air and the streams of other gas in it[1], or mercury and the tube of air in it (2407.), or water and the piece of bismuth in it (2301.); and though exhibited only in the action of masses, the latter must no doubt owe their differences to the qualities of the particles composing them. Yet it is to be observed, that no attempt to separate the perfectly mixed particles of very different substances has ever succeeded, though made with most powerful magnets. Oxygen and nitrogen differ exceedingly, yet no appearance of the least degree of separation occurred in very powerful magnetic fields[2]. In other experiments I have enclosed a dilute solution of sulphate of iron in a tube, and placed the lower end of the tube between the poles of a powerful horseshoe magnet for days together, in a place of perfectly uniform temperature, and yet without the least appearance of any concentration of the solution in that end which might indicate a tendency in the particles to separate.

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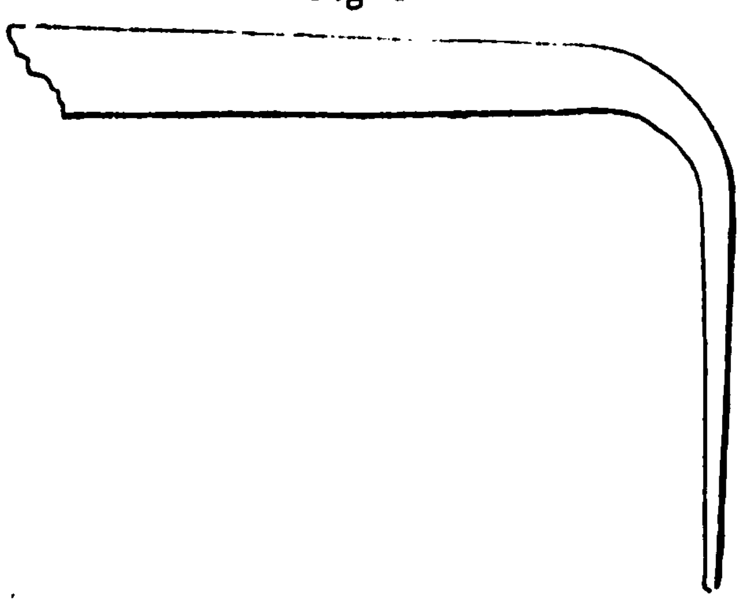
27 58. The diamagnetic phænomena of the gases, when considered as the differential result of the action of volumes of these bodies, may be produced and examined in a very useful manner by the employment of soap-bubbles, as follows:—A glass tube was fitted with a cap, stopcock and bladder, so that any given gas contained in the bladder might be sent through it, and also with a foot or stand so that it might be placed in any required position. The end of the tube was drawn down, bent at right angles, and cut off straight across at the extremity, being of

1 Philosophical Magazine, 1847, vol. xxxi. p. 409. 2 Ibid. p. 416.

Fig. 4.



the size and shape represented in fig. 4.

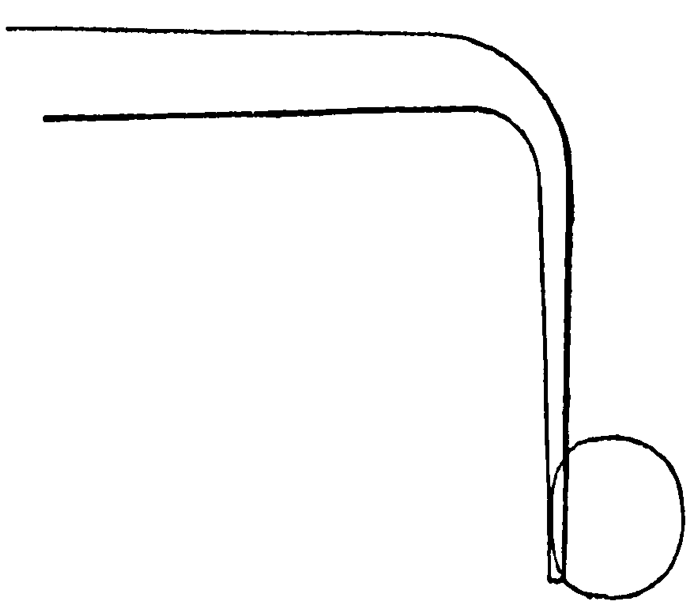
2759. It is easy to blow soap-bubbles at the end of such a tube, of any size up to an inch in diameter, and retain them for the time required by the action of the stopcock. The soapy water should be prepared, when wanted (and not beforehand), by putting a cutting or two of soap into a little cold distilled water, for then bubbles of the thinnest and most equable texture can be blown, which are more mobile than if thicker suds be used, and if a little care be taken, quite permanent enough for every useful experiment. The end of the pipe should be perfectly clean and free from heterogeneous matter (which is often destructive of the bubble), and should be wetted both inside and outside with the soap-water, and *left awhile in it before use.*

2760. If a bubble be blown with the end of the tube downwards, and be half an inch in diameter, it will usually have a ittle extra water at the bottom, and will hang from the slender extremity of the tube by an attachment so small as to allow it great freedom of motion. Hence it will swing to and fro like a pendulum; and according as there is more or less water at the bottom, it will vibrate more or less rapidly, will, as a whole, gravitate more or less powerfully, and therefore will retain its perpendicularly dependent position with more or less stability, —circumstances which are very useful in the employment of the bubble as a magnetic or diamagnetic indicator.

2761. The regulation of the relative quantity of water which is in or upon the bubble is easily obtained within certain limits. If, after the pipe is dipped in the soap-water, the end be touched with a piece of wood or glass rod, which has also been kept in the soap-water, more or less of the liquid may be removed; and by observing the height at which the fluid stands by capillary action within the tube, which may be varied between $\frac{1}{20}$th and $\frac{1}{2}$ an inch, it is easy, after a few experimental trials, to observe how much is required to make a bubble charged with a certain amount of water, and how little to give a bubble without any dependent water below; and then it is just as easy, by arranging the amount of water beforehand, to blow a bubble of any required character. Even when no drop of water is left at the bottom, still a range of thickness or thinness in the film itself can be obtained.

2762. As the bubbles contain less and less of water, so are they rendered more sensitive in their action. They vibrate slower, and are more easily moved by forces applied laterally to them. The diamagnetic effect of the soap-water constituting them is less, and therefore that of the gas contained within them comparatively greater. If the bubble is very thin, the dependent position becomes a position of unstable equilibrium, for any inclination of the tube, or any lateral force, however small, then causes the bubble to pass to one side, and to run up and adhere to the side of the tube, fig. 5. The dependent position supplies, in inclosed spaces or atmospheres, an exceedingly delicate indicator; and even when the bubble is on the side of the tube it still forms a very valuable instrument, for it freely moves round the tube as axis; and as it possesses a certain degree of steadiness, it can be held in the magnetic field in any position, and by its motion to or from the axial line, shows very well the magnetic or diamagnetic condition of the gas contained in it in relation to the surrounding air.

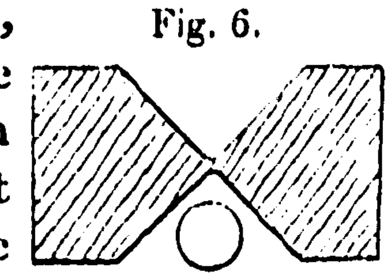
Fig. 5.



2763. If the mouth of the tube be turned upwards, bubbles of the thinnest texture can be blown; but they are then also very unstable in position, and run to the side of the tube; they can be used as indicators, as above (2762.). If the mouth of the tube be made broader, the bubbles, being thin, can be retained standing on the extremity; but as their attachment is larger, so they require more force to move them sideways, and teey lose in delicacy of indication. It is convenient, in working with such bubbles, to make them nearly equal in size and thickness for the same set of comparative experiments. I usually employ them about half an inch in diameter.

2764. On blowing such a bubble with *air*, in the dependent position, placing it in the angle of the double pole on a level with the axial line (fig. 6), and then putting on the magnetic power by the use of twenty pair of plates, the bubble was deflected outwards from the axial line (or equatorially) with a certain amount of force, and returned to its first position on the interruption of the electric current. The deflection was not great, and being due to the water of the bubble, gave an indication of the amount of that effect, to be used as a correction in experiments with other gases.

Fig. 6.



2765. *Nitrogen in air.*—A bubble of nitrogen went outwards or equatorially in common air with a force much surpassing the outward tendency of a bubble of air (2764.), in a very striking and illustrative manner. It was often driven up from the end to the side of the tube; and when on the side, if presented inwards, it was driven to the outside of the tube, and however the tube was turned round, kept that position as long as the magnetic force was maintained. This effect is the more striking when it is considered that four-fifths of the air itself is nitrogen gas.

2766. *Oxygen in air.*—The effect was very impressive, the bubble being pulled inwards or towards the axial line sharply and suddenly, exactly as if the oxygen were highly magnetic. The result was expected, being in accordance with the phænomena presented by oxygen and nitrogen in a former investigation of the diamagnetic phænomena of the gases[1].

2767. *Nitrous oxide and olefiant gases in air.*—The bubbles went outwards or diamagnetically with a force much greater than that due to the effect of the water of the bubble, proving the relation of these gases to air, and according with the results formerly obtained with streams of these substances[2].

2768. There is no difficulty in applying this method of observation to experiments with gases in atmospheres of other gases than air, provided they be such as do not destroy the bubble; but I do not consume time by detailing the results of such experiments, which accorded perfectly with those before obtained[3]. The description given is quite sufficient to illustrate the point stated, namely, that the motion of the gases, one in

[1] Philosophical Magazine, 1847, vol. xxxi. pp. 410, 415.
[2] Ibid. p. 411. [3] Ibid. pp. 414, 415.

another, when in the magnetic field, is a differential result, and supply sufficient cases for reference hereafter.

2769. The same conclusion, that the effect is a differential result of the masses of matter present in the magnetic field, is also manifest from the consideration of the cases of gaseous, liquid, and solid diamagnetic bodies, advanced in a former part of these Researches (2405–14.); and a conclusion of the same kind, as regards magnetic bodies, may also be drawn from experiments then described (2361–68.).

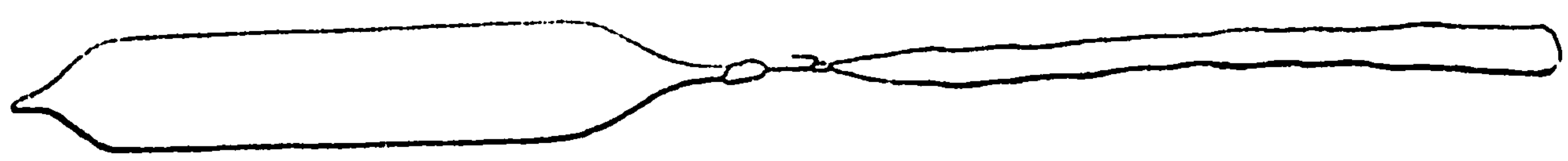
¶ iii. *Magnetic characters of Oxygen, Nitrogen, and Space.*

2770. The differential action of two portions of gas, or of any two bodies, may, by a more elaborate method, be examined in a manner far more interesting and important than that just described. The mode of action referred to may even be made the basis of instruments, by which, probably, most important indications and measurements of both magnetic and diamagnetic actions may be obtained, leading to results which are not even as yet contemplated by the imagination.

2771. If two portions of matter, gaseous or liquid, are tied together and placed in a symmetric magnetic field, on opposite sides of the magnetic axis, they will be simultaneously affected. If both are diamagnetic, or less magnetic than the medium occupying the magnetic field, both will tend to go outwards or equatorially; equally if they are alike, but unequally if they differ. The consequence will be, that, if they are placed, in the first instance, equidistant from the magnetic axis, the supervention of the magnetic force will not alter their position, provided they be alike; but if they differ, then their position will be changed; for the most diamagnetic will move outwards equatorially, pulling the least diamagnetic inwards until the two are in such new positions that the forces acting on them are equipoised, and they will assume a position of stable equilibrium. Now the distance through which they will move may be used indirectly, or better still, the force required to restore them to their equidistant position may be employed directly to estimate the tendency each had to go from the magnetic axis; that is, to give their relative diamagnetic intensities.

2772. That I might submit gases to such a method of examination, I selected a piece of very thin and regular flint-glass tube, about $\frac{5}{16}$ths of an inch external diameter, and not more than $\frac{1}{50}$th of an inch in thickness, and drawing at the blow-pipe lamp two equable portions of this tube into the shape and size represented, fig. 7, in which the barrel part is 1½ inch long, I filled one with

Fig. 7.



oxygen gas and the other with nitrogen gas, and then sealed them up hermetically. The end of the prolonged part of each was touched whilst warm with sealing-wax and a thread fastened to it, which thread was tied into a loop, also represented of full size. By these the tubes were to be suspended perpendicularly from a torsion balance, so that the middle of each should, when in place, be on a level with the magnetic axis.

2773. The torsion balance consisted of a bundle of sixty equally-stretched cocoon silk fibres, made fast above to a vertical axis carrying a horizontal index and graduated plate, and below to a horizontal lever. A cross bar, about 1½ inch long, was attached to one end of this lever, also in the horizontal plane; and on the extremities of this cross bar, and 8½ inches from the centre of motion, were hung the two tubes of oxygen and nitrogen (2772.), counterbalanced by a weight on the other arm of the horizontal level. The whole was thus so placed and adjusted in relation to the electro-magnet, furnished at the time with the double cone core or keeper (2764.), that the middle part of each tube was level with the middle of the core, and equidistant on each side from it. Under these circumstances, if any motion was given to the balance, so as to make its arm vibrate, the vibrations were made with great slowness, in consequence of the weight of the whole moving arrangement, and the small amount of torsion force in the cocoon silk.

2774. The moment the magnetic force was thrown into action all things changed. The oxygen tube was immediately carried inwards towards the axis, and the nitrogen tube driven outwards on the contrary side. The balance swung beyond its new place of rest and then returned with considerable power, vibrating many times in the period, which before was filled by a single oscillation; and when it had come to its place of rest, or of stable equilibrium, the oxygen tube was about one-eighth of an inch from the iron of the core, and the nitrogen tube four-eighths distant. Ten revolutions of the torsion axis altered only in a slight degree these relative distances.

2775. The actions which determine the mutual self-adjustment of the oxygen and nitrogen, as regards their place in relation to the magnetic axis, are very simple and evident. In the first place, the *glass* of the tubes is more diamagnetic than the surrounding medium or air (2424.), and therefore each tends to move outwards; but being equal in nature and condition to each other, they tend to move with equal force when at equal distances, and at those distances compensate each other. If one be driven inwards, it is subjected to a greater exertion of force by coming into a more intense part of the magnetic field; and the other, being at the same time carried outwards, is for a corresponding reason in a place of less intense action; and therefore, as soon as the constraint is removed, the system returns to its position of stable equilibrium, in which the two bodies are equidistant from the magnetic axis.

2776. The *contents* also of the tubes are subject to the magnetic forces, and as the result shows (2774.), in very different degrees. Either the oxygen tends inwards much more forcibly than the nitrogen, or the nitrogen tends outwards more powerfully than the oxygen; and the difference must exist to a very great degree, for it is such as to carry the glass of the oxygen tube up to a position so near the axis that it could not by itself, or with mere air inside, retain it for a moment without the aid of considerable restraint. The power with which the tubes only would retain their equidistant position, combined with the extent to which they are displaced from this position, shows the great amount of force which this conjoint action of the oxygen and the nitrogen leaves free to be exerted in the one direction, namely, from the oxygen inwards or axially, for though the action be complicated the result is simple. By former experiments, the nitrogen is known to pass equatorially and the oxygen axially in air[1], and the nitrogen tube will pass equatorially according to a certain differential force, depending on the flint-glass and the nitrogen on the one hand, and the bulk of air

[1] Philosophical Magazine, 1847, vol. xxxi. p. 409.

displaced by them on the other. The oxygen tube in like manner will tend to pass axially by a differential force, the amount of which will depend upon the tendency of the oxygen to go axially, of its tube to go equatorially, and of their joint relation to the air they displace. But both the tubes and their contents are by their joint relation to the air and their mechanical connexion so related to each other, that when a force (as of torsion) is employed to restore them to their equidistant position from the magnetic axis, all consideration of the matter of the tubes and of the air as a surrounding medium may be dismissed. The gases within them may be considered as in immediate relation with each other and the magnetic axis, and disembarrassed from all other actions: and the force which may be found needful to place them *equidistant*, is the measure of their magnetic or diamagnetic differences.

2777. Having thus explained the general principles of action, I will not at present go into their application in the construction of a measuring instrument or the results obtained with it, further than is required for the general elucidation of magnetic and diamagnetic bodies, and the determination of the true zero-point (2721. 2722.).

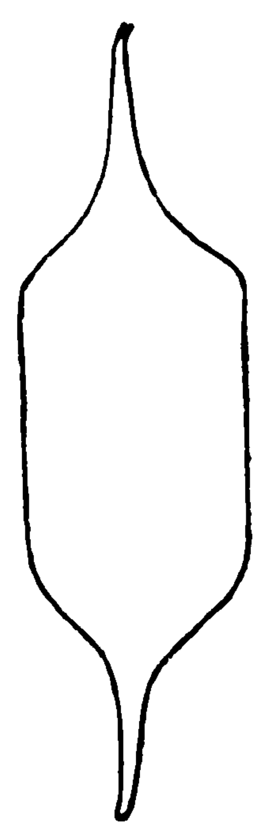
2778. The principles just described enabled me to return to a method of investigation which on a former occasion greatly excited my hopes (2433.), but which seemed then suddenly cut off by want of power. Various bodies, whether considered as magnetic or diamagnetic substances, admit of two modes of treatment, which promise to be exceedingly instructive as regards their properties and their destined purposes in natural operation. A gas may be *heated* or *cooled*, and the effect of temperature, which is known to be very influential[1], may now be ascertained without any change in the bulk of the gas; or it may be *rarefied* and *condensed* through a very extensive range, and the effect of this kind of change upon it ascertained independent of temperature or the presence of any other substance. Solids and liquids do not admit of these methods of examination, and do not therefore assist in the determination of the zero-point and of the true distinction of magnetic and diamagnetic bodies in the same manner that the gases do.

2779. It appeared to me that if a gaseous body were mag-

[1] Philosophical Magazine, 1847, vol. xxxi. pp. 406, 417.

netic, then its magnetic properties ought to be diminished in proportion as it was rarefied, *i. e.* that equal volumes of such a gas at different pressures ought to be more magnetic, as they are denser; on the other hand, that if a gas were diamagnetic, rarefaction ought to diminish its diamagnetic character, until, when reduced to the condition of a vacuum, it should disappear. In other words, if two opposed portions of the same magnetic gas, one rarer than the other, were subjected at once to the magnetic force, the *denser* ought to approach the axial line, or be drawn into the place of most intense action; whereas if two similarly opposed portions of a diamagnetic gas were subjected to the magnetic action, the more expanded or *rarer* gas ought to go inwards to the place of strongest action.

Fig. 8.



2780. Several bulbs of oxygen (fig. 8), similar in arrangement to those already described (2772.), and very nearly alike in size, were prepared and hermetically sealed, after that the quantity of gas within them had been reduced to a certain degree by the air-pump. The first contained the gas at the pressure of one atmosphere; the second had the gas at half an atmosphere, or 15 inches of mercury; the third contained gas at the pressure of 10 inches of mercury; and the fourth, after being filled with oxygen, was reduced to as good a vacuum as an excellent air-pump could effect. When the first of these was compared with the other three, the effect was most striking; opposed to the half atmosphere, it went towards the axis, driving the expanded portion away; when in relation to the one-third atmosphere, it went inwards or axially with still more power; and when opposed to the oxygen vacuum, it took its place as close to the iron core as in the former case, when contrasted with nitrogen (2774.); and it was manifest that the diamagnetic power of the glass tube which inclosed it (2775.), was the only thing which prevented the oxygen from pressing against the iron core occupying the centre of the magnetic field.

2781. On experimenting with the other tubes exactly the same result was obtained. Thus the tube with one-third of an atmosphere, in association with the vacuum tube, went inwards, driving the other outwards, *i. e.* it was more magnetic than the vacuum; but in association with the one-half atmosphere tube it went outwards, whilst the denser gas passed inwards. Any one of the tubes, if associated with another having a rarer atmosphere, passed inwards or magnetically, whilst if associated with others having denser atmospheres it passed outwards, being driven off by the superior magnetic force of the denser gas. As far as I could ascertain in these preliminary forms of experiment, the tendency inwards or axially appeared to be in proportion to the density of the gas; but the exact measurement of these forces will be given hereafter.

2782. Thus oxygen appears to be a very magnetic substance, for it passes axially, or from weaker to stronger places of force, with considerable power; a conclusion in accordance with the result of former observations[1]. Moreover it passes more powerfully when dense than when rare, its tendency inwards being apparently in proportion to its density. Hence, as the oxygen is removed, the magnetic force disappears with it, until when a vacuum is obtained, little or no trace of attraction or inward force remains. No doubt it may be said that dense oxygen is less diamagnetic than rare oxygen, or a vacuum. This however would imply, that the acting force of a substance, as the oxygen, could increase in proportion as the quantity of the substance diminished, which is not, I think, a philosophical assumption; and besides that, other reasons will soon appear to show that the magnetic condition which disappears as the oxygen is removed, belongs to, and is dependent upon that substance, and that oxygen is therefore a truly magnetic body.

2783. *Nitrogen*, being the other and larger part of the atmosphere, was then subjected to experiment, and three tubes, one containing the gas at a pressure of 30 inches of mercury, another with the gas at the pressure of 15 inches, and the third reduced as nearly as it could be to a vacuum, were prepared (2780.). When these were compared one with another in the magnetic field, they were found to be so nearly alike as not to be distinguishable from each other, *i. e.* they remained equidistant from the magnetic axis. I do not mean to imply that nitrogen at these different pressures is absolutely the same bulk for bulk (an instrument now under construction will enable me hereafter to compare and measure with infinitely greater accuracy, and to

[1] Philosophical Magazine, 1847, vol. xxxi. pp. 410, 415.

ascertain these points); but as compared with oxygen, the great and extraordinary differences produced by rarefaction there, have no corresponding differences here. If there are any, they are insensible at present, and may, for the chief purpose of this paper and the determination of the zero-point between magnetics and diamagnetics, be taken as nothing.

2784. Nitrogen therefore appears to be neither magnetic nor diamagnetic; if it were either, it could not but fall in its specific condition as it was rarefied; as it is, it is equivalent to a vacuum. If a given space be considered as a vacuum, into which oxygen or nitrogen is to be gradually introduced, as oxygen is added the space becomes more and more magnetic, *i. e.* more competent to admit of the kind of action distinguished by that word; but the corresponding gradual addition of nitrogen to an empty space produces no effect of that kind, or the contrary, and the nitrogen is therefore neither magnetic nor diamagnetic, but like space itself.

2785. As yet I have found no gas, which, being on the diamagnetic side of zero, can at all compare with oxygen in the range of effect produced by rarefaction. For the present, I may mention olefiant gas and cyanogen as substances which appear to proceed inwards, or towards the axial line, as they are more rarefied. They are therefore not merely at zero, but are in opposition to oxygen and are diamagnetic bodies. But if we want a body that is strongly and undeniably diamagnetic, and which, being added to or introduced into space, will make it diamagnetic, as oxygen renders it magnetic, then flint glass or phosphorus presents us with such a substance. When these bodies are made into forms similar to the volumes of nitrogen, or the vacua in size and shape, and are compared with them on the torsion balance, they pass outwards with much force; and it is probably the great diamagnetic force of the glass of the tubes that prevents the effect of rarefaction from being more evident in olefiant and other gases.

2786. When a tube has been filled with a particular gas, then exhausted as much as possible, and sealed up hermetically, it may be considered as inclosing what is commonly called a vacuum. I have prepared many such vacua, and may be permitted to distinguish them by the name of the gas, traces of which still remain. In comparing these vacua in the magnetic field (2773.), they appeared to me to be in all respects alike; the oxygen vacuum was not more magnetic than the hydrogen, nitrogen, or olefiant vacuum. Their differences, if any, were far smaller than the differences which could be produced by the variations of size and other conditions of the glass bulbs, and can only be made manifest by the means hereafter to be used (2783.); and I am fully persuaded that they will ultimately be nearly alike, ranging close *up to and about a perfect* vacuum.

2787. Before determining the place of zero amongst magnetic and diamagnetic bodies, we have to consider the true character and relation of *space* free from any material substance. Though one cannot procure a space perfectly free from matter, one can make a close approximation to it in a carefully prepared Torricellian vacuum. Perhaps it is hardly necessary for me to state, that I find both iron and bismuth in such vacua perfectly obedient to the magnet. From such experiments, and also from general observations and knowledge, it seems manifest that the lines of magnetic force (2149.) can traverse pure space, just as gravitating force does, and as static electrical forces do (1616.); and therefore space has a magnetic relation of its own, and one that we shall probably find hereafter to be of the utmost importance in natural phænomena. But this character of space is not of the same kind as that which, in relation to matter, we endeavour to express by the terms magnetic and diamagnetic. To confuse them together would be to confound space with matter, and to trouble all the conceptions by which we endeavour to understand and work out a progressively clearer view of the mode of action and the laws of natural forces. It would be as if, in gravitation or electric forces (1613.), one were to confound the particles acting on each other with the space across which they are acting, and would, I think, shut the door to advancement. Mere space cannot act as matter acts, even though the utmost latitude be allowed to the hypothesis of an ether; and admitting that hypothesis, it would be a large additional assumption to suppose that the lines of magnetic force are vibrations carried on by it (2591.); whilst as yet, we have no proof or indication that time is required for their propagation, or in what respect they may in general character assimilate to, or differ from, the respective lines of gravitating, luminiferous, or electric forces.

2788. Neither can space be supposed to have those circular currents round points diffused through it, which Ampère's theory assumes to exist around the particles of ordinary magnetic matter, and which I had for a moment supposed might exist in the contrary direction round the particles of diamagnetic matter (2429. 2640. &c.). The imagination, restrained by philosophical considerations, fails to find anything in pure space about which the currents could circulate, or to which they could by any association be attached; and the difficulty, if already not immeasurable, would be still greater to those, if there be any, who, assuming that magnetic and diamagnetic bodies are alike in nature, must assume that there are like currents in both; for it does not seem possible to add (for instance) phosphorus having such a magnetic constitution, to space supposed to be of a similar constitution, and yet to have as a result a diminution of the magnetic powers of the space so occupied.

2789. As space therefore comports itself independently of matter, and after another manner, the different varieties of matter must, in relation to their respective qualities, be considered amongst themselves. Those which produce no effect when added to space, appear to me to be neutral or to stand at zero. Those which bring with them an effect of one kind will be on the one side of zero, and those which produce an effect of the contrary kind will be on the other side of zero; by this division they constitute the two subdivisions of magnetic and diamagnetic bodies. The law which I formerly ventured to give (2267. 2418.), still expresses accurately their relations; for in an absolute vacuum or free space, a magnetic body tends from weaker to stronger places of magnetic action, and a diamagnetic body under similar conditions from stronger to weaker places of action.

2790. Now that the *true zero* is obtained, and the great variety of material substances satisfactorily divided into two general classes, it appears to me that we want another name for the magnetic class, that we may avoid confusion. The word *magnetic* ought to be general, and include *all* the phænomena and effects produced by the power. But then a word for the subdivision, opposed to the diamagnetic class, is necessary. As the language of this branch of science may soon require general and careful changes, I, assisted by a kind friend, have thought that a word not selected with particular care might be provisionally useful; and as the magnetism of iron, nickel and cobalt, when in the magnetic field, is like that of the earth as a whole, so that when rendered active they place themselves parallel to its axes or lines of magnetic force, I have supposed that they and their similars (including oxygen now) might be called paramagnetic bodies, giving the following division:—

$$\text{Magnetic}\begin{cases}\text{Paramagnetic.}\\ \text{Diamagnetic.}\end{cases}$$

If the attempt to facilitate expression be not accepted, I hope it will be excused.

2791. From the presence of oxygen in the air, the latter is, as a whole, a magnetic medium of no small power. Hence all the comparative experiments on the diamagnetic condition of other gases, made by passing streams of them through it and through each other[1], require a correction, which occasionally may place some of these bodies on the paramagnetic side of zero. Even solid and fluid substances may be thus affected; and the preliminary list, which I formerly gave (2424.), will need alteration in this respect. I hope soon however to have the means of ascertaining, not only the place of bodies, but also their relative degrees of force, at the same and at different temperatures, with a degree of accuracy that will serve great purposes in the further development of this branch of science.

2792. Amongst the gases hitherto examined there is nothing that compares with oxygen. The following are comparatively indifferent by the side of it:—chlorine and bromine vapour, cyanogen, nitrogen, hydrogen, carbonic acid, carbonic oxide, olefiant gas, nitrous oxide, nitric oxide, nitrous acid vapour, muriatic acid, sulphurous acid, hydriodic acid, ammonia, sulphuretted hydrogen, coal-gas, æther vapour and sulphuret of carbon vapour; for though some, as olefiant and cyanogen gases, appear to be a little diamagnetic, and others, as nitrous oxide and nitric oxide, are magnetic, yet their effects disappear in comparison with the results produced by oxygen.

2793. I hope to give the correct expression of the paramagnetic force of oxygen (2783.) hereafter, in the meantime I am tempted to give one or two rough illustrations of its degree in

[1] Philosophical Magazine, 1847, vol xxxi. pp. 407, 420, &c.

this place, in addition to the former one (2774.). The capacity of the oxygen bulb, containing one atmosphere, is not quite 0·34 of a cubic inch, and the weight therefore of the oxygen within 0·117 of a grain. I endeavoured to compare this quantity in the first place with soft iron, and therefore attached a portion of that metal, having one-tenth of this weight or 0·012 of a grain, to a fine platina wire fixed into one end of a vessel, corresponding in size to that containing the oxygen, so as to bring the iron into the middle, and then the bulb was exhausted and hermetically sealed. Being now opposed to the oxygen tube in the magnetic field, it was found, as expected, far to surpass the oxygen in magnetic power. As it was inconvenient further to reduce the iron or to enlarge the oxygen, another magnetic substance was employed for the comparison.

2794. One hundred grains of clean, good, crystallized protosulphate of iron were dissolved in distilled water, and diluted until a glass bulb, of nearly the same size as the oxygen bulb when filled with the solution, was equal to the oxygen bulb in force, and stood equidistant from the axial line, as far as I could judge by the present modes of observation. When the solution had this strength, it occupied the bulk of 17½ cubic inches. As the bulk of the oxygen is only 0·34 of a cubic inch (2793.), that volume of this solution would contain very nearly two grains of crystallized sulphate of iron, equivalent to 0·4 of a grain of metallic iron; so that, bulk for bulk, oxygen is equally magnetic with a solution of sulphate of iron in water containing seventeen times the weight of the oxygen in crystallized protosulphate of iron, or 3·4 times its weight of metallic iron in that state of combination.

2795. Again, the oxygen tubes, containing respectively one atmosphere and a vacuum (2780.), were adjusted about an inch apart, and placed on each side of the magnetic axis, and the force of the magnet developed. The oxygen of course approached the magnetic axis, and the vacuum passed equatorially. A slender glass filament, about 6 inches in length, had been drawn out at the lamp and fixed to a foot; and the end of this filament was then employed to press back the oxygen tube into its original place, and render it equidistant from the magnetic axis with the vacuum tube. In this position the two tubes would, as respects the glass, neutralize each other (2775.); and considering the vacuum as zero, the oxygen alone may be considered as active, and the force required to hold it out may be looked upon as the force with which the oxygen, at that distance of half an inch, tended to go to the magnetic axis. The deflection of the glass filament or spring, at the place where the oxygen tube was held by it, was rather more than one inch from its position when relieved from the pressure of the tube. Being taken away, it was set up in the horizontal position (after being turned 90° on its axis, so that the flexure might be in the same direction, relative to the filament, as before); and the position of the end being marked, weights were put on it at the place of former contact with the oxygen tube, until they produced the same amount of deflection as before. It required rather more than the tenth of a grain to produce this effect; and this, considering that the whole oxygen only weighed 0·117 of a grain, and that no part of it was nearer than half an inch, whilst the average distance of the mass was above an inch from the magnetic axis, gives a high expression for the magnetic power.

2796. It is hardly necessary for me to say here that this oxygen cannot exist in the atmosphere, exerting such a remarkable and high amount of magnetic force, without having a most important influence on the disposition of the magnetism of the earth as a planet, especially if it be remembered that its magnetic condition is greatly altered by variations in its density (2781.) and by variations in its temperature[1]. I think I see here the real cause of many of the variations of that force, which have been, and are now, so carefully watched on different parts of the surface of the globe. The daily variation and the annual variation both seem likely to come under it; also very many of the irregular continual variations which the photographic process of record renders so beautifully manifest. If such expectations be confirmed, and the influence of the atmosphere be found able to produce results like these, then we shall probably find a new relation between the aurora borealis and the magnetism of the earth, namely, a relation established, more or less, through the air itself in connexion with the space above it; and even magnetic relations and variations which are not as yet suspected, may be suggested and rendered manifest and measurable, in the further development of what I will venture to call *Atmo-*

[1] Philosophical Magazine, 1847, vol. xxxi, p. 417.

spheric Magnetism (2847. &c.). I may be over-sanguine in these expectations, but as yet I am sustained in them by the apparent reality, simplicity and sufficiency of the cause assumed, as it at present appears to my mind. As soon as I have sufficiently submitted these views to a close consideration and the test of accordance with observation, and where applicable with experiments also, I will do myself the honour to bring them before the Royal Society.

Royal Institution, August 2, 1850.

TWENTY-SIXTH SERIES[1].

§ 32. *Magnetic conducting power.* ¶ i. *Magnetic conduction.* ¶ ii. *Conduction polarity.* ¶ iii. *Magnecrystallic conduction.* § 33. *Atmospheric magnetism.* ¶ i. *General Principles.*

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¶ i. *Magnetic conduction.*

2797. The remarkable results given in a former series of these Researches (2757. &c.) respecting the powerful tendency of certain gaseous substances to proceed either to or from the central line of magnetic force, according to their relation to other substances present at the same time, and yet the absence of all condensation or expansion of these bodies (2756.) which might be supposed to be consequent on such an amount of attractive or repulsive force as would be thought needful to produce this tendency and determination to particular places, have, upon consideration, led me to the idea, that if bodies possess different degrees of *conducting power* for magnetism, that difference may account for all the phænomena; and, further, that if such an idea be considered, it may assist in developing the nature of magnetic force. I shall therefore venture to think and speak freely on this matter for a while, for the purpose of drawing others into a consideration of the subject; though I run the risk, in doing so, of falling into error through imperfect experiments and reasoning. As yet, however, I only state the case hypothetically, and use the phrase *conducting power* as a general expression of the capability which bodies may possess of affecting the transmission of magnetic force; implying nothing as to how the process of conduction is carried on. Thus limited in sense, the phrase may be very useful, enabling us to take, for

[1] Philosophical Transactions, 1851, p. 29.

[2] Revised by the author and returned by him November 12, 1850.

a time, a connected, consistent and general view of a large class of phænomena; may serve as a standard of meaning amongst them, and yet need not necessarily involve any error, inasmuch as whatever may be the principles and condition of conduction, the phænomena dependent on it must consist among themselves.

2798. If a medium having a certain conducting power occupy the magnetic field, and then a portion of another medium or substance be placed in the field having a greater conducting power, the latter will tend to draw up towards the place of greatest force, displacing the former. Such at least is the case with bodies that are freely magnetic, as iron, nickel, cobalt and their combinations (2357. 2363. 2367. &c.), and such a result is in analogy with the phænomena produced by electric induction. If a portion of still higher conducting power be brought into play, it will approach the axial line and displace that which had just gone there; so that a body having a certain amount of conducting power, will appear as if attracted in a medium of weaker power, and as if repelled in a medium of stronger power by this differential kind of action (2367. 2414.).

2799. At the same time that this idea of conduction will thus account for the place which a given substance would take up, as of oxygen in the axial line if in nitrogen, or of nitrogen at a distance if in oxygen, it also harmonizes with the fact, that there are no currents induced in a single gas occupying the magnetic field (2754.), for any one particle can then conduct as well as any other, and therefore will keep its place; and it also agrees, I think, with the unchangeability of volume (2750.).

2800. In reference to the latter point, we have to consider that the force which urges such a body as oxygen towards the middle of the field, is not a central force like gravitation, or the mutual attraction of a set of particles for each other; but an axial force, which, being very different in character in the direction of the axis and of the radii, may, and must produce its effect in a very different manner to a purely central force. That these differences exist, is manifest by the action of transparent bodies, when in the magnetic field, upon a ray of light; and also by the ordinary action of magnetic bodies: and hence, perhaps, the reason, that when oxygen is drawn into the middle of the field, in consequence of its conducting power, still its particles are not compressed together (2721.) by a force that otherwise would seem equal to that effect (2766.).

2801. So when two separate portions of oxygen or nitrogen are in the magnetic field, the one passes inwards and the other outwards, without any contraction or expansion of their relative volumes; and the result is differential, the two bodies being in *relation to and dependence on* each other, by being simultaneously related to the lines of magnetic force which pass conjointly through them both, or through them, and the medium in which they are conjointly immersed.

2802. I have already said, in reference to the transference onwards of magnetic force (2787.), that pure space or a vacuum permits that transference, independent of any function that can be considered as of the same nature as the conducting power of matter; and in a manner more analogous to that in which the lines of gravitating force, or of static electric force, pass across mere space. Then as respects those bodies which, like oxygen, facilitate the transmission of this power more or less, they class together as magnetic or paramagnetic substances (2790.); and those bodies, which, like olefiant gas or phosphorus, give more or less obstruction, may be arranged together as the diamagnetic class. Perhaps it is not correct to express both these qualities by the term *conduction*; but in the present state of the subject, and under the reservation already made (2797.), the phrase may I think be employed conveniently without introducing confusion.

2803. If such be a correct general view of the nature and differences of paramagnetic and diamagnetic substances, then the internal processes by which they perform their functions can hardly be the same, though they might be similar. Thus they *may* have circular electric currents in opposite directions, but their distinction can scarcely be supposed to depend upon the difference of force of currents in the *same* direction. If the view be correct also, though the results obtained when two bodies are simultaneously present in the magnetic field may be considered as differential (2770. 2768.) even though one of them be the general medium, yet the consequence of the presence of conducting power in matter renders a *single* body, when in space, subject to the magnetic force; and the result is, that when a paramagnetic substance is in a magnetic field of unequal force, it tends to proceed from weaker to stronger places of action, or is *attracted*; and when a diamagnetic body is similarly circumstanced, it tends to go from stronger to weaker places of action, or is *repelled* (2756.).

2804. Matter, when its powers are under consideration, may, as to its quantity, be considered either by weight or by volume. In the present case, where the effects produced have an immediate reference to mere space (2787. 2802.), it seems proper tha the volume should be considered as the representation, and that in comparing one substance with another, equal volumes should be employed to give correct results. No other method could be used with the differential system of observation (2772. 2780.).

2805. Some experimental evidence, other than that of change of situation, of the existence of this conducting power, by differences in which, I am endeavouring to account for the peculiar characteristics of paramagnetic and diamagnetic bodies, may well be expected. This evidence exists; but as certain considerations connected with polarity preclude me from calling too freely upon iron, cobalt, or nickel (2832.) for illustrations, and as in other bodies which are paramagnetic, as well as in those that are diamagnetic, the effects are very weak, they will be better comprehended after some further general consideration of the subject (2843.).

2806. I will now endeavour to consider what the influence is which paramagnetic and diamagnetic bodies, viewed as conductors (2797.), exert upon the lines of force in a magnetic field. Any portion of space traversed by lines of magnetic power, may be taken as such a field, and there is probably no space without them. The condition of the field may vary in intensity of power, from place to place, either along the lines or across them; but it will be better to assume for the present consideration a field of equal force throughout, and I have formerly described how this may, for a certain limited space, be produced (2465.). In such a field the power does not vary either along or across the lines, but the distinction of direction is as great and important as ever, and has been already marked and expressed by the term axial and equatorial, according as it is either parallel or transverse to the magnetic axis.

2807. When a paramagnetic conductor, as for instance, a sphere of oxygen, is introduced into such a magnetic field, considered previously as free from matter, it will cause a concentration of the lines of force on and through it, so that the space occupied by it transmits more magnetic power than before (fig. 1). If, on the other hand, a sphere of diamagnetic matter be placed in a similar field, it will cause a divergence or opening out of the lines in the equatorial direction (fig. 2); and less magnetic power will be transmitted through the space it occupies than if it were away.

Fig. 1.

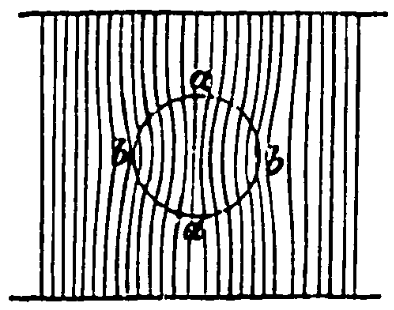
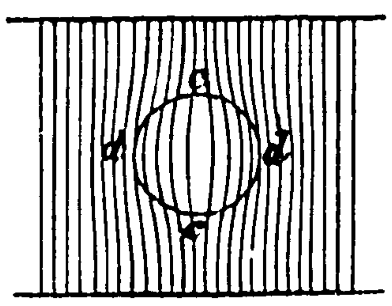


Fig. 2.



2808. In this manner these two bodies will be found to affect, *first* the *direction* of the lines of force, not only within the space occupied by themselves, but also in the neighbouring space, into which the lines passing through them are prolonged; and this change in the course of the lines will be in the contrary direction for the two cases.

2809. *Secondly*, they will affect the *amount* of force in any particular part of the space within or near them; for as every section across the line of such a magnetic field must be definite in amount of force, and be in that respect the same as every other section, so it is impossible to cause a concentration within the sphere of oxygen (fig. 1) without causing also a simultaneous concentration in the parts axially situated as *a a* outside of it, and a corresponding diminution in the parts equatorially placed, *b b*. On the other hand, the diamagnetic body (fig. 2) will cause diminution of the magnetic force in the parts of space axially placed in respect of it, *c c*, and concentration in the near equatorial parts, *d d*. If the magnetic field be considered as limited in its extent by the walls of iron forming the faces of opposed poles (2165.), then even the distribution of the magnetism within the iron itself will be affected by the presence of the paramagnetic or diamagnetic bodies; and this will happen to a very large extent indeed, when, from among the paramagnetic class, such substances as iron, nickel or cobalt are selected.

2810. The influence of this disturbance of the forces upon the place and position of either a paramagnetic or a diamagnetic body placed within the magnetic field, is readily deduced upon consideration and easily made manifest by experiment. A small sphere of iron placed within a field of equal magnetic power, bounded by the iron poles, has a position of unstable equilibrium, equidistant from the iron surfaces, and at such time a great concentration of force takes place through it, and at the iron faces opposite to it, and through the intervening axial spaces. If the sphere be on either side of the middle distance, it flies to the nearest iron surface, and then can determine the greatest amount of magnetic force to or upon the axial lines which pass through it.

2811. If the iron be a spheroid, then its greatest diameter points axially, whether it be in the position of unstable equilibrium, nearer to or in contact with the iron walls of the field. As the circumstances are now more favourable for the concentration of force in the axial line passing through the body than before, so this result can be produced by much weaker paramagnetics than iron, and I have no doubt could easily be produced by a vessel of oxygen or nitric oxide gas (2782. 2792.). It now becomes indeed a form, though not the best, of that experiment by which the magnetic condition of bodies is considered as most sensitively tested.

2812. The relative deficiency of power in diamagnetic bodies renders any attempt to obtain the converse phænomena to those of iron somewhat difficult; in order therefore to exalt the conditions, I used a saturated solution of protosulphate of iron in the magnetic field; by this means I strengthened the lines of power passing across it, without disturbing its equality in the parts employed, or introducing any error into the principle of the experiment, and then used bismuth as the diamagnetic body. A cylinder of this substance, suspended vertically, tended well towards the middle distance, finding its place of stable equilibrium in the spot where the paramagnetic body had unstable equilibrium. When the cylinder was suspended horizontally, then the direction it took was equatorial; and this effect also was very clear and distinct.

2813. These relative and reverse positions of paramagnetic and diamagnetic bodies, in a field of equal magnetic force, accord well with their known relations to each other, and with the kind of action already laid down in principle (2807.) as that which they exert on the magnetic power to which they are subjected. One may retain them in the mind by conceiving that if a liquid sphere of a paramagnetic conductor were in the place of action, and then the magnetic force developed, it would change in form and be prolonged axially, becoming an oblong spheroid; whereas if such a sphere of diamagnetic matter were placed there, it would be extended in the equatorial direction and become an oblate spheroid.

2814. The *mutual action* of two portions of paramagnetic matter, when they are both in such a field of equal magnetic force, may be anticipated from the principles (2807. 2830.), or from the corresponding facts, which are generally known. Two spheres of iron, if retained in the same equatorial plane, repel each other strongly; but as they are allowed to depart out of that plane, they first lose their mutual repulsive force and then attract each other, and that they do most powerfully when in an axial direction.

2815. With diamagnetic bodies the mutual action is more difficult to determine, because of the comparative lowness of their condition. I therefore resorted to the expedient, before described, of using a saturated solution of protosulphate of iron as the medium occupying the field of equal magnetic force, and employing two cylinders of phosphorus, about an inch long and half an inch in diameter, as the diamagnetic bodies. One of these was suspended at the end of a lever, which was itself suspended by cocoon-silk, so as to have extremely free motion, and the adjustments were such, that when the phosphorus cylinder was in the middle of the magnetic field, it was free to move equatorially or across the lines of magnetic force; it however had *no tendency* to do so under the influence of the magnetic force. The other cylinder was attached to a copper wire handle, and could be placed in a fixed position on either side of the former cylinder; it was therefore adjusted close by the side of it, and the two retained together, until all disturbance from motion of the fluid or of the air had ceased; then the retaining body was removed, the two phosphorus cylinders still keeping their places; finally, the magnetic power was brought into action, and immediately the moveable cylinder separated slowly from the fixed one and passed to a distance. If brought back again whilst the magnet was active, when left at liberty it receded;

but if restored to close vicinity, when the magnetic force was away, it retained that situation. The effect took place either in the one direction or the other, according as the fixed cylinder was on this or that side of the moving one; but the motion was in both cases across the lines of magnetic force, and was indeed mechanically and purposely limited to that direction by the mode of suspension. When two bismuth balls were placed, in respect of each other, in the direction of the magnetic axis, so that one might move, but only in the direction of that axis, its place was not sensibly affected by the other; the tendency of the free one to go to the middle of the field (2812.) overpowered any other tendency that might really exist.

2816. Thus two diamagnetic bodies, when in the magnetic field, do truly affect each other; but the result is not opposed in its direction to that of paramagnetic bodies, being in both cases a separation of the substances from each other.

2817. The comparison of the action of para- and diamagnetic bodies on each other, was completed by using water as the medium in a field of equal magnetic force, and suspending a piece of phosphorus from the torsion balance. When the magnetic power was on, this phosphorus was repelled equatorially, as before, by another piece of phosphorus, but it was attracted by a tube filled with a saturated solution of protosulphate of iron; so paramagnetic and diamagnetic bodies attract each other equatorially in a mean medium, but each repels bodies of its own kind (2831.).

¶ ii. *Conduction polarity.*

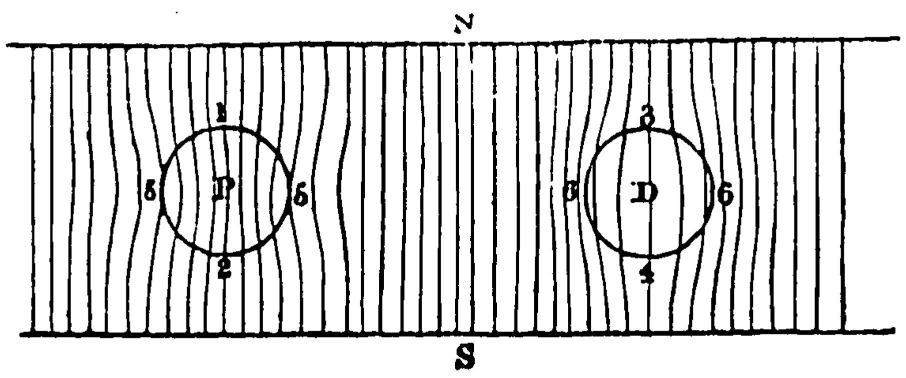
2818. Having thus considered briefly the effects which the disturbance of the lines of force, by the presence of paramagnetic and diamagnetic bodies, is competent to produce (2807. &c.), I will ask attention to that which may be considered as their polarity: not wishing by the term to indicate any internal condition of the substances or their particles, but the condition of the mass as a whole, in respect of the state into which it is brought by its own disturbance of the lines of magnetic force; and that, both in regard to its condition with respect to other bodies similarly affected; and also in regard to differences existing in different parts of its own mass. Such a condition concerns what may be called conduction polarity. Bodies in free space, when under magnetic action, will possess it in its simplest condition; but bodies immersed in other media will also possess it under more complicated forms, and its amount may then be varied, being reversed or increased, or diminished to a very large extent.

2819. Taking the simplest case of paramagnetic polarity, or that presented in fig. 1 (2807.), it consists in a convergence of the lines of magnetic force on to two opposed parts of the body, which are to each other in the direction of the magnetic axis. The difference in character of the two poles at these parts is very great, being that which is due to the known difference of quality in the two opposite directions of the line of magnetic force. Whether polar attraction or repulsion exists amongst paramagnetic bodies, when they present mere cases of conduction (as oxygen, for instance), is not yet certain (2827.), but it probably does; and if so, will doubtless be consistent with the attraction and repulsion of *magnets* having correspondent poles.

2820. When we consider the conduction polarity of a diamagnetic body, matters appear altogether different. It has not a polarity like that of a paramagnetic substance, or one the mere reverse (in name or direction of the lines of force) of such a substance, as I, Weber and others have at times assumed (2640.), but a state of its own altogether special. Its polarity consists of a divergence of the lines of power on to, or a convergence from the parts, which being opposite, are in the direction of the magnetic axis; so that these poles, having the *same* general and opposite relations to each other, which correspond to the differences in the poles of paramagnetic bodies, have still, under the circumstances, that striking contrast and difference from the polarity of the latter bodies which is given by convergence and divergence of the lines of force.

2821. Let fig. 3 represent a limited magnetic field with a paramagnetic body P, and a diamagnetic body D, in it, and let N and S represent the two walls of iron associated with the magnet (2465.) which form its boundary, we shall then be able to obtain a clear idea of the direction

Fig. 3.



of the lines of magnetic force in the field. Now the two bodies, P and D, cannot be represented by supposing merely that they have the same polarities in opposite directions. The 1 polarity of P is importantly unlike the 3 polarity of D; but if D be considered as having the reverse polarities of P, then the one polarity of P should be like the 4 polarity of D, whereas it is more unlike to that than to the 3 polarity of D, or even to its own 2 polarity.

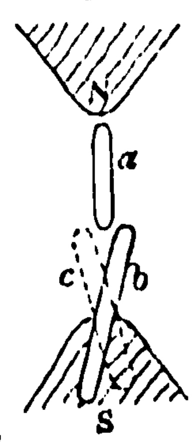
2822. There are therefore two essential differences in the nature of the polarities dependent on conduction, the difference in the direction of the lines of force abutting on the polar surfaces, when the comparison is with a magnet reversed, and the difference of convergence and divergence of these lines, when compared with a magnet not reversed; and hence a diamagnetic body is not in that condition of polarity which may be represented by turning a paramagnetic body end for end, while it retains its magnetic state.

2823. Diamagnetic bodies in media more diamagnetic than themselves, would have the polar condition of paramagnetic bodies (2819.); and in like manner paramagnetic conductors in media more paramagnetic than themselves, would have the polarity of diamagnetic bodies.

2824. Besides these differences the bodies must have an equatorial condition, which, in the two classes of conductors, would be able to produce corresponding effects. The whole of the equatorial part of P (fig. 3) is alike in polar relation to the body P, or to the lines of force in the surrounding space; and there is a like correspondence in the equatorial parts of D, either to itself or to space; but these parts in P or in D differ in intensity of power one from the other, and both from the general intensity of the space. Such equatorial conditions must, I think, exist as a consequence of the definite character of any given section of the magnetic field (2809.).

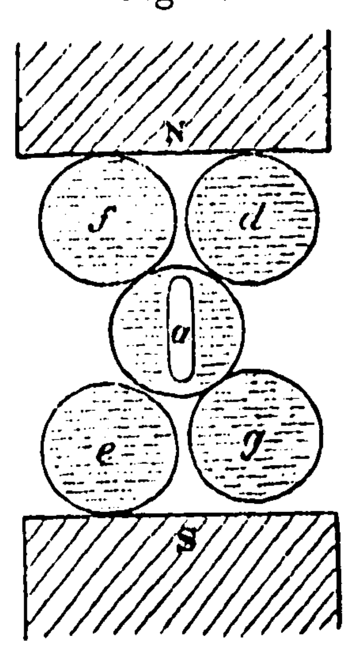
2825. Though the experimental results of these polarities are not absent, still they are not very evident or capable of being embodied in many striking forms; and that because of the extreme weakness of the forces brought into play, as compared with those larger forces exhibited in the mutual action of magnets. Hence it is, that the many attempts to show a polarity in bismuth have either failed, or other phaenomena have been mistaken for those properly referable to such a cause. The highest, and therefore the most delicate, test of polarity we possess, is in the subjection of the polar body to the line of direction of magnetic forces of a very high degree, when developed around it; and hence it is, that the pointing of a substance between the poles of a powerful magnet is continually referred to for such a purpose. It would be, and is utterly in vain to look for any mutual action between the poles of two weak paramagnetic or diamagnetic conductors in many cases, when the action of these same poles is abundantly manifest in their relation to the almost infinitely stronger poles of a powerful horseshoe or electro-magnet.

Fig. 4.



2826. I took a tube *a* (fig. 4), filled with a saturated solution of sulphate of cobalt, and suspended it between the poles of the great electro-magnet; it set readily and well. Another tube, *b*, was then filled with a saturated solution of sulphate of iron, and being associated with the S pole of the magnet, was brought near the cobalt tube in the manner shown, but not the slightest effect on the position of *a* was observable. The tube *b* was changed into the position *c*, to double any effect that might be present, but no trace of mutual action between the poles of *a* and *b* was visible (2819.).

Fig. 5.



2827. To increase the effect, the magnetic solution tube was suspended in water, as a good diamagnetic medium, between flat-faced poles (fig. 5). It pointed well. Two bottles of saturated solution of sulphate of iron were placed at *d* and *e*, but they did not alter the position of *a*; being removed into the positions *f* and *g*, neither was any sensible alteration of the position of *a* produced. I made the same kind of experiment with an air-tube in water, in which case it points axially (2406.), with the same negative result. I do not mean to assert that there was absolutely no effect produced in these cases (2819.); but if any, it must have been inappreciably small, and shows how unfit such means are to compare with those which are supplied by the pointing of a body when under the influence of powerful magnets. If polarity cannot be found by these methods in paramagnetic bodies so strongly influential as saturated solution of iron, nickel or cobalt, it can hardly be expected to manifest itself by analogous actions in the much weaker cases of diamagnetic substances.

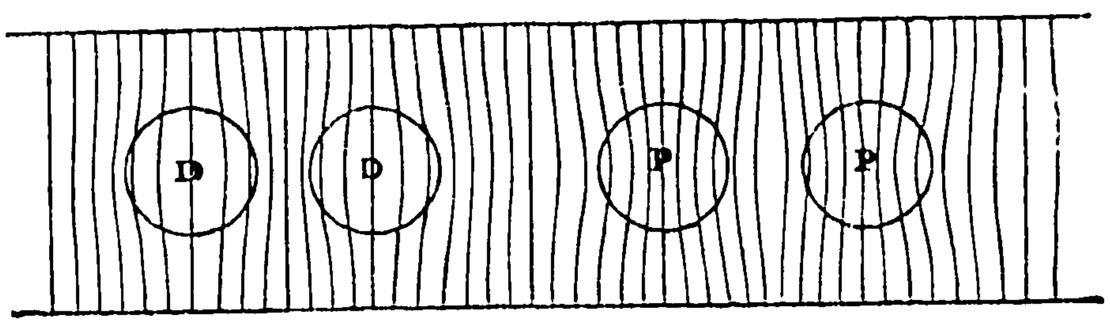
2828. When a spherical paramagnetic conductor is placed midway in a field of equal magnetic force, it occupies a place of unstable equilibrium, from which, if it be displaced ever so little, it will continue to move until it has gained the iron boundary walls of the field (2465. 2810.); this is a consequence of its particular polar condition. If the sphere were free to change its form, it would elongate in the direction of the magnetic axis; or if it were a solid of an elongated form, it would point axially, both consequences of its polar condition (2811.).

2829. So also in the case of diamagnetic bodies, their peculiar condition of polarity is shown by corresponding facts, namely, by a spherical portion having its place of stable equilibrium in the middle of the magnetic field (2812.), by a fluid portion tending to expand equatorially and become an oblate spheroid (2813.), and by the equatorial pointing of an elongated portion (2812.). If pointed magnetic poles are used, then the effects are very much stronger, but are exactly the same in kind, and dependent upon the same causes and polar conditions.

2830. There are another set of effects produced, which are either the results of the axial polarity just referred to, or else may be considered as consequences of the condition of the equatorial parts of the conductors (2824.). Two balls of iron, in a field of equal force, if retained in a plane at right angles to the line of force, *i. e.* with their equatorial parts in juxtaposition, separate from each other with considerable power (2814.), and the probability is that two infinitely weaker bodies of the paramagnetic class would separate in like manner. Two portions of phosphorus, being a diamagnetic substance, have been found also to separate under the same circumstances (2815.).

2831. The motions here are of the same kind, whereas they might have been expected to be the reverse (2816.) of each other; still they are perfectly consistent. The diamagnetics ought to separate, for the field is stronger in lines of magnetic force between them than on the outsides, as may easily be seen by considering the two spheres D D in fig. 6; and therefore this motion is consistent, and is in accordance generally with the opening or set equatorially, either of separate portions or of a continuous mass of such substances (2829.), in their tendency

Fig. 6.



to go from stronger to weaker places of action. On the other hand, the two balls of iron, P P, have weaker lines of force between them than on the outside; and as their tendency is to pass from weaker to stronger places of action, they also separate to fulfil the requisite condition of equilibrium of forces. Finally, a paramagnetic and a diamagnetic body attract each other (2817.); and they ought to do so, for the diamagnetic body finds a place of weaker action towards the paramagnetic body, and the paramagnetic substance finds a place of stronger action in the vicinity of the diamagnetic body, D P, fig. 6.

2832. I have frequently spoken of iron in illustration of the action of paramagnetic conductors, and considered the polarity which it acquires as the same with that of these conductors; but I must now make clear a distinction, which exists in my mind, with regard to the polarity of a magnet, and the polarity, as I have called it, due to mere conduction. This distinction has an important influence in the case of iron. A permanent magnet has a polarity in itself, which is possessed also by its particles; and this polarity is essentially dependent upon the power which the magnet inherently possesses. It, as well as the power which produces it, is of such a nature, that we cannot conceive a mere space void of matter to possess either the one or the other, whatever form that space may be supposed to have, or however strong the lines of magnetic force passing across it. The polarity of a conductor is not necessarily of this kind; is not due to a determinate arrangement of the cause or source of the magnetic action, which in its turn overrules and determines the special direction of the lines of force (2807.); but is simply a consequence of the condensation or expansion of these lines of force, as the substance under consideration is more or less fitted to convey their influence onwards. It is evidently a very different thing to originate such lines of power and *determine their direction* on the one hand, and only to assist or retard their progression without any reference to their direction on the other. Speaking figuratively, the difference may be compared to that of a voltaic battery and the conducting wires, or substances, which connect its extremities. The stream of force passes through both, but it is the battery which originates it, and also determines its direction; the wire is only a better or worse conductor, however by variation of form or quality it may diffuse, condense, or vary the stream of power.

2833. If this distinction be admitted, we have to consider whether iron, when under the influence of lines of magnetic power, becomes a magnet and has its proper polarity, or is a mere paramagnetic conductor with conducting powers of the highest possible degree. In the first place, it would have the real polarity of the magnet, in the second only that which I assign to oxygen and other conducting bodies. To my mind the iron is a magnet. It can be raised as a *source* of lines of magnetic power to an extreme degree of energy in the electro-magnet; and though, when very soft, it usually loses nearly all this power upon the cessation of the electric current, yet such is not the case if the mass of metal forms a continuous circuit or ring, for then it can retain the force for hours and weeks together, and is evidently for the time an original source of power independent of any voltaic current. Hence I think that the iron under the influence of lines of magnetic power becomes a magnet; and though it then has the same kind of polarity, as to direction, as a mere paramagnetic conductor, subject to the same lines of force, still with a great difference; for as the internal particles of iron become in a degree each a system producing magnetism, so their polarity is correlated and combined together into a polar whole, which, being infinitely more intense, may also be very different in the disposition of its force in different parts, to that equivalent to polarity, which a mere conductor possesses.

2834. It appears to me also as very probable, that when iron, nickel and cobalt, are heated up to the respective temperatures at which they lose their wonderful degree of power (2347.) and retain only so small a portion of it as to require the most sensible test to make it manifest (2343.), they then have passed into the condition of paramagnetic conductors, have lost all ability to acquire that state of internal polarity they could assume as magnets, and now have no other polarity than that which belongs to them as masses of paramagnetic matter (2819.). It is also probable that in many states of combination these metals may take up the mere conducting state; for instance, that whilst in the protoxide, iron may constitute a magnet, in the peroxide it is only a conductor; and in this respect it is not a little curious to find oxygen, which as a gas is a paramagnetic body (2782.), reducing iron down to, and indeed far below its own condition, weight for weight. In their various salts also and solutions, these metals may, in conjunction with the combined matter, be acting only as conductors.

2835. Perhaps I ought not to have called the condition of concentration or expansion of the lines of magnetic force in the bodies acting as conductors, a polarity; inasmuch as true magnetic polarity depends essentially and entirely on the *direction* of the line of force, and not on any mere compression or divergence of these lines. I have done so only that I might point with the more facility to facts and views that have heretofore been associated with some supposed polarity in the bodies which, whether paramagnetic or diamagnetic, I have been considering as mere conductors, and I hope that no mistake of my meaning will arise in consequence. I have already asked for such liberty in the use of phrases (lines of force, conducting power, &c.) (2149. 2797.) as may, for the time, set me free from the bondage of preconceived notions; these are, for that very reason, exceedingly useful, provided they are for the time sufficiently restricted in their meaning, and do not admit of any hurtful looseness or inaccuracy in the representation of facts.

¶ iii. *Magnecrystallic conduction*[1].

2836. The beautiful researches of Plücker in relation to magneoptic phænomena cannot have been forgotten, and I hope that my own experiments on magnecrystallic results (2454, &c.) are

[1] I must refer here to the important paper by MM. Tyndall and H. Knoblauch on this subject in the Philosophical Magazine, 1850, vol. xxxvii. p. 1. M. F.—January 6, 1851.

remembered in conjunction with his; the phænomena described by us are, as I believe, due to a common cause, and are the same in kind; and as far as they are presented by pure transparent bodies, are I think brought by Plücker into a proper relation to the positive and negative optic axis of such bodies[1]. In these cases a crystalline body sets powerfully, or takes up a particular position when placed in a field of magnetic force (2464. 2479. 2550.), without reference to its paramagnetic or diamagnetic character (2562.), and also without assuming any state which it can on its removal bring away with it (2504.).

2837. If the idea of conduction be applied to these magnecrystallic bodies, it would seem to satisfy all that requires explanation in their special results. A magnecrystallic substance would then be one which in the crystallized state could conduct onwards, or permit the exertion of the magnetic force with more facility in one direction than another; and that direction would be the magnecrystallic axis. Hence, when in the magnetic field, the magnecrystallic axis would be urged into a position coincident with the magnetic axis, by a force correspondent to that difference, just as if two different bodies were taken, when the one with the greater conducting power displaces that which is weaker.

2838. The effect of position would thus be accounted for (2586.); and also the greater aptness for magnetic conduction in one direction than in another (2588. 2591.): and, what appeared to me as an anomaly in the supposition, that a line of force could have reference indifferently to any part of a plane (2600.) disappears. That heat should take away this conducting power (2570.) seemed perfectly consistent with what we know of the effect of heat on the magnetic condition of iron, oxygen, &c., and also upon the conducting power for electricity in such cases as platina, sulphuret of silver, &c. Finally, the assumption did not appear inconsistent with the state which the body seems to assume for the time during which it is under the magnetic force (2609. &c.).

2839. But if such a view were correct, it would appear to follow that a diamagnetic body like bismuth ought to be less diamagnetic when its magnecrystallic axis is parallel (as nearly as may be) to the magnetic axis, than when it is perpendicular

[1] Philosophical Magazine, 1849, vol. xxxiv. p. 450.

to it. In the two positions it should be equivalent to two substances having different conducting powers for magnetism, and therefore, if submitted to the differential balance, ought to present differential phænomena, corresponding in kind to those of oxygen and nitrogen (2774.), or phosphorus and bismuth, or any other two differing bodies. Though I have given certain results on a former occasion which seemed to bear on this point (2551. 2552. 2553.), they are not satisfactory in the present state of our knowledge, because the difference, if any, would be small (2552.), and quickly hidden by the employment of a single pointed pole. Other experiments, formerly described (2551–2561.), would not show a small difference in diamagnetic force (though quite fitted for their intended purpose), because they were made with flat-faced poles, and a field nearly equal in magnetic power.

2840. The differential torsion balance (2773.) enabled me to return to this matter with better hopes of success. A consistent group of bismuth crystals was selected (2457.) and hung up on one side of the double cone core (2738.), whilst a cylinder of flint-glass was opposed to it on the other. The flint-glass was to be a standard of reference, and therefore neither its place on the balance nor condition was altered during the experiment. The bismuth group was placed with its magnecrystallic axis horizontal, and so that it could be turned in a horizontal plane, that the axis might be at one time parallel to the magnetic axis (or lines of force), and at other times perpendicular to it, but without any alteration of the distance of its centre of gravity from the opposed glass cylinder. Hence, having either one position or the other, it could still be compared with the cylinder.

2841. The magnecrystallic axis was first made parallel to the core or magnetic axis, the magnetic power developed, and when the diamagnetic bodies had taken their position of rest or stable equilibrium, the place of the balance lever was observed and recorded by means of a ray of light reflected from a mirror attached to it. Then the bismuth was turned through 90°, or until its magnecrystallic axis was perpendicular to the axis of the double cone core; and now, when the magnet was excited, the place of the bismuth was found to be further out from the core than before. On being turned through 90° more, so as to be in a position diametral to the first (2461.), its place was again a little nearer to the magnet; and when in the fourth position, which is diametral to the second, then it was further out. Thus the crystallized bismuth proved to be diamagnetic in different degrees, according with certain directions of its magnecrystallic axis, being more diamagnetic when this axis was perpendicular or transverse to the lines of magnetic force, than when it was parallel to them; and thus the expectation founded upon theoretical considerations (2839.) was confirmed.

2842. I tried to obtain similar results with a cube of calcareous spar (2597.); for it is evident that if its optic axis, being in a horizontal plane, is first placed parallel to the magnetic axis and then perpendicular to it, the body ought to be more diamagnetic in the first position than in the second, inasmuch as the latter is the position which it takes up under the influence of its magnecrystallic or magneoptic condition. I could not however obtain any distinct results, partly because its power is in all respects very inferior to the bismuth, partly because of the present imperfection of my torsion balance, and partly because of the size and shape of the calcareous spar. A sphere or a cylinder, having the optic axis perpendicular to the axis of the cylinder, would be more correct as forms of the substances to be tried.

2843. In concluding this part of the subject relating to the magnetic conducting power, I will now refer to some of the cases which I think experimentally establish its *existence* in the two subdivisions of magnetic bodies (2805.). The place and position of iron in a field of equal force (2810. 2811.) is no doubt a result of the extraordinary power which this body has of transmitting the magnetic force across the space which it occupies, whether the particles of the iron be considered as polar or not (2832.), and therefore I accept the converse phænomena as to place and position of a diamagnetic body (2812. 2813.) as proof that it has less power of transmitting the magnetic force than the space it occupies, and from that conclude that it conducts diamagnetically (2802.).

2844. The separation of paramagnetic bodies in the equatorial direction is a proof of the manner in which, by their better conduction, they disturb the position of the lines of force in the medium around them (2831.). The separation of two diamagnetic bodies, under the same circumstances, is an equal proof of the manner in which, by difference of conducting power, they also disturb the disposition of the force (2831.). The equatorial attraction of a paramagnetic and a diamagnetic body for each other, when they are in a medium, which in conducting power is between the two (2831.), is a proof not only of conduction in both, but also of their reverse condition in respect of each other and the medium.

2845. The place of a crystal of bismuth, either nearer to or further from the magnetic axis (2841.), according as its magnecrystallic axis is parallel or perpendicular to the axial line, is also a case of the difference of conducting power, and therefore of the possession of that power by the diamagnetic body. Many other cases might be quoted in illustration of the existence of that power which I assumed as conducting power (2797.), and which probably nobody may be inclined to deny. I will suppose that the above are enough to explain my meaning.

2846. It is hardly necessary for me to say that magnetic conduction does not mean electro-conduction, or anything like it. The very best electro-conductors, as silver, gold and copper, are below mere space in their ability to favour the transmission of magnetic force, so deficient are they in what I have called magnetic conduction. There is a striking analogy between this conduction of magnetic force and what I formerly called specific inductive capacity (1252. &c.) in relation to static electricity, which I hope will lead to further development of the manner in which lines of power are affected in bodies, and in part transmitted by them.

§ 33. *Atmospheric magnetism*[1]*.

¶ 1. *General principles.*

2847. It is to me an impossible thing to perceive, that two-ninths of the atmosphere, by weight, is a highly magnetic body,

[1] A most important paper by Professor Christie, "On the Theory of the Diurnal Variation of the Magnetic Needle," appears in the Philosophical Transactions for 1827, p. 308. Led by the discoveries of Seebeck in thermo-magnetism and the experiments of Cumming, he was induced to search how

* I ought to refer the readers of my paper to a theory of the *cause* of the daily variations by M. A. de la Rive, founded upon the idea of thermo-electric currents in the atmosphere and earth; it will be found in a memoir

subject to great changes in its magnetic character, by variations in its physical conditions of temperature and condensation or rarefaction (2780.), and at the same time subject to these phy-

far the idea of thermo-currents or thermo-magnetic polarity would apply to the natural phænomena, and concludes (p. 327), that, admitting that the *earth and the atmosphere* are substances in which such action can under any circumstances take place, these experiments would indicate that *any portion of the earth bounded by parallel planes with the atmosphere surrounding it, would become similarly polarized if one part were more heated than another.* Thus considering alone *the equatorial regions of the earth*, we should have *two magnetic poles on the northern side, and on the southern side two poles similarly posited*; *the poles of different names being opposed to each other on the contrary sides of the equator.*

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entitled 'On the Diurnal Variation of the Magnetic Needle.' Annales de Chimie, 1849, xxv. p. 310.

A friend has recently called my attention to an observation by M. E. Becquerel, which has reference to the present subject, and is in the following words. "If we reflect that the earth is encompassed by a mass of air, equivalent in weight to a layer of mercury of 30 inches, we may inquire whether such a mass of magnetic gas, continually agitated and submitted to the regular and irregular variations of pressure and temperature, does not intervene in some of the phænomena dependent on terrestrial magnetism. If we calculate in fact what is the magnetic force of this fluid mass, we find that it is equivalent to an immense plate of iron, of a thickness a little more than $\frac{1}{10}$th of a millimetre of diameter (?), and which covers the whole surface of the globe." This passage is at pp. 341, 342 of vol. xxviii. Annales de Chimie, 1850, being contained in an excellent memoir, in which the author has well worked out those differential actions of different media, which I developed generally *five* years ago:—Experimental Researches, 2357. 2361. 2406. 2414. 2423. &c. By such means he has rediscovered the magnetic character of oxygen and taken measurements of its force, being evidently unacquainted with the account that I gave of this substance in relation to nitrogen and other gases *three* years ago, in a letter published in the Philosophical Magazine for 1847, vol. xxxi. p. 401, and also in Poggendorff's Annalen and elsewhere;—hence the observations above. I cannot wonder at this, for I myself was not aware of M. E. Becquerel's paper until very lately. In my letter of 1847, I speak of oxygen as being magnetic in common air, p. 410; in carbonic acid, p. 414; in coal gas, p. 415; in hydrogen, p. 415, its power then being equal to its gravity. I say that air owes its place to the oxygen and nitrogen in it, p. 416, and tried to separate these constituents by attracting the oxygen and repelling the nitrogen. At the end of the paper I hesitate in deciding where the true zero between magnetic and diamagnetic bodies is to be placed, and refer to the atmosphere as being liable to affections under the magnetic influence of the earth. It was these old results which led me on to the present researches, M. F.—Nov. 28, 1850.

sical changes in a high degree, by annual and diurnal variations, in its relation to the sun, without being persuaded that it must have much to do with the disposition of the magnetic forces upon the surface of the earth (2796.), and may perhaps account for a large part of the annual, diurnal and irregular variations, for short periods, which are found to occur in relation to that power. I cannot pretend to discuss this great question with much understanding, seeing that I have very little of that special knowledge which has been accumulated by the exertions of the great and distinguished labourers, Humboldt, Hansteen, Arago, Gauss, Sabine, and many others, who have wrought so zealously at terrestrial magnetism over the surface of the whole earth. But as it has fallen to my lot to introduce certain fundamental physical facts, and as I have naturally thought much upon the general principles which tend to establish their relation to the magnetic actions of the atmosphere, I may be allowed to state these principles as well as I can, that others may be placed in possession of the subject. If the principles are right, they will soon find their special application to magnetic phænomena as they occur at various parts of the globe.

2848. The earth presents us with a spheroidal body, which, consisting of both paramagnetic and diamagnetic substances, disposed with much irregularity as regards its large divisions of earth and ocean, are also equally irregularly disposed and intermingled in its smaller portions. Nevertheless it is, on the whole, a magnet, and, as far as we at this moment are concerned, an original source of that power. And though we cannot conceive at present that all the particles of the earth contribute, as sources, to its magnetism, inasmuch as many of them are diamagnetic, and many non-conductors of electric currents, yet it is difficult to say that any large portion is not concerned in the production of the force; hereafter it may be necessary, perhaps, to consider certain parts as mere conductors, *i. e.* as parts merely permeated by the lines of force, originating elsewhere, but for the present the whole may be assumed, according to the theory of Gauss, as a mighty compound magnet.

2849. The magnetic force of this great system is disposed with a certain degree of regularity. We have the opportunity of recognising it only as it is exhibited in one shell or surface, which, being very irregular in form, is always the same to us, for we rarely, if ever, pass out of it; or if we do, as in a balloon, only to an insensible extent. This is the surface of the earth and water of our planet. The magnetic lines of force which pass in or across *this surface* are made known to us, as respects their direction and intensity, by their action on small standard magnets; but their average course or their temporary variations *below or above*, i. e. in the air above, or the earth beneath, are only dimly indicated by variations of the force at the surface of the earth, and these variations are so limited in their information, that they do not tell us whether the cause is above or below.

2850. The lines of force issue from the earth in the northern and southern parts with different but corresponding degrees of inclination, and incline to and coalesce with each other over the equatorial parts. Their general disposition is represented by the system, which emanates from a globe having within one or two short magnets adjusted in relation to the axis. There seems reason to believe, from the analogy of such globes to the earth, that the lines of magnetic force which proceed from the earth return to it; but in their circuitous course they may extend through space to a distance of many diameters of the earth, to tens of thousands of miles. Messrs. Gay-Lussac and Biot, in their ascent in a balloon, perceived some indication of a diminution in the intensity of the magnetic force at a height of about four miles from the surface; but we shall shortly perceive that they might be at the time in the midst of influences sufficient to account for all the effect, so that none of it might be occasioned by removal from the earth as a magnet. The increase of the intensity of the magnetic force, as we proceed from the equator towards the poles, accords with the idea of the enormous extension of this power.

2851. These lines proceed through space with a certain degree of facility, of which a general idea may be gained from ordinary knowledge, or from experiments and observations formerly made (2787.). Whether there are any circumstances which can affect their passage through mere space, and so cause variations in their condition; whether variations in what has been called the temperature of space could, if they occurred, alter its power of transmitting the magnetic influence, are questions which cannot be answered at present, although the latter does not seem to be entirely beyond the reach of experiment.

2852. This space forms the great abyss into which such lines of force as we are able to take cognizance of by our observing instruments, which issue from the earth, proceed, at least at all parts of the globe where there is a sensible dip. But, as it were, between the earth and this space, there is interposed the atmosphere; which, however considerable we may estimate it in height, is so small when compared to the size of the earth, or to the extent of space beyond it into which the lines of force pass, that the idea of its being a changeable, active something interposed *between* two systems far more extensive and steady in their nature and condition, will not lead to any serious error. It is at the bottom of this atmosphere that we live and make all our inquiries, whether by observation or experiment.

2853. The atmosphere consists, as far as we are concerned at present, of four volumes of nitrogen and one volume of oxygen, or by weight, of three and a half parts of the former and one part of the latter. These substances are nearly uniformly mixed throughout, so that, as regards their manner of investing the earth, they act magnetically as a single medium; nor does there seem to be any tendency in the terrestrial magnetic forces to cause their separation[1], though they differ very strikingly in their constitution as regards this power.

2854. *The nitrogen* of the air does not appear to be either paramagnetic or diamagnetic; if removed from zero, in either of these respects, it is only to a very small extent (2783. 2784.). Whether dense or rare, it has apparently the same relation to and equality with space, as far as the present means of observation have proceeded. As respects the other element of change, namely, temperature, I concluded, from former imperfect experiments[2], that nitrogen became more diamagnetic when heated than before; but as it was then mixed with the oxygen of the air, and the results were mingled together, I have, for the purposes of the present research, repeated the experiments far more carefully.

2855. A small helix of platinum wire, fixed at the end of thicker copper wires, could be placed in any position beneath the poles of the great electro-magnet, and being ignited by a voltaic battery, served to raise the temperature of the gas around it. The magnetic poles were raised; were terminated by hemi-

1 Philosophical Magazine, 1847, vol. xxxi. p. 416. 2 Ibid. p. 418.

spheres of soft iron 0·76 of an inch in diameter and 0·2 of an inch apart; and were covered by a glass shade, resting upon a thick flat bed of vulcanized caoutchouc. A tube passed through the bed, rising up to the top of the shade, by which any required gas could be introduced. A very thin plate of mica, about 3 inches square, was covered with an attenuated coat of wax on the upper side, and fixed horizontally over the magnetic poles within the shade. The small platinum helix was so placed as to be beneath the space, between the poles, and a little on one side of the axial line, so that a current of hot air rising upwards from it, could pass to the mica plate, and by melting the wax show where it came against the mica.

2856. All acted exceedingly well, *air* being in the glass shade. When there was no magnetic power on, the hot air from the ignited helix rose perpendicularly, and melted a neat round portion of the wax, showing the place of the current under natural circumstances; but when the magnet was thrown into action, then the wax on the mica remained unchanged, the hot air being thrown so far away from the axial line, and so cooled by its forcible mixture with the neighbouring air, as to be unable to melt a spot of wax anywhere. The moment the magnetic power was suspended, the column of hot air rose vertically and regained its original position.

2857. *Carbonic acid gas* was then sent into the shade, until twice as much as the contents of the shade had passed through the pipe (2855.); but as it was heavy and the common air could make its way out only at the bottom of the shade, there was no doubt air mixed with the carbonic acid, which at last remained about the poles. The platinum coil being now heated, the column of hot gas rose vertically, as before. On putting on the magnetic force it was deflected from the axial line, passing equatorially, and melted the wax about half an inch off from the former place. Believing that even this effect might be due to the air mingled with the gas, other two volumes of carbonic acid gas were directed into and through the vessel. After this the magnetic force caused much less deflection of the rising column. Two volumes more of carbonic acid were sent through, and now the hot current of gas rose so nearly vertical that there was scarcely any sensible difference of its place when the magnetic power was in full action, or when it was entirely absent.

Hence I conclude that carbonic acid gas is very little affected in its diamagnetic relations by a difference of temperature equal to that between natural temperatures and a full red heat.

2858. *Nitrogen.*—This gas was prepared by passing common air slowly over burning phosphorus, and after being washed for twelve or fourteen hours, was sent into the shade so as to displace the carbonic acid. As it was lighter than the latter, it performed that service very well, and the portion remaining in the vessel probably contained no other oxygen or air than that it carried in with it. This nitrogen being then heated by the platina coil, was almost as indifferent to the magnet as the carbonic acid. The heated column rose (nearly) to the same spot against the mica, whether the magnetic power was active or not. It went outwards or equatorially a very small degree when the magnet was active, but this I attributed to a little oxygen still left with the nitrogen; and indeed nitric oxide gas shows oxygen in nitrogen so prepared. The platina coil was raised to as high a temperature as it could well support without fusion, and yet there was only this small effect sensible; hence I conclude that hot nitrogen is not more diamagnetic than cold nitrogen, and that indeed its magnetic relation is noways affected by such change of temperature.

2859. I raised the French shade (2855.) an inch for a moment, and then instantly placed it down again; and now, on making the magnet active and the coil hot, there was so much effect of dispersion of the gas within, that the melted spot of wax appeared nearly an inch outside of the standard place, yet only a very small portion of air or of oxygen could have entered the vessel under these circumstances.

2860. The nitrogen of the air is therefore, as regards the magnetic force, a very indifferent body; it does not appear to be either paramagnetic or diamagnetic; neither does it present any difference in its relation, whether it be dense or rare, or at high or low temperatures. I formerly found that the diamagnetic metals, when heated, did not seem to change in their relation to the magnet (2397.), and this now appears to be the case with such neutral or diamagnetic bodies as nitrogen and carbonic acid gases.

2861. *The oxygen* of the air differs in a most extraordinary degree from the nitrogen. It is highly paramagnetic, being, bulk for bulk, equivalent to a solution of protosulphate of iron, containing, of the crystallized salt, seventeen times the weight of the oxygen (2794.). It becomes less paramagnetic, volume for volume (2780.), as it is rarefied, and apparently in the simple proportion of its rarefaction, the temperature remaining the same. When its temperature is *raised*, the expansion consequent thereon being permitted[1], it loses very greatly of its paramagnetic force; and there is sufficient reason, from a former result with air[2], to conclude, that when its temperature is lowered its paramagnetic condition is exalted. How much its paramagnetic intensity might be increased by lowering it to the temperature of freezing mercury, as at the north or south poles of the earth, we cannot at present tell. Though a gas, it is apparently like the solid metals, iron, nickel or cobalt, when they are within the range of temperature which affects their magnetic forces; and it may, perhaps, like them, rise by cooling to a very high state.

2862. These relations it preserves when mingled with nitrogen in the air, as long as its physical and chemical conditions remain unchanged; but it is not irrelevant to remark, that every operation by which this active part of the atmosphere changes in its nature and passes into combinations, takes away its paramagnetic powers, whether the result be solid, liquid or gaseous.

2863. Hence the atmosphere is, in common phrase, a highly magnetic medium. The air that stands upon every square foot of surface on the earth, is equivalent, in magnetic force, to 8160 lbs. of crystallized protosulphate of iron (2794. 2861.). This medium is, by every change in its density, whether of the kind indicated by the barometer, or caused by the presence or absence of the sun, changed in its magnetic relations. Further, every variation of temperature produces apparently its own change of force, in addition to that caused by the mere expansion or contraction in volume, and none of these alterations can happen without affecting the magnetic force emanating from the earth, and causing variations, both in its intensity and direction, at the earth's surface. Whether these changes are in the right direction and sufficient in quantity to supply a cause for the variations of the terrestrial magnetic power, is the point now to be considered, for the illustration of which I will endeavour to construct

[1] Philosophical Magazine, 1847, vol. xxxi. p. 417.
[2] Ibid. p. 406.

a type case, and then apply it, as well as I can, to the natural facts.

2864. Let us assume the existence of two globes of air distinct from the surrounding atmosphere, by a difference of temperature or by a difference of density: the assumption is not too extravagant for an illustration, since Prout showed that there were masses of air, larger or smaller, floating about in the atmosphere, and singularly distinct from the surrounding parts, by temperature and other circumstances. Not to complicate the expression, we will leave out of view, at present, the attenuation upwards, and will consider one of these globes as *colder* or denser than the contiguous parts, and that it is in a portion of space which without it would present a field of equal magnetic force, *i. e.* having parallel lines of equal intensity of force passing across it.

2865. The air of such a globe will *facilitate* the transmission of the magnetic force through the space which it occupies (2807.), making it superior, in that respect, to the surrounding atmosphere or space, and therefore more lines of magnetic force will pass through it than elsewhere (2809.). The disposition of these lines, in respect of the line of the dip of the place, will be something like what is represented in fig. 7 (2874.), and consequently the globe will be polarized as a conductor (2821. 2822.) of the paramagnetic class. Hence the intensity of the magnetic force and its direction will vary, not only within but without the globe, and these will vary in opposite directions, in different places, under the influence of laws which are perfectly regular and well known.

2866. First, as regards the *intensity*, which before was uniform (2864.). If the intensity is to be considered as expressing the amount of force which passes through any given place, then, in consequence of the definite amount of power which belongs to any section, as *a a*, of a given amount of lines of magnetic force (2809.), a concentration of these lines towards the middle, P, will cause an increase of intensity at that part, and a diminution at some other parts, as *b b*, from whence the influence of the power has been partly removed. Hence, supposing the normal condition to exist at *a*, if a test of intensity were carried from *a* to P, it would gradually enter parts *b* and *c*, in which the intensity was less than the normal condition, and these might be either without or within the globe P, or both (according to its temperature relative to the surrounding air, its size and other circumstances); it would then arrive at parts having the normal intensity; and lastly, at parts, P, having an intensity greater than the surrounding space; as it went outwards, on the opposite side of P, corresponding variations would occur in the reverse order.

2867. On transporting the test upwards, in the direction of the dip from *e*, where the intensity may be considered as normal, it would gradually occupy positions at *f g*, &c., in which the intensity would increase until it arrived at P, after which it would pass through places of less and less intensity, until at *p* it would again find the force in the normal state. If the test, in being carried upwards, be not taken along *the line of the dip*, then it will of course pass through variations like those described on the line *a* P, growing more and more in extent until the direction coincides with the line *a* P, which is at right angles to the dip and where they are at a maximum. Hence, to pass upwards through such a globe of cold air in our latitude, where the dip is 70° nearly, and at the equator, where it is 0°, would be a very different matter, and the necessary natural results of such a difference ought to appear hereafter.

2868. But a magnetic needle or bar is not a test of such intensity, *i. e.* it will not tell these differences, or it may tell them in a contrary direction. To understand this point, we have to consider that a needle vibrates by gathering upon itself, because of its magnetic condition and polarity, a certain amount of the lines of force, which would otherwise traverse the space about it; and assuming that it underwent no change by change of temperature, it would be affected in proportion to any variations in the intensity of these lines, provided everything else remained the same. But being under natural circumstances surrounded by the atmosphere, which is a medium liable to variation in its magnetic condition, both by heat and rarefaction, and by these variations affects the intensity or quantity of the force, it will vary in its indications by variations in these conditions. Thus, for instance, if it were in a large sphere of oxygen, I expect that it would, by its number of vibrations or otherwise, indicate a certain intensity; if the oxygen were expanded, that it would indicate a higher intensity, although the same amount of lines of force and magnetic energy were passing through the oxygen as before. If the oxygen were made dense, then becoming a better conductor, I presume it would convey onwards *more* of the force and the magnet *less*, for the power would be partly transferred from the unchanging magnet to the improving conductor around it.

2869. These experiments can hardly be made with oxygen except by means of extremely delicate apparatus, but like effects are easily shown experimentally in selected analogous cases. Thus let a thin small tube of flint-glass, about 1 inch long and $\frac{1}{2}$ an inch in diameter, be filled with a saturated solution of protosulphate of iron, and suspended horizontally by cocoon-silk (2279.) between the poles of the electro-magnet, in a vessel which may either contain air or water, or other media (2406.). In air it will point axially, and will be analogous to a needle under the earth's influence, and it will point with a certain amount of force. Fill the vessel with water, and now it will point with more force than before, though the water is a worse magnetic conductor than the air which was previously there; and it is precisely because the water *is* a worse conductor that the liquid magnet or test indicates more power. Increase the conducting power of the surrounding medium by adding sulphate of iron to it, and the indication of strength by the tube goes on diminishing, first returning to the degree of power it had in air, and then descending to lower gradations, for it returns with less and less force to its axial position when disturbed from it. So the magnetic needle employed for measuring intensity or magnetic force (for the same meaning is at present understood by the two terms), indicates, in a certain manner, the power thrown upon itself, and, I conclude, accurately, provided the condition of the surrounding medium remains magnetically unchanged; but if it be placed in different media or in an altering medium, I expect that it will not measure accurately the intensity in them, *i. e.* it will not measure directly the amount of force passing relatively through them. The difference in air under different conditions would be very small, still it is that difference which concerns us in *atmospheric magnetism*; and it is very important to know, whether, when the magnet indicates an increased intensity of force, it is altogether due to a real increase of the amount of the power at its source as it comes to us from the earth, or in part to a change in the magnetic constitution of the space around the magnet hitherto unknown to us.

2870. If what is now often indifferently called magnetic force or intensity have its results distinguished as of two kinds, namely, those of *quantity* and those of *tension*, then we shall more readily comprehend this matter. At present a needle shows both these as magnetic force, making no distinction between them, yet they produce effects on it often in opposite directions; for as they increase or diminish they both affect the needle alike; but as it is assumed that the tension can change whilst the quantity remains the same, and the quantity can be altered, yet the tension remain unaffected, the result by the needle will then be uncertain. If the tension in a given region be increased by diminishing the conducting power, the needle will show *increased force*; if it be increased by an increase of magnetic power in the earth from some internal action, the needle will still show *increased force*, and will not distinguish the one effect from the other. If the quantity in a region be increased by increasing the conducting power, the needle will show no such increase; on the contrary, it will indicate *diminution* of force, because the tension is diminished; or if the quantity be diminished by diminishing the conducting power, it will show *increased* force. The force might even lose in quantity and gain in tension in such proportions that the needle should show no change; or it might gain in quantity and lose in tension, and the needle still be entirely indifferent to the whole result.

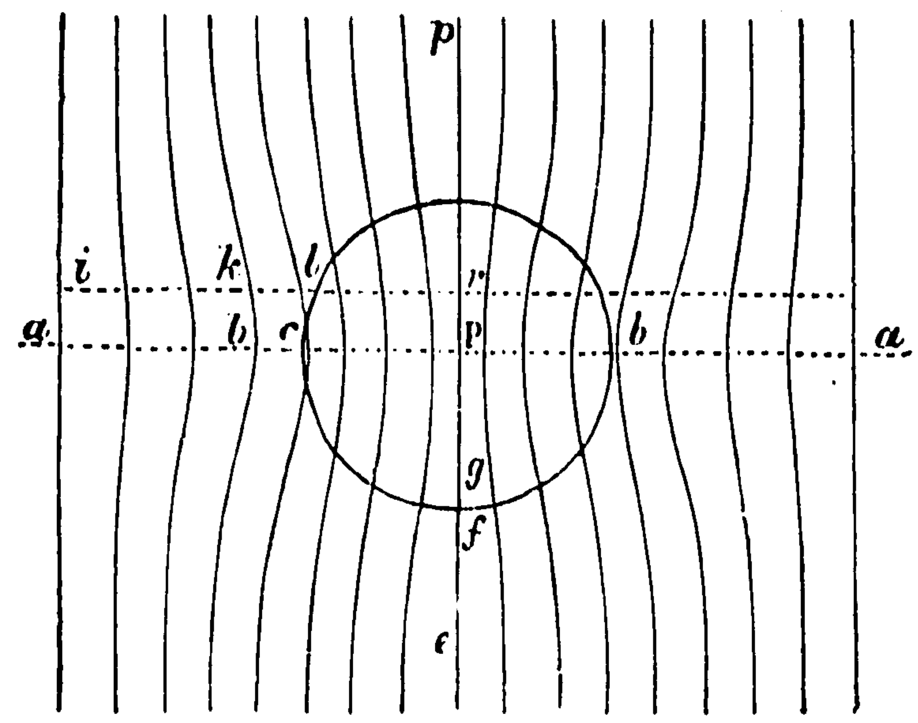
2871. If my view be correct, then the magnet is not, as at present applied, a perfect measure of the earth's magnetic force; for that may not change when the magnet by the influence of the different conditions of day and night, or of summer and winter, may show a difference. How *far* these uncertainties in its indication may affect the value of the observations made on the horizontal and vertical components of the earth's magnetic force as indications of that which they are expected to tell us, I do not know; but involving, as the effects do, two very different conditions, namely, variation of the conducting power and variation of the amount of force at its source, the one of which is chiefly in the atmosphere and the other in the earth, it seems to me to be of great consequence to the development of the theory of terrestrial magnetism, to have some method, if possible, of distinguishing these two points or effects from each other.

2872. Referring again to the model globe, fig. 7 (2874.), it appears to me, that if a magnet be used as the intensity test, it will indicate a less intensity at P rather than a greater one, for the very reason that the conducting power of the whole globe has been increased; and also, though the apparent diminution of intensity will probably be greater there than elsewhere, that the effect will occur in other parts, especially those on the right and left, and even at *b* and *b*, where the power transmitted, instead of being more, as at P, is really less than the portion transmitted in the normal or equable state of the magnetic field. With a diamagnetic globe of air, *i. e.* one warmer or more rarefied than the surrounding space (2877.), though it would convey less power as being a worse conductor, still it should cause the magnet to set with greater force, and so give an indication of increased intensity, and that also both within and equatorially without the globe.

2873. If it be true that the changes of the medium (2869.) can thus affect the magnet, and that such changes can rise up to a sensible degree in the gases, then a magnet might make a different number of vibrations in a given time in oxygen and nitrogen gases of the same density, for they are very different in their magnetic relations. It should make the greatest number in nitrogen; perhaps a delicate torsion balance would be a still more sensible test of such a result; but it is probable that the space around the needle should be large, and it would be requisite to ascertain that the two media opposed equal mechanical resistance to the vibrating needle.

2874. The variation of the *direction* caused by the typical globe (2864.) might be oblique to the horizontal and vertical planes, and consequently give results of declination and inclination, either separately or together. The direction would not vary in a central line parallel to the general dip of the surrounding space (fig. 7). Along another central line perpendicular to this (*i. e.* any line in the equatorial plane), *a* P, there would also be no variation of the direction, but in any other position there would be variations. Thus in the line *i r*, as the free needle passed from *i* to *k*, its lower end would be carried inwards towards the central line of dip P; this effect, after attaining a maximum, perhaps at *l*, would gradually diminish again, and by the time the needle had reached *r* the dip would be normal.

Fig. 7



Corresponding effects would occur on the opposite side of the axial line *p e*; and if a needle be considered as in any place the dip of which is thus affected, and then be conceived as travelling in a circle round the axial line *p e*, it would always be in the surface of a cone, the apex of which is below.

2875. On the other hand, if the variations of the dip below the equatorial plane *a* P be considered, they will be equal in amount, but in the reverse direction, so that the magnetic needle, when deflected from its normal position, would have its upper end inclined inwards towards the axial line *p e*; or if moved round the axial line would always be in a conical surface, the apex of which is above.

2876. So the dip would vary in such a globe of air in every azimuth; and it would also vary in opposite directions in the upper and lower parts of the globe, and of the affected surrounding space.

Fig. 8.

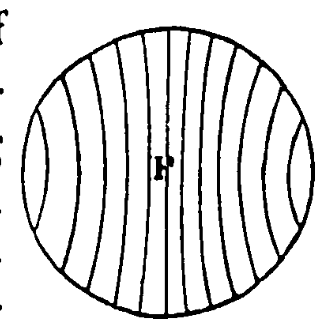
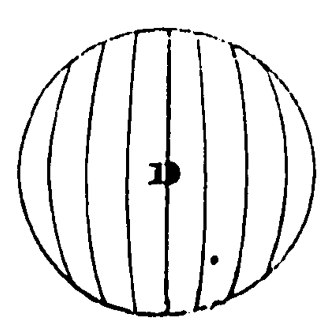


Fig. 9.



2877. If we assume the existence of another typical globe of air (2864.), having a higher temperature than the surrounding atmosphere, then its condition will be that of a diamagnetic conductor, and will be represented by fig. 9 (2807.); and it will have power to affect both the intensity and the direction of the lines of force, in conformity with the action of the former globe, but in the contrary order. As regards the action of these globes, consequent upon the direction of the lines of force in and about them upon a needle coming within their influence, it may, in part, be represented by a magnet placed either in the direction of the needle for the cold globe, or in the reverse direction for the warm one; but as the lines of force of the combined system of the earth and such a magnet are very different in their arrangement to the lines of the earth affected by masses of warm or cold air having only conduction polarity (2820.), it would be too much to say that they correspond, or that the effects on the intensity or direction would be the same for similar distance from the centre of the globe of air and the representative magnet.

2878. In endeavouring to proceed, from these hypothetical and comparatively simple cases, which are given only to lead the mind on from the results of experiment to the supposed condition of matters as regards our atmosphere and the earth, we have to consider, that though there will be an effect, and though the intensity and direction of the magnetic force, upon the surface of the earth, must vary with changes of temperature and density of the atmosphere, still it will be in a manner very different from that represented by the typical globe of air, for the latter is a case which will never occur, though the variations of the natural case are almost infinite. Still the comparison holds in principle, and we may expect that as the sun leaves us on the west, some effect, correspondent to that of the approach of a body of cold air from the east, will be produced, which will increase and then diminish, and be followed by another series of effects as the sun rises again and brings warm air with him.

2879. The atmosphere diminishes in density upwards, and that diminution will affect the transmission of the magnetic force, but as far as it is constant, the effect produced by it will be constant too. The portion of the atmosphere which lies under the heating influence of the sun, as compared to its depth, will more resemble a slice of air wrapped round the earth than a globe. Still the inflection of the lines of force, both above and below this stratum, will occur, extending into space above and into the earth beneath (2848.), according to the known influence of magnetic power and its perfectly definite character (2809.). We are placed at the bottom of this layer of air, but as the atmosphere is denser there than higher up, and is also in many cases more affected there by changes of temperature, we are probably in a position where the inflections and variations due to the assumed causes exist in a considerable degree.

2880. There are innumerable circumstances that will break up, more or less, any general or average arrangement of the air temperature. For instance, the diversity of sea and land causes variations of temperature differently in different times of the year, and the extent to which this goes may be learned from the beautiful isothermal charts of Dove, now fortunately to be had in this country[1]. These variations may be expected to give, not merely differences in the regularity, direction and degree of magnetic variation; but because of vicinity, differences so large as to be manifold greater than the mean difference for a given short period, and they may also cause irregularities in the times of their occurrence.

2881. On considering the probable results of the magnetic action of the atmosphere, it appears to me that if the terrestrial magnetic force could be freed from all periodical and small perturbations, and its disposition ascertained for any given time, it might still include certain effects constituting a part of atmospheric magnetism. For instance, there is more air, by weight, over a given portion of the surface of the earth at latitudes from 24° to 34°, than there is either at higher latitudes or at the equator; and that should cause a difference from the disposition of the lines of force which would exist if there were equality in that respect, or if the atmosphere were away. Again, the temperature of the air is greater at the equatorial parts than in latitudes north or south of it; and as elevation of temperature diminishes the conducting power for magnetism, so the proportion of force passing through these parts ought to be less, and that passing through the colder parts greater, than if the temperature of the air were at the same mean degree over the whole surface of the globe, or than if the air were away. Again, there is a greater difference in range of temperature of the air at the

[1] Report of the British Association, 1848, Reports, p. 85.

equator as we rise upwards than in other parts, and hence the lower part is not so good a conductor proportionately to the upper part, or to space, as elsewhere, where the difference is not so great; the magnetic power, therefore, should be in some degree weakened there, the lines of force being diverted, more or less, from the warm air and thrown into other parts, as the cooler atmosphere and space above, or the earth beneath, according to the principles before explained (2808. 2821. 2877.).

2882. The result of *annual variation* that may be expected from the magnetic constitution and condition of the atmosphere seems to me to be of the following kind. Assuming that the axis of rotation of the earth was perpendicular to the plane of its orbit round the sun, and dismissing for the present other causes of magnetic variation than those due to the atmosphere, the two hemispheres of the earth, and the portions of air covering them, would be affected and warmed alike by the sun, or at least would come into a constant relative state, dependent upon the arrangement of land and water; and the lines of magnetic force having taken up their position under the influence of the great dominant causes, whatever they may be, would not be altered by any annual change due to the atmosphere, since the daily mean of the atmospheric effect in a given place would at all parts of the year be alike. Under such circumstances the intensity and direction of the magnetic forces might be considered constant, presuming no sensible change to take place by the difference in distance from the sun which would occur in different parts of the orbit; and, as regards the two magnetic hemispheres, each would be the equivalent of and equal to the other, and they may for the time be considered in their mean or normal state.

2883. But as the axis of the earth's rotation is inclined 23° 28′ to the plane of the ecliptic, the two hemispheres will become alternately warmer and colder than each other, and then a variation in the magnetic condition may arise. The air of the cooled hemisphere will conduct magnetic influence more freely than if in the mean state, and the lines of force passing through it will increase in amount, whilst in the other hemisphere the warmed air will conduct with less readiness than before, and the intensity will diminish. In addition to this effect of temperature, there ought to be another due to the increase of the ponderable portion of the air in the cooled hemisphere, consequent upon its contraction and the coincident expansion of the air in the warmer half, both of which circumstances tend to increase the variation in power of the two hemispheres from the normal state. Then as the earth rolls on in its annual journey, that which at one time was the cooler becomes the warmer hemisphere, and consequently in its turn sinks as far below the average magnetic intensity as it before had stood above it, whilst the other hemisphere changes its magnetic condition from less to more intense.

2884. As the sum of the magnetic forces which crop out from the earth wherever there is dip on one side of the magnetic equator must correspond to the sum of like force on the other side (2809.), so they would not become more intense in one hemisphere, or more feeble in the other, without a corresponding contraction on the one hand, and enlargement on the other. The line of no dip round the globe may therefore be expected to move alternately north and south every year, or some effect equivalent to that take place. The condition of the two hemispheres under this view may be conceived by supposing an annual undulation of the force to and fro between them, during which, though neither the character nor the general disposition of the power be altered, there is in our winter a concentration and increase of intensity in the northern parts coincident with a diffused and diminished intensity in the south, and in summer the reverse.

2885. In respect of *direction*, alterations may also be anticipated. In the first place, and assuming that the magnetic poles and the poles of the earth coincide, the dip would increase in the cooling hemisphere towards the middle and polar parts; but it ought to diminish towards the magnetic equator, to accord with the concentration of the hemisphere of stronger power and enlargement of the weaker one; whilst on the other hand the dip ought to diminish at the polar and middle parts of the warming hemisphere and increase towards the magnetic equator. The magnetic equator would shift a little north and south of its mean place during each year, simultaneously with the whole system of magnetic lines. But as the magnetic poles do not coincide with those of the earth, or with what may be called the poles of the changing temperatures, so a cause of difference in direction will here arise.

2886. Again, it may be that as oxygen is cooled, its paramagnetic power may increase in a more rapid proportion than that of the change of temperature, so that the chief alteration of the disposition of the earth's force may be in the extreme northern and southern parts; and in combination with the holding power of the earth (2907.) may even cause a change the reverse of that expected above in lower latitudes. If in our winter the lines of force were to close together in the polar parts and to open out in lower latitudes, the balance of magnetic force would just as well be sustained as if *all* the lines in our hemisphere were to be compressed and strengthened, and be compensated for by a corresponding change in the south. In the former case, each hemisphere would balance its own forces, in the latter they would be balanced against each other. There can, I think, be no doubt, that as far as the mass of the earth and the space above our atmosphere are unchangeable in relation to annual and diurnal variation, so far they would tend to restrain any variation which might depend only on the varying temperature and state of the air; holding as it were the two sides of the variations, the increase and diminution of intensity, or the right and left hand in change of direction, nearer together than they otherwise would be.

2887. Further, if it be supposed that the whole of a hemisphere *is affected at once* in the same direction by change of temperature, it will *not be affected alike, but differently in different latitudes*, because of the difference in amount of that change.

2888. The difference of land and water (2880.) will still further break up any expected uniformity of the general result, and cause that certain parts of the cooling hemisphere shall increase in power more in proportion than other parts; and when these parts lie on opposite sides of the magnetic meridian of any given place, they would probably have power to cause an alteration in the declination of the needle at that place.

2889. As the annual changes of temperature are less at the equator than in parts more north or south, so there, probably, little or no annual variation would occur: none indeed as regards the varying temperature or expansion of the air, but only that portion which is consequent upon the alternate changes of the parts on its opposite sides (2884.).

2890. Another effect, which may be considered as an annual variation, but which is connected with the diurnal change, may be expected. As the daily changes in temperature of the atmosphere, influential upon a given place in north or south medium latitudes, are greater in extent in summer than in winter, so the corresponding magnetic variations may be expected to vary also, being larger in the northern hemisphere, when the sun is on the north side of the equator, and less when he is present in the southern hemisphere, and producing like correspondent change there.

2891. From a most important investigation by Colonel Sabine[1], founded on the results of observations at Toronto and Hobarton, the facts appear to be that the magnetic intensity is greater in both hemispheres in those months which are winter in the northern hemisphere, and summer in the southern. Similar results are greatly wanted for other localities, and would show whether the different disposition of land and sea has anything to do with the question, or whether the results at Toronto and Hobarton are true exponents of hemispherical effects. Assuming Toronto and Hobarton as being such exponents, the dip in both hemispheres is greater (i. e. greater north dip at Toronto and south dip at Hobarton) in those months which are winter in the northern, and summer in the southern hemisphere. Whether there is any *annual* variation of the dip or total force in the equatorial parts of the globe is very important to determine. It would be well worth while to take up a station for the express purpose; the instruments are very simple, and the observations would require only a single observer. They are described in the paper referred to. Unfortunately such observations are not even made in Great Britain.

2892. The manner in which the diurnal variation may be produced or affected by the action of the sun on our atmosphere as the earth revolves in its beams, has been already generally referred to. The whole portion of atmosphere ex-

[1] On the means adopted for determining the Absolute Values, Secular Change, and Annual Variation of the Magnetic Force, Philosophical Transactions, 1850, p. 201.

posed to the sun receives power to refract the lines of magnetic force which traverse it, and the whole of that which covers the darker hemisphere assumes an equally altered, but contrary state relative to the mean condition of the air. It is as if the earth were inclosed within two enormous magnetic lenses competent to affect the direction of the lines of force passing through them.

2893. I have already said that the action of the atmosphere thus affected might in some degree be compared at night time to that of an enormous, diffuse, and very feeble ordinary magnet, having the position that it would naturally take according to the line of dip, passing over us from east to west, and including us for the time within its influence: in the daytime the action would be like that of the similar journey, not of a corresponding magnet reversed in direction, but of a corresponding globe of diamagnetic matter (2821.). Assuming the maximum heat and cold to occur at midday and midnight, we might expect that the maximum effects would also occur near those periods as regards the variations of intensity (2824. 2866.); for, other things being the same, the central parts of the heated and cooled masses are those where the difference of intensity should be greatest.

2894. It might be expected that this variation in the *intensity* would be greatest at those parts of the globe over which the sun passes vertically, or nearly so; but that may depend upon two circumstances at least; first, whether the difference in the day and night temperature is greater there than at other places, because the extent of the variation may be dependent in part upon that difference; and next, whether the amount of effect to be expected is the same for the same difference in number of degrees of temperature at every part of the scale (2886.). If the conducting power of oxygen (2800.) should be found by future experimental measurements (2960.) to increase in a greater proportion for a fall of a given number of degrees at lower temperature than at high ones (including the effect of contraction for that fall (2861.)), then it may be that parts more distant from the sun will be more affected than those under it; or if the contrary be the case, less affected than otherwise would be expected.

2895. With regard to the daily variations, as respects the *direction* of the lines of terrestrial magnetic force, or the inclination and declination of the magnetic needle, the principles of the changes that may be expected to occur have been already referred to (2879.); and it remains for me to compare these expectations with a few simple cases of observation, in such a general manner as will tend to show whether the *direction* of action is, both in theory and fact, the same; and whether there is any probability that the effect has been assigned to its true cause; for this purpose I will confine myself entirely at present to a part of the daily variation, namely, the effect of the sun and air as the luminary arrives at and passes over the meridian.

2896. Profiting by the last volume which has issued from the powerful mind and careful hands of Colonel Sabine[1], I will take the case of Hobarton. The observatory there is in latitude 42° 52′·5 south, and longitude 147° 27′·5 east of Greenwich. The absolute declination is 9° 60′·8 east, and the dip is 70° 39′ south. In order to have the place of the sun and the time of maximum and minimum temperatures at hand, I have transferred the mean temperature for January (summer) for seven years, 1841–48, and the mean temperature for June (winter) for the same period, corresponding to every hour in the day and night, from pp. lxxxiv. and cviii. to fig. 10, Plate I., where the middle series of numbers represents the hours, the line next below them a base line of temperature at 30° Fahr., and the two curves still lower down the mean hourly temperature for summer and winter. The short lines show generally the direction of the needle east or west of its mean position, the upper end being of course the north extremity. The positions about noon are distinguished by full lines, being those required for more immediate illustration.

2897. The north end of the magnetic needle at Hobarton is most east at 2 o'clock, and most west about 21 o'clock. Being at the extreme west at the latter hour, it passes through the full range of variation, or to the extreme east in five hours, or by 2 o'clock, and then requires the remaining nineteen hours to return to the utmost west. The maximum east and west declination is at 2 and 21 o'clock for summer, and at 3 and 22 o'clock for winter. The vertical positions show at what hours the declination was 0, and correspond with Sabine's zero. From 21 to 2 o'clock the needle passes from one extremity of its variation to the other, the north or upper end travelling in the reverse

[1] Magnetical and Meteorological Observations, Hobarton, vol. i. 1850.

direction to the sun, so that it and the sun cross the meridian together in opposite directions, nearly about or a little before noon. About 2 o'clock the needle is arrested, and after that time returns west, following the sun. It will be proper to state, that the north end of the needle, the motion of which has just been described, is the end towards the equator, and also, the upper end of a dipping-needle at Hobarton. This distinction will receive more significance presently.

2898. Hence the cause which affects the needle appears to be far more powerful, and more concentrated in time when the sun is present than when he is away. In this there is accordance between the time of the effect and the time when the sun could exert most influence on those magnetic conditions of the atmosphere, which are for the present supposed to govern that effect.

2899. It will be seen by examination of fig. 10, that the time of maximum temperature is not when the sun is on the meridian, but two hours after it, both in summer and winter. But in reference to temperature and its effect on the magnetic condition of the air, and through that on the needle, it is not the local temperature which is supposed to influence the needle, but that which affects enormous masses of air, above as well as below, and of which the temperature at the spot, however important it may become when we can properly interpret it, gives us as yet little or no knowledge. Still there are some points on which temperature has a more direct bearing. Thus the amount of variation of temperature is in summer double what it is in winter, and the amount of variation in the declination increases in the same proportion (2890.). The minimum temperature in winter is later than in summer, and the extreme western declination of the needle is also later at the same period.

2900. The varying *direction* of the magnetic lines of the earth is made known to us by observations in two planes, one the horizontal plane, to which the position east and west is referred, constituting declination, and the other a vertical plane passing through the line of mean declination, and supplying observations of inclination. The direction of the line of force referred to this plane might change so as either to increase or diminish the inclination, and it does increase at some places for the same hour of local time for which it diminishes at others; thus it increases at Greenwich whilst it diminishes at St. Helena, which is nearly in the same meridian. At Hobarton it changes rapidly at the east and west extremes of the variation, *i. e.* about 2 and 21 o'clock. From noon it diminishes until about 3 o'clock; it then continues nearly the same in summer, when the variation is greatest until 18 or 19 o'clock, from that time it increases until about 22 o'clock, and is nearly a maximum from thence till noon. Hence it will be understood, that the inclination is generally greatest during the rapid journey of the north end of the needle from west to east between 21 and 2 o'clock, and least in the other or prolonged half of the journey; and though this is partly broken up in the night effect, to be considered hereafter, still as a general result it always appears.

2901. All this may be roughly represented by fig. 11 (2909.), in which E. W. represents the path of the sun between the tropics as he comes up with the hours 21^h, 22^h, &c. in his daily journey, and *e* the path described by the north or upper end of the needle, freely suspended at Hobarton, and therefore showing both declination and inclination, *i. e.* the whole direction. Looking down upon such a needle, its upper end will take the course indicated by the arrow, and its position at any given hour is shown sufficiently by the leading lines.

2902. This relation of the motion of the needle to that of the sun has long been known; it has great significance in relation to my hypothesis of the physical cause of these variations. As regards the part of the action which I am considering, it is as if the pole of a magnet came on with the sun, of like nature to the upper end of the Hobarton needle, and at first drives that end west. Towards 19 o'clock the tendency westward diminishes, but the tendency south increases. At 21 o'clock, the increase in the sun's power, acting not directly from the sun but from a region in the atmosphere beneath it, is not sufficient to compensate for his more unfavourable position; the earth's force brings the needle back as regards declination, and then it passes eastwards, but the southerly motion or inclination still increases; about 24 o'clock, or noon, the sun is as to east or west declination indifferent, but powerful in southern action, making the inclination then, or soon after, a maximum. Then as the sun goes west of the needle, its power in driving the pole behind it eastward, will increase for a time, whilst the power producing inclination will diminish, until at 2 or 3 o'clock the earth's force will regain preponderance as the sun's power diminishes by distance, and the needle will return towards its least dip and mean inclination.

2903. All this may be represented experimentally by carrying a magnetic pole north of the dipping-needle, so as to represent the place of the sun-heated air to Hobarton, provided that pole be of the same kind as the north or upper pole of the needle. I have already stated (2877. 2863.), that when a portion of air is heated in a field of magnetic power, it loses in magnetic conduction power, and if in association with air less heated deflects the lines, assuming the state which I have distinguished as that of diamagnetic conduction polarity; then presenting the very polarity, or rather the very inflection of the lines of force, which would affect the needle, as it is affected. As the sun rises and passes north of such a place as Hobarton, the atmosphere under his coming influence becomes more and more heated and expanded; and referring to the model globes of air (2864. 2877.), it is as if such a warm mass passed with the sun through all the regions of the equator, extending also far north and south of it; and, having Hobarton within its influence, produced the effects there observed.

2904. In such a view one sees a reason for the short time occupied in the return of the needle from west to east as the sun passes immediately over its meridian, and for the long time during which it is passing from east to west as the influence of the sun is slowly withdrawn, and then again slowly renewed during the remaining part of his journey, exception being made for the present of the paramagnetic effects due to cold.

2905. I will now consider the Toronto case of diurnal variation as it is presented to us in the volume of magnetical observations, issuing from the same authority and hands as the former volume[1], and also in further observations down to 1848, sent to me by the kindness of Colonel Sabine. The position of the observatory is in lat. 43° 39′ 35″ N. and long. 79° 21′ 30″ W. The absolute declination is 1° 21′ 3″ W., and the mean or absolute dip is 75° 15′ N., so that as regards Hobarton it is on the other side of the equator, and nearly on the other side of the world. The results for the months of June and December are

[1] Magnetical and Meteorological Observations. Toronto, 1840, 1841, 1842, Sabine.

placed in a diagram corresponding to that for Hobarton (2896.), employing the Toronto time for the hours, Plate I. fig. 12.

2906. The north end of the needle is that universally referred to in speaking of the declination; its course at Toronto, during the immediate sun effect, is as follows:—Having gradually moved east from 16^h, it is at extreme east at 20 o'clock, and then returns from the east to extreme west in six hours, after which it moves eastward from the sun. But if we convert this into the motion of the equatorial extremity of the needle, for that is the *upper* end if the needle be free, and concerns us most in the comparison with Hobarton, then it will be seen that this end is most west at 19^h or 20^h; and leaving that position at that hour, it travels quickly eastward, passing through the full range of variation or to extreme east in six hours, or until 2^h, and then returns, following the sun.

2907. Looking at these results, I might repeat the words used in illustration of the Hobarton effects, but for the sake of brevity will simply refer to them. As before, the amount of variation in the declination is in summer double what it is in winter. The difference of temperature is three times greater. The extreme west and east declination is both in summer and winter at 20 and at 2 o'clock, so that the magnet holds to the time in both seasons; but the maxima and minima of cold, as shown before, vary in the two seasons, for the former is at 4 o'clock in summer and 2 in winter, whilst the latter is at 16 o'clock in summer, and 20 o'clock in winter. But this is a variation with consistency; for it will be seen by a moment's inspection, that in winter the maximum of heat has moved towards the time of most powerful action in the one direction, and the minimum has moved towards it in the other. The passage of the sun, therefore, over the meridian, and the period of rapid motion of the needle from west to east, still coincide.

2908. The other element of direction is the inclination. Its variation is very small, but changes thus. A principal maximum dip occurs at 22 o'clock, and the extreme minimum dip at 4 o'clock.

2909. So all the effects may again be generally represented by an ellipse (fig. 13) as they were for Hobarton; and I may refer to the words then used, substituting Toronto for Hobarton and north for south (2901.). As the sun comes up from the east in his course between the two places, he drives, by the altered atmosphere beneath him, the upper ends of their needles before him, and outwards from the line of his path, as if he

Fig. 13.

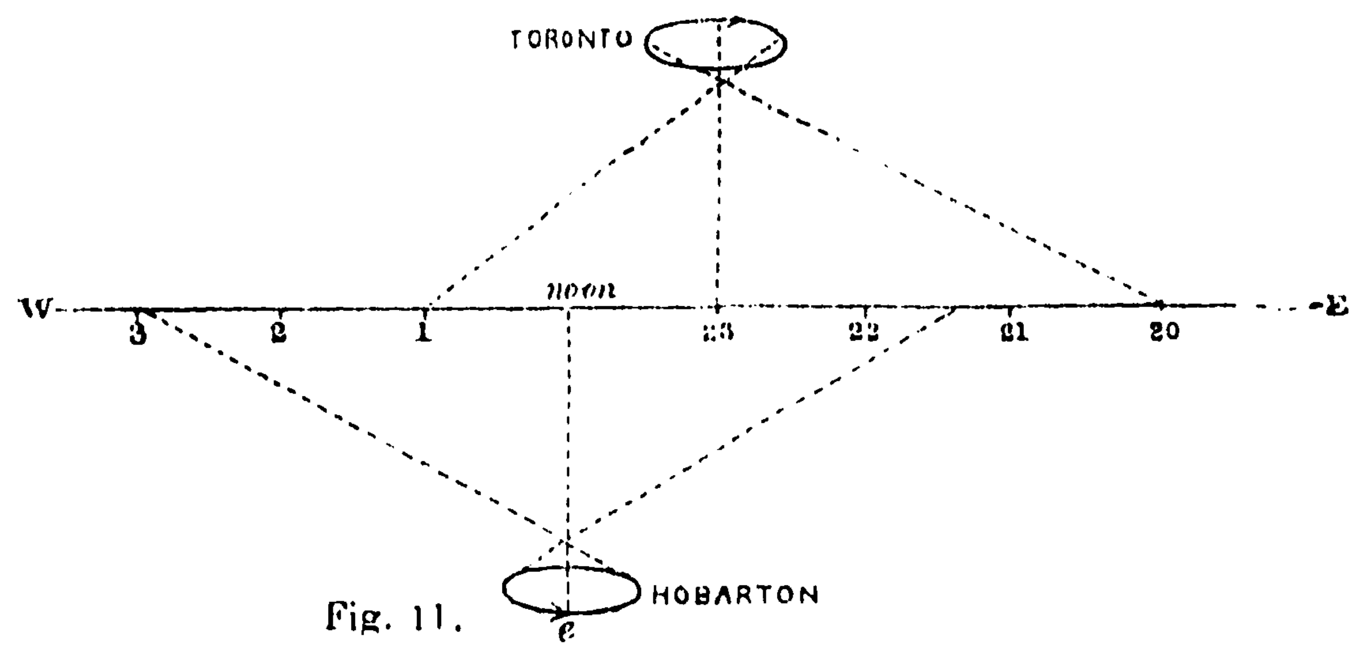


Fig. 11.

were a north pole to the Hobarton magnet, and a south pole to the Toronto magnet. By 22 o'clock, the earth's force, and the action of the air due to the sun's position, permit a return to the east, though the inclination for a time increases (2902.); both swing rapidly round from west to east as he passes over the meridian, and then having attained their maximum position eastward, soon follow after him under the influence of the earth's force, less and less counteracted by the retreating sun. So striking is the similarity between Hobarton and Toronto, that Colonel Sabine has already especially distinguished and described it[1], and has shown, that, laying down the direction of motion in both cases by curves, and bringing the two curves together by their faces, they coincide almost exactly, with this single difference, that the Hobarton changes precede those at Toronto by an hour, or rather more, of local time.

2910. We cannot represent this day effect experimentally upon two such needles as those at Hobarton and Toronto by *one* pole of a magnet, though we can do it with each separately with different poles: but we see at once from the hypothesis, the reason why the sun acts in this manner (2877.), and how it is that the region of influential atmosphere that accompanies him in his journey round the globe, acts with one effect in the northern

[1] Hobarton Magnetical Observations, 1850, p. xxxv.

latitude and another in southern positions (2903.). The reasons also for the short time of the day journey and the lengthened period of the night return (2904.), are manifest. The occurrence of disturbances or secondary waves of power in the night time, and the condition both of the chief variation and the subordinate oscillations in summer and winter, will be considered hereafter.

2911. *Greenwich.*—The following results are taken from the volume of Greenwich Observations for 1847. The latitude is 51° 31′ N., and being removed nearly 80° in longitude from Toronto, the station is well contrasted with it and also with Hobarton. The mean declination is 22° 51′ 18″ W., and the mean inclination is 69° N. As it is the upper end of the dipping-needle which we have to consider for the purpose of a ready comparison with the sun's observed day action (2906.), I will describe those parts of its course and place for Greenwich time which concern us now. Moving westward before 19^h and 20^h, it then returns towards the east, and in six hours, or by 1^h or 2^h, has completed the great sun swing, after which it returns west, following the luminary. The vertical force is given as greatest between 3 and 4 o'clock, and least between 11 and 13 o'clock. The south end of the needle therefore is more upright at the former time and less at the latter; and as the latter occurs during the prolonged return part of the journey from east to west, including the night hours, so we perceive that the upper end of the needle performs its daily journey in an irregular closed curve, which the ellipse for Toronto, fig. 13 (2909.), may generally represent; it passes from east to west slowly during the night hours, approaching the equator at the same time, and then it returns from west to east with far greater rapidity, performing this part of its journey at a greater distance from the equator and nearer to the pole.

2912. *Washington, U.S.*—Latitude 38° 54′ N.; longitude 77° 2′ W.; the mean declination 1° 25′ W.; the mean dip 71° 20′ N. The south or upper end of the needle is in the morning at extreme west, about 20 to 22 o'clock, and at extreme east about 2 o'clock; it then returns slowly west, with the night action as in former cases, regaining extreme west at 20 to 22 o'clock. This is exactly the same movement for declination, in relation to the place of the sun, as for the former localities. I have not the variation of the dip, but theory would lead one to conclude that it is greatest between 22 and 2 o'clock, and least in the evening and night time. The total amount of declination variation is greatest in summer, as before, being 9′·87 in July and only 4′ in December. The greatest difference in the earth's temperature is also in July, being then nearly 20° Fahr., whereas in December and January it is only 10° Fahr. The shortest period between the extreme temperature, including therefore the quickest change of temperature, is from 16 or 18 to 2 o'clock, and consequently includes noon. All these conditions combine to produce the greatest magnetic action, and it is in the direction pointed out by the hypothesis.

2913. *Lake Athabasca.*—Latitude 58° 41′ N.; longitude 111° 18′ W. of Greenwich; mean declination 28° E. The observations are only for five months, but as the position is in a high latitude and may be important for future considerations, I give the results here. The extreme western position of the upper end of the needle is about 17 or 18 o'clock, and its extreme eastern position about 1 or 2 o'clock; so that as far as declination is concerned, the action of the sun and atmosphere is as in former cases. The amount of declination variation is very great, being in October 21′·32; in November 10′·8; in December 9′·78; in January 16′·29, and in February 14′·87.

2914. *Fort Simpson.*—Latitude 61° 52′ N.; longitude 121° 30′ W. of Greenwich; mean declination 38° E. These observations are only for two months, *i. e.* April and May 1844. The extreme western place of the upper or south end of the needle was at 19 o'clock, and its extreme eastern position at 2 o'clock. The result therefore is in perfect accordance with the preceding observations and conclusions. The amount of variation, as given in the horizontal plane, is very large, being 36′·26 in April and 32′ in May.

2915. *St. Petersburgh.*—Latitude 59° 57′ N.; longitude 30° 15′ E. of Greenwich; mean declination 6° 10′ W.; the dip 70° 30′ N. The observations are the mean of six years, and show that the upper end of the needle is extreme west in regard of noon, about 19^h and 20^h for the months of March to August, and that for the other months there is a western position about the same hours. The extreme east position is, for *all* the months, about half-past 1 o'clock, so that the sun's effect in passing over at the noon period is as in former cases. The greatest amount of variation is 11′·52 in June; in winter it dwindles away to as little as 1′·77. From theory the dip may be expected to increase during the day hours and diminish at night.

2916. Thus these cases, which, including the chief feature of diurnal variation and sun action, were selected as a first and trial-test of the hypothesis, join their evidence together, as far as they go, in favour of that view which I am offering for their cause; nor have I yet found any instance of even an apparent contradiction in regard to the sun action. They assist the mind greatly in forming a precise notion of the manner in which the influence of the sun and air is supposed to act, not only in similar cases, but in respect of other consequences, *i. e.* in all that properly comes under the term of atmospheric magnetism; I will therefore now restate more particularly the principles which, according to the hypothesis, govern them, in hopes that I may be fortunate enough to assist in developing by degrees the *true physical cause* of the magnetic variations in question.

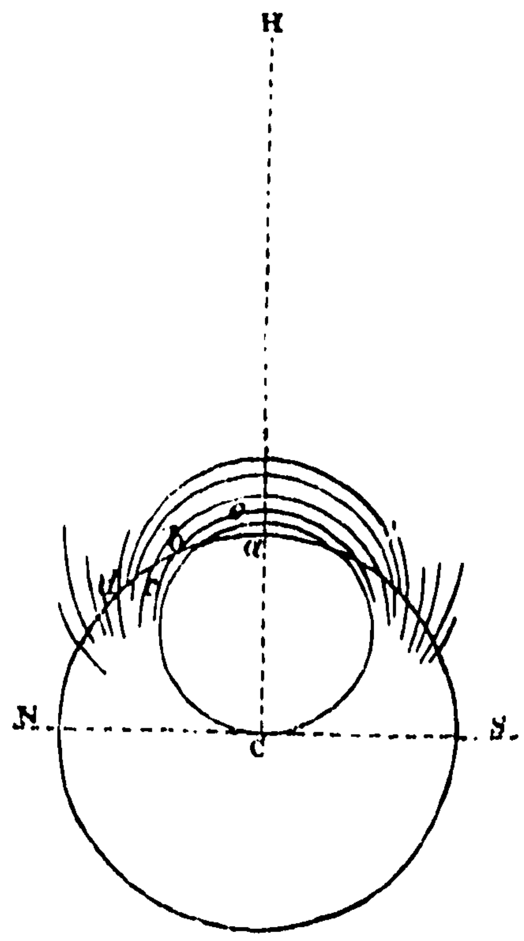
2917. Space, void of matter, admits of the transmission of the magnetic force through it (2787. 2851.). Paramagnetic and diamagnetic bodies either increase or diminish the degree in which the transmission takes place (2789.). This, their influence, I have expressed, for the time, by the phrase of magnetic conducting power, and I think have given sufficient first experimental evidence of the existence of the power and its effects in disturbing the lines of magnetic force (2843.). The atmosphere is, by the oxygen it contains (2861. 2863.), a paramagnetic medium, and has its conducting force greatly diminished by elevation of temperature (2856.) and by rarefaction (2782. 2783.), as has also been fully proved by experiment. The sun is an agent which both heats and rarefies the atmosphere, and in its diurnal course, the place of greatest heat and rarefaction must, speaking generally, be beneath it. The irregularities in the condition of the earth's surface and other causes do produce local departures from an exact relation of place, but they probably disappear partly, if not altogether, in the upper regions of the air.

2918. Assuming that *the air under the sun* is most changed magnetically, and confining the attention to a spot where the sun is vertical, for the purpose of considering the condition of the atmosphere there and at other parts in relation to it, the supposition of a globe of air over the spot will of course find no fit application (2877.). We are first to suppose the sun far away and the atmosphere in a mean state as to temperature, and then consider the sun as present in the meridian of a given place; and it is *the degree of alteration* in temperature and expansion of the air beneath and around the place of the sun, and the manner in which the change comes on and passes away, which concern us. In relation to the surface of the earth, that alteration will be greatest somewhere beneath the sun, and will diminish in every direction around, becoming nearly nothing as to direct action at that part or circle of the earth where the sun's rays are tangent. In relation to elevation, it is a question yet whether the effect is greatest in amount at the surface, diminishing upwards. As regards the atmosphere, it must of course end with it, though as respects space itself (2851.), a reservation-thought may arise. With regard to any alteration occasioned by the sun's influence in the opposite hemisphere, though there is none produced directly, yet indirectly there is that due to the falling of the temperature of the air, from the condition to which the sun, whilst above the horizon, had brought it. This change must be more tardy, irregular and disturbed, by local and other circumstances, than the opposite alterations produced by the direct influence of the luminary; and is that which occasions, by the hypothesis, the second maximum or minimum or other recurring night actions, made manifest by the needle in the hours when the sun is away.

2919. The lines of force which issue from a magnet are, as it were, located and fixed by their roots in a way well understood experimentally by those who have worked upon this subject. In the same manner the lines which issue from the earth more or less suddenly, according to the amount of inclination, are held beneath by a force of location; and because of the unchanging action of the earth in respect of atmospheric effect, are restrained more or less from alteration beneath during the changing action of the atmosphere. This fixation in the earth is a chief cause of certain peculiarities in the atmospheric phænomena as we observe them; and is productive of that rotation of the line of force about the mean position which we have already considered during the sun swing, and shall meet with again under the action of cold air. This condition of fixation at the lower parts of the lines of force occurs at every station where there is any dip at all, and gives for each the point of convergence round which the motion of the upper end of the needle takes place (2909. 2932.).

2920. So the atmosphere, under the influence of the sun, lies upon the earth altered most at the part beneath the luminary. It has received power to affect the lines of magnetic force differently to the manner in which it affected them in the sun's absence. It has become a great magnetic lens, able to refract the lines, and the manner in which it does so appears to be of the following nature. All the lines passing through this heated and expanded air, surrounded by other air not so much heated, will, because of its being a worse magnetic conductor than the latter (2861. 2862.), tend to open out (2807.); and the mass of heated air, as a whole, will assume the condition of diamagnetic polarity. If, therefore, for the sake of simplicity, the magnetic and astronomical poles of our earth be supposed as coincident, and fig. 14 represent a section taken through them and the place of the sun, then N and S will be the magnetic poles, and the different curves cutting the outline of the circle will sufficiently represent the course of the magnetic lines as they occur at or about the surface of the earth, H being the sun, and *a* the place immediately beneath it, which is also coincident with the magnetic equator. By this diagram we shall have an illustration of the hypothetical effect on the inclination of the needle.

Fig. 14.



2921. Considering the point *a* first, and assuming as yet that the maximum of change in the air is always at the surface of the earth, we shall find that there the lines of force will open out, preserving in some degree their parallel or concentric relation. Consequently a magnetic needle, free to move in every direction, and therefore taking up its position *in* the line of force, ought not, if placed at this spot, to be altered in its position. It ought to show perhaps a diminution of magnetic force transmitted through that spot; but, for the reason before given (2868.), I conclude it would indicate a greater intensity, the increased power thrown upon it through the diminution of the conducting power of the air in that place causing it to act as a more powerful needle.

2922. Proceeding to a point *b*, there the lines of force have dip. The same physical effect will be produced upon them here as before, *i. e.* the portions in the atmosphere will open out; but neither here nor in the former case will they continue to have the same curvature as before, for towards and in the earth, where they have their origin, they are restrained more or less from altering by the unchanging action of the earth (2919.); whilst at their more advanced parts, as at *c*, they enter into portions of the atmosphere which are nearer to the most intense lines of solar action, H C, probably also into the region of most intense action, and also into space, circumstances which cause more displacement of the lines, tending to separate by the tension of the parts altered in the air, than can happen *in* the earth (2848.). So the magnetic line of force at *b* will not move parallel to itself, but being inclined a certain degree to the horizon, when in the normal condition, will be more inclined, *i. e.* will have more dip given to it by the presence of the sun. This is the fact made manifest by the needle when indicating the position of the line as to inclination (2908.) at Hobarton, Toronto or elsewhere, by the motion of its upper end; for it is manifest that whatever happens on one side of the place of the sun and magnetic equator, when, as in our supposition (2920.), they coincide, will happen on the other.

2923. The case may be more simply stated, for the facility of recollection, by saying that the effect of the sun is to raise the magnetic curves, over the equatorial and neighbouring parts, from their normal position, in doing which the north and south dip are simultaneously affected and increased.

2924. At the place *d* like effects on the inclination must be produced, and theoretically it should be affected in the same direction even at N. and S. At the point *a* the inclination is supposed to be not at all altered, but going either north or south, the changes appear and increase. It is not probable that the maximum alteration will be at N. or S., but the latitude where it will occur must depend upon the many conjoined circumstances that belong to the case of a globe round which a magnetic lens, such as I have endeavoured to describe, is continually revolving.

2925. Instead of assuming that the sun is at H, let us suppose that we are looking at the diagram in a vertical position and towards the east; the sun coming up from the east and passing over our heads, and bringing with it that condition of our atmosphere which is the cause of the change. As it does so, all the magnetic curves would rise; the inclination would increase at *b*, *d*, and every place where there was any beforehand, in opposite directions on the two sides of *a*; this would go on until the sun was in the zenith, and then as it passed away and sank behind us, the lines would draw in again and the dip diminish to what it was at first. The maximum of dip would be when the sun was near the zenith, and the minimum when he was quite away.

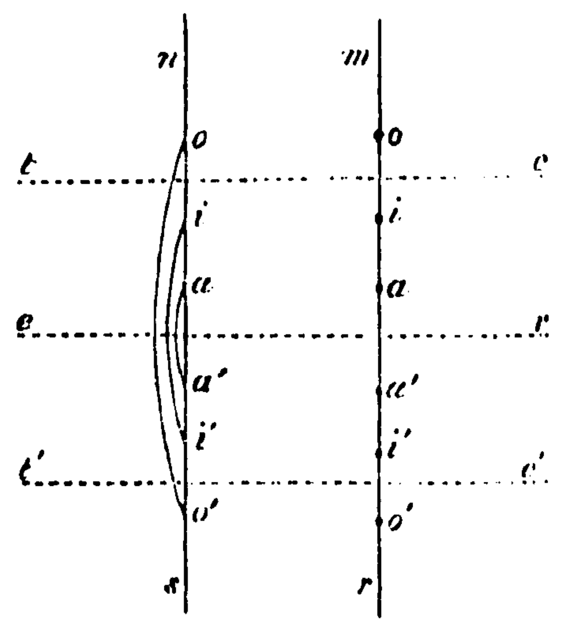
2926. But if the resultant of force be above in the atmosphere (2937.), which is by far the most probable, as it is the whole atmosphere which acts by heat diamagnetically, then the results would be modified; for if over *a*, the lines of force might be *depressed*, and any inclination there would be diminished; at *b* it might not for the moment be affected; whilst in higher latitudes it would be increased, according as the line of force from the resultant in the atmosphere, wherever that might be, fell outside of the angle formed by the inclination with the horizon of a given place or within it. St. Helena, the Cape of Good Hope, and Hobarton, furnish instances of the three cases.

2927. At the same time the total force would undergo a change in its amount; that transmitted through a given space would be least when the sun was in the zenith, and most when he was away (2863.). The total variation in the force should be greatest at *a*, and diminish from thence towards north and south. The daily variations of the inclination are so imperfectly known to us at present, that we cannot say how far the natural changes will accord with these expected variations, but as far as the observations go they agree with the theory.

2928. If the sun, instead of being over the equator, is at a tropic and so vertical, for instance, over *b*, then the effects will be modified; and the resultant still being assumed as above, the lines of force which before were not affected, may be expected to descend and lessen the inclination, whilst other lines in higher latitudes, which before were increased in inclination, may now be but little affected, and other lines in still higher latitudes have, as before their inclination, increased. On the other side of the equator, the tendency of the lines would be to increase in inclination.

2929. Proceeding to that part of the expected change of position of the free needle which produces variations of *declination*, let *e r* in fig. 15 represent the sun's path in the equator, and *t c*, *t' c'* the same at the tropics; let *m r* be a magnetic meridian, and *a a'*, *i i'*, *o o'* places of equal north and south inclination on opposite sides of the equator. The curves of magnetic force seen in front in fig. 14, are now in the plane of the magnetic meridian, but may be considered as rising on opposite sides of the equator and coalescing over it. If the air on all sides were in its mean condition and the sun entirely away, these curves would be in the vertical plane *m r*; or if the sun near midday was so placed that the resultant of the heated and changed atmosphere was in the meridian *m r*, though effects of inclination would occur (2922.), still the curves would remain in the same vertical plane. But if the resultant were either to the east or the west of *m r*, variations of declination would be produced. For suppose the sun to be advancing from the east or *r*; because it gives the air a diamagnetic condition, the lines of force would tend to expand (2877.), and therefore move westward, as represented in the meridian *n s*; and the deflection caused thereby would be greatest upon the surface of the earth, because it is there that the curves as they enter the earth are held and restrained in respect of their normal position (2919.). As the warmed atmosphere came on, the western deflection would increase to a certain extent, and then diminish to nothing when the resultant was in the meridian; but as the latter passed on, the deflection would grow up on the eastern side of *n s*, and, after attaining a maximum, would diminish and cease as the warm air retreated.

Fig. 15.



2930. If the sun's path was in the northern tropic, *t c*, and the resultant in the atmosphere therefore to the north of the stations *a* or *i*, though that would make a difference in the amount of the declination variation, it would not alter its *direction*, for still the curves *a a'* and *i i'* would bear to the west as the sun came up, and would be on the meridian when the resultant was there also. There would be more effect produced at *i* than at *i'*, but the contrary character of the dip, in respect of the sun's place, would not alter the direction of the declination variation.

2931. A cold region of air acting, as at the coming on of night, upon the lines of magnetic force of the earth, would, by virtue of its paramagnetic character (2865.), produce corresponding effects both of inclination and declination, but in the contrary direction.

2932. Thus the lines of force which issue from the earth at all places upon its surface where there is any dip, will, by the hypothesis, under the daily influence of the sun, describe by their ascending parts a closed curve or irregular cone, the apex of which is below. As a fact this result is perfectly well known, but its accordance with the hypothesis is important for the latter. The mean position of the free needle will be in the axis of this curve or cone, and its return, either in declination or inclination, to the mean is an important indication of the amount and position of the variable forces which influence it at such times.

2933. My hypothesis does not at all assume that the heated or cooled air has become magnetic so as to act directly on the needle after the manner of a piece of iron, either magnetically polar or rendered so under induction. There is no assumed polarity of the oxygen of the air other than the conduction polarity (2822. 2835.) consequent upon a slight alteration of the direction of the lines of force. The change in the magnetic conducting power causes this deflection of the lines; just as a worse conductor of heat introduced into a medium of better conducting power disturbs the previous equable transfer of heat, and gives a new direction to that which is conducted; or as in static electricity, a body of more or less specific inductive capacity introduced into a uniform medium disturbs the equable lines of force which were previously passing across it.

2934. The sole action of the atmosphere is to bend the lines of force. The needle being held by these lines and, when free, being parallel to them, changes in position with the changes of the lines. It is not necessary even that the lines, which are immediately affected in direction by the altered air, should be those about the needle, but may be very distant. The whole of the magnetic lines about the earth are held by their mutual tension in one connected sensitive system, which has no sluggishness anywhere, but feels in every part a change in any one particular place. There may be, and is continually, a new distribution of force, but no suppression. So when any change in direction happens, near or distant, the needle in a given place will feel and indicate it, and that the more sensibly according to the vicinity of the place and the kind of change induced; but the disposition of the *whole* system has been affected at the same moment, and therefore all the other needles will be affected in obedience to the change in the lines of force which govern them individually.

2935. The needle is a balance on which all the magnetic power around a given locality fastens itself, even to the antipodes, and it shows for each place every variation in their amount or disposition, whether that occurs near or far off. Its mean position is the normal position; and as regards atmospheric changes, the fixation of the lines of force in the earth (2919.) is that which tends to give the lines a standard position (exclusive of secular changes), and so bring them and the needle back from their disturbed to their normal state. Hence, whilst considering the causes which disturb either the declination or the inclination, arises the importance of keeping in mind the mean position or place of the needle (2932.), and not merely the direction in which it is moving.

2936. So the well-known action of the sun on the needle is, by my hypothesis, very indirect; the sun at a given place affects the atmosphere; the atmosphere affects the direction of the lines of force; the lines of force there affect those at any distance, and these affect the needles which they respectively govern.

2937. I have, for the sake of convenience in considering a special action of the atmosphere, spoken of the resultant in the atmosphere dependent on the sun's presence; and will do so a little while longer without implying any direct action of this resultant, or that portion of air which yields it, upon the needle (2933.), for the sake of considering at what probable height it is situated in the air. That it cannot be on the surface of the earth, is shown by the depression of the lines and diminution of the dip at St. Helena and Singapore during the middle of the day; and that it is not even under the sun, is shown by the manner in which the greatest action precedes, in some degree, the sun, as at Hobarton and Toronto, and other places by different amounts of time; neither the time when the sun is on the meridian, nor the time when the observed temperature is highest (for that is after the sun), is the time of greatest action, but one before either of these periods. The changes in the temperature of the air produced by the sun, will not take place below and above at the same time. The upper regions of the atmosphere over a given spot are affected by the sun at his rising and afterwards, before the air below is heated; and therefore the effect from above would be expected to precede that below. The temperature observed on the earth does not show us, for the same time, the course of the changes above, and may be a very imperfect indication of them. The maximum temperature below is often two, three, or four hours after the sun, whereas, whatever heat the sun gives by his rays directly to the atmosphere, must be acquired far more rapidly than that. It is very probable, and almost certain, that at 4 or 5 o'clock A.M. in the summer months, the upper regions may be rising in temperature, whilst on the surface of the earth, through radiation and other causes, it is falling. The well-known effect of cold just before sunrise in some parts of India, and even in our country, is in favour of such a supposition. We must remember that it is not the absolute temperature of the air at any spot that renders it influential in producing magnetic variations, but the *differences* of temperature between it and surrounding regions. Though the upper regions be colder than the lower, their changes may be as great or greater; they happen at a range of temperature which is probably more influential than a higher range (2967.); and, what is of importance, they occur more quickly and directly upon the presence of the sun. The quantity of heat which the atmosphere can take directly from the sun's rays, is indicated by the different proportions we receive from him when he is either vertical or oblique to us, and so sending his beams through less or more air; and when he has departed, the upper parts of the air are far more favourably circumstanced for rapid cooling by radiation than the portions below. So that the final changes may be as great or greater than below, and we may learn little of them, or their order, or time, by observations of temperature at the earth's surface. In addition therefore to observations of magnetic effect, as depression of the lines of force at St. Helena, &c., there are apparently reasons deducible from physical causes, why the chief seat of action should be above in the atmosphere.

2938. In the midday effect the upper end of the needle passes the mean position (2935.) on its return to the east generally before the sun passes the meridian going westward. At Toronto it is about an hour in advance; at St. Helena and Washington an hour and a half; at Greenwich and Petersburgh two hours; at Hobarton and the Cape of Good Hope the passage is about noon. Such results appear to indicate that the place of maximum action is in advance of the sun; and it probably is so in some degree, but not so much as at first may be supposed, as will appear I think from the following considerations.

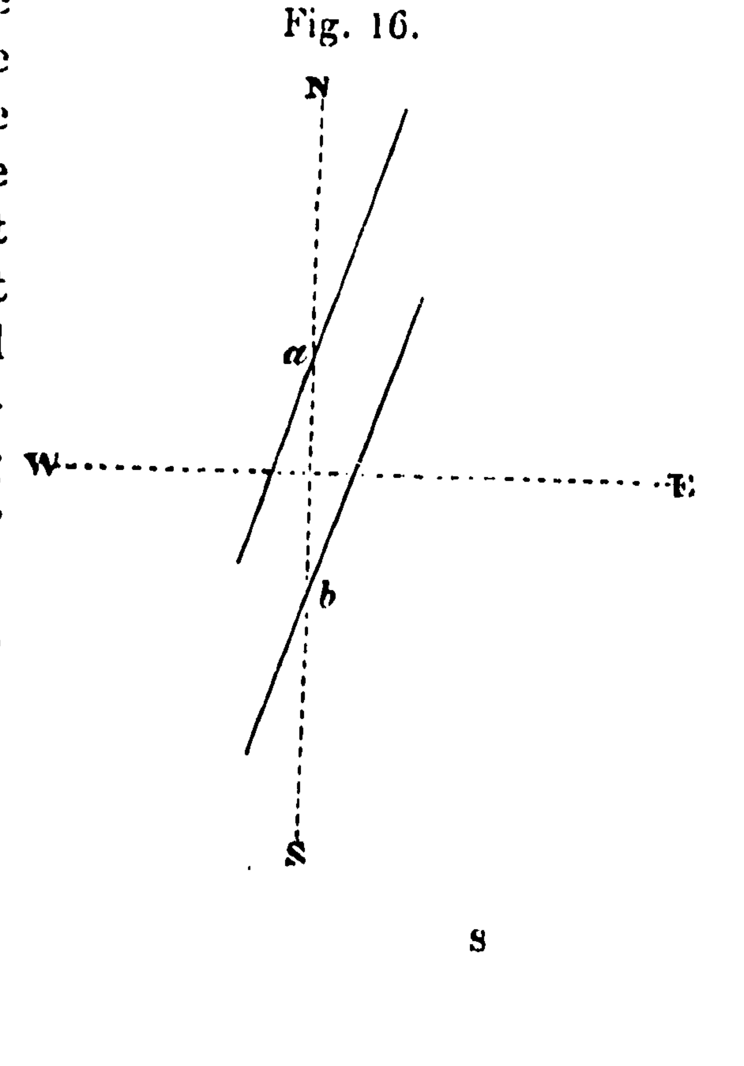
2939. The precession of the time of maximum action may depend in part upon some such condition as the following. As the sun advances towards and passes over a meridian, the air is first raised in temperature and then allowed to fall, and these actions produce the differences in different places on which the magnetic variations depend. But they depend also upon the *suddenness* with which or the vicinity at which these differences occur. Thus two masses of air, having equal differences of temperature, will affect the lines of force more if they be near together, and to the needle, than if they be far apart. And again, if a body of air were of a certain low temperature at one part, and, proceeding horizontally, were to increase rapidly to a certain high temperature and then diminish slowly to the first low temperature, such a body passing across a set of lines of magnetic force would affect them in opposite directions at the fore and after part; but it would affect them most on the rapidly altering side.

2940. Now the air as heated by the sun must be in this condition. According to analogy with solid and liquid bodies, being exposed to heat and then withdrawn, the changes of temperature that it would undergo would be more rapid in the elevation than in the falling, and so the changes in the preceding would be more rapid than in the following parts. To this would be added the effect of the atmosphere warmed by the earth; for as that is slower in attaining heat, as is shown by the time of maximum temperature, so its effects being gradually communicated to the air above, as the sun passed away, would tend to retard its fall and enlarge the difference already spoken of. Applying these considerations to the natural case, the strongest effect and the greatest variation should be towards the west, and the following or lesser action towards the east of the sun; and the mean condition of the needle for the whole change would be in advance of that body.

2941. Mr. Broun has made observations of the daily variation at different heights, namely, at Makerstoun and the top of the Cheviot Hills, where the height differs by nearly half a mile, and finds, I believe, no difference in the intensity, but that the progress is *first* at the higher station. It would be very interesting to have an observatory up above, but to give the results required it should have air and not solid matter beneath it.

2942. There is another circumstance which importantly influences the *times* of the passages of the declination variation. If two places north and south of the equator have equal dip and contrary declinations, *i. e.* if both their upper ends point east or west, then the effects ought to correspond and form a pair. But if both have east or west declination, according to the usual mode of marking this effect by the north end of the magnet, then the variations already described should come on as the sun passes midway between them, but there should be a difference in *time*. As the luminary appears and approaches, the needles *a* and *b* (fig. 16) will most probably be affected together; but, as he draws nigh, if the places have eastern declination, the one that is south will be soonest affected, and for the time most strongly, but will in a period more or less extended, be followed by the corresponding action at the other place. For as each needle will have returned from the first half of its series of changes to 0° by the time the sun is on its magnetic meridian, and as it will arrive at this meridian, as regards the

Fig. 16.



south needle, before it does so for the north needle, so the south magnet should precede the other in its changes. If the declination of both were westerly, then the north needle would precede the south.

2943. The hypothesis advanced, besides agreeing with the facts regarding the direction of the needle's motions, as is the case generally, and if my hopes are well-founded, will be the case also in more careful comparisons; should also agree in the *amount* of force required for the observed declinations at given hours. I have endeavoured to obtain experimental evidence of the difference of action of oxygen and nitrogen on needles subjected to the earth's power, but have not yet succeeded. This however is not surprising, since a saturated solution of protosulphate of iron has failed under the same circumstances. More delicate apparatus may perhaps yield a positive result.

2944. That small masses of oxygen should not give an indication of that which is shown by the atmosphere as a whole is not surprising, if we consider that the mass of air is exceeding great, and includes a vast extent of the curves on which it, by the hypothesis, acts; and yet that the effect to be accounted for is exceeding small. The extreme declination at Greenwich is 12′, equal to about 4′ 24″ of east and west alteration on the free needle, so that that is the whole of what has to be accounted for. One could scarcely expect such an effect to be shown by small masses of oxygen and nitrogen acting on only a few inches in length of the magnetic curves passing through them, unless one could use an apparatus of extreme and almost infinite sensibility; but from what I have seen of oxygen when compared at different degrees of dilution (2780.), or at different temperatures (2861.), I am led to believe that the effects on it produced by the sun in the atmosphere will ultimately be found competent to produce these variations.

2945. Where the air is changed in temperature or volume, there it acts and there it alters the directions of the lines of force; and these by their tension carry on the effect to more distant lines (2934.), whose needles are accordingly affected. The transferred effect will be greater or less according as the distances are less or greater, and hence a change near at hand may overpower that at a distance, and a cloud close to a station may for the moment do more than the rising sun. These are the irregular variations; and the extent of their influence is well shown by the photographic records of Greenwich and Toronto. The volume of Greenwich Observations for 1847 contains a photographic record of the declination changes, February 18—19, 1849. Between 6 and 7 o'clock there is a variation of 16′ occurring in 18 minutes of time, or at the rate nearly of 1′ for each minute of time. The course of the mean variation for the same date and time is 1′·95 in two hours, or at the rate of 1 second for each minute of time, so that the irregular variation (which may be considered as a local variation in respect of the sun's power for the time) is sixty times that due to the effect of the great resultant; moreover it was in the reverse direction, for the temporary variation was from east to west, whilst the mean variation was from west to east.

2946. Another mode of showing how much the action of nearer portions of the atmosphere may overpower and hide the effect of the whole mass, is to draw the line of mean variation for the twenty-four hours through such a photographic record as that just referred to, and then it will be seen in every part of the course how small the mean effect on the needle is, compared to the irregular or comparatively local effect for the same moment of time. The magnet with which these observations were made, is a bar of steel 2 feet long, $1\frac{1}{2}$ inch broad and a quarter of an inch thick, and therefore not obedient to sudden impulses; it is probable that a short, quick magnet would show numerous cases in which the irregular variation would be several hundred of times greater than the mean. Still all these irregularities and overpowering influences of near masses are eliminated by taking the mean of several years' observation, and thus a true result is obtained, to which the hypothesis advanced may be applied and so tested.

2947. Returning for a short time to the annual variation (2882.), I may observe, that it has been a good deal considered in discussing the daily variation. The arrangement of the magnetic effects by Colonel Sabine at Hobarton, Toronto, St. Helena and elsewhere, into monthly portions, proves exceedingly instructive and important, especially for places between and near the tropics. It supplies that kind of analysis of the annual variation which is given by the hours for the daily variation.

Every month, by a comparison of its curve with those of other months, tells its own story, at the same time that it links its predecessor and successor together.

2948. I shall have occasion to trace these monthly means hereafter; but in the meantime refer to the effect of the sun's annual approach and recession indicated by these means, as according with the hypothesis in respect of near and distant actions (2945.). Hobarton and Toronto are in opposite hemispheres, so that the sun whilst approaching one recedes from the other, and the amount of variations therefore changes in opposite directions. Below is the average for each month, derived in the case of Hobarton from a mean of seven years, and in that of Toronto from a mean of two years.

	Hobarton. Lat. 42° 52′·5 S.		Toronto. Lat. 43° 39′·35 N.	
	′		′	
January	11·66		6·51	
February	11·80		6·40	
March	9·50		8·50	
April	7·26		9·52	
May	4·56		10·34	
June	3·70	Winter.	11·99	
July	4·61		12·70	Summer.
August	5·89		12·68	
September	8·24		9·72	
October	11·01		7·59	
November	12·05	Summer	5·75	
December	11·81		4·47	Winter.

The two stations are in latitudes differing only 47′ from each other; and the extreme difference of the atmospheric effect between summer and winter differs as little, being at Hobarton, which has the highest latitude, 8′·35, and at Toronto 8′·23.

2949. According to Dove, the northern hemisphere is warmer in July than the southern hemisphere by 17°·4 Fahr., and colder in winter by only 10°·7; the numbers being as follows:—

		°	
July.	Northern hemisphere	71·0	62°·3 the whole globe.
	Southern hemisphere	53·6	
January.	Northern hemisphere	48·8	54°·15 the whole globe.
	Southern hemisphere	59·5	

The mean for the whole year is 59°·9 for the northern hemisphere, and $56^\circ{\cdot}5$ for the southern. Therefore, as Dove further shows, the whole earth is in July, when the sun is shining over the terraqueous parts, 8° higher in temperature than in January, when it is over the watery regions: and from the influence of the same cause, the mean of the southern hemisphere is $3^\circ{\cdot}4$ below the mean of the northern half of the globe. The difference between January and July is for the northern hemisphere $22^\circ{\cdot}2$, and for the southern only $5^\circ{\cdot}9$. These differences are so peculiar in their arrangement and so large in amount, that they must have an effect upon the distribution of the magnetic forces of the earth, but the data are not yet sufficient to enable one to trace the results. Sabine indicates a probability from his analysis of observations, that the sum of the earth's magnetic force is increased in intensity when the sun is in the southern signs, *i. e.* in our winter (2891.). I should have expected from theory that such results would have been the case, at least in those parts where the dip was not very great; because a colder atmosphere ought to conduct the lines of magnetic force better, and therefore the systems round the earth ought at such a time to condense, as it were, in the cooler parts. It would be doubtful, however, whether the needle would show this difference, because the lines of power would not be restrained above, as in the case formerly supposed (2922.), but could gather in from space freely. From what has been said, however, it will be evident that such a conclusion can only be drawn with any degree of confidence from observations made pretty equally over both hemispheres.

2950. If we should ever attain a good knowledge of the annual variation for several stations in different parts of both hemispheres, it would help to give data by which the depth at which the magnetic power is virtually situated might be estimated; for, as this power is expected to undergo undulations over very large portions of the earth's surface by the annual changes of temperature (2884.), so they would differ in character and extent according as the origin of the lines should prove to be more or less deeply situated.

2951. With regard to the many variations of magnetic force, not periodic or not so in relation to the sun, which yet produce the irregular and overruling changes already referred to (2945.), dependent, as I suppose, on local variations of the atmosphere, I may be allowed to notice briefly such points as have occurred to my mind.

2952. The *varying pressure* of the atmosphere, over a given part of the earth's surface, ought to cause a variation in the magnetic condition of that part of the earth. It is represented to us by a difference of 3 inches of mercury, or one-tenth of the weight of the atmosphere. Now the oxygen in a given space is paramagnetic in proportion to its quantity (2780.), and therefore it does not seem possible that that quantity over a given space of the earth's surface, whether it be recognized by volume as above, or by weight as in a given volume at the earth's surface, should be varied to the extent of one-tenth of the whole sum without producing a corresponding alteration in the distribution of the magnetic force; the lines being drawn together and the force made more intense by an increase of the quantity or of the barometric pressure, and the reverse effects produced at the occurrence of diminished pressure.

2953. At any spot which is towards the confines of that space where the air is increasing or diminishing in pressure, there will probably occur variations in the directions of the lines of force, and these will be more marked at such places as happen to be between two others, in one of which atmosphere is accumulating, whilst from the other it is retreating. Whether these changes (which I think must occur) produce by vicinity, effects large enough to become sensible in our magnetic instruments, is a question to be resolved hereafter. To suggest the cause is useful, because to know of the existence, nature, and action of a cause, is important to the arrangement of the best means of observing and evolving its effects.

2954. *Winds and large currents of air above* may often be accompanied by magnetic changes if they endure for a time only. A constant stream like the trade-wind, may have a constant effect; but if, when the arrangement of the lines of magnetic force through the atmosphere is in a given state consequent upon the condition of the atmosphere at that time, a wind arises which mixes regions of cold and warm air together, or makes the air more dense in one region than another, or proceeding from one to another, balances regions which before were in different conditions, then every such change will be accompanied by a corresponding change in the disposition of the magnetic force, to which we may perhaps hereafter be able to refer by means of our instruments. Even tides in the air ought to produce an effect, though it may be far too small to be rendered sensible.

2955. The precipitation of *rain or snow* is a theoretical reason for the change of magnetic relations in the space where it takes place; because it alters the temperature where such precipitation occurs, and relieves it from a quantity of diluting diamagnetic or neutral matter. A *chilling hail-storm might* affect the needle in a summer's day. *Clouds* may have a sensible influence in several ways; acting at one time by their difference from neighbouring regions of clear air, and at other times by absorbing the sun's rays, and causing the evolution of sensible heat at different altitudes in the atmosphere at different places, or preventing its evolution more or less at the surface of the earth. Those masses of warmer or colder air of which meteorologists speak, which being transparent are not sensible to the eye, will produce their proportionate effect. And hypothetically speaking, it is not absolutely impossible that the hot and partially deoxygenated air of a large town like London, may affect instruments in its vicinity; and if so, it will affect them differently at different times, according to the direction of the wind.

2956. If one imagines on the surface of the earth a spot which shall represent the resultant there of the atmospheric actions above, and can conceive its course as it wanders to and fro, under the influence of the various causes of action which have been in part referred to, whilst it still travels onwards with the sun, one may have an idea of the manner in which it may affect the various observatories scattered over the earth. I believe that its course, as regards the east and west direction of its wanderings, is partly told in the photographic registerings of Greenwich and Toronto, being there mingled in effect with other causes of variation. This spot may be concentrated or diffuse; it may pass away and reappear elsewhere; there may even be two or more at once sufficiently strong to cause vibrations of the needle between them.

2957. The aurora borealis or australis can hardly be independent of the magnetic constitution of the atmosphere, occurring as it does within its regions, and perhaps in the space above. The place of the aurora is generally in those latitudes the air of which has a distinct magnetic relation, by difference of temperature and quantity, to that at the equator, and the magnetic character both of the aurora and of the medium in which it occurs ties them together; therefore, to be aware of and to understand in some degree the latter, will probably direct us to a better comprehension of the former. The aurora is already connected with magnetic disturbances and storms; it may in time connect them with changes in the atmosphere in a manner not at present anticipated, and as the suggestion is founded upon principle it seems deserving of consideration.

2958. Can the magnetic storms of Humboldt be due to atmospheric changes? This is a question on which I would offer the following observations. Supposing a magnetic rest in the atmosphere, and that all local or irregular variations remained unchanged for the time, then if a change happened in one place it would be felt instantly everywhere else over the whole earth, and in proportion to the distance from the place of change. It would be felt instantly, because the impulse would not be conveyed chiefly or importantly through the matter of the earth or air, but through the space above, for the lines there are affected by changes in that part of them which passes through the atmosphere, and, as I conceive, would affect the other lines in space round our globe, which would in turn affect those parts of their lines, which, passing downwards to the earth, govern the needles below. In space, I conceive that the magnetic lines of force, not being dependent on or associated with matter (2787. 2917.), would have their changes transmitted with the velocity of light, or even with that higher velocity or instantaneity which we suppose to belong to the lines of gravitating force, and if so, then a magnetic disturbance at one place would be felt instantaneously over the whole globe.

2959. But the difficulty is to conceive an atmospheric change sufficiently extensive and sudden to make itself perceived everywhere at the same time amongst the comparatively local variations that are continually occurring. Still, if there were a lull in these disturbances by the opposition of contrary actions or otherwise for the same moment of time at two or more places, those places might show a simultaneous effect of disturbance, and that even when the cause might be very little or not at all sensible in the place where it occurred. A simultaneous change over an area of 600 or 800 miles in diameter, might produce less alteration in the middle of that area than at the extremities of radii of 1000 miles.

2960. It becomes a fair question of principle to inquire how far masses of the air may be *moved* by the power of the magnetic force which pervades them. When two bulbs of oxygen in different states of density are subjected to a powerful magnet with an intense field of force, the mechanical displacement of one by the other is most striking. Whether in nature the enormous volumes of air concerned, and the difference in intensity of the earth's magnetic force at the different latitudes where these may be supposed to be located, combined with the difference of temperature, are sufficient to compensate for the small portions of oxygen in the air and the smaller variations in density, is a matter that cannot at present be determined. The differential result of motion, as has been shown, is very great where the direct result, as of compression, is not merely very small but nothing (2774. 2750.), and the atmosphere is a region where the differential action of enormous masses is concerned.

2961. Now in the matter of difference of intensity, Gay-Lussac and Biot conclude from their observations[1], that the magnetic force is the same at a height of four miles as at the surface of the earth. M. Kupffer, however, draws from Gay-Lussac's results the conclusion, that there was a little diminution, and Professor Forbes, from his experiments made in different parts of Europe[2], concludes that there is a decrease of the force upwards. Such decrease may be a real consequence due to the difference of distance from the source of the terrestrial magnetic force; or, as is more likely, it may be due to the different proportions of oxygen there and at the surface of the earth. According to Gay-Lussac's account of the air brought from above, it was as 0·5 to 1·0, compared with the density below. Hence the paramagnetic power, added to space in the place above, from whence the air was taken, would not be more than one-half of that added by the presence of the denser atmosphere below. This I think ought to make a change in the dis-

[1] Annales de Chimie, Ann. xiii. vol. lii. p. 86.
[2] Edin. Phil. Trans. 1836, vol. xiv. p. 25.

tribution of the magnetic force; it would almost certainly do so at the equator, where the lines of force are parallel to the general direction of the atmosphere (2881.); and I think it would do so, as to the horizontal component, in the latitude where Gay-Lussac and Biot made their aërial voyages. It is also just possible that the observers may have been in such relation to the heated or cooled air about them as to have had the difference observed produced, or rather affected, by some of the circumstances just described (2951.).

2962. Whether the result obtained by Gay-Lussac and Biot indicate a change of power due to distance or not, this we know, that there *are* great changes from the magnetic equator toward the north and south; and that, as Humboldt and Bessel say, it is doubled in proceeding from the equator to the western limits of Baffin's Bay. And when so little as one-third of a cubic inch of oxygen can exert a force equal to the tenth of a grain, subject to the action of our powerful magnet, we may well conceive that the enormous sum of oxygen present, in only a few miles of heated or cooled atmosphere, can compensate for the great difference of magnetic force, and so by a change of place, cause currents or winds having their origin in magnetic power. In such a case we should have a relation of magnets to storms; and the magnetic force of the earth would have to do with the mechanical adjustments and variations of the atmosphere, sometimes causing currents which without it might not exist, and at other times opposing those which might else arise, according as the great differential relations by which it would act (2757.) should combine with or oppose the other natural causes of motion in the air. Such movements would react upon the magnetic forces, so that these would readjust themselves, and so there would be magnetic storms, both material and potential, in the atmosphere, as there are supposed to be of the latter kind in the earth.

2963. In bringing this communication to a close, I have to express my obligations to two kind and able friends, Colonel Sabine and Professor Christie, for the interest they have taken in the subject, and on the part of the former for the extreme facilities afforded me in the use of observations and the data derived from them; but in doing so I must be careful not to convey any idea that they are at all responsible for the peculiar views I have ventured to put forth. I may well acknowledge that much which I have written has been upon very insufficient consideration; but hoping that there might be some foundation of truth in the account of the physical cause of the variations which I have ventured to suggest, I have not hesitated to put it forth, trusting that it might be for the advantage of science. The magnetic properties and relations of oxygen are perfectly clear and distinct, and are established by experiment (2774. 2780.); and it is no assumption to carry these properties into the atmosphere, because the atmosphere, as a mere mixture of oxygen and nitrogen, is shown to possess them also (2862.)[1]. It varies in its magnetic powers, by causes which act upon it under natural circumstances, and make it able to produce some such effects as those I have endeavoured generally to describe.

2964. If it be a cause, in part only, of the observed magnetic variations, it is most important to identify and distinguish such a source of action, even though imperfectly; for the attention is then truly and intelligently directed in respect of the action and the phænomena it can produce. The assigned cause has the advantage of occurring periodically and for the same periods, as a large class of the effects supposed to be produced by it; and if the agreement should appear at first only general, still that agreement will greatly strengthen its claim to our attention. It has the advantage of offering explanations and even suggestions of many other magnetic events besides those which are periodical, and it presents itself at a time when we have no clear knowledge of any other physical cause for the variations, but are constrained vaguely to refer them to imaginary currents of electricity in the air or space above, or in the earth beneath.

2965. The causes, both of the original power and of its secular variations, are unknown to us. But if, accepting the earth as a magnet, we should be able to distinguish largely between internal and external action, and so separate a great class of phænomena from the rest, we should be enabled to define more exactly that which we require to know in both directions, should be competent to state distinctly the problems which need solution, and be far better able to appreciate any new hints from nature respecting the *source* of the power and the effects that it presents to us.

2966. The magnetic constitution of oxygen seems to me won-

[1] Philosophical Magazine, 1847, vol. xxxi. pp. 406, 409.

derful. It is in the air what iron is in the earth. The almost entire disappearance of this property also, when it enters into combination, is most impressive, as in the oxynitrogens and oxycarbons, and even with iron, which it reduces into a condition far below either the metal or the oxygen, weight for weight. Again, its striking contrast with the nitrogen, which dilutes it, impresses the mind, and by the difference recalls that which also exists between them in relation to static electricity (1464.) and the lightning flash. Chlorine, bromine, cyanogen and its congeners, chemically speaking, have no magnetic relation to oxygen. In nature it stands in this respect, as in all its chemical actions, alone.

2967. There is much to do with oxygen relative to atmospheric magnetism. Its proportion of paramagnetic force at different temperatures and different degrees of rarefaction, will require to be accurately ascertained, and this I hope to effect by a torsion balance, in course of construction (2783.). Indeed, I hope that this great subject may be largely touched and tried by experiment as well as by observation, and therefore gladly make it part of these experimental researches.

2968. One can scarcely think upon the subject of atmospheric magnetism without having another great question suggested to the mind (2442.), What is the final purpose in nature of this magnetic condition of the atmosphere, and its liability to annual and diurnal variations, and its entire loss by entering into combination either in combustion or respiration? No doubt there is one or more, for nothing is superfluous there. We find no remainders or surplusage of action in physical forces. The smallest provision is as essential as the greatest. None are deficient, none can be spared.

Royal Institution,
September 14, 1850.

APPENDIX.

Received November 12, 1850.

The following Tables of data obtained at Toronto, St. Petersburgh, Washington, Lake Athabasca and Fort Simpson, supplied to me by the kindness of Colonel Sabine, have not yet been published. The data for Hobarton and Greenwich are in the volumes of observations for those stations.

Toronto.—Longitude 77° 5′ W. Latitude 43° 40′ N. Approximate declination 1° 25′ W. Mean inclination 75° 15′ N.

Diurnal variation of the Declination in the several months, from July 1842 to June 1848 inclusive.

Increasing numbers denote a movement of the south or upper end of the magnet towards the West.

Toronto mean time.	0h. Noon.	1h.	2h.	3h.	4h.	5h.	6h.	7h.	8h.	9h.	10h.	11h.	12h. Midn.	13h.	14h.	15h.	16h.	17h.	18h.	19h.	20h.	21h.	22h.	23h.	Daily means.
	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	
January	0·87	0·00	0·00	0·53	1·44	2·28	2·89	3·47	4·43	4·53	4·39	4·06	3·66	3·42	3·63	3·87	4·46	3·84	3·88	4·44	5·48	5·80	4·79	3·03	3·30
February......	0·78	0·00	0·06	0·94	1·58	1·91	2·73	3·13	4·02	4·44	4·69	4·45	4·31	3·81	3·54	4·07	3·97	4·86	4·95	5·20	5·97	5·70	4·64	2·53	3·43
March.........	1·44	0·01	0·00	0·59	1·74	2·90	3·82	4·77	5·63	6·60	6·47	6·86	6·04	6·42	6·55	6·34	6·69	6·59	7·10	8·30	9·38	9·40	7·15	4·22	5·21
April	1·25	0·00	0·16	1·02	2·76	4·12	5·14	6·31	7·05	7·14	7·60	7·66	7·39	7·32	7·65	7·82	8·08	8·33	9·46	10·09	10·20	9·10	6·87	3·74	6·09
May............	1·16	0·00	0·29	1·56	3·24	5·11	6·08	6·27	6·18	6·75	7·36	7·49	7·40	7·26	6·72	7·10	7·80	9·70	11·02	12·16	12·03	10·28	6·90	3·52	6·39
June	1·37	0·00	0·02	0·94	2·53	4·35	5·45	6·11	6·10	6·66	6·66	6·87	6·77	6·50	6·37	6·36	7·29	9·03	11·34	12·34	12·09	10·54	7·67	4·04	6·12
July............	1·53	0·09	0·00	0·84	2·18	3·98	5·20	5·77	6·31	6·34	7·31	7·38	7·67	7·06	6·42	6·37	6·86	8·53	10·54	12·01	12·22	10·69	7·65	4·27	6·13
August	1·11	0·00	0·73	2·61	4·52	6·12	7·60	7·52	8·05	8·97	8·51	8·40	8·14	7·69	7·65	7·83	8·24	9·50	12·09	13·89	13·79	11·51	7·31	3·73	7·31
September ...	0·03	0·00	0·76	2·92	4·62	6·04	6·78	6·83	7·62	7·84	7·83	7·08	7·50	7·66	7·53	7·77	8·45	8·38	9·79	11·16	10·24	8·60	5·34	2·25	6·37
October	0·48	0·00	0·31	1·38	2·38	3·06	3·84	4·33	4·92	5·76	5·68	5·48	5·00	5·21	4·89	5·16	5·82	5·74	5·50	6·39	7·32	7·02	5·17	2·63	3·89
November ...	0·75	0·00	0·34	1·37	2·21	2·91	3·96	4·89	5·16	5·70	5·54	5·26	4·51	3·79	3·80	4·51	4·44	4·99	4·95	5·70	6·31	6·08	4·57	2·35	3·92
December ...	1·20	0·18	0·00	0·61	1·67	2·56	3·30	3·89	4·39	4·62	4·95	4·48	4·09	3·49	3·04	3·56	3·92	4·02	3·81	4·18	4·50	5·22	4·67	2·83	3·30

Mean Diurnal variation of the Inclination in the several months, from July 1842 to June 1848.

Increasing numbers denote increasing inclination.

January	1·02	0·77	0·53	0·22	0·00	0·09	0·16	0·21	0·25	0·33	0·34	0·38	0·47	0·54	0·58	0·45	0·33	0·28	0·22	0·21	0·36	0·54	1·04	1·06	0·43
February......	0·86	0·61	0·36	0·12	0·01	0·00	0·09	0·12	0·19	0·26	0·27	0·31	0·37	0·45	0·49	0·44	0·41	0·30	0·27	0·44	0·69	0·70	0·79	0·86	0·39
March.........	1·02	0·77	0·47	0·14	0·00	0·00	0·12	0·22	0·24	0·35	0·40	0·44	0·45	0·53	0·50	0·50	0·46	0·46	0·47	0·62	0·81	0·95	1·00	1·05	0·50
April	0·99	0·77	0·54	0·17	0·05	0·00	0·14	0·35	0·49	0·53	0·49	0·61	0·56	0·59	0·64	0·61	0·60	0·66	0·74	0·80	0·90	1·03	1·16	1·16	0·61
May............	0·83	0·52	0·22	0·06	0·00	0·11	0·27	0·33	0·47	0·62	0·64	0·66	0·76	0·86	0·87	0·89	0·92	1·04	1·00	1·01	1·11	1·29	1·30	1·12	0·70
June	0·89	0·60	0·26	0·05	0·00	0·00	0·16	0·34	0·53	0·68	0·73	0·83	0·84	0·90	0·91	0·98	1·03	1·05	1·08	1·12	1·17	1·27	1·37	1·19	0·75
July............	0·67	0·55	0·17	0·07	0·00	0·06	0·17	0·24	0·39	0·40	0·51	0·56	0·62	0·56	0·66	0·78	0·83	0·87	0·81	0·80	0·88	1·03	1·14	0·93	0·57
August	0·85	0·51	0·21	0·00	0·02	0·04	0·25	0·40	0·42	0·44	0·48	0·55	0·53	0·64	0·65	0·73	0·74	0·84	0·89	1·00	1·09	1·25	1·26	1·13	0·62
September ...	1·35	0·82	0·35	0·10	0·00	0·10	0·29	0·49	0·53	0·55	0·57	0·63	0·68	0·70	0·70	0·75	0·57	0·65	0·68	0·95	1·28	1·64	1·85	1·71	0·75
October	0·93	0·67	0·39	0·14	0·00	0·06	0·21	0·30	0·37	0·39	0·40	0·49	0·62	0·45	0·40	0·33	0·22	0·22	0·32	0·55	0·85	1·04	1·15	1·11	0·48
November ...	0·94	0·75	0·45	0·22	0·11	0·18	0·12	0·18	0·20	0·20	0·23	0·33	0·33	0·31	0·21	0·22	0·12	0·07	0·00	0·08	0·42	0·14	0·89	1·00	0·32
December ...	0·91	0·88	0·51	0·27	0·05	0·01	0·02	0·16	0·21	0·27	0·33	0·30	0·38	0·34	0·28	0·22	0·14	0·08	0·09	0·00	0·17	0·34	0·66	0·81	0·31
Hourly means	0·94	0·69	0·37	0·13	0·02	0·05	0·17	0·28	0·36	0·42	0·45	0·51	0·55	0·57	0·57	0·58	0·53	0·54	0·55	0·63	0·81	0·93	1·13	1·10	0·54

Toronto.—Mean Diurnal variation of the Total Force in the several months, from July 1842 to June 1848. Increasing numbers denote increasing Force. Mean Total Force at Toronto 13·9. The figures express the changes in parts of the whole Force.

Toronto mean time	0h.	1h.	2h.	3h.	4h.	5h.	6h.	7h.	8h.	9h.	10h.	11h.	12h.	13h.	14h.	15h.	16h.	17h.	18h.	19h.	20h.	21h.	22h.	23h.	Daily means.
	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00
January...	005	011	017	023	022	024	023	023	023	020	019	015	010	009	004	003	004	005	008	009	010	003	000	000	012
February .	006	012	019	022	024	026	026	027	025	023	019	017	009	005	003	004	003	006	009	010	016	004	000	001	013
March ...	004	011	019	026	029	031	031	031	028	025	020	014	005	002	002	002	003	003	010	013	013	007	002	000	014
April	012	020	031	039	045	045	044	039	034	027	020	012	008	007	003	000	009	011	013	012	012	009	006	006	019
May	004	013	015	031	038	042	038	035	029	025	017	011	004	002	002	003	008	010	015	013	009	003	000	000	015
June	003	008	017	025	029	031	030	028	022	018	014	011	006	002	002	001	006	011	014	014	010	005	002	000	013
July	015	019	030	038	044	047	043	039	031	026	021	015	007	004	000	002	005	011	016	017	015	014	010	002	020
August ...	027	034	044	042	057	057	051	046	041	032	023	010	006	001	003	000	006	018	026	026	024	019	019	021	026
September	029	043	053	060	060	056	052	050	044	038	034	029	019	007	000	001	004	009	018	022	028	015	016	021	030
October...	013	021	027	032	032	033	033	034	031	026	023	018	007	003	005	002	000	005	011	014	018	011	008	006	017
November	007	014	022	026	028	025	025	028	024	018	015	011	007	004	001	003	000	003	007	009	008	002	001	003	011
December	004	010	019	020	023	021	021	021	019	018	017	012	010	008	006	007	003	003	007	006	006	006	001	000	011
Means ...	011	018	026	032	036	037	035	033	029	025	020	015	008	005	003	002	004	008	013	014	014	008	005	006	017

Mean Temperature of the Air in the several months, from July 1842 to June 1848, in degrees of Fahrenheit's scale.

	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
January...	27·8	28·3	28·6	28·5	28·0	27·1	26·3	25·8	25·5	25·3	26·3	24·6	25·6	23·5	23·4	21·6	23·2	23·0	23·7	23·6	23·9	24·8	26·0	27·1	25·48
February .	27·4	28·2	28·5	28·5	28·0	26·9	25·6	24·6	23·8	23·2	22·7	22·2	22·0	21·7	21·3	20·9	20·5	20·2	20·0	19·9	20·9	22·9	24·9	26·3	23·80
March ..	33·8	34·4	35·0	34·9	34·3	33·6	31·9	30·5	29·5	28·6	28·0	27·3	27·3	26·8	26·3	26·2	25·8	25·5	25·0	25·9	27·8	29·9	31·6	32·8	29·71
April......	47·6	48·5	49·0	49·0	48·6	47·9	46·1	43·5	41·8	40·7	39·8	39·3	39·1	38·9	37·9	37·3	37·0	36·6	36·8	39·2	41·5	43·6	45·2	46·6	42·55
May	59·0	59·9	60·2	60·3	60·3	59·9	58·1	55·2	52·6	50·7	49·7	48·8	48·0	46·9	46·3	45·6	45·1	45·2	47·6	50·6	53·0	55·2	56·8	58·0	53·04
June	66·7	67·5	67·9	68·3	68·5	68·1	66·6	63·9	60·6	58·4	57·0	56·1	55·7	54·8	54·1	53·3	52·7	52·9	55·6	58·5	60·8	62·7	64·4	65·6	60·86
July	72·9	73·7	74·5	74·8	74·7	74·3	72·7	69·2	65·1	62·9	61·8	60·7	59·8	58·9	58·3	57·6	56·9	56·9	59·8	63·4	66·0	68·2	70·1	71·5	66·02
August ...	73·0	73·9	74·5	74·8	74·6	74·1	72·5	68·4	65·3	63·6	62·4	61·6	60·7	60·0	59·4	58·7	58·3	58·1	59·6	63·2	66·3	68·7	70·6	72·1	66·43
September	63·9	64·5	64·9	64·9	64·7	63·7	61·1	58·3	57·1	56·0	54·9	54·2	53·8	53·3	52·7	52·2	51·6	51·1	51·7	54·2	57·0	59·5	61·5	62·9	57·90
October...	49·7	50·3	50·5	50·3	49·6	47·8	45·8	44·7	43·9	43·1	42·3	41·6	41·0	40·4	40·1	39·9	39·5	39·4	39·6	40·4	42·8	45·6	47·5	48·9	44·35
November	39·5	40·0	40·1	39·9	39·0	37·7	36·9	36·3	35·9	35·7	35·3	34·9	34·3	34·0	33·7	33·4	33·2	33·3	33·6	33·6	34·6	36·2	37·7	38·8	36·15
December	29·9	30·4	30·7	30·5	29·8	28·9	28·2	27·9	27·5	27·3	27·0	26·9	26·6	26·0	25·7	25·5	25·5	25·5	25·1	25·0	25·4	26·6	27·9	29·1	27·44
Means ...	49·27	49·97	50·37	50·39	50·01	49·17	47·65	45·69	44·05	42·96	42·27	41·52	41·16	40·41	39·93	39·35	39·11	38·97	39·84	41·46	43·33	45·32	47·02	48·31	44·48

St. Petersburgh.—Longitude 38° 18′ East. Latitude 59° 57′ North. Mean declination 6° 10′ West.

Mean Diurnal variation of the Declination in the several months, from 1841 to 1845 inclusive.

Increasing numbers denote a movement of the south or upper end of the magnet towards the West.

Mean time.	Noon. h m 0 21½	h m 1 21½	h m 2 21½	h m 3 21½	h m 4 21½	h m 5 21½	h m 6 21½	h m 7 21½	h m 8 21½	h m 9 21½	h m 10 21½	h m 11 21½	Midn. h m 12 21½	h m 13 21½	h m 14 21½	h m 15 21½	h m 16 21½	h m 17 21½	h m 18 21½	h m 19 21½	h m 20 21½	h m 21 21½	h m 22 21½	h m 23 21½	Means.
	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	
January...	0·27	0·00	0·36	1·24	1·59	1·99	2·39	3·28	4·03	4·65	4·56	4·65	4·43	3·72	3·77	3·10	2·57	2·22	2·35	2·17	2·08	1·86	1·42	1·02	2·49
February .	0·84	0·00	0·09	0·57	1·55	2·84	3·54	3·63	4·03	5·18	5·67	5·49	5·27	4·52	4·34	4·65	4·34	3·90	4·03	3·77	3·50	3·23	2·61	1·46	3·29
March ...	0·89	0·00	0·04	1·15	2·88	4·16	5·36	6·29	6·73	6·78	7·40	7·84	6·69	6·51	5·76	6·11	6·38	6·16	6·65	6·56	6·73	6·38	4·96	2·79	5·05
April	1·59	0·00	0·36	2·17	4·03	5·54	7·04	7·93	8·33	8·02	9·30	9·17	8·68	8·46	8·73	8·59	9·17	9·30	9·48	10·01	10·68	9·75	7·58	4·61	7·01
May	1·02	0·00	0·62	2·13	3·68	5·49	6·96	7·44	7·18	6·82	7·44	8·24	8·55	8·73	8·82	9·66	10·23	10·72	11·12	11·39	11·12	9·92	7·49	3·54	7·01
June	1·77	0·00	0·00	1·37	2·92	4·52	5·76	6·25	6·25	6·56	7·13	7·13	7·49	8·28	8·68	9·70	10·23	10·94	11·30	11·39	11·52	10·10	7·35	4·12	6·70
July	1·46	0·04	0·00	1·11	2·39	3·94	4·96	5·27	5·98	5·98	6·78	7·49	7·66	7·71	8·42	8·55	9·30	9·52	9·83	9·75	9·04	8·46	6·96	3·90	6·02
August ...	0·27	0·00	0·40	2·44	4·16	5·94	7·04	6·73	7·49	7·27	7·35	7·27	8·02	8·11	8·06	8·55	9·30	9·88	10·19	10·19	9·44	8·42	5·67	3·15	6·47
September	0·57	0·00	0·84	2·57	4·30	6·33	6·78	7·49	7·97	8·06	8·33	7·49	7·04	7·31	7·40	6·78	7·04	6·91	7·27	7·31	7·13	6·25	4·12	1·95	5·72
October...	0·31	0·00	0·48	1·77	3·59	4·08	4·87	5·76	6·02	6·56	6·91	6·20	5·71	5·18	4·78	4·39	4·16	3·99	4·21	4·83	5·09	4·65	3·32	1·59	4·10
November	0·09	0·00	0·36	1·55	1·90	2·71	2·79	3·15	3·81	4·39	4·47	4·34	3·68	3·06	2·75	2·35	2·22	1·82	1·82	1·90	1·77	1·77	0·93	0·57	2·26
December	0·53	0·00	0·36	1·24	1·82	2·39	2·79	2·97	4·03	4·56	4·56	4·52	3·68	3·10	2·88	2·53	2·04	1·82	1·77	1·77	1·82	1·68	1·28	0·93	2·29
Means ...	0·80	0·00	0·33	1·61	2·90	4·16	5·02	5·52	5·99	6·24	6·66	6·65	6·41	6·22	6·20	6·25	6·42	6·43	6·67	6·75	6·66	6·04	4·47	2·47	4·87

Mean Temperature of the Air in the several months, from 1841 to 1845 inclusive. Fahrenheit's scale.

	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	
January...	20·28	20·71	20·67	20·41	19·82	19·89	19·73	19·71	19·74	19·76	19·67	19·49	19·38	19·33	19·29	19·19	19·04	18·90	18·79	18·79	18·74	18·81	19·24	19·98	
February .	18·34	18·58	18·70	18·65	18·27	17·35	16·88	16·52	16·32	16·09	15·95	15·67	15·55	15·48	15·39	15·19	14·97	14·88	14·76	14·65	15·19	15·66	16·61	17·35	
March ...	25·61	26·30	26·97	27·01	27·01	26·13	24·74	23·88	23·20	22·44	21·94	21·31	20·70	20·07	19·62	19·26	18·84	18·54	18·34	18·74	19·87	21·38	22·95	24·53	
April	37·85	38·07	38·58	38·81	39·53	38·63	36·54	34·62	33·35	32·38	31·42	30·72	30·07	29·55	29·08	28·61	28·36	28·42	29·21	30·63	32·16	33·75	35·39	36·79	
May	53·46	53·80	54·31	54·72	55·10	54·29	52·23	49·84	47·91	46 19	45·38	43·90	43·16	42·37	41·83	41·47	41·56	42·73	44·19	46·06	47·88	49·57	51·17	52·65	
June	63·03	63·48	63·83	64·36	64·97	64·07	61·90	59·97	58·19	56·48	55·04	54·07	53·26	52·58	52·00	51·50	52·27	53·33	54·95	56·57	58·21	59·72	59·02	62·15	
July	66·44	66·93	67·21	67·46	67·93	67·26	65·28	63·61	62·01	60·57	59·56	58·86	58·05	57·33	56·86	56·48	56·68	57·53	58·86	60·59	62·06	63·43	64·76	65·90	
August ...	67·10	68·00	68·56	68·69	69·23	68·56	65·73	63·70	62·26	60·89	60·08	59·22	58·14	57·60	57·20	56·57	56·14	56·73	57·65	59·45	61·29	63·12	64·71	66·06	
September	54·50	55·66	55·91	55·78	55·83	54·56	52·62	51·16	50·15	49·43	48·89	48·31	47·75	47·25	46·85	46·44	46·26	46·06	46·17	47·21	48·76	50·56	52·20	53·37	
October...	41·92	44·80	42·76	41·98	41·78	40·92	40·30	39·89	39·56	39·31	38·86	38·52	38·32	38·14	38·00	37·85	37·73	37·67	37·53	37·71	38·28	39·11	40·12	41·04	
November	29·73	30·22	30·88	29·75	29·26	28·96	28·83	28·67	28·58	28·41	28·52	28·49	28·47	28·45	28·42	28·27	28·13	28·04	28·00	27·95	28·07	28·42	28·83	29·37	
December	26·04	26·45	26·40	26·27	26·13	25·91	25·70	26·02	25·52	25·48	25·48	25·32	25·21	25·12	24·96	24·85	24·85	24·92	25·03	25·03	25·14	25·25	25·39	25·79	

Washington, U.S.—Longitude 77° 2′ West. Latitude 38° 54′ North. Mean declination 1° 25′ West. Mean dip 71° 20′ North.

Mean Diurnal variation of the Declination in minutes, and temperature in Fahrenheit's scale, of the months of the years 1840, 1841, 1842, which are specified.

Increasing numbers denote a movement of the south or upper end towards the East.

Mean time ...	Noon. h m 0 12	h m 2 12	h m 4 12	h m 6 12	h m 8 12	h m 10 12	h m 12 12	h m 14 12	h m 16 12	h m 18 12	h m 20 12	h m 22 12	Mean.
	′	′	′	′	′	′	′	′	′	′	′	′	′
Jan. 1841–42.	4·10	5·20	3·96	2·52	0·87	0·68	1·01	1·62	1·66	1·71	0·91	0·29	2·04
Feb. 1841–42.	3·55	5·28	4·22	2·89	1·59	0·94	0·84	1·26	0·81	0·82	0·60	0·35	1·76
Mar. 1841–42.	6·34	7·51	6·26	4·25	2·88	2·31	2·50	3·00	1·71	1·13	0·00	1·76	3·30
Apr. 1841–42.	6·56	8·33	6·42	4·41	3·22	1·97	2·24	1·35	0·73	0·46	0·00	2·18	3·15
May 1841–42.	7·72	8·57	6·36	4·45	3·82	3·47	3·25	3·05	2·63	0·33	0·00	4·25	3·99
June 1841–42.	8·55	9·47	8·00	5·33	4·91	4·24	4·05	4·32	3·44	0·63	0·00	4·53	4·79
July 1840–41.	8·42	9·87	8·07	5·75	4·57	3·52	3·60	3·40	2·99	0·98	0·00	3·62	4·56
Aug. 1840–41.	10·94	10·81	7·95	6·00	4·03	3·55	4·48	4·64	4·04	1·35	0·00	5·89	5·30
Sept. 1840–41.	8·76	8·44	5·43	4·45	2·62	3·31	2·83	2·86	2·44	0·87	0·00	4·22	3·85
Oct. 1840–41.	5·65	5·83	4·35	2·47	1·41	0·58	1·41	1·80	1·51	1·35	0·00	1·75	2·34
Nov. 1840–41.	4·69	4·79	3·33	1·60	0·51	0·74	1·14	1·41	0·82	0·71	0·00	1·83	1·79
Dec. 1840–41.	3·93	4·90	3·39	1·92	0·10	0·36	0·62	1·53	0·93	2·00	1·02	0·87	1·79

Temperature.

Mean time ...	Noon. h m 0 12	h m 2 12	h m 4 12	h m 6 12	h m 8 12	h m 10 12	h m 12 12	h m 14 12	h m 16 12	h m 18 12	h m 20 12	h m 22 12
Jan. 1841–42.	38°28	40°83	40°18	36°68	35°47	34°24	32°37	32°10	31°71	30°53	31°63	36°96
Feb. 1841–42.	40·03	42·51	42·28	38·22	35·38	33·86	32·58	31·22	30·51	30·18	31·44	36·72
Mar. 1841–42.	51·39	53·61	53·28	49·96	46·20	44·37	42·48	41·26	40·06	39·87	42·28	48·06
Apr. 1841–42.	57·68	59·81	60·20	57·21	52·18	49·12	47·91	46·91	46·12	46·49	49·93	54·02
May 1841–42.	66·37	68·48	68·69	65·93	59·83	56·70	55·19	53·34	52·42	55·50	59·72	63·23
June 1841–42.	79·32	81·85	82·75	76·89	72·29	68·70	66·83	66·04	65·07	68·26	73·63	77·37
July 1841......	81·13	81·53	84·60	81·33	74·93	71·56	68·78	68·09	66·78	70·64	75·19	78·38
Aug. 1840–41.	78·70	80·73	80·09	75·93	71·48	68·00	66·82	65·12	64·17	65·69	65·73	76·09
Sept. 1841 ...	74·66	76·50	76·30	72·30	68·59	64·90	62·70	61·90	61·00	61·29	65·73	71·02
Oct. 1841......	55·30	57·00	56·20	52·94	48·40	46·60	44·90	43·70	42·30	41·70	45·00	51·61
Nov. 1841 ...	48·00	49·20	48·50	47·30	44·20	43·20	41·80	40·70	39·40	38·80	39·50	44·10
Dec. 1841 ...	39·20	41·30	40·60	37·95	36·26	34·70	33·50	33·16	32·20	31·60	31·69	36·00

Lake Athabasca.—Longitude 111° 18′ West. Latitude 58° 41′ North. Mean declination 28° East.
Diurnal variation of the Declination in the months of October, November and December 1843, and Jan. and Feb. 1844.
Increasing readings denote a movement of the south or upper end of the magnet towards the West.

Mean time.	h m 12 55	h m 13 55	h m 14 55	h m 15 55	h m 16 55	h m 17 55	h m 18 55	h m 19 55	h m 20 55	h m 21 55	h m 22 55	5 minutes to noon. h m 23 55	h m 0 55	h m 1 55	h m 2 55	h m 3 55	h m 4 55	h m 5 55	h m 6 55	h m 7 55	h m 8 55	h m 9 55	h m 10 55	5 minutes to midnight. h m 11 55	Mean time.
1843.																									
Oct. ...	1′·13	7′·49	6′·23	18′·95	21′·32	8′·55	13′·35	9′·90	10′·72	7′·03	2′·29	2′·70	0′·00	0′·29	0′·02	0′·98	0′·49	2′·88	1′·88	3′·75	3′·47	3′·30	8′·42	0′·49	13 days obs. in Oct. 1843.
Nov....	3·73	5·17	6·74	9·07	10·35	10·80	9·40	8·80	7·63	6·54	1·10	0·00	1·06	1·57	1·93	2·73	2·94	3·34	4·52	4·97	5·43	7·08	3·04	3·63	25 days obs. in Nov. 1843.
Dec....	3·04	6·16	5·57	5·65	9·23	7·61	9·78	6·73	6·58	5·33	1·94	0·55	0·03	0·00	0·83	2·37	2·63	3·81	4·18	3·57	4·35	7·19	4·05	6·05	25 days obs. in Dec. 1843.
1844.																									
Jan. ...	5·69	8·48	6·97	12·57	16·29	10·05	9·97	10·02	9·96	7·95	4·99	2·10	0·39	0·00	2·75	1·67	2·47	3·63	4·72	5·15	5·44	3·97	6·71	2·18	25 days obs. in Jan. 1844.
Feb. ...	4·27	4·93	8·46	10·55	14·80	14·87	14·06	12·10	12·76	8·64	2·82	2·93	1·55	0·00	1·48	3·24	3·76	4·52	4·96	5·48	5·58	4·99	6·98	5·40	25 days obs. in Feb. 1844.

Fort Simpson.—Longitude 121° 30′ West. Latitude 61° 52′ North.
Diurnal variation of the Declination in the months of April and May 1844.
Increasing numbers denote a movement of the south or upper end of the magnet towards the West.

Mean time.	15 minutes past midnight. h m 12 15	h m 13 15	h m 14 15	h m 15 15	h m 16 15	h m 17 15	h m 18 15	h m 19 15	h m 20 15	h m 21 15	h m 22 15	h m 23 15	15 minutes past noon. h m 0 15	h m 1 15	h m 2 15	h m 3 15	h m 4 15	h m 5 15	h m 6 15	h m 7 15	h m 8 15	h m 9 15	h m 10 15	h m 11 15	Mean time.
1844.																									
April ..	5′·82	6′·03	17′·45	25′·16	22′·90	36′·80	36′·26	37′·60	30′·14	25′·27	18′·77	8′·27	5′·72	2′·16	0′·00	1′·37	1′·23	0′·43	0′·30	4′·04	4′·62	1′·26	2′·86	5′·55	20 days obs. in Apr. 1844.
May ...	1·56	7·77	11·71	15·63	17·19	23·73	28·22	32·69	29·80	20·29	15·51	9·49	4·74	3·75	0·70	0·75	0·80	0·00	0·19	1·90	6·72	3·51	2·29	1·67	18 days obs. in May 1844.

TWENTY-SEVENTH SERIES[1].

§ 33. *On Atmospheric Magnetism—continued.*

Received November 19,—Read November 28, 1850.

¶ ii. *Experimental inquiry into the laws of atmospheric magnetic action, and their application to particular cases.*

2969. Believing that experiment may do much for the development of the general principles of atmospheric magnetism, and produce rapidly a body of facts on which philosophers may proceed hereafter to raise a superstructure, I endeavoured to find some means of representing practically the action of the atmosphere, when heated by the sun, upon the terrestrial magnetic curves. The object was to obtain some central arrangement of force which should deflect these curves or lines as they are deflected in a diamagnetic conductor or globe of hot air (2877.), and then apply the results obtained by such an arrangement as a partial test to the various cases supplied by the magnetic observatories scattered over the earth. At first I endeavoured, for the sake of convenience, to attain this desired end by means of a horseshoe magnet, employing the lines which passed from pole to pole to disturb and re-arrange the earth's force; but the comparative weakness of the terrestrial force near the magnet, and the great prominence of the poles of the latter, gave rise to many inconveniences, which soon caused me to reject that method and have recourse to a ring-helix and voltaic apparatus. Considering the new use to which this helix is to be applied, the interest of the results, and the instruction that may be drawn from them, I shall be excused for being somewhat elementary in the description of its character and action.

2970. The helix consisted of about 12 feet of covered copper wire formed into a ring having about twenty-five convolutions, and being $1\frac{1}{2}$ inch in external diameter. The continuations of the wire were twisted together so as to neutralize any magnetic

[1] Philosophical Transactions, 1851, p. 85.

effect which they could produce, and were long enough to reach to a voltaic arrangement, and yet allow free motion of the helix. The requisite amount of magnetic power in the helix may be judged of by the following considerations:—Suppose a declination needle freely suspended; and then the *helix* placed at a distance in the prolongation of the needle with its axis in a line with the latter, and with that side towards the needle which will at small distances cause repulsion. The needle will point, in the magnetic meridian, with a certain amount of force; but as the helix is brought near it will point with less force, and within a certain distance will no longer point in the magnetic meridian, but either on one or the other side of it. There is a given distance within which the needle, when in the magnetic meridian, is in a position of unstable equilibrium, but beyond which it has a position of stable equilibrium, the distance varying with the strength of the exciting electric current. The power of the helix should be such, that when end on to the needle the latter has a position of stable equilibrium in the meridian. One pair of plates is quite sufficient to make the helix as magnetic as is needful for distances varying from 4 to 24 inches. When a needle is properly arranged with either a magnet or a helix to the north or south of it as above described, if the magnet or helix be moved west the near end of the needle will move east, and contrariwise.

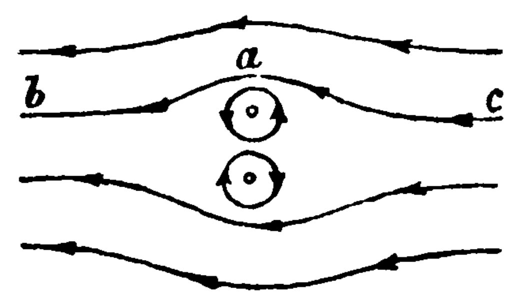
2971. As is well known, such a helix has a system of magnetic lines, which, passing through its axis, then opens out and turning round on the outside re-enters again at the axis, the circles of magnetic force being everywhere perpendicular to the electric current traversing the convolutions of the helix; and now I had, at a moment's notice, a source of lines of magnetic power exactly of the kind required to produce, in association with those of the earth, a disposition of the forces coinciding either with those of paramagnetic or diamagnetic polarization (2865. 2877.).

2972. For let fig. 17 represent a section parallel to the axis of the ring-helix, then the two circles may represent the disposition of the magnetic force in that section, and the arrow-heads may serve to indicate that magnetic direction which belongs to lines of force issuing out of the north end of a magnet. If such a system be suddenly produced in the midst of the earth's

Fig. 17.

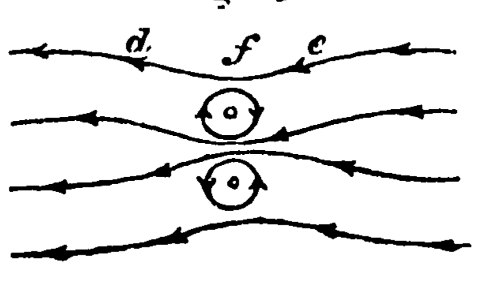
lines, it acts upon them according to the position of the helix in relation to the direction of the earth's power. Choosing the two positions in which the axis of the helix is parallel to the natural direction of the power, as shown by a free needle, at the place of observation, then two contrary effects are produced, which, as regards the lines exterior to the helix system, correspond to the polarity of paramagnetic and diamagnetic conductors. If, for instance, the helix is so placed that the polarity of its magnetic lines, exterior to and in the plane of the ring, accords with that of the earth's force, as in fig 18, then the earth's lines are deflected as represented, and a magnetic needle placed at *a*, which had taken up its position by the earth's influence, will not tend to alter its position as the helix approaches it, though it will be acted on with more power. In other parts of the line, *b a c*, it will alter its position, standing as a tangent to the curvature, and therefore will be deflected sometimes one way and sometimes another, as it is carried along the line (or through the neighbouring lines), in place of remaining parallel to itself, as it would do if the electro-magnetic helix were away.

Fig. 18.



2973. On the other hand, if the helix were turned round into the second position (2972.), then the effect upon the direction of the neighbouring lines of force would be as in fig. 19. Needles placed at *d* and *e* would again be deflected from the natural position given to them by the earth, but they would be deflected in a contrary direction to that which would be taken if they were in corresponding situations under the former arrangement. This figure and state of things represents the paramagnetic disposition of the forces, as the former did the diamagnetic condition.

Fig. 19.



2974. It is not pretended that the whole of these arrangements of forces are like those of the cases of paramagnetic and diamagnetic conductors. Independent systems are here introduced into the midst of the earth's magnetic power, and the central part of each arrangement must therefore be excepted; there are also attractions inwards and repulsions outwards, when the needle is at *a* and *f*, which do not take place in the cases of mere magnetic conduction. But external to these helix systems, the arrangement imposed upon the lines of force from the earth is in accordance with that produced by diamagnetic and paramagnetic conductors, and at distances from 2 inches to 2 or 3 feet; the lines of force thus altered, and those contorted by the sun and atmosphere in the great field of nature, are comparable in their direction, and may be considered as representing each other.

2975. In order to obtain a simple result of the action of such a centre of force on the magnetic lines of the earth, I adjusted a rod in the direction of the dipping-needle, and also a plane at the foot of it parallel to the magnetic equator at London. Then suspending a small magnet, half an inch long, from cocoon-silk, so that when hanging it should be parallel to the magnetic equator, it was adjusted so as to be near to the plane at the foot of the rod representing dip. The ring-helix (2970.) was then associated with the voltaic pair, so that contact could be completed at any moment, and being always retained parallel to itself and to the plane of the magnetic equator, could be brought into the vicinity of the needle on all sides, above or below, and its action upon it observed. As the object was to represent the sun's action, the current was so sent through the helix that its upper face would repel the north end of a magnetic needle; for then a magnet, outside of and in the plane of the ring, would not tend to have its position changed, and the disposition of the forces of the earth under the influence of the helix was as in fig. 18, or like that of a diamagnetic conductor.

2976. In making observations of this kind, and especially if the ring-helix is purposely retained at a considerable distance from the needle, it is better not to connect the helix permanently with the battery and then carry it towards and by the needle, but rather to choose the place where the helix action is to be observed, and when the helix is there to make contact with the battery; the motion and direction of the needle is then easily observed; or if it still, through reason of distance, be feeble, making and breaking contact a few times isochronously with the vibrations of the needle soon raises the effect to any degree required.

2977. There are certain positions in respect of the needle as a centre which must be clearly comprehended. The magnetic axis is a line through the centre of the free regular needle parallel to the direction of the earth's lines of force, whatever that may be, at the place where the experiments may be made. The magnetic equator plane is a plane passing through the centre of the needle perpendicular to the magnetic axis. The plane of the magnetic meridian is that plane which coincides with the magnetic axis, and also with the direction in which the declination needle points. This position always occurs with the magnets that are employed for observation, being a consequence of the method in which they are supported; it would not be taken by a needle placed at right angles on its mechanical axis, the latter being in the magnetic axis.

2978. When the ring-helix, situated as before explained (2975.), was anywhere in the plane of the *magnetic meridian*, it exerted no action on the declination needle tending to change its position. When the helix was anywhere in the plane of the magnetic equator, it exerted no action on the needle to make it change its direction. These are the only places in which the helix does not affect the position of the needle.

2979. These two planes of no variation divide the space around the magnet into four quadrants, and the helix being in any one of these, affects the needle, altering its declination. The deflection of the line of force for two neighbouring quadrants is in the contrary direction, so that as the helix passes from the neutral line into one or the other quadrant, the declination of the needle changes.

2980. If the helix be above or below the magnetic equator and be carried round the magnetic axis travelling along a line of latitude, then the needle makes *one* large oscillation to the right, and another to the left during the circuit. Supposing that the experiment commences with the helix above the equator, and in the plane of the magnetic meridian north of the needle, if it then proceeds by west to south and on by east to its original position the north end of the needle will first go westward; will then stop and return eastward, passing the mean position, and will finally return westward and settle in its first or original direction. All the time the helix is to magnetic east of the needle it will cause the same deflection, and also as long as it is in the west; the deflection will be more or less, but not change in direction as regards the neutral place. The position of the helix north or south of the needle is of no consequence as to the direction of the declination, provided it remain on the same side of the magnetic meridian, though it is to the amount. If the helix be below the magnetic equator the direction of the declination is reversed, but then again it does not change whilst the helix remains east or west of the needle and its plane of mean declination.

2981. If we carry the helix round the needle in a plane perpendicular to the planes of the magnetic equator and meridian, so as to traverse in succession the four quadrants, then the needle makes *two* to and fro vibrations (instead of one) during the circuit. Thus, beginning with the helix in the neutral position over the needle and going round by west and below, and then upwards on the east side to its first position, the north end of the needle will first pass westward, then eastward, then westward, after that eastward, and finally westward to its original or neutral position.

2982. As the helix is carried from the neutral planes (2978.) into any of the quadrants, the power of affecting the declination of the needle is first developed, and then increases every way, from the edges of the quadrant until it attains its maximum force at the middle. Hence the maximum deflection east or west is when the helix is in the middle of each quadrant. Therefore, when the helix is carried from the middle of one quadrant to the middle of the next, only *one motion* in the needle appears; as for instance, an increasing westerly declination, though the direction of the declination in relation to the mean position has been reversed in that time, and there was a moment when the needle had no extra declination, but was *in* that mean position. So also as the helix moves over one quadrant from one neutral plane to another, though the declination of the needle produced by it has not changed in direction, but has been, for instance, all the time west, still the needle will have exhibited two motions going first west during the increase of the power, and then east whilst it is diminishing; and hence it is that though there are *four* departures of the needle from and return to the neutral or mean position, whilst the helix circumscribes it in an east and west vertical plane (2981.), there are only *two* complete journeys of the needle.

2983. The amount of the deflection diminishes as the distance of the helix from the needle increases; and the contrary.

2984. Two other needles were slung (2975.) very oblique to the magnetic axis, one with its north end upwards and the other with its north end downwards, and these were submitted to the action of the helix as the former had been (2978.). They were affected exactly in the same manner, showing no difference; *i. e.* a given end always moved the same way for the same change in position of the helix. If the helix was very near, then one pole was a little more influenced than the other in certain positions; but its removal further off took away that difference (which is easily accounted for (2970.)) and produced pure results. The place of the helix above or below the prolongation of the line of the needle made *no difference*, provided it was in the same place as regarded the magnetic equator of the earth's lines of force passing through the needle.

2985. For the purpose of establishing the nature of the action which such a helix, always in the given or diamagnetic position (2975.), would exert upon the *inclination*, a small dipping-needle was submitted to its action and the following results obtained. The needle could move in the plane passing through the magnetic meridian of London.

2986. There was no deflection of the needle when the helix was in the plane of the magnetic equator, or in a plane perpendicular to that containing the mechanical axis of the needle. In every other position it affected it; so that these two planes divided the sphere of action into four segments, as before.

2987. As the helix passes from one quadrant to another, the direction in which the needle is deflected changes as before (2982.). If the helix is in the upper north segment or the lower south segment, the upper or south end of the needle is deflected towards the south; if the helix be in the upper south or lower north segments, the upper or south end of the needle is deflected towards the north. If the helix be carried round the needle in the direction of the plane of motion, which in this case is that of the magnetic meridian, the end of the needle starting from a mean or unaffected position will move first one way, as for instance, north and then south; north again and south again, and finally north to regain its place of rest: so that there are two extreme deflections of the end in each direction, as before, in the case of the declination magnet (2982.).

2988. In other words, when the helix was anywhere below the magnetic equator, the *lower* or north end of the needle tended to point outwards from it or outside of it, being as it were *repelled* by the axis of the helix, but *drawn* by the outer curved lines of force, fig. 20 (2992.). Or if the helix were above the equator, then the *upper* or south end of the needle went outwards from the helix, moving exactly in the same direction in relation to the helix as the lower pole did before.

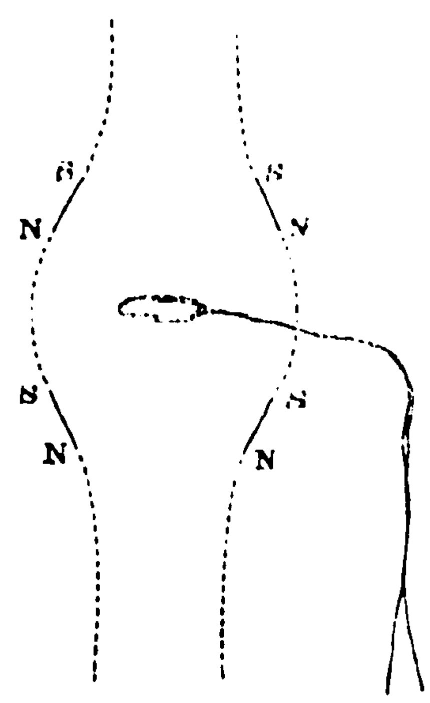
2989. The support of the needle was turned round 90°, which therefore removed the plane in which the needle could move 90° from the magnetic meridian. This carried the plane of no action on the needle 90° round, so that it now coincided with the magnetic meridian; and the plane, which, standing east and west, was before neutral, was no longer a plane of indifference, but in fact passed at the middle of the segments through the places of strongest action.

2990. Here, with *inclination*, as before with *declination*, it is not the direction in which the needle stands that determines what action the helix may have upon it; for it may be loaded or otherwise restrained, as all horizontal needles are; but it is the *direction* of the lines of force at the needle which, with the helix, governs all. The helix may be above or below the prolongation of the needle indifferently; for if it still continues on the same side of the line of force, under the influence of which the needle acts, then the end of the needle moves in the same direction, though it may travel towards the helix in one instance and from it in another.

2991. I suspended a needle so that it was free to move in every direction, and now I obtained the simple natural effect of the helix, or a diamagnetic globe (2877.) on a given line of force, and it is well to have it in mind. For, though we are obliged for the sake of practical observation to divide the position into two parts, declination and inclination, yet the results in each case are much better compared and remembered when the simple law of change in the whole line of force is ready in the mind for reference. The equatorial plane and the magnetic axis are now the only parts in which the helix can be without affecting the position of the needle; the first gives places (for the helix) with a stable position for the needle, and the second such as have either stable or unstable positions, according to the helix distance.

2992. If the helix be out of the plane and axis, then the end of the needle nearest to it leans from it as if repelled. If the helix be carried round in a circle of latitude, the end of the needle moves round before it just like the upper end of the needles at Hobarton and Toronto, in respect of the sun, during the midday hours. Instead of moving the helix round the needle, we may carry the needle into different positions as regards the helix, and then fig. 20 will represent the result. A result exceedingly simple, and in perfect accordance with the diamagnetic disposition of the forces produced by the helix (2972.), as the two dotted lines indicate.

Fig. 20.



2993. As an expression of the facts for use in applying them to the explanation and illustration of natural phænomena, it may be said in respect of *declination*, that the helix being above the needle in a plane having dip, and therefore above its magnetic equator, if on the east of a needle having north dip, it will send the south or upper end west, or if on the east of a needle having south dip (being of course then itself inverted (2972.)), it will cause the north or upper end to pass westward; seeming to repel the end of the free needle or part of the line of force nearest to it. In reference to the *inclination*, it may be said, that the helix being above the needle, tends to send the upper end of the needle or line of force from it. If the helix is north of the magnetic axis, it will tend to send the upper end of the needle south; if it is south, the upper end will go north. As in the case of the declination, it is as if the end of the free needle or line of force nearest to it was repelled. In fact every case is included in this result, that if the helix be diamagnetically adjusted (2975.) for a free needle, whether it is above or below the needle, or on this side or that, the nearest end of the needle will be as if repelled, provided the helix is not in a neutral position.

2994. I repeated all these experiments with the helix reversed, so as to give the effect of a paramagnetic globe of air (2865. 2973.). I need only say, that the effects were precisely the same in nature and order, only in the *reverse* direction. They will be required in the explication of the night and early morning actions, due to the cooling of the atmosphere (3003. 3010.).

2995. In these experiments, that the laws of deflection might appear in their simplicity, the needle was suspended in the air, and the representation of the sun's action carried round it in all directions. But in nature the air is only above the needle, and the earth as a magnet is beneath it. In the natural case also, there is the fixation of the lines in the earth (2919.), which tends, by holding them below the surface, to give them an amount of deflection *at* the surface, far beyond what they would have if they were as free to move in the earth beneath as in the space above[1]; and though this deflection would coincide with that produced by the helix alone, still it was important to verify its effect. I therefore took a bar magnet 30 inches long, and weak in condition, and suspended the needle above it in various parts, so as to have the effect of north or south dip to any degree, or no dip at all near the middle parts. The effect of absence of air from beneath was also in a certain degree represented; and to make this point more striking, I occasionally put masses of iron on and under the middle part of the magnet. The results with the helix were now influenced greatly in the *amount* of the deflection, but not in the direction. When the helix affected the direction of the needle, it was according to the above laws.

2996. In the consideration of natural phænomena, the magnetic axis, and also the planes of the magnetic equator and meridian, being circles or planes of no deflection, are very important. Changing as they do with every change, either of place or declination or dip, they require some ready means of illustration, and can hardly be comprehended in their effects without a model. I have prepared a globe on which, after marking the places of the observatories, I have drawn the magnetic meridians of these places as they were last estimated. I have then in another colour drawn for each place its magnetic equator, making that a great circle parallel to the equatorial plane of the dipping-needle at the place. I have also marked on the globe the mean path of the sun for each month, and by the use of adjustible pins to indicate the hours before and after

[1] Referring to the typical globe of cold air (2874.), it is manifest that if the space below the horizontal lines *a*, *c*, &c. were occupied by matter holding the lines in it, then the deflections now represented on the lower parts would appear above the holding surfaces, and to a much greater degree, though extending downwards to a much smaller space.

noon of any given place, I have the means of ascertaining with sufficient accuracy when the sun is in any particular quadrant, or what part of the quadrant; when it passes a neutral line, and what its position in relation to the place of observation is, in a manner which no diagrams or figures could supply. I have found the globe very useful; and I accustom myself to place it always in a certain position, namely, with the axis of rotation horizontal, the north pole to my right hand, and the astronomical meridian of the place of observation towards the zenith. The observer can then regard it as from the place of the rising sun.

2997. Though we thus have the experimental conditions of a needle under an action like that resulting in nature from the presence of the sun (2920.), I do not pretend that they can be applied without modification to natural phænomena, but only that they give very important aid in the study of the latter and the rationale of their action. The atmosphere, instead of being illimitable, wraps the earth round as a garment; the influence, as it extends from the region of action, must, in respect of that portion which is conveyed through its mass (2920.), curve with its curvature, and give a result in any particular place, which only refined calculations founded upon careful observations can determine accurately. In regard to the development of the air action, it would, I think, be very interesting to ascertain, even roughly, the daily variations of a magnet at the bottom of a deep mine, half-way up, and at the mouth of the shaft. The results might tell us much about the holding power of the earth and the depths to which the deflections of the magnetic lines of force penetrate, and might even give us a rough expression of the changes of the internal power (or the absence of such changes) when freed from those dependent upon the atmosphere.

2998. Another reason why the experimental results must not be applied too closely is as follows. If the lines of force of the earth were perfectly regular, then the change produced amongst them by the sun and air would be regular also. But as the natural system is not regular either between the tropics, as at Sister's Walk and Longford in St. Helena, or in the higher latitudes, as at Hudson's Bay, so apparent inconsistences may and must result. The probability is that the greatest irregularities in the arrangement of the earth's magnetism are in and near the surface of the earth, and that above they tend to adjust with each other into a more regular order. Still the irregularities must extend their influence very far upwards, so that the contortions of the magnetic meridians or lines of force are not likely to be effaced, or much diminished, at the region coinciding with the place of the atmosphere's effect.

2999. But though the lines are irregular in the large space affected by the sun, the result will be expansion of the whole as a system and diamagnetic polarity. The lines of force below will be affected by those above; and so, though a perfect similarity between different places is not to be expected, still the kind of change at the earth's surface is not likely to be so uncertain as it might at first appear. Therefore I believe the globe (2996.) will be found very useful in giving information regarding the probable effects of the magnetic meridian and equator, due to the place of the sun in the two chief quadrants for any given month of the year, or hour of the day.

3000. The passage of the magnetic meridian is important, and appears far more so after the experiment described (2978.) than it did on a former occasion (2942.). Being very often inclined to the astronomical meridian, it must have great influence in deciding when the daily declination changes in its direction. The place of greatest action, and its travelling north or south along a line of magnetic force according as the declination was west or east in relation to the helix as a sun, was confirmed by an experiment; and the further observation (a consequence of the former), that when the sun was equidistant from a place more north or south than itself, its action was far stronger on that side at which its path and the declination direction made an acute angle than on the other side where it was obtuse, was also confirmed. Thus if the helix, moving from east to west, were passing a place north of it having western declination, then the action was stronger on the western side of the place than on the east for equal distances of the helix from the magnet.

3001. The passage of the magnetic equator by the sun is also important, since the direction of the diurnal variation of the experimental needle is then altered; and this is of the more consequence, because by the great degree of natural declination in many places, even far north and south, this passage is thrown forward towards the astronomical meridian, either on the east side or the west, and comes into effect during the more influential hours of the sun or the cold. In all those places too where the dip is little, as at St. Helena, and in or near the sun's path, it may be important in influencing the amount of action. From the change of place of the sun between the tropics, and the variety of dip and declination at different places, the passing of the neutral planes by the sun and acting region must take place under an extreme variety of conditions; the unravelling of which I think will be much assisted by knowledge such as that which the preceding experiments and principles give. The sun may be astronomically either north or south of the needle, and yet the declination of the needle not change in direction (2980.); or if there were much mean declination, as at Greenwich, then it might be astronomically east or west of it, and yet the declination produced not change its direction. The sun region may be south of a place and yet send its upper end further south (2990.); for all will depend upon its position in relation to the magnetic meridian and the magnetic axis, which are in most cases very far removed from those that are astronomical: added to all these causes of variety, there is the fixation of the lines of force in the earth (2919.), which tends to give a further diversity to them.

3002. In the former paper I considered only the effect of air raised in temperature above the mean condition (2895.), illustrating it by the sun's effect in the middle of the day; *now* I purpose considering that which will be produced by the cold of night, which reduces the air of a given region below the mean air temperature of that place. When a portion of air is so cooled, its conduction power is increased; in conjunction with the warmer air of surrounding regions it deflects the lines of magnetic force passing through both, as indicated by the type globe (2864.2874.), and acquires what I have called conduction polarity (paramagnetic), meaning thereby simply that the lines of force draw together in the middle of the cooled air.

3003. Theoretically, the effect of a cold region of air coming up from the east would be to make the magnetic lines of force, as they leave the earth, advance or bend towards it, because those in and about the cold air are inflected into it; and as those immediately west of the cold region move into or towards it, so those further west, being in part relieved from their tension, will also move east, and thus an effect, the reverse of that of the sun (2877. 2972.), or the same as that of the helix in the paramagnetic position (2973. 2994.), will be produced. The upper ends of needles at places having dip show this deflection of the upper part of the lines of force, because they move by, with, and in them.

3004. So as cold approaches, the lines will lean towards it until it is in the position of maximum action in the eastern quadrant; then they will return (in declination) before the cold, until both it and the line (or needle) are in the magnetic meridian; after which, as the cold travels on westward, the needle will follow it west until the cold has attained its place of maximum action in the west quadrant (2982.); and then, as the cold retreats, the needle will return east to its mean place; assuming that there is no other action for the time than that of the cold region. The upper end of the free needle, therefore, at any given place will tend *towards* the cold region, just as before it tended *from* a warm region; and as the declination is affected, so also the inclination will be. If the cold be on the magnetic meridian of a place within the tropics, as St. Helena or Singapore, it will increase the dip there, whilst at the same moment it is diminishing the dip at places south or north of it having considerable dip, a result which follows directly from the inflection of the lines of force into or towards the cold region.

3005. The chief regions of heat and cold on the same parallel of latitude, do not follow each other at equal intervals of time. It is difficult to make a judgement regarding their interval in the atmosphere above; but the maximum of cold on the earth for the twenty-four hours, is assumed by many as being seventeen hours after the preceding noon, and only seven hours from the coming noon. This brings into consideration the joint effect of hot and cold regions in deflecting the lines of force, especially during the forenoon and middle of the day. If a cold region be only three and a half hours west of a place at the same time that the warm region is three and a half hours east of it, it is very manifest that the joint effect of the two, for both act then to cause the same deflection, will be far greater than that of the heat or cold alone, or than any corresponding effect at other periods, for neither twelve hours after, nor at any other time will there be an equivalent condition of circumstances; and so it is also for other combinations of hot and cold regions, the effect of which will vary both by position and by their extent. A free needle is held in tension by the lines, which are themselves governed by the hot and cold regions of atmosphere; it probably never occupies its mean place, but is always in the resultant of these ever-present and ever-varying causes of change.

3006. As the earth revolves under the sun, each place would have, speaking generally, a maximum and a minimum of temperature for its atmosphere in the twenty-four hours. But looking at the globe as a whole, there would be one maximum and two minima, *i. e.* there would be a maximum region somewhere beneath the sun in his path, and a minimum in each of the polar regions; which, as regards the twenty-four hours, would not be at the pole, but in some place of high latitude, and perhaps, as before, seven or eight hours before noon. These cold regions will be very seriously affected in their extent and place and power by the position of the sun between the tropics; for as he advances to one tropic the cold region there will diminish in extent and force, whilst the other will grow up in importance; and whilst they thus vary in their power of influencing the general direction of the lines of force, they will vary in their own position also, and have at different times very various relations of place to the sun in different months, and so produce very various effects. It is these differences which are made manifest to us, as I believe, in the night and morning actions at the numerous observatories scattered over the globe.

3007. I will proceed to apply these views, and the additional knowledge gained by experiment, to the localities formerly considered, and to some new ones between the tropics, for the purpose of explaining, if I can, the principles of night action; of retardation, more or less, of the effects in relation to local time; of the difference in direction of the declination variation in different months, for the same place at the same hours, as pointed out by Colonel Sabine; of the diminution of dip in one place, and increase of it at another for the same local time. In doing so, it will be necessary to refer continually to that place which may be considered, in respect of the station, as the centre of hot or cold action for the time. I will endeavour to use the word region for that purpose, meaning thereby not the whole extent of heated or warmed air, nor the centre, but the chief place of the altered portion. It is very manifest that in some days in March or September, *all* the air that is east of the meridian at 21^h or 22^h may be considered warm in comparison of that which is then west of the same meridian, and that a resultant of action, which shall be the same for all places, cannot exist.

3008. We are to remember that the eastening and the westening of the upper end of the needle, of which I always speak, is produced in two ways. The needle travels as positively by the withdrawal of a direct cause of action as it does under the immediate direct action of that cause, but in the contrary direction (2982.). A westening may be the result either of the coming up of the sun on the east of the place of observation, or of its withdrawal in the west, after he has passed over the meridian and produced the great east swing.

3009. *St. Petersburgh* has a mean declination of 6° 10′ W., and a dip of 70° 30′ N.; therefore, though the magnetic and astronomical meridians are not very oblique to each other, still the sun or warm region reaches the former from 20′ to 40′ before the latter, and hence the time of the great sun-swing, which is from 20 to 1 o'clock, is made earlier than it otherwise would be. The magnetic equator of the needle (2977.) forms an angle of about 40° with the earth's equator, and being thus tilted, it disposes the two quadrants chiefly concerned in the daily variation (2979.), so, that in the St. Petersburgh summer the warmest region is not only far nearer to the needle, but passes through the strongest places of action of the quadrants, whereas in winter it is further off, and also in much weaker positions. Hence a cause, as I believe, of the great difference in the amount of variation of declination, and also of its character: in November, December and January, it is from 4′·47 to 4′·65 only, whilst in June it is 11′·52[1]. See the Tables, p. 271, and the Curves, Plate II.

3010. In December or January, being St. Petersburgh winter,

[1] The eastening and westening of a free dipping-needle are not properly represented by the movements of a horizontal needle, inasmuch as at places with different dip the angle is read off on planes differently inclined to the dip itself, and in high latitudes the effect is greatly exaggerated. But though different places may not be compared without a correction, the variations for the same place as St. Petersburgh are comparable and proportionate.

the sun-swing east almost disappears. It is over by 1 o'clock[1], after which the upper end of the needle follows the sun until 9^h, having passed its mean position at 5^h. It then stops, after which it moves east until 16^h or 17^h; then again stops, or nearly so, until 21^h, and then the sun-swing comes on, carrying it to extreme east. So here there are two very important points to explain, namely, why the needle moves eastward after 9^h, and why it does not travel westward from 13^h to 20^h, but, on the contrary, is travelling eastwards or standing still: the explanation, according to my view, is as follows:—St. Petersburgh is a place in which, from its position, the upper cold, consequent upon the daily withdrawal of the sun, would produce a paramagnetic action (2994. 3003.). This action, as the sun set, would begin to appear on the east, and I conclude that at 9^h–11^h the cold region coming up from the east, not on the latitude of the sun's path, which is far to the south, but probably near to that of St. Petersburgh itself, is able at 9–11 o'clock, during which the needle is stationary, to counteract any remaining tendency westward, and after that to draw the line of force and the needle end eastward until 17 o'clock, and to hold it there, after which the sun sends it eastward in the great swing. That the cold, considering its probable position, may well direct the needle end eastward till 17^h, and the sun region not send it westward from 17^h to 20^h or 21^h, is seen, I think, to be a very natural consequence of the probable position of the two regions between these hours. For letting the sun (whose place we know) represent the warm region at 17^h, he is then in the eastern quadrant below the horizon, so that if he could affect the needle through or round the earth (2995.), it would be to easten it, and it continues in that quadrant until 19^h. Then at 19^h, when he enters the quadrant, in which he begins to exert a westening action on the sun, he is in such a position as respects the needle at St. Petersburgh (as is seen by a line drawn over the surface of the globe (2996.) and compared with the magnetic meridian and dip), and in so inefficient a part of the quadrant (2982.), and also so far off, that it has no power to send the needle westward, but only in association with the retreating cold region to

[1] The St. Petersburgh observations are at $21\frac{1}{2}$ minutes after each hour; but I mention the hour without the minutes as sufficient for a general statement.

hold it there, until at 21^h, or thereabout, the sun-swing from west to east occurs as in other cases. After this the needle follows the sun from 1 o'clock, being, as the hours advance, gradually arrested and taken up by the cold region of the next twenty-four hours, as already described.

3011. I have considered the cold eastening as continued until as late as 17^h, which would imply probably that until that hour the cold region was east of St. Petersburgh. It is very difficult to speak, even in a general manner, of the places or times of things so little identified as yet, as the warm and cold regions in the upper atmosphere; but referring to the temperatures on the earth at St. Petersburgh, I may point out, that the extreme cold is, in the month of January, as late as 19 and 20 o'clock, and *five hours* later than it is in the summer months. I may also point out here, for use in the summer months, that the maximum heat varies three hours in the *opposite direction*; so that whilst from the highest to the lowest temperature in the day is only eleven hours in summer, it is nineteen hours in winter, as may be seen by the Temperature Table, p. 271. As the day comes on, therefore, in January the highest temperature is only five hours after the lowest, which accords generally with the assumed cause of the effects on the needle[1].

3012. As I am endeavouring to make St. Petersburgh a general case of night action for the explanation of corresponding effects at other places, so I may notice that the night action must contain a portion of sun effect which combines with that of the cold. The action of the sun is known by observation to be very extended; in the case of St. Petersburgh, the sun, when at the southern tropic and on the meridian, is between 80° and 90° from the station, and yet we see by the observations and curves how large an effect he produces (3009.). Wherever the sun may be, he is by his motion causing changes which are felt simultaneously over the whole globe; and at 9 and 10 o'clock he is in an effectual part of that quadrant which would send the

[1] In relation to the cold of the upper atmosphere and the occurrence of its maximum (at certain levels at least), not at midnight but hours after, how often do we in this country see a clear bright night, and then just before the sun rises, the formation of a veil of clouds high up, and, upon his appearing, their dissolution and passing away! In these cases the clouds show the time of greatest cold above by their formation, and by their dissolution its quick reversion and change into increasing warmth.

needle eastward if the earth were replaced by air, and in the representative experiments with a helix (2995.) does so send it eastward when a magnet is interposed. The night action ought therefore to be greatest in winter, as it is, because the cold is then most intense, and also because the action of the distant sun coincides with it. It is very probable that many of the curious contortions of the night action which appear in the curves of Hobarton, Toronto and elsewhere, may depend upon the manner in which, at different hours, these two causes (probably with others) combine together.

3013. Though the declination varies little or nothing between 17^h and 21^h, no westening then appearing (3010.), still I should expect a marked action on the inclination at that time, and conclude that it will be on the increase; but I have not been able to obtain a table of the daily variation of inclination.

3014. In the month of February the same remarks apply; but as the sun is now coming from the southern signs and drawing nearer to St. Petersburgh its power is increasing, and this is shown by making the cold eastening for 15^h, 16^h and 17^h less in extent than before by more than half a minute (of a degree), and by absolutely overcoming it and making a return westwards between 17^h and 18^h, before the swing to the east comes on. In March the effect is still more striking; the paramagnetic eastening is arrested at 14^h, and the following diamagnetic westening extends to 20^h; then follows the swing. In April the westening by the warm region is as early as 13^h and continues to 20^h, being very strong. It is interesting to look at the Table of Temperatures for these months, even as they are obtained at the earth's surface. As the months come on the eastening from the cold ceases sooner and sooner, being in January and April 17^h and 13^h respectively. The minimum of temperature also retreats, being for the same month 20^h and 16^h. On the contrary, the maximum of heat *advances* from the winter to the summer months, being also greatly increased; and the effect on the sun-swing is seen both in the *advanced* time of change and the *increased* amount of variation.

3015. In May and June the night or cold eastening has disappeared, or is shown only by a little hesitation; and from midnight the coming on of the sun region sets the needle end west. If we look at the globe (2996.), we should be led to expect that it would do this. The sun is then in the northern tropic nearly, wheeling round St. Petersburgh and comparatively near to it; and a free dipping-needle would in twenty-four hours make one revolution in the same direction as the sun region, but at the opposite end of the line, joining the two together. If the needle were at the astronomical pole of the earth, having great dip, it would describe almost a circle with nearly uniform motion; but being really much nearer to the warm region in one part of the uniform daily course of the latter than another, the radius vector joining it with the region then makes a much greater angle in a given time than when it is further off, and hence the greater rapidity of the motion between 20^h and 1^h, and the production of what I have familiarly called the sun-swing from west to east.

3016. It will be seen from the Table of Curves (Plate II.), that we have at St. Petersburgh a fine example of that kind of result which Colonel Sabine called attention to so strongly in his paper upon the St. Helena phænomena[1]; and those occurring at Hobarton, Toronto and elsewhere; namely, a declination variation in *different* directions for the same hours in different months. Thus, in the present case, the needle end goes eastward for the hours 13^h to 20^h in October, November, December, January and February, whilst it goes west for the same hours in April, May, June, July and August: March and September curves fall midway. But this difference is now I hope by the hypothesis accounted for (3010. 3015.), and I trust that equally satisfactory reasons will appear for St. Helena (3045.) and other places (3022. 3039. 3065.).

3017. The paramagnetic character of the eastening effect by cold in the winter months after 10 o'clock, would probably be illustrated by inclination observations for the same time; for if the cold region passes to the south of St. Petersburgh the inclination will be decreased by the paramagnetic action, but increased by the diamagnetic resultant; and the manner in which these two elements of direction, *i. e.* inclination and declination, are combined at any given moment, is very important to the full elucidation of the magnetic effect of the atmosphere. I have not been able to give these data for St. Petersburgh. The total force variations would also help greatly to clear up the

[1] Philosophical Transactions, 1847, p. 51.

subject. Indeed it is not fair to endeavour to explain the results of the assigned cause by taking only one element of three into consideration. What we require ultimately to know, is all the changes of a free needle in position and in respect of power. All are important, and all should be considered at once. I presume that the theory of the variations cannot advance very far without their joint consideration.

3018. *Greenwich* presents a fine case of the night episode, and the different directions of the magnetic variation for the same hours in different months. In these respects it is very much like St. Petersburgh, but has great additional interest, because of the large western declination[1], and the effect produced by it on the places of the active quadrants (2979. 3000.), and the times of the variation phænomena. On setting up its position on the globe (2996.), it will be seen that the equatorial plane is not likely to be much concerned in the midday action, and that the sun or warm region passes nearly across the middle of the two chief quadrants in summer; which with its nearness at the same time, ought to make the midday swing to east very great. In winter it is further off and in much weaker parts of the quadrant, so that the swing ought to be far less, and such is the case. The greatest summer variation is 11′·30, and the least winter variation only 5′·88. In April, May, June, July and August, the great west declination of the south or upper end of the needle is at $19^h\ 20'$, and the chief east position at $1^h\ 20'$. The latter position remains the same all the year round, but the extreme westening is in the other seven (cold) months at $9^h\ 20'$ and $11^h\ 20'$[2], or verging towards midnight; it then surpassing the morning west deflection. Thus the sun's effect in summer, in weakening the cold night effect (3005.), is very evident; and so also is the manner in which the night action grows up, until very prominent, in the winter months, through the strengthening of the cold action (3006.), when the sun is towards the southern tropic and in the weaker parts of the segments. The assumed principles of this action have been already given in the case of St. Petersburgh (3010. &c.).

[1] Mean declination 22° 51′ W. Mean inclination 69° N.

[2] See the Curves, Plate II. The observations are only for every two hours, so that no degree of nicety can be expected in assigning the time of any given change.

3019. The magnetic meridian is much to the east of the astronomical meridian, where the warm region passes it, especially in winter, for then the sun crosses it about 10 o'clock, and in summer about 11 o'clock. Hence the swing ought to be earlier in winter than in summer, though, because of the slower angular motion of the warm region in relation to Greenwich (3015.), it ought then to occupy a longer time; and yet, as above said (3018.), be, by reason of distance, of smaller amount. All this appears to accord remarkably with the fact. The swing begins at 17^h in the winter but not until 19^h in the summer, and ending at the same hour at both seasons, namely, 1 o'clock, is much longer in its occurrence in winter than in summer. It begins earlier, because the magnetic meridian is sooner passed than in summer; and the reason also appears why the extension in time is at the beginning rather than at the termination of the swing; for, because of the declination, the warm region is at the same hours much less east of the magnetic meridian in the morning and much further west of it in the afternoon, in winter than in summer; hence the swing is thrown forward in time in winter; and though prolonged, its termination coincides with the termination in summer, as far at least as these two-hour observations can indicate.

3020. As the region precedes the sun, the degree of mean declination here ought to make the day-swing come on early, *i. e.* earlier than at Hobarton, and especially earlier than at Toronto, unless other causes of variation interfere. Now the beginning is earlier than at Toronto, but the end the same. Both the beginning and the end is an hour earlier than at Hobarton. The latter difference I believe due to the difference of mean declination: at Toronto, I think we shall find another cause influencing the time (3032.).

3021. We are to remember also that in winter the sun or warm region passes the magnetic meridian two hours before he passes the astronomical meridian; and therefore his effect in giving west position to the south or upper end of the needle ceases long before it does in summer, and perhaps even before it ceases to come nearer; and so the eastern after-effect on it ought to be greater, which it is. This eastern effect should be strengthened also, because the action of the warm region on the needle ought to be comparatively great after passing the magnetic meridian.; for its path forms an obtuse angle with the meridian before the passage and an acute one afterwards (3000.), and therefore is more powerful. To all these causes of action will be added the effect for the time of the cold in the distant west (3005.).

3022. The case of difference of direction before 19^h (3016.) is very marked at Greenwich, as may be seen by looking at the Curves for the months, Plate II. The south or upper end of the needle goes west in May, June, July and August, from 12^h to 19^h, *i. e.* from midnight to five hours before noon; but in October, November, December and January, it is eastening at the same hours. Considering first a summer month, as June, the upper end of the needle is westward as the sun comes onward (as it ought to be) until 19^h, when he is almost in the middle of his passage through the east quadrant; and in respect of distance and angular relation to the magnetic meridian, the warm region is then, probably, in the place of greatest power to produce westening of the needle end[1]. In the next six hours the needle passes to extreme east, performing, according to the observations, a fourth of the whole swing in the first two hours, a half in the next two, and a fourth in the remaining two, the journey being no doubt with first rapidly increasing and then rapidly diminishing velocity. In this transit of the region, the sun is for about two-thirds of the time in the eastern quadrant, and one-third in the western; and his path in the latter third forms almost the base of an equilateral triangle with Greenwich, having the magnetic meridian for one side, so that all that time it is close to and therefore has strong action on the needle (3000.). The sun is at 1^h in such a position as respects this angle, that if we assume the region to be somewhat in advance of it, the latter would be in that place where it could exert its maximum eastening effect; and therefore after that, as it recedes westwards, would let the needle return from east to west, as it does, following it. The needle continues to go west, passing its *mean* place for the month about 7^h; in the meantime, before that, at a little after 6 o'clock, the sun has left the western segment by

[1] It must not be forgotten, that the return from an extreme east or west position is not when the sun or warm region passes by a neutral line, or from one quadrant to another, but when it passes its point of greatest action in a quadrant (2982.).

passing the magnetic equator; it has not yet set to Greenwich, and if it have any action, it will, because of the segment it is now in (2979.), still be to carry the needle end *westward*. The end in fact continues to go westward, slowly only, after 10 o'clock, gaining a little from 10^h to 15^h; and then, as the sun comes up, passing more rapidly west, as it ought to do, until 19^h, and finally making the great swing to the east as before. The whole progression here is very simple, and apparently a natural result of the assumed cause. Effects of cooling no doubt come in; but the cold region has diminished in intensity and extent (3006.), has retreated northward, and its action appears in combining with the former to produce only variations in the velocity of the change.

3023. Then for the winter, let us consider January; and, as the eastening is a maximum in all the months at 1 o'clock, after the sun's passage across the meridian, let us begin the cycle there. At 1^h the upper end of the needle is at extreme east, and the amount of the variation not half what it was in summer, the sun being now far off. The sun and warm region pass the magnetic meridian about 21^h or 22^h; and therefore, in the hours before and after that, should produce the full west to east effect. At 1 o'clock the needle returns west, following the retreating sun, and does so quickly for seven or eight hours, or up to 9^h, during which time the warm region, and also the early morning cold region, are in quadrants and positions, which, if they have any action at all like that referred to in the experiments (2975. 2995.), would then set or hold the needle end west of its mean position. Then an action of the following kind supervenes; the needle remains stationary until 11^h, after which it goes east at midnight and until 15^h; again remains stationary, or nearly so, for two hours; then eastens again, slowly at first and afterwards more rapidly, until 1^h, when it has attained its maximum eastening and the place from whence it set out.

3024. This night action is another case of the action of a cold region like that considered in respect of St. Petersburgh (3010.). It appears to me that at 11^h the immediate sun action and return west after it, were over; that the cold region which was coming round from the east did then act by its paramagnetic condition (combined with the complementary effects of the sun's action on the other side of the globe), and set the needle eastward, as it would be competent to do (2994. 3010.) until 14^h or 15^h. In eastening, the needle does not arrive at the mean place, but is still 1′ west of it; and the reason why it hangs there from 15^h to 17^h and then begins to go east again, more and more under the sun's action, is probably that, as the sun rises in the southern tropics, his distance and position bring the resulting distant warm region gradually into action with that of the nearer cold; that at first he stops the action of the latter, and then as he advances combines with and finally replaces it; causing the usual swing to come on, slowly at first and then quickly, from west to east by 1^h. How this would happen is well seen, both as respects the place of the sun in the southern hemisphere, and in the two magnetic segments, by reference to the globe (2996.) and the diagram of the curves of variation, Plate II.

3025. Considering another and an intermediate month as March; at 1^h the upper end of the needle is at extreme east; then until 9^h it follows the sun as before (3023.). From 9^h to 11^h it is stationary; then the paramagnetic action of cold from the east occurs, and the needle moves east until 13^h. It is then stopped, and two hours sooner than before; for the sun now appears to Greenwich as early as 6 o'clock, and in a more favourable position for effect, both as regards the magnetic meridian and the segment in which it has for the time its place; and so the needle is actually sent west for a couple of hours. It is then almost held steady until 19^h, after which the great sun-swing occurs. The holding west and yet the absence of more westening between 15^h and 19^h, is not inconsistent with the southern place of the warm region, and it is probable that at that time the *dip* is increasing; an effect which would accord very harmoniously with the condition of matters at the time.

3026. Other months are on this or that side of March in respect of their effects; the corresponding month on the opposite side of the year (September) is the same as March, except in that portion of effect which is consequent upon a month following one that is warmer or colder than itself (3053.). Greenwich therefore satisfactorily illustrates the application of the hypothesis to the case of a difference in direction for the same hour in different months (3016. 3022.); and also the occurrence of the night effect, and its transition into the very marked eastening of the early morning.

3027. The cases of *Hobarton* and *Toronto* are so similar, though in opposite hemispheres, that they may be considered together. A very important comparison of the phænomena at both places has been already made by Colonel Sabine in relation to the variations of declination, inclination, and total force[1]. When examined by the globe (2996.) the distribution of the quadrants is nearly alike, the sun being in two chief east and west quadrants, from about 18 to 6 o'clock, or during the day. The sun is in more influential parts of the quadrants in summer than in winter, and the effect is seen in the difference of the amount of declination variation. At Hobarton it is 12′·05 in summer and only 3′·6 in winter. At Toronto it is 14′ in summer and 5′·2 in winter. The night action at both is alike in character, and has been sufficiently explained according to the hypothesis in the former cases (3010. 3024.).

3028. Colonel Sabine has given the data by which the variations of the inclination and of the total force at Hobarton and Toronto may be compared with and applied to the hypothesis; but I hesitate to enter upon them in this general view, inasmuch as these and the declination variations should be closely considered and compared together at every hour for each particular place. The inclination variation at Hobarton is greatest in its summer, being then 2′·18, and least in winter, or 1′·28, as was to be expected. The great variation occurs in the daytime, as with the declination; the dip being most as the sun region passes over the meridian. The greatest dip is not at the same hour for all the months; it occurs at 23 o'clock for December, February and March, at 24 o'clock for September, at 1 o'clock for June and July; as it moves on so do the points of least dip on each side of it, so that the whole curve advances in time in the order of these months. There is also another affection of it, for the *quickest* transition is from *most* to *least* dip in some months, as December, February, and from *least* to *most* dip in other months, as June, July, September. At Toronto the dip variation, though peculiar in some points, may be said to have the same general character.

3029. For the variation of the total force at both places, I will

[1] Hobarton Observations, 1850, vol. i. p. lxviii., &c.; also Philosophical Transactions, 1847, p. 55, and 1850, pp. 201, 215, &c. See the Curves, Plate II., and Tables for Toronto, pp. 269, 270.

at present only refer to Colonel Sabine's volumes, and the observations he has made thereon.

3030. There is a remarkable difference between the time of the day changes at Hobarton and Toronto, to which Colonel Sabine has called attention. It consists in the occurrence of those at the latter place, about an hour before those of the former. If this had depended upon the declination, then the change should have taken place first at Hobarton, for there the sun arrives at the magnetic meridian before he comes to the astronomical meridian, and for like hours of local time he is in a better position in the quadrant in the afternoon than at Toronto; still it is the later of the two.

3031. If the *time* of the sun-swing from west to east be considered, the middle of it ought to be somewhere near the period when the warm region is passing the magnetic meridian (2982.), and in that way supplies an approximative expression of the relative positions of the region and the sun. The swing is at Hobarton from 21 to 2 o'clock, or five hours, and the magnetic meridian is passed by the sun nearly in the middle of the time, or $23^h\ 20'$ o'clock. But according to the supposition just made, this is also the time at which the warm region ought also to pass, and so the sun and the region in this place appear to arrive at the meridian together. At *Toronto* the sun-swing is about four hours in winter, or from 21 to 1 o'clock, and five hours in summer, or from 20 to 1 o'clock. Of the latter five hours the middle is $22\frac{1}{2}$ o'clock, at which time the region ought to pass over the magnetic meridian, and as that coincides nearly with the astronomical meridian, it appears that the region is about $1\frac{1}{2}$ hour *before* the sun. By a similar comparison for winter, the region would then appear to be about an hour before the sun[1].

3032. I am inclined to refer much of this precession of the warm region at Toronto to the geographical distribution of land and water there. The Atlantic is on the east and the continent of America on the west of the station, and as Dove's charts and results intimate, the temperature may rise higher and sooner

[1] In reference to the position in advance of the sun, of the resultant of those actions which set the needle end westward, we must remember that the preceding cold, being perhaps seven hours only to the west, is by its action on the general system of the curves aiding the westening of the needle, whilst the sun is in the east and even over the meridian (3005.).

over the land than over the water, and so throw the warm region in respect of Toronto in advance of the time or of the sun. In the case of Hobarton the arrangement is different; and, in fact, what land there is is between the advancing sun and the station, and would tend to hold the warm air region back, and tend to cause its time to coincide with that of the sun. Even the greater difference in summer than in winter at Toronto appears to be explicable in the same manner, by reference to the relative position of the sun at the two seasons to the land and water arrangement.

3033. Though the temperature on the earth's surface is a very uncertain indication of that above (2937.), yet as far as it goes it harmonizes with this view. The maximum temperature occurs sooner after midday at Hobarton than at Toronto; in the former place it is at 2 o'clock and very regular, and the minimum at 16–19 o'clock, being earlier in summer and later in winter. At Toronto the maxima are from 2 to 4 o'clock, and the minima at 16–18 o'clock. The maxima are later in summer than in winter; the minima are as at Hobarton, being later in winter than in summer. The mean temperature is lower at Toronto than at Hobarton, being as 44°·48 and 53°·48: the range of variation is also greater, being at Toronto 43° and for Hobarton only 18°.

3034. It is probable that effects of retardation and acceleration, in respect of the passage of the local part of the warm region for a given place, may occur in many parts of the globe, and these will require to be ascertained for every locality and for the different seasons there. A place having the reverse position of Toronto would have a reversed or retarded effect; and hence it might happen that needles in the same latitude might be affected at very different local times, and yet all be regularly affected every twenty-four hours. The region would in that time make its diurnal revolution, but vary in the velocity of its different parts at different periods of its journey, and that in a different degree and order for different latitudes, and for different parts of the same parallel of latitude. Even the time during which the effect (as for instance the sun-swing) continued would probably be altered; one place holding the influence longer and another dismissing it sooner, analogous to two conditions of stable and unstable equilibrium.

3035. *Cape of Good Hope* [1].—This station is in longitude 18° 33′ east and latitude 33° 56′ south. The mean declination is 29° west and the dip 53° 15′ south. The amount of dip, combined with the position of the place, gives a magnetic equator, which passes nearly through the astronomical poles, and so the sun's path in every part of the year intersects it almost at right angles and at the same hour, namely, about 20′ past 7 o'clock in the morning and evening, or at 19^h 20′ and 7^h 20′. But because of the great declination, the sun is in the astronomical meridian two hours before he arrives at the magnetic meridian in Cape winter, and half an hour or more before in Cape summer.

3036. The sun passes obliquely through both the chief quadrants and across their central parts pretty equably; but because of the western character of the mean declination he is much nearer the Cape when in the eastern than when in the western quadrant for all the months, and so the coming up effect, *i. e.* the westening before the midday swing commences, ought to be more powerful than the eastening after it is over, and such is the case. This is in beautiful and striking contrast to Greenwich, which, having the same kind of mean declination and nearly in the same degree, is on the north of the sun's path, and therefore the luminary passes its magnetic meridian before 12 o'clock, and for a time still approaches the station: the result is the reverse effect to what we have at the Cape; for the eastening effect at the end of the midday swing is more powerful than the westening effect before it, as is well seen by the curves given in Plate II.

3037. Selecting July as the month in which the effect of winter occurs at the Cape, we find that the day-swing is very feeble, as it ought to be, the sun being in the northern tropic and far away; and the swing east is at an end by three o'clock when the sun has passed by about one hour over the magnetic meridian. The upper or north end of the needle then westens for two hours, following the sun until 5^h, when the luminary is low to the Cape and at its setting. After that the needle end eastens slowly until 10^h, then a little more quickly until midnight (passing the mean position at 11^h); quicker still until 16^h or 17^h, and still more quickly until 19^h, when it has attained its maximum east position. This effect I believe to be due to the

[1] See Tables, pp. 317, 318, and curves of variation, Plate II.

cold, which in these hours is approaching from the east, and setting by its paramagnetic action (3003.) the needle end eastward. On the surface of the earth the maximum cold in this month is at 17^h or 18^h, and as far as it goes this result accords with the effect above described. At 19^h, the sun in rising not only stops the eastening but quickly drives the needle back again, and the latter very rapidly goes westward until about 23^h, at which time the sun-swing from west to east comes on, being over by 2^h or 3^h, completing the daily variation, after which the needle goes west, following the sun as before. In this sun-swing is seen the effect of an inclined magnetic meridian (3000.); for though the sun is, at the beginning, only an hour east of the astronomical meridian, he is full three hours to the east of the magnetic meridian. As the swing occupies about four hours, the warm region is probably near the magnetic meridian about half-past 12 or 1 o'clock.

3038. *January* presents a case of Cape summer. The day-swing is then from 21^h to 1^h or 2^h. After 2^h the needle upper end follows the sun westward until 6^h, and then moves a little eastwards for two hours; after this it moves slowly westward again, the whole effect being as if a cold region had occurred on the east, had passed over and gone away west, and the temperature below at this time is within 2° of the minimum. This night effect of drawing the needle westward (3004.) proceeds slowly until 15^h or 16^h, being assisted by the rising temperature on the east urging the end still more rapidly west until 20^h, when having reached its maximum in that direction, it at 21^h turns back and is driven to extreme east in the sun-swing, through an amount of variation more than twice as great as that produced in July or Cape winter.

3039. I think the above is a true explanation of the reverse motion of the needle in the months of July and January, or Cape winter and summer. In winter the paramagnetic effect of cold air is on between 12^h and 19^h, remaining longer on the east side of the magnetic meridian; as it passes forward, both it and the sun region conspire at 19^h to carry the needle westward, for though they have opposite actions they are then also on opposite sides of the magnetic meridian (3005.). In the summer the cold region has much less power, occurs earlier [1], soon passes

[1] The minimum temperature below is three hours earlier.

over, for the summer sun is behind it, and then rather aids the sun in carrying the needle westward.

3040. Some of the other months are still more striking in summer effect. February has a swing through 8′ from west to east between 21^h and 1^h; then from 1^h to 3^h it scarcely changes; from 3^h to 6^h it follows the sun west; from 6^h to 16^h it varies but little, showing the merest trace of east effect about 8^h; and after 16^h it passes west more and more rapidly, so that by 21^h it is at a maximum west, ready to swing back as the sun region passes over. The other and intermediate months are easily traced, and found to be beautifully consistent with the same principles of the hypothesis. As is evident, in almost every case each month partakes of the character of the preceding month in some degree, though not so much in this case of the Cape as in some others (3053). The curves of December and January are more equal.

3041. The time of the sun-swing illustrates exceedingly well the effect of the inclined magnetic meridian (3000.). In November, December and January, the swing is from 20^h to between 1^h and 2^h. In these months the sun crosses the astronomical meridian about half an hour before he arrives at the magnetic meridian. In October, February and March, the swing is later, being from 21^h to 2^h or 3^h, for the sun then passes the magnetic meridian an hour or more later than the astronomical or time meridian. In September, April and May, the swing is still later, being from 22^h to 2^h or 3^h, and the sun is still longer than before in reaching the magnetic meridian. In June, July and August, the swing is latest, being from 23^h to 3^h, and the sun is proportionately late in arriving at the magnetic meridian. What I describe as the passage of the sun is of course true of the warm region which precedes it; but I prefer referring to the visible type rather than to the invisible reality, because it ties the considerations of time more simply together.

3042. The inclination at the Cape varies singularly in the twenty-four hours, depending, I think, upon its mean degree. It is such that the warm and cold resultants of action for the Cape will sometimes be above the line of the dip and sometimes below it, not only for different times of the year, but I think in some seasons, even at different times of the day. It would require much attention to unravel the whole effect. In June, July and August, when the sun and its warm region are greatly to the north of the Cape, it appears that the dip is increased as the region passes, which would give a rotation of the upper end of the needle like that at Hobarton (2909.); but in November, December, January, February, March and April, the dip diminishes at that time, and the resulting rotation of the pole is of the contrary kind, or like that at St. Helena (3057.) and Singapore (3061. 3067.).

3043. The daily variations of intensity at the Cape are remarkable. In the months October to April it is at a chief maximum at 19^h or 20^h; by noon it is reduced to a minimum as the sun passes over; gradually rises to a second maximum about 4^h or 5^h, and then, after sinking a little about 8^h or 9^h, reaches the chief maximum about 18^h or 19^h next morning. In the months from May to September the chief maximum is at 21^h or 22^h, which is followed by a minimum at 1^h or 2^h, due to the day effect. Then comes on the 5^h maximum, and after thirteen hours or more the second minimum as low almost as the former, and only three hours before the chief maximum; so that this maximum is placed between minima close on each side of it.

3044. These are exactly the months during which the upper end of the needle moves eastward in early morning up to 19^h, and that is just the hour when the minimum intensity occurs. From 18^h or 19^h to 21^h the intensity rises to a maximum, precisely as the lines of force are moving westward before the sun region, prior to their quick return east; and as they return in their quick journey so the intensity falls to a minimum again, and is at that minimum at 1^h or 2^h, just as the swing is over. Here is a very close connexion, and it is curious to see the needle end at east with minimum power at 18^h, and again also at 1^h, remembering that in that time it has swung from east to west and back to east again.

3045. *St. Helena* [1].—This is a station which Colonel Sabine has distinguished as of the highest interest; being near the line of least force, within the tropics, and with little magnetic inclination [2]. It was here also that he called attention to the striking fact, that the course of the needle is in some months in one direction, and in other months in the contrary for the same hours

[1] See Tables, pp. 319, 320, and curves of variation, Plate II.

[2] Magnetical Observations, St. Helena, 1840 to 1843.

of the day[1]. De la Rive attempted to explain this fact[2], but Sabine has stated that this explanation is not satisfactory[3].

3046. St. Helena being a small island in the south Atlantic ocean is removed about 1200 miles from the nearest land. The longitude is 5° 40′ west, the latitude 15° 56′ south; the mean declination 23° 30′ west, and the mean dip 22° south. Hence there are three quadrants concerned in the day action of the sun, especially when that luminary is south of the equator. The sun is south of St. Helena itself in the months of November, December, January and February, or for nearly that time; it is north of the island for the rest of the year. At one time the sun passes the astronomical meridian before it arrives at the magnetic meridian, and at another time the contrary is the case. In addition to these peculiar circumstances, St. Helena is a place of great local differences, and also its dip is so low that the sun's day effect is almost constantly to depress and lessen it.

3047. In June and July the sun rises to St. Helena in the south-east quadrant; about an hour after, it passes into the north-east quadrant, and crosses it towards the southern end, being then at mid-distance in the quadrant about one-third of the length, or nearly 60° from the southern termination. It leaves this quadrant about 1^h 20^m, crossing the magnetic meridian at that time (and consequently so long *after* passing the astronomical meridian), and entering the third or north-west quadrant traverses it obliquely towards its northern extremity. In our winter, December and January, the sun also rises to St. Helena in the south-east quadrant, as before; but it now remains in it until 22^h, being for much of the time in strong places of action; it enters the north-east quadrant to the south of St. Helena, and does not remain in it two hours, being then only in the weakest part of it; it leaves it again *before* arriving at the astronomical meridian, then enters the north-west quadrant, gliding along near to its southern side, and is within two-thirds of an hour of leaving it when it sets to St. Helena.

3048. As June presents the aspect of circumstances approaching nearest to that of a station further south, as Hobarton or the Cape, so I will consider the variations for it first. The

[1] Philosophical Transactions, 1847, p. 51.
[2] Annales de Chimie et de Physique, March 1849, tome xxv. p. 310.
[3] Proceedings of the Royal Society, May 10, 1849, p. 821.

north or upper end of the needle is then nearly at its mean place at midnight or 12^h : it advances east (slowly at first) until 16^h, and then more and more rapidly up to 19^h, when it stops and goes as quickly west until about 22^h, after which it changes but little until 3^h, when it moves *west* till 5^h, and then slowly east up to 12^h, and then onwards to 16^h and 19^h, as already said. The eastening from midnight and before, I refer to the paramagnetic action of the cold, which comes up from the east as before (3003. 3025. 3037.); the rapid increase of the eastening from 16^h to 19^h is consistent with the increasing cold of the early morning, and also with the circumstance, that the sun and its representative region are then passing from the south-east into the north-east quadrant, and must be not far from the neutral line, for that is the time of quickest transit of the needle. As the sun advances into the north-east quadrant, it first stops the eastening, as at 19^h, and then converts it into westening (3014.), which goes on consistently with all former observations until 22^h; the needle is then retained a little west of its mean position until 1^h, at which time it has not yet attained coincidence with the magnetic meridian, and after this hour it is determined east a little until 3^h. This effect, from 22^h to 3^h, I consider as the sun-swing to the east; and I think, examining the globe (2996.), its small amount in declination is quite consistent with the relative positions of St. Helena and the warm region, combined with those of the active and neutral parts of the quadrants traversed during the time. From 3^h to 5^h the needle end moves westward, following the sun; and that effect harmonizes with the idea that the previous holding of the needle in an eastern position, from 22^h to 3^h, is the sun effect: then the slow eastening from 5^h to midnight and beyond, is the cold effect coming on.

3049. Colonel Sabine has shown that the months of May, June, July and August, may be classed together, so that I will not speak of each. Whilst they show the analogies they have between themselves, they also indicate the transitions to and from the other months. Let us consider September. From 7^h, through midnight on to 16^h, the needle stands nearly at the mean. From 16^h to 18^h, the upper or north end eastens through the effect of the early morning cold. That the eastening should be fully effected an hour sooner than before (3048.) is quite consistent with the principles, for the path of the sun and its diamagnetic region is far nearer to the station than before, being now about the equator. From 18^h to 22^h it sends the end westward in conformity with all former observations, and then comes on the sun-swing from west to east, between 22^h and 24^h, and a hold at extreme east an hour longer. The shortness in time of this transit is, I think, a beautiful point. The sun is still north of St. Helena, but is now so much nearer that he passes through the *same angle* east and west, in respect to the place of observation, in less than half the time of the former sun effect in June (3041.). After this the needle end travels west from 1^h to 6^h, following the sun as on other occasions; and then from 6^h to 9^h it moves a little east by the evening cold in the east, and remains near the mean position until the greater cold before sunrise (3005. 3011.) takes it more east between 16^h and 18^h of the coming day.

3050. In looking at the curves of variation (Plate II.), it will be seen that the curve for the next month, October, is remarkable for being like in general character with, and yet far removed in place from, that of September; and this effect appears due to the circumstance that the sun has now arrived at the latitude of St. Helena, or nearly so. According to my supposition, there has been a feeble night effect (3010.); and at midnight the needle is at the mean position and moving slowly westward, when the greater cold which precedes the sunrise coming into action on the east, counterbalances and arrests the western progress, and even draws the needle, as before, east a little for a couple of hours, till 18^h. Even the sun region is at 16^h in that quadrant (the south-east one), that if it could act on the needle it would combine with the cold in the next or north-east quadrant in setting it eastward. By 18^h both the preceding cold region and the following sun region are so far advanced in their respective quadrants that they combine to carry the needle end west as before, until 20^h, and then comes on the swing from west to east until 24^h. Why this begins sooner, lasts longer, and is above four times the extent of the September swing, appears to be that the sun region comes up upon the latitude of St. Helena, and so acts in respect of the magnetic meridian more powerfully, and also sooner and longer: also that because of the mean declination west it arrives at equidistant points from and passes over the magnetic meridian sooner; and also from the effect of an accumulative action added to it from former months (3053.).

3051. At 1 o'clock the needle begins to turn from extreme east, *i. e.* sooner than in the former months, because the magnetic meridian is sooner passed, and follows the sun until 4^h, when it stops, and then the evening or night action due to cold appearing in the east, carries the needle back eastward till 10^h, and then as it advances in the quadrant lets the needle return back again (3004.) until between 12^h and 13^h, when the latter is in its mean position. The cold region then appears to draw it westward until 16^h, when its distance increasing, it releases the needle and lets it return east until 18 o'clock, when the latter is still west of its normal position, and then the sun region rising up, helped perhaps by the cold that immediately precedes it, which is probably now over or beyond the magnetic meridian, sets it toward the west prior to the sun-swing.

3052. In December and January the sun is south of the station. This makes no difference in the general character of the curve for these months; nor should it according to the hypothesis, except in this point, that though the sun is very near to St. Helena and has the cumulative effects of the preceding months (3050. 3053.) added to its own effect at the time, still it is in weaker parts of the quadrant, and whilst in the chief segment is almost up in the corner and near the place where the two neutral planes cross each other; hence its effect ought to be less, and so it is; for the sun-swing of November and February is larger than that of December and January. The sun-swing happens in December at the same time as for October, though in the latter month it crosses the magnetic meridian after, and in the present before midday: still there is only half an hour's difference from one to the other, and the observations are perhaps not close enough to allow one to separate its peculiar effect out of an interval of four hours. Besides, accumulative causes may interfere. The places of the December curve are altogether a little more west than those for October.

3053. The *cumulative* effect of preceding months is very important and well-shown at St. Helena (3050.). Thus, taking the September curve and comparing it with that for October or the following month, we have a great difference of a certain kind;

then again comparing September with the month in which the sun is returning from the southern tropic instead of proceeding to it, and has arrived at the same position as it had in October, another striking difference appears. March is the nearest month for the second comparison. Up to 20^h its curve changes like the October curve, but the upper end of the needle is all the time about half a minute east of its place in October. At 20^h the needle in October begins to swing from west, and reaches extreme east at 24^h: in March it westens until 21^h, then returns and reaches extreme east at 1^h; so that the swing is an hour later, and during that time the end is from half a minute (of space) to a minute more west than in October. This difference I believe to be due to the cumulative effect of the months between October and March, during which time the heat has been diminishing in the northern hemisphere, and increasing in the south. Similar results in other months make it probable that the effect of the atmosphere, though induced by the sun, lags behind the luminary considered as in his astronomical position all the year round; and that therefore in advancing to and receding from a tropic, he seems to do less in the first instance and more in the second than is due to his place for the time.

3054. But where circumstances are apparently equal, a difference also arises. Thus from March to April in one direction, and from September to October in the other, might be expected to be alike, except for a little of the lagging effect (3053.), which would appear on both sides: nevertheless March and April are in Sabine's curves *between* September and October, and near together, whilst the other two are far apart. This effect I refer to the different conditions of the two hemispheres as regards heat (Dove). From September to October the sun is passing from a hemisphere having a mean temperature in summer $17^{\circ}{\cdot}4$ above that of the other hemisphere for its winter; but in March and April it is departing from a hemisphere having a mean summer temperature of only $10^{\circ}{\cdot}7$ above that of the other hemisphere for its winter (2949.); and these respective differences must tend to separate September and October, and bring together March and April, as is seen to be the case by the curve charts, Plate II.

3055. I need not go further into the declination variation of St. Helena: the lines for the other months are subject to the observations already made. Colonel Sabine's important query of the cause of difference in direction for different months (3045.), appears to me at present to be answered for this station, as well as for the other stations, in very various latitudes where it makes its appearance (3016. 3022. 3039.).

3056. The *dip* at St. Helena is a daily variation very simple in character, being a maximum at 7^h and a minimum at 22^h and 23^h, with only one progression. It proceeds to its minimum therefore in the middle of the sun-swing, *i. e.* the upper end of the needle proceeds to west, and descends from 16^h to 19^h or 20^h, during which time therefore the dip is decreasing; then it returns east until it reaches the neutral position, the dip decreasing the while still more. The needle still continues to go east to complete the sun-swing, but now the dip increases; at 24^h or 1^h the needle returns (in declination) after the sun westward, but still with increasing dip; at 5^h or 6^h the westening has almost ceased, and an hour after the dip is at its maximum.

3057. So as the sun and its region pass over they diminish the dip by depressing the upper ends of the lines of force, and as they pass away the lines rise (2926. 2937.) and the dip increases. The ellipse or curve, therefore, which represents the motion of the upper end of the needle at St Helena, as the sun comes up from the east, is above westward and downward, and back below to east; then rising to be repeated in the next twenty-four hours. This is the reverse direction to the representative ellipse for Hobarton, having like south dip in a greater degree. But that is in perfect consistency with the hypothesis; for as the region is located above in the air, it is above the angle which the dip makes with the horizon at St. Helena, and therefore ought to depress the line of force and lessen the dip. At Hobarton, the region being in the tropical parts, is within the angle formed by the line of dip with the horizon, and therefore deflects the lines of force upwards and *increases* the dip, and so the portions of ellipse at the two places, which correspond in time and direction as to declination, have contrary variations of inclination.

3058. *Singapore*[1].—This is a very interesting station: being

[1] See Tables, pp. 321, 322, and the curves of variation, Plate II. The data for Singapore are deduced from the recent very valuable labours of Captain Elliot.

in longitude 103° 53′ E., it has only 1° 16′ N. latitude, and so is close to the equator. Its declination too is only 1° 40′ E., and its inclination 12° S. It is also near the line of weakest force round the earth. The magnetic equator of the needle is almost parallel to the earth's equator, and the quadrants (2929.) are distributed with great simplicity, the magnetic and astronomical meridian nearly coinciding. In our summer the sun passes through the east and west northern quadrants during the day-time; in our winter through the east and west southern quadrants; and in certain months through all four quadrants, following nearly the neutral line of the magnetic equator.

3059. Hence if the line of force were free, *i. e.* if it had no hold in the earth (2919.), we should expect from the hypothesis little or no change in the needle, especially in the months when the sun was over the magnetic equator; but because there is dip, and the lines of force which govern the needle are to the south tied up in the earth (2929.), whilst they are free to move in the air and space toward the north, so there is variation both of the declination and inclination in a perfectly consistent manner; and keeping this in mind, I think we shall have no difficulty in tracing the monthly results according to the hypothesis.

3060. In the first place, the curves of day variation are so like those of St. Helena, month for month, that the account given of them there will suffice for the present occasion (3048.). The sun-swing occurs at the same period, and the effect, dependent, as I suppose, on the character of the two hemispheres, is produced (3054. 2949.). There are however striking differences in the latter part of the sun turn, and also in the night hours, from 5^h to 14^h. The amount of variation appears small; but this is chiefly due to the circumstance that the horizontal plane on which we read it, almost coincides with the free needle, and so the correction before referred to (3009. note) necessary to give the true value of the variation is here very small.

3061. Considering *June* first, as at St. Helena, the upper needle end moves east as before until 19^h, under the influence of the morning cold, after which it stops and is sun-driven west until 22^h, when it swings downwards and beneath to east by 3^h; then follows the sun west until 7^h; it then stops and returns, creeping more and more east because of the coming cold (3065.). In July the needle eastens a little more before 19^h; westens until

23^h, and then eastens until 4^h. The sun-swing is thus thrown an hour later than in June, which I believe to be connected with the accumulation of heat over the land (3054.), combined with the lagging effect of the sun (3053.). In August the needle end eastens until 19^h; more than in July, and most of all the months: it then westens strongly before the sun until 23^h, after which the sun-swing comes on and continues until 5^h, as if the warm region were behind the sun, perhaps even 2^h. The time of the swing is much prolonged, and not unnaturally, as the place is at the equator and therefore under the sun. In September the eastening is less, the westening is less, and the sun-swing is less. April is like September, except that the latter shows the effect of the previously warmed hemisphere (3053.).

3062. Then there are four months in the year, November, December, January and February, when the sun is south of Singapore, and altogether during the day in the southern quadrants (3058.). As the sun comes on from 16^h or 17^h, the upper part of the line of force moves westward (the lower being fixed in the earth) until 19 or 20 o'clock. The sun is at this time in the south-east quadrant, and it might be expected perhaps that the motion of the north or upper end of the needle should be to the east if there were any change at all. But there are two or three reasons, from the hypothesis, why this should not be. For that effect there should first of all be no dip; and in the next place, if there were no dip, the sun is so nearly in the neutral line of the magnetic equator, that the deflection, if any, would have been very small. On the other hand, the lines of force have dip to the south, and being therefore held in the earth, that travelling of the sun along the neutral line, which in its coming up would have sent the whole line of force west, and so caused no variation of declination, can now only send the northern parts, as they rise out of the earth and are carried on with the general system of lines, west, and so cause that western travelling of the needle which does occur. Besides this, though the sun be south of that neutral line and also of Singapore, there is reason to suppose that the middle or resultant of the warm region is north even of both (3063.), which would aid the westening of the needle just described.

3063. For if we recall to mind Dove's results, they show that the northern hemisphere, as a whole, is warmer than the southern (2949.). Again, if we look at the meridian of Singapore, we shall find that there is far more continent on the north of it, to produce a higher temperature, than to the south; and even by the local tables of temperature below, we shall find that May, June, July and August are the hottest months for Singapore, and November, December, January and February the coldest; all tending to make us suppose that the warm region of the atmosphere is relatively north of the sun's place, and perhaps even of Singapore (3067.).

3064. At 20^h the sun-swing from west to east comes on, and continues until 2^h, after which the needle moves west, following the sun, until 10^h or 11^h when it is near the mean; it still goes on westening very slowly until 17^h, when the morning sun action takes it up and drives it more quickly west, until about 20^h, when the sun-swing east occurs. The curve in these months is very simple in its character; the night or cold effect appears to be but small, being indicated rather by a hesitation than by a distinct movement east.

3065. The easterly movement of the needle end in May, June, July and August, and the westerly movement in November, December, January and February, for the same hours, up to 19 o'clock, are in striking contrast; and I have attributed the difference to the effect of a cold region coming on from the east during the former months (3061.), which is absent in the latter months. In reference to this point, we have again to consider that the warm region is on the north of the equator (3063.), and that as the sun moves north and south it also will move with it, but still keeping north of it. Hence the two cold regions, which come up to the meridian in higher latitudes (3006.) before the sun, will not be in the same relation to Singapore, for the one on the south will be nearer to it than the one on the north, or at all events more powerful. So when the sun is near and at the southern tropic, the warm region probably passes *over* Singapore, at which time, therefore, whilst it is the nearest, the most powerful and most direct in position, the cold regions will be *least* in force at the station, and also least favourably disposed by position. But when the sun is at the northern tropic, then the power of the warm region is diminished, both by distance and direction, and the southern cold region grows up into importance by increased strength and closer vicinity, and so produces the eastening before 19^h.

3066. A striking difference in the direction of the night curves.

from 5^h to 14^h, at St. Helena and Singapore may be observed. At the former place the needle end tends first east and then west, whilst at the latter it moves first west and then east. The difference is, I believe, due to the appearance of night cold action at St. Helena to a greater extent than at Singapore. Singapore shows that action in June, July and August, as just described (3065.), but only in a weak degree and at a late hour. At St. Helena, which is in latitude 16° S., the cold effect should, for the reasons given above (3065.), appear in more power, and hence the eastening at 6^h and after; and that this is the cause is indicated also in a degree by the tables of temperature; for whilst at Singapore the difference between the maximum and minimum in the twenty-four hours is only from 3° to 4°, at St. Helena it is from 4°·5 to 7°, and four-fifths or even five-sixths of this depression occurs by 9 o'clock: so that four or five hours before that, there was in the east a cold region coming up and producing the eastening effect recorded in the curves.

3067. The *inclination* variation at Singapore is beautifully simple, and such as might be expected from the hypothesis; the sun or warm region, when passing the meridian, always being over the lines to depress them. It is alike in all the months, being greatest at night-time and least at mid-day; it is nearly the same from 8^h to 18^h; then as the sun comes up it decreases quickly until 23^h or 24^h, after which, as the sun passes away, it increases nearly as quickly until 7^h or 8^h. The amount of variation is greatest when the sun is over or to the south of Singapore. It is least in June and July, when he is near the northern tropic. In December and January, when he is near the southern tropic, it is considerably more than in June and July, which again seems to show that the warm region is chiefly north of the sun (3063.).

3068. The total force variation is simple, being a maximum from 9^h 12^h, and a minimum at 22^h or 23^h, near noon. The greatest variation is in April and October, or at the equinoxes, and the least in December and June, when the sun is at the tropics. The force is the least towards noon, when I suppose that the air above is in the worst condition of conduction, and would cause a magnet in it to show more power. But how that may affect the curves beneath on the surface of the earth, where they are compressed together, is doubtful, and the whole matter of intensity is too uncertain and has too many bearings for me to consider it usefully here.

3069. I hope soon to give further experimental data for the purpose of illustrating and testing the view of the physical cause of the magnetic variations which I have put forth, namely, those I expect to obtain by the differential balance, and others concerning the sensible influence of oxygen in causing, under different conditions, deflection of the lines of magnetic force.

Royal Institution,
November 16, 1850.

Cape of Good Hope.—Longitude 18° 30′ E. Latitude 33° 56′ S. Declination 29° 05′ W. Inclination 53° 15′ S.

Mean Diurnal variation of the Declination in each month of the years 1841 to 1846.

Increasing numbers denote a movement of the north or upper end of the magnet towards the East.

Mean time	Noon. 0h.	1h.	2h.	3h.	4h.	5h.	6h.	7h.	8h.	9h.	10h.	11h.	Midn. 12h.	13h.	14h.	15h.	16h.	17h.	18h.	19h.	20h.	21h.	22h.	23h.	Daily means.
	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′
January ...	4·11	5·43	5·50	5·04	4·64	4·29	3·96	4·16	4·43	4·36	4·27	4·13	3·98	3·79	3·56	3·32	3·09	2·67	2·01	1·15	0·37	0·36	1·39	2·93	3·28
February .	5·51	7·48	8·21	7·86	7·06	6·11	5·74	5·83	5·90	5·82	5·74	5·69	5·78	5·73	5·55	5·50	5·38	5·27	4·90	3·36	1·26	0·00	0·82	2·96	4·93
March ...	5·34	6·74	7·29	6·93	6·17	5·48	5·32	5·33	5·42	5·39	5·28	5·40	5·34	5·52	5·59	5·54	5·52	5·39	4·95	3·22	1·04	0·02	1·26	3·44	4·66
April	4·07	5·00	4·76	4·21	3·75	3·53	3·42	3·39	3·44	3·45	3·51	3·56	3·53	3·98	3·96	4·07	4·17	4·32	4·15	3·29	1·32	0·00	0·39	0·06	3·30
May	1·83	2·49	2·81	2·92	2·50	2·15	2·17	2·31	2·33	2·30	2·45	2·41	2·64	2·83	2·87	3·10	3·24	3·36	3·80	3·91	2·31	0·68	0·00	0·62	2·42
June	0·74	1·27	1·87	1·87	1·36	1·05	1·11	1·19	1·24	1·34	1·49	1·63	1·74	2·00	2·04	2·25	2·22	2·28	2·59	3·21	2·38	0·86	0·00	0·10	1·58
July	0·60	1·18	1·96	2·09	1·54	1·06	1·06	1·09	1·19	1·31	1·46	1·74	2·03	2·22	2·23	2·32	2·46	2·61	3·08	3·56	2·74	1·35	0·28	0·02	1·71
August ...	0·62	1·52	2·50	3·23	2·93	2·22	2·27	2·34	2·46	2·56	2·49	2·64	2·77	2·91	3·01	3·32	3·45	3·63	4·35	4·98	3·65	1·74	0·36	0·00	2·71
September	1·58	2·64	3·22	3·27	2·94	2·57	2·64	2·73	2·75	2·76	2·82	2·84	2·91	3·05	3·06	3·19	3·22	3·36	4·33	4·10	2·43	0·84	0·00	0·49	2·65
October ...	4·90	5·81	5·96	5·23	4·29	3·69	3·84	4·05	4·09	4·17	4·18	4·16	4·16	4·15	3·96	3·79	3·63	3·40	2·78	1·37	0·14	0·00	1·27	3·18	3·59
November	5·19	6·15	6·36	5·98	5·43	4·98	5·08	5·26	5·32	5·39	5·37	5·37	5·34	5·13	4·84	4·55	4·15	3·41	2·05	0·68	0·00	0·57	2·19	3·88	4·28
December.	4·64	5·48	5·51	5·08	4·47	4·41	4·24	4·42	4·54	4·60	4·66	4·67	4·62	4·36	4·07	3·74	3·38	2·63	1·42	0·39	0·04	0·55	2·05	3·56	3·62

Mean Diurnal variation of the Inclination in each month of the year, from April 1841 to June 1846. Increasing numbers denote increasing inclination. Inclination 53° 15′ South.

January ...	0·03	0·00	0·34	0·56	0·72	1·00	1·25	1·35	1·46	1·51	1·50	1·41	1·37	1·40	1·30	1·22	1·18	1·10	0·94	0·99	0·97	0·92	0·66	0·23	0·98
February ..	0·13	0·00	0·44	1·05	1·55	1·91	2·01	2·14	2·14	2·09	2·94	1·94	1·70	1·63	1·65	1·56	1·63	1·70	1·66	1·94	2·26	2·08	1·45	0·66	1·56
March ...	0·11	0·00	0·46	1·05	1·45	1·86	2·09	2·21	2·16	2·02	1·94	1·79	1·79	1·46	1·35	1·46	1·45	1·50	1·51	1·69	1·81	1·66	1·00	0·41	1·38
April	0·00	0·49	1·05	1·55	1·76	1·89	2·16	2·21	2·11	2·06	1·88	1·65	1·45	1·18	1·18	1·07	1·22	1·12	1·10	1·10	1·48	1·53	0·92	0·33	1·35
May	0·00	0·38	0·70	0·86	1·08	1·16	1·27	1·31	1·49	1·59	1·39	1·32	1·16	0·89	0·93	0·89	0·76	0·68	0·30	0·15	0·53	0·66	0·56	0·12	0·84
June	0·41	0·53	0·58	0·76	0·64	0·79	0·96	1·22	1·34	1·40	1·29	1·22	1·04	0·96	0·89	0·81	0·73	0·78	0·36	0·00	0·07	0·38	0·36	0·43	0·75
July	0·62	0·64	0·59	0·64	0·77	0·87	1·18	1·20	1·46	1·43	1·43	1·21	1·07	0·95	0·77	0·87	0·67	0·60	0·28	0·00	0·18	0·51	0·64	0·56	0·80
August ...	0·12	0·38	0·76	0·99	1·16	1·27	1·34	1·60	1·55	1·52	1·45	1·30	1·16	1·04	0·84	0·83	0·63	0·68	0·27	0·00	0·13	0·33	0·43	0·30	0·84
September	0·00	0·23	0·66	1·09	1·40	1·65	1·78	1·68	1·75	1·75	1·40	1·27	0·91	0·97	0·56	0·81	0·51	0·73	0·18	0·31	0·07	0·35	0·03	0·12	0·84
October ...	0·00	0·08	0·53	1·17	1·75	1·98	1·98	1·89	1·88	1·75	1·60	1·50	1·28	1·23	1·20	1·10	1·09	1·07	1·07	1·09	1·04	0·77	0·36	0·05	1·14
November.	0·14	0·00	0·29	0·64	1·12	1·53	1·41	1·46	1·48	1·33	1·25	1·08	0·99	0·94	0·90	0·87	0·87	0·94	0·80	0·62	0·46	0·26	0·06	0·00	0·81
December.	0·13	0·30	0·87	1·22	1·40	1·53	1·78	2·01	2·04	1·81	1·70	1·57	1·50	1·48	1·47	1·50	1·50	1·40	1·05	0·81	0·48	0·15	0·02	0·00	1·15
Means ...	0·14	0·25	0·61	0·96	1·23	1·45	1·60	1·69	1·74	1·69	1·49	1·44	1·28	1·18	1·09	1·08	1·02	1·02	0·79	0·72	0·79	0·80	0·54	0·27	1·04

Cape of Good Hope.—Mean Diurnal variation of the Intensity in each month of the year, from April 1841 to June 1846.

Increasing numbers denote increasing intensity.

The numbers express the changes in parts of the whole Force. Approximate Total Intensity 7·5.

Mean time	Noon. 0h.	1h.	2h.	3h.	4h.	5h.	6h.	7h.	8h.	9h.	10h.	11h.	Midn. 12h.	13h.	14h.	15h.	16h.	17h.	18h.	19h.	20h.	21h.	22h.	23h.	Daily means.
	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00
January...	000	003	018	031	041	047	048	039	036	038	040	044	045	047	049	050	052	061	071	079	078	056	028	005	042
February .	000	000	020	047	069	083	076	072	068	069	071	071	074	075	078	076	077	080	092	115	127	105	062	026	068
March ...	000	007	027	052	070	079	074	069	064	064	067	066	070	068	066	068	068	068	081	103	119	094	047	011	063
April	000	017	040	058	070	071	068	065	060	059	059	060	060	059	060	063	061	060	065	083	102	093	059	021	059
May	003	000	008	019	036	038	035	028	025	023	021	021	022	020	020	019	019	018	013	026	056	063	050	020	025
June	008	000	003	015	029	030	026	021	017	014	017	010	010	007	009	007	010	008	004	003	030	041	039	017	015
July	009	004	005	016	032	033	026	023	018	013	010	007	007	007	008	009	008	007	000	005	027	043	041	023	016
August ...	028	013	007	010	028	035	029	024	019	016	017	016	016	016	017	013	013	009	000	009	044	061	064	044	023
September	007	002	005	015	023	028	021	017	013	012	013	012	019	015	020	014	019	008	005	018	051	057	050	023	019
October...	000	009	033	057	070	064	053	047	045	047	047	049	051	055	056	056	056	060	073	088	084	060	025	005	050
November	000	005	021	038	049	057	048	044	043	044	044	047	050	053	056	057	052	074	090	096	082	054	022	007	047
December	000	003	015	029	037	035	034	030	027	026	028	031	035	036	038	042	047	060	074	076	066	042	013	000	034
Means ...	005	005	017	032	046	050	045	040	036	035	036	036	038	038	040	040	040	043	047	058	072	064	042	017	461

Mean Temperature of the Air from April 1841 to June 1846 inclusive. Fahrenheit's scale.

	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
January...	73·16	73·30	73·03	72·43	71·52	70·30	67·94	66·11	65·45	65·01	64·51	64·10	63·77	63·54	63·32	63·06	62·85	63·18	65·20	67·24	68·78	70·05	71·46	72·59	67·58
February .	74·04	74·07	73·63	72·85	71·74	70·36	68·20	66·69	66·13	65·75	65·34	64·93	64·54	64·23	64·00	63·60	63·41	63·12	64·46	66·33	67·94	69·72	71·62	73·10	67·91
March ...	71·74	72·40	72·19	71·40	69·96	68·07	65·98	64·98	64·38	63·69	63·20	62·85	62·37	62·06	61·69	61·44	61·15	60·97	61·31	63·03	65·39	67·48	69·50	71·00	65·76
April	68·39	68·94	68·90	68·06	66·56	64·22	62·69	62·13	61·54	60·98	60·46	59·87	59·53	59·27	59·00	58·80	58·40	58·28	58·17	59·49	61·82	64·14	65·95	67·35	62·62
May	62·32	62·55	62·43	61·74	60·49	58·99	58·08	57·41	56·81	56·39	55·85	55·48	55·37	55·21	55·05	54·81	54·59	54·32	54·04	54·44	56·42	58·48	60·09	61·29	57·61
June	58·45	58·79	58·78	58·32	57·01	55·58	54·79	54·25	53·71	53·25	52·77	52·32	52·16	51·86	51·69	51·53	51·21	50·95	50·80	50·83	52·20	54·42	56·17	57·42	54·14
July	59·15	59·66	59·65	59·21	57·77	56·13	55·15	54·47	53·83	53·34	52·86	52·37	52·08	51·72	51·42	51·19	50·96	50·83	50·51	50·88	52·67	54·94	56·87	58·28	54·41
August ...	58·86	59·23	59·07	58·61	57·61	55·91	55·01	54·35	53·91	53·60	53·17	52·91	52·71	52·31	52·07	51·87	51·54	51·19	51·15	51·88	53·19	55·57	57·05	58·30	54·63
September	61·60	61·79	61·68	60·87	59·65	58·02	56·78	56·09	55·62	55·22	54·58	54·47	54·36	53·95	53·64	53·31	53·10	53·20	53·13	54·94	56·90	58·61	59·94	60·94	56·77
October...	65·48	65·71	65·46	64·66	63·38	61·56	59·51	58·68	58·05	57·51	57·14	56·74	56·70	56·33	56·11	55·88	55·64	55·63	57·14	59·08	61·00	62·75	64·07	65·04	59·97
November	68·00	68·03	67·63	66·98	66·19	64·82	62·49	61·12	60·38	59·99	59·52	59·12	58·82	58·43	58·00	57·69	57·48	58·14	60·55	62·50	63·96	65·27	66·50	67·50	62·46
December	71·02	71·23	70·84	70·21	69·63	68·23	65·69	63·94	63·19	62·64	62·07	61·59	61·18	60·82	60·49	60·17	59·91	60·83	63·41	65·80	67·17	68·42	69·62	70·42	65·35

St. Helena.—Longitude 5° 40′ West. Latitude 15° 56′ South. Declination 23° 36′ West. Inclination 21° 40′ South.

Mean Diurnal variation of the Declination for the years 1841 to 1845 inclusive.

Increasing numbers denote increasing eastening of the north or upper end of the needle. Mean Declination 23° 36′·6 W.

Mean time	Noon. 0h.	1h.	2h.	3h.	4h.	5h.	6h.	7h.	8h.	9h.	10h.	11h.	Midn. 12h.	13h.	14h.	15h.	16h.	17h.	18h.	19h.	20h.	21h.	22h.	23h.	Daily means.
	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′
January ...	3·72	3·19	2·41	2·03	2·09	1·84	1·92	2·41	2·80	2·90	2·86	2·81	2·63	2·43	2·23	2·01	1·73	1·68	1·50	0·59	0·00	0·57	1·75	3·15	2·13
February ..	4·53	4·51	4·11	3·39	2·80	2·55	2·61	3·02	3·21	3·35	3·48	3·52	3·42	3·22	3·12	2·98	2·83	2·75	2·64	1·14	0·00	0·31	1·52	3·34	2·85
March	3·76	4·11	3·48	2·57	2·15	2·20	2·48	2·64	2·69	2·68	2·85	2·84	2·80	2·77	2·68	2·70	2·69	2·70	2·96	1·90	0·26	0·00	1·13	2·72	2·49
April	3·28	2·48	1·65	1·27	1·03	1·01	1·23	1·50	1·56	1·60	1·71	1·81	1·96	1·92	1·92	2·00	2·16	2·22	2·47	2·15	0·62	0·00	0·82	2·57	1·70
May	0·71	0·44	0·57	0·71	0·55	0·21	0·25	0·48	0·56	0·72	0·78	0·90	1·02	1·09	1·11	1·18	1·32	1·57	2·04	2·58	1·50	0·32	0·00	0·49	0·88
June	0·71	0·73	0·88	0·94	0·44	0·00	0·10	0·39	0·56	0·73	0·90	1·06	1·17	1·22	1·25	1·29	1·40	1·66	2·16	3·31	2·69	1·39	0·75	0·69	1·10
July	0·82	0·71	0·78	0·84	0·48	0·02	0·00	0·22	0·46	0·64	0·84	0·98	1·08	1·14	1·14	1·24	1·31	1·46	2·02	3·11	2·58	1·31	0·63	0·67	1·02
August ...	0·11	0·00	0·40	0·86	0·82	0·42	0·25	0·52	0·68	0·78	0·88	1·02	1·02	1·02	1·04	1·18	1·27	1·61	2·37	3·43	2·52	1·19	0·36	0·05	0·99
September	0·89	0·85	0·65	0·44	0·35	0·28	0·28	0·53	0·62	0·67	0·79	0·78	0·75	0·64	0·61	0·65	0·70	0·95	2·12	1·89	0·85	0·17	0·00	0·43	0·70
October ...	4·41	4·35	3·43	2·06	1·23	1·32	1·80	2·04	2·21	2·34	2·45	2·43	2·23	2·00	1·92	1·81	1·72	1·74	1·99	0·49	0·00	0·72	2·08	3·61	2·10
November.	3·97	3·82	3·31	2·52	1·85	1·85	2·29	2·77	2·99	3·13	3·21	3·13	2·96	2·68	2·46	2·22	1·97	1·80	1·53	0·21	0·00	1·03	2·22	3·41	2·39
December.	3·65	3·48	2·70	1·94	1·66	1·85	2·15	2·58	2·93	3·12	3·20	3·13	2·89	2·64	2·40	2·17	1·93	1·73	1·38	0·32	0·00	0·86	2·09	3·32	2·25

Mean Diurnal variation of the Inclination in each month, from January 1841 to December 1845.

Increasing numbers denote increasing inclination. Mean Inclination 21° 40′ South.

January ...	0·14	0·46	1·07	1·49	2·18	2·53	2·77	2·91	2·87	2·83	2·77	2·63	2·40	2·23	1·99	1·85	1·80	1·77	1·65	1·37	0·88	0·53	0·00	0·02	1·71
February ..	0·23	0·61	1·33	2·00	2·39	2·78	3·01	3·15	3·12	2·98	2·86	2·73	2·61	2·46	2·44	2·21	2·15	2·00	2·04	1·82	1·40	0·94	0·10	0·00	1·97
March	0·11	0·56	1·33	1·86	2·21	2·47	2·80	2·97	2·99	2·89	2·89	2·67	2·46	2·37	2·26	2·12	1·99	2·01	1·84	1·53	1·06	0·43	0·00	0·10	1·83
April	0·45	0·89	1·54	2·14	2·53	2·83	3·05	3·17	3·26	2·88	2·97	2·63	2·41	2·18	2·15	2·08	1·88	1·77	1·47	1·30	0·94	0·43	0·27	0·00	1·88
May	0·20	0·74	1·16	1·62	1·90	2·10	2·22	2·36	2·28	2·21	2·12	1·99	1·77	1·69	1·56	1·33	1·21	1·16	1·18	1·09	0·81	0·36	0·00	0·03	1·38
June	0·18	0·56	1·04	1·58	1·65	1·91	2·21	2·26	2·24	2·12	2·02	1·98	2·06	1·77	1·68	1·54	1·46	1·36	1·22	0·98	0·80	0·43	0·09	0·00	1·38
July	0·30	0·60	1·06	1·44	1·82	2·01	2·42	2·47	2·40	2·35	2·21	2·04	1·80	1·58	1·52	1·30	1·25	1·08	1·00	0·84	0·53	0·27	0·00	0·03	1·35
August ...	0·25	0·72	1·21	1·61	1·94	2·16	2·35	2·40	2·39	2·19	2·12	1·98	1·80	1·69	1·58	1·40	1·33	1·25	1·13	1·07	0·79	0·54	0·19	0·00	1·42
September	0·18	0·70	1·29	1·72	2·02	2·19	2·40	2·40	2·32	2·13	2·01	1·99	1·80	1·68	1·47	1·50	1·51	1·44	1·40	1·27	0·92	0·60	0·36	0·00	1·47
October ...	0·24	0·74	1·36	1·90	2·34	2·56	2·55	2·88	2·65	2·56	2·46	2·28	2·09	2·04	1·89	1·80	1·70	1·69	1·63	1·31	0·88	0·37	0·00	0·02	1·66
November.	0·08	0·27	0·74	1·19	1·63	2·00	2·35	2·45	2·38	2·30	2·18	2·07	1·96	1·83	1·71	1·59	1·66	1·48	1·35	0·92	0·55	0·23	0·00	0·10	1·38
December.	0·26	0·73	1·06	1·59	2·08	2·41	2·69	2·78	2·61	2·45	2·39	2·15	2·01	1·80	1·72	1·56	1·48	1·38	1·26	0·85	0·40	0·10	0·00	0·07	1·49
Means ...	0·22	0·63	1·18	1·68	2·06	2·33	2·57	2·68	2·63	2·49	2·42	2·26	2·10	1·94	1·83	1·69	1·62	1·53	1·43	1·20	0·83	0·44	0·08	0·03	1·58

St. Helena.—Mean Diurnal variation of the Total Intensity in each month, from Ja nuary 1841 to December 1845.

Increasing numbers denote increasing intensity.

Mean time	Noon. 0h.	1h.	2h.	3h.	4h.	5h.	6h.	7h.	8h.	9h.	10h.	11h.	Midn. 12h.	13h.	14h.	15h.	16h.	17h.	18h.	19h.	20h.	21h.	22h.	23h.	Daily means.
	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00
January ...	130	129	116	098	081	063	046	027	016	004	000	001	001	008	015	015	019	019	024	040	055	080	109	124	051
February ..	117	109	092	077	063	050	029	014	007	001	002	003	004	002	000	002	002	003	009	023	046	070	095	114	039
March	153	147	124	096	070	055	032	020	010	000	006	004	001	016	014	018	018	021	026	040	068	102	132	149	055
April	162	150	122	088	068	042	021	010	005	001	004	000	007	014	015	016	025	024	024	031	054	090	102	150	051
May	163	142	115	087	065	049	036	023	016	011	004	000	008	008	011	018	019	019	025	034	048	095	132	159	054
June	141	128	101	064	063	047	028	015	006	004	001	000	002	003	004	004	004	009	013	022	042	075	110	137	043
July	126	117	095	073	051	042	027	015	011	002	002	000	003	005	006	011	015	018	019	024	038	068	097	116	041
August ...	139	126	103	077	053	038	026	013	004	001	000	000	002	005	009	015	015	016	016	018	037	068	103	131	039
September	143	131	105	077	056	037	026	013	004	000	003	007	011	014	018	024	022	024	023	022	038	069	101	134	046
October ...	135	123	105	088	070	051	030	015	007	001	000	005	011	013	019	020	019	020	017	032	058	093	123	135	050
November.	112	111	101	090	076	055	029	018	007	000	002	009	011	011	016	017	017	017	026	048	071	087	105	112	048
December.	116	110	105	089	071	051	027	011	004	000	000	003	003	006	008	010	011	012	024	046	064	086	107	117	045
Means ...	136	127	107	084	066	048	030	016	008	002	002	003	006	009	011	014	015	017	021	032	052	082	110	132	047

Mean Temperature of the Air from 1841 to 1845 inclusive.

January ...	67·68	68·05	67·93	67·79	67·30	66·04	64·58	65·39	62·93	62·62	62·42	62·27	62·06	61·88	61·66	61·60	61·49	61·39	61·37	61·84	62·79	64·09	65·38	66·55	63·98
February ..	69·14	69·61	69·85	69·59	68·95	67·89	66·59	65·43	64·91	64·66	64·46	64·32	64·13	63·96	63·78	63·59	63·51	63·44	63·42	63·62	64·43	65·72	67·08	68·24	65·87
March	69·13	69·57	69·89	69·71	69·10	68·10	66·86	65·84	65·47	65·15	65·00	64·83	64·65	64·50	64·31	64·17	64·15	64·03	64·05	64·29	65·08	66·39	67·35	68·38	66·24
April	68·54	68·89	68·98	68·64	67·99	66·98	65·84	65·15	64·84	64·61	64·45	64·29	64·11	64·02	63·88	63·77	63·66	63·51	63·49	63·69	64·50	65·70	66·74	67·86	65·60
May	65·76	66·01	66·09	65·84	65·17	64·11	63·56	63·32	62·73	62·13	62·03	61·88	61·67	61·57	61·40	61·24	61·19	61·19	61·04	61·06	62·01	63·22	64·11	65·22	63·05
June	62·40	62·76	62·73	62·39	61·83	60·98	60·14	59·68	59·49	59·34	59·20	59·11	59·00	58·86	58·74	58·61	58·56	58·53	58·47	58·52	59·13	60·16	60·98	61·80	60·07
July	60·37	60·83	60·77	60·51	59·87	58·98	58·14	57·70	57·48	57·25	57·13	56·91	56·81	56·72	56·60	56·51	56·43	56·36	56·31	56·38	57·07	58·09	58·90	59·69	57·99
August ...	59·68	59·99	60·12	59·72	59·11	58·11	57·23	56·83	56·60	56·45	56·32	56·20	56·10	55·98	55·76	55·68	55·53	55·50	55·39	55·56	56·20	57·22	58·07	58·96	57·17
September	59·89	60·26	60·18	59·77	59·11	58·03	57·05	56·56	56·35	56·21	56·04	55·91	55·82	55·67	55·51	55·37	55·29	55·21	55·21	55·44	56·12	57·18	58·31	59·14	57·07
October ...	61·50	61·98	61·91	61·31	60·46	59·31	58·18	57·57	57·64	57·12	56·99	56·84	56·64	56·48	56·30	56·18	56·11	56·01	56·06	56·50	57·38	58·61	59·71	60·73	58·23
November.	63·44	63·88	63·97	63·52	62·63	61·32	59·94	59·11	58·76	58·57	58·41	58·24	58·08	57·90	57·71	57·62	57·55	57·43	57·53	58·03	58·94	60·20	61·35	62·35	59·84
December.	65·19	65·81	65·87	65·72	64·88	63·62	62·17	61·10	60·70	60·46	60·27	60·12	59·95	59·75	59·57	59·47	59·34	59·31	59·39	59·77	60·69	61·97	63·03	64·19	61·77

Singapore.—Latitude 1° 16′ N. Longitude 103° 53′ E. Declination 1° 40′ E. (Approx.). Inclination 12° S. (Approx.).

Mean Hourly oscillation of the Magnetic Declination for each month of the years 1843, 1844 and 1845.

Increasing numbers denote a movement of the north or upper end of the magnet towards the East.

Mean time	Noon. 0¼h.	1¼h.	2¼h.	3¼h.	4¼h.	5¼h.	6¼h.	7¼h.	8¼h.	9¼h.	10¼h.	11¼h.	Midn. 12¼h.	13¼h.	14¼h.	15¼h.	16¼h.	17¼h.	18¼h.	19¼h.	20¼h.	21¼h.	22¼h.	23¼h.	Daily means.
	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′	′
January ...	2·04	2·52	2·79	2·72	2·52	2·24	2·11	2·18	2·04	1·97	1·84	1·70	1·63	1·63	1·50	1·43	1·29	1·02	0·61	0·00	0·00	0·41	0·82	1·22	1·59
February ..	2·31	2·92	3·13	3·06	2·79	2·52	2·31	2·31	2·18	2·04	1·90	1·90	1·97	1·90	1·90	1·90	1·84	1·70	1·29	0·54	0·00	0·00	0·34	1·43	1·84
March	1·29	1·43	1·63	1·77	1·63	1·43	1·29	1·09	0·95	0·88	0·82	0·82	0·88	0·88	0·88	0·82	0·82	0·82	0·82	0·48	0·00	0·07	0·54	1·02	0·96
April	0·34	0·61	0·75	1·02	1·16	1·16	0·95	0·82	0·61	0·54	0·61	0·68	0·82	0·95	0·95	0·95	0·88	0·82	1·16	0·95	0·34	0·00	0·20	0·41	0·70
May ...	0·07	0·48	0·82	1·16	1·36	0·95	1·02	0·88	0·75	0·68	0·75	0·82	1·09	1·16	1·29	1·36	1·36	1·43	2·11	2·24	1·43	0·68	0·27	0·00	0·75
June	0·20	0·41	0·54	0·61	0·54	0·34	0·14	0·00	0·00	0·00	0·07	0·20	0·41	0·54	0·61	0·68	0·68	0·82	1·36	1·56	0·82	0·41	0·00	0·14	0·46
July	0·07	0·41	0·61	0·88	1·02	0·88	0·48	0·34	0·27	0·20	0·27	0·34	0·54	0·68	0·82	0·75	0·82	1·16	1·63	1·97	1·29	0·54	0·14	0·00	0·67
August ...	0·20	0·68	1·02	1·56	1·63	1·84	1·43	1·29	1·09	1·02	1·02	1·16	1·22	1·36	1·43	1·50	1·50	1·70	2·52	2·79	1·77	0·75	0·14	0·00	1·27
September	0·48	0·88	1·36	1·63	1·63	1·56	1·50	1·29	1·16	1·09	1·02	1·09	1·16	1·22	1·29	1·29	1·36	1·43	1·97	1·77	0·75	0·20	0·00	0·20	1·14
October ...	1·90	2·31	2·31	2·18	1·97	1·77	1·77	1·63	1·50	1·36	1·29	1·29	1·43	1·36	1·36	1·29	1·22	1·09	1·16	0·54	0·14	0·00	0·27	1·02	1·33
November.	2·45	2·92	2·99	2·86	2·45	2·11	1·97	1·84	1·70	1·56	1·36	1·36	1·43	1·43	1·36	1·29	1·16	1·02	0·48	0·00	0·00	0·27	0·82	1·63	1·51
December.	2·52	2·99	2·99	2·99	2·86	2·45	2·18	2·04	1·97	1·84	1·70	1·63	1·56	1·56	1·56	1·50	1·43	1·16	0·61	0·00	0·00	0·41	1·02	1·77	1·70

Mean Diurnal variation of the Inclination in the several months during the years 1843, 1844 and 1845.

Increasing numbers denote increasing inclination. Approximate inclination 12° South.

January ...	0·00	0·43	0·98	1·28	1·90	2·07	2·38	2·57	2·62	2·71	2·76	2·76	2·66	2·71	2·71	2·72	2·72	2·72	2·73	2·52	1·97	1·09	0·32	0·02	1·97
February ..	0·04	0·64	1·24	1·75	2·00	2·21	2·56	2·82	2·94	3·02	3·10	3·07	3·08	3·03	2·98	2·99	2·97	2·97	3·03	2·88	2·35	1·21	0·40	0·00	2·22
March	0·06	0·29	0·75	1·36	1·75	1·88	2·22	2·55	2·65	2·70	2·74	2·72	2·72	2·67	2·71	2·72	2·72	2·75	2·75	2·54	1·84	0·75	0·11	0·00	1·91
April	0·28	0·82	1·54	2·09	2·47	2·71	3·04	3·00	3·27	3·36	3·41	3·49	3·43	3·40	3·33	3·29	3·29	3·27	3·15	2·92	2·18	1·15	0·28	0·00	2·46
May	0·00	0·32	0·80	1·17	1·62	1·96	2·29	2·34	2·42	2·43	2·42	2·20	2·38	2·40	2·42	2·46	2·52	2·51	2·25	1·87	1·25	0·49	0·03	0·00	1·69
June	0·04	0·20	0·57	0·98	1·44	1·50	2·20	2·25	2·19	2·16	2·10	2·07	2·15	2·21	2·15	2·20	2·21	2·29	2·02	1·71	1·25	0·69	0·22	0·00	1·53
July	0·00	0·15	0·45	0·89	1·34	1·76	2·10	2·16	2·19	2·20	2·20	2·07	2·18	2·19	2·19	2·21	2·21	2·27	2·02	1·74	1·28	0·69	0·27	0·05	1·53
August ...	0·12	0·34	0·69	1·01	1·35	1·68	1·99	2·32	2·21	2·27	2·26	2·28	2·26	2·20	2·18	2·22	2·21	2·22	1·93	1·68	1·30	0·62	0·14	0·00	1·56
September	0·41	1·00	1·60	1·94	2·11	2·32	2·58	2·69	2·78	2·82	2·85	2·84	2·85	2·79	2·74	2·75	2·75	2·79	2·66	2·39	1·66	0·79	0·15	0·00	2·09
October....	0·38	1·17	1·94	2·38	2·48	2·59	2·86	3·07	3·23	3·29	3·34	3·23	3·33	3·23	3·22	3·20	3·24	3·20	3·25	2·96	2·18	1·14	0·21	0·00	2·46
November.	0·16	0·63	1·37	1·96	2·32	2·68	2·88	2·87	3·02	3·08	3·07	3·06	3·06	3·03	3·02	2·95	3·01	3·00	3·15	2·80	2·01	1·04	0·17	0·00	2·26
December	0·09	0·54	1·13	1·62	1·92	2·26	2·53	2·66	2·85	2·86	2·85	2·86	2·76	2·76	2·72	2·69	2·65	2·69	2·44	2·38	1·81	1·08	0·34	0·00	2·02
Means ...	0·13	0·54	1·09	1·54	1·89	2·14	2·47	2·61	2·70	2·74	2·76	2·72	2·74	2·72	2·70	2·70	2·71	2·72	2·62	2·37	1·76	0·89	0·22	0·01	1·98

Singapore.—Mean Diurnal variation of the Total Intensity in the several months during the years 1843, 1844, 1845.

Increasing numbers indicate increase of total intensity.

The numbers express the changes in parts of the whole force. Approximate total intensity 8·21.

Mean time	Noon. 0¼h.	1¼h.	2¼h.	3¼h.	4¼h.	5¼h.	6¼h.	7¼h.	8¼h.	9¼h.	10¼h.	11¼h.	Midn. 12¼h.	13¼h.	14¼h.	15¼h.	16¼h.	17¼h.	18¼h.	19¼h.	20¼h.	21¼h.	22¼h.	23¼h.	Daily means.
	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00
January...	086	066	047	030	028	021	015	009	009	003	002	000	007	002	008	007	010	012	022	033	054	074	099	100	031
February .	099	080	061	042	027	024	013	008	004	000	000	002	000	001	005	005	009	011	014	025	049	082	103	110	032
March ...	114	085	055	035	026	020	013	007	003	001	000	002	005	008	007	009	011	012	012	027	057	087	112	122	035
April	122	094	065	040	029	020	016	011	005	003	005	002	000	004	009	011	013	015	020	043	076	110	139	137	041
May	104	081	060	036	020	013	014	011	010	006	006	000	004	006	006	006	008	012	025	044	073	097	114	118	036
June	096	075	051	026	011	042	004	004	001	000	000	000	001	001	003	004	004	008	020	038	062	085	102	105	031
July	100	081	056	033	019	013	007	007	004	004	002	000	002	004	006	004	010	015	026	044	070	091	105	110	034
August ...	101	081	055	033	016	009	010	005	006	000	002	002	004	010	011	011	016	018	028	046	077	096	115	114	036
September	093	065	037	029	018	009	009	008	002	001	000	003	003	008	014	018	019	017	023	045	079	104	117	115	035
October...	102	073	050	036	029	024	013	007	003	000	000	001	006	011	011	016	019	017	020	037	070	101	117	121	037
November	080	063	044	030	021	015	009	003	001	000	001	000	002	006	007	013	012	013	017	034	059	082	098	095	029
December	077	061	044	031	026	021	012	004	000	000	001	002	003	006	009	013	014	014	019	035	056	078	095	091	030
Means ...	098	075	052	033	022	019	011	007	004	002	002	001	003	006	008	010	012	014	021	038	065	091	110	112	034

Mean Temperature of the Air observed on the Standard Thermometer inside the Observatory, during the years 1841, 1842 and 1843.

	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
January...	79·7	79·5	80·0	79·5	79·7	79·1	79·0	78·4	78·5	78·0	78·1	77·5	77·9	77·3	77·6	76·9	77·4	76·5	77·0	76·3	77·3	77·4	78·6	78·8	78·2
February .	80·7	81·6	81·2	82·1	81·2	82·1	80·2	80·6	79·7	80·2	79·2	79·6	78·6	79·0	78·3	78·6	78·2	78·2	77·7	78·2	78·0	79·4	79·6	81·8	79·8
March ..	81·4	82·1	81·7	82·3	81·6	82·0	80·9	81·5	80·5	81·2	80·1	81·0	79·9	80·6	79·4	80·3	79·1	79·9	78·7	79·8	79·4	80·6	80·6	81·5	80·7
April	81·8	81·8	82·1	81·8	81·9	81·6	81·4	81·2	81·0	80·9	80·7	80·2	80·3	80·3	79·9	80·0	79·5	79·6	79·2	79·8	79·9	80·6	81·1	81·4	80·8
May	82·5	......	82·7	......	82·5	......	82·1	......	81·8	......	81·4	......	80·9	......	80·5	......	80·3	......	79·9	......	80·9	......	81·8	......	81·4
June	82·5	82·9	82·7	83·1	82·5	82·7	82·2	82·0	81·7	81·6	81·4	81·4	81·2	81·2	80·7	80·7	80·3	80·2	80·1	80·4	81·0	81·7	81·8	82·6	81·6
July	82·4	83·0	82·8	83·1	82·7	82·7	82·2	82·1	81·8	81·9	81·5	81·5	81·1	81·2	80·7	80·9	80·3	80·6	80·8	80·8	80·8	81·9	81·9	82·6	81·7
August ...	81·7	81·9	81·9	81·9	81·8	81·8	81·4	81·3	81·0	81·2	80·8	80·8	80·4	80·3	80·1	80·1	79·8	79·8	79·5	79·8	80·2	80·8	81·2	81·6	80·9
September	82·1	82·6	82·4	82·3	82·2	82·1	81·7	81·6	81·5	81·2	81·0	80·8	80·5	80·3	80·1	80·0	79·7	79·7	79·4	79·4	80·3	80·9	81·4	81·8	81·0
October...	81·5	81·3	81·7	81·4	81·7	81·2	81·2	80·6	80·7	80·2	80·1	79·8	79·6	79·8	79·4	79·3	79·0	79·3	78·7	79·2	79·5	80·0	80·7	80·9	80·3
November	81·1	81·0	81·3	80·8	81·0	80·6	80·4	80·1	80·1	79·7	79·7	79·6	79·3	79·1	78·9	78·7	78·6	78·5	78·4	78·5	79·2	79·6	80·2	80·5	79·8
December	81·0	81·3	81·3	81·3	80·9	80·8	80·2	80·0	79·6	79·4	79·2	79·2	79·0	79·0	78·6	78·4	78·1	78·1	77·9	78·2	78·7	79·5	80·1	80·7	79·6
Means ...	81·5	81·7	81·8	81·8	81·6	81·5	81·1	80·9	80·7	80·5	80·3	80·1	79·9	79·8	79·5	79·4	79·2	79·1	78·9	79·1	79·6	80·2	80·7	81·3	80·4

On Atmospheric Magnetism.

[Royal Institution Proceedings, April 11, 1851.]

On a former evening (January 24) it was shown that oxygen gas was magnetic, being attracted towards the poles of a magnet; and that like other magnetic bodies, it lost and gained in power as its temperature was raised and lowered, and that the change occurred within the range of natural temperatures. These properties it carries into the atmosphere; and the object, this evening, was to show how far they might be applied to explain certain of the observed variations of the terrestrial magnetic force.

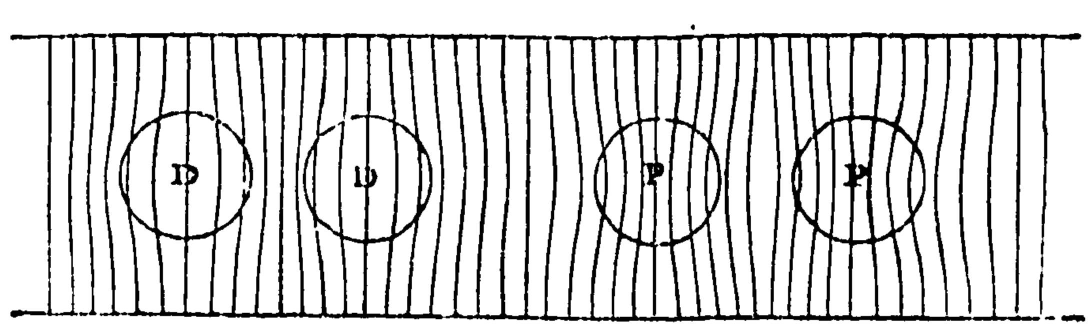
If a source of magnetic power be considered (as a magnet), it presents us with a system having polarity; and if the parts which are called the poles be taken as representing the most concentrated condition of the polarity, then the contrary polarities, manifest externally in relation to the magnet, are perfectly definite, being exactly equal to each other. If the magnet be irregular in the disposition of its force, still the same definite character of the sum of the contrary polarities holds good.

External to the magnet those concentrations which are named poles may be considered as connected by what are called magnetic curves, or lines of magnetic force, existing in the space around. These phrases have a high meaning, and represent the ideality of magnetism. They imply not merely the directions of force, which are made manifest when a little magnet, or a crystal or other subject of magnetic action is placed amongst them, but those lines of power which connect and sustain the polarities, and exist as much when there is no magnetic needle or crystal there as when there is; having an independent existence analogous to (though very different in nature from) a ray of light or heat, which, though it be present in a given space, and even occupies time in its transmission, is absolutely insensible to us by any means whilst it remains a ray, and is only made known through its effects where it ceases to exist. The form of a line of magnetic force may vary exceedingly from a straight line to every degree of curvature, and may even have double and complicated curvatures impressed upon it. Its direction is determined by its polarity, the two changing together. Its powers are such, that a magnetic needle placed in it finds its place of rest parallel to it; a crystal of calcareous spar turns until its optic axis is transverse to it; and a wire which is unaffected when moved in or along it, has an electric current evolved the instant that it passes across it: by these and by other means the presence of the magnetic line of force and its direction are rendered manifest.

The earth is a great magnet; its power, according to Gauss, being equal to that which would be conferred if every cubic yard of it contained six one-pound magnets; the sum of the force therefore is equal to 8,464,000,000,000,000,000,000 such magnets. The disposition of this magnetic force is not regular, nor are there any points on the surface which can be properly called poles: still the regions of polarity are in high north and south latitudes; and these are connected by lines of magnetic force (being the lines of direction) which, generally speaking, rise out of the earth in one (magnetic) hemisphere, and passing in varied directions over the equatorial regions into the other hemisphere, there enter into the earth to complete the known circuit of power. A free needle shows the presence and direction of these lines. In London they issue from the earth at an angle of about 69° with the horizon (being the dip or inclination); and the plane in which they rise forms an angle of 23° W. nearly with true north, giving what is called west declination. Where the dip is small, as at the magnetic equator, these lines scarcely rise out of the earth and pass but a little way above the surface; but where it is large, as in northern or southern latitudes, they rise up at a greater angle, and pass into the distant realms of space, from whence they return again to the earth in the opposite magnetic hemisphere; thus investing the globe with a system of forces like that about an ordinary magnet, which, wherever it passes through the atmosphere is subject to the changing action of its magnetic oxygen. There is every reason to believe that these lines are *held* in the earth, out of which they arise and by which they are produced, just as the lines which originate in a magnet are held by it, though not in the same degree; and that any disturbance from above affecting them will cause a greater change in their place and direction in the atmosphere and space above, than in the earth beneath.

The system of lines of magnetic force around a magnet or the earth is related by a lateral tension of the whole, analogous in some degree to the lateral tension of lines of static electrical force; both the one and the other being easily made manifest by experiment. The disturbance of the tension in one part is accompanied instantly by a disturbance of the tension in every other part; for as the sum of the external powers of a system, unaltered at its origin, is definite and cannot be changed; so any alteration either of intensity or direction amongst the lines of force at one place, must be accompanied by a corresponding change at every other. So if a mass of soft iron on the east side of a magnet causes a concentration of the lines of force from the magnet on that side, a corresponding expansion or opening out of the lines on the west side must be and is at the same time produced; or if the sun, on rising in the east, renders all the oxygen of the air on that side of the globe less magnetic and less able therefore to favour the transition of the lines of terrestrial force there, a greater number of them will be determined through the western region; and even though the lines of force may be doubted by some as having a separate existence such as that above assumed, still no error as to the effects on magnetic needles would in that case be introduced, for they by experiment would be and are the same.

The power of a magnetic body as iron or oxygen to favour the transmission of lines of force through it more than other bodies not magnetic, may be expressed by the term conduction. Different bodies, as iron, nickel, oxygen, conduct in various degrees, and not only that, but the same body as iron or oxygen conducts in different degrees at different temperatures. When space traversed by uniform lines of magnetic force is occupied by a uniform body as air, the disposition of the lines is not altered; but if a better conducting substance than the air is introduced, so as to occupy part of the space, the lines are concentrated in it, and drawn from other parts, as shown by P, P in the figure, or if a worse conducting substance is intro-



duced, the lines are opened out as at D, D. In both cases the lines of force are inflected, and a small magnetic needle standing in them at the inflected part would have its direction changed accordingly. Experimental illustrations of these changes in direction are given in Mr. Faraday's paper in the Philosophical Transactions for 1851, Part I. par. 2843, &c.

Now this by the hypothesis is assumed to take place in the atmosphere. Supposing it all at mean temperature, the lines of force would have the direction determined by the arrangement of the power within the earth. Then the sun's presence in the east would make all the atmosphere in that region a worse conductor, and cause it to assume the character of D; and as the sun came up to and passed over the meridian and away to the west, the atmosphere under his influence would bring up changes in direction like those shown in either D or D; it would therefore manifestly set a needle in a given latitude in opposite directions as it passed by; and as evidently set two needles in north and south latitudes in opposite directions at the same moment of time. As the night came on and a temperature lower than the mean came up from the east and passed over, the lines of force would be inflected as in P or P, and a reverse variation of the needle to that which occurred before would now take place.

That natural effects of variation must be produced consequent upon the magnetic nature of oxygen and its daily variations of temperature is manifest; but whether they cause the observed variations, or are competent to do so, is a question that can only be decided after very careful inquiry. Observations are now made on the surface of the earth with extreme care in many places, and these are collated, and the average or mean result, as to direction and intensity of the earth's force, ascertained for every hour and season; and also many remarkable, anomalous, and extra results evolved. A theory of the causes of any or all of these variations may be examined first by the *direction* which the varying needle does or ought to assume, and then by the *amount* of the variation. The hypothesis now brought forward has been compared with the mean daily variation for all the months in the year at north and south stations, as Toronto and Hobarton, and at many others near to and far from the equator, and agrees in direction with the results observed far beyond what the author anticipated. Thus the paths described by the upper ends of free needles in the north and south hemispheres should be closed curves, with the motion in opposite and certain directions, and so they are:—the curves described by needles in north or south latitudes should be larger in summer and smaller in winter, and so they are:—a night or cold action should grow up in the winter months, and such is the case:—the northern hemisphere ought to have a certain predominance over the southern, because of its superior temperature, and that is so:—the disposition of land and water ought to have an influence, and there is one in the right direction:—so that in the first statement and examination of the hypothesis it appears to be remarkably supported by the facts. All these coincidences are particularly examined into and stated in the Philosophical Transactions already referred to. The next step will be to ascertain what is the amount of change in the conducting power of the air for given changes of temperature, and then to apply that in the endeavour to ascertain whether the amount of change to be expected is (as well as the direction) accordant with that which really occurs.

TWENTY-EIGHTH SERIES[1].

§ 34. *On lines of Magnetic Force; their definite character; and their distribution within a Magnet and through space.*

Received October 22,—Read November 27 and December 11, 1851.

3070. From my earliest experiments on the relation of electricity and magnetism (114. note), I have had to think and speak of lines of magnetic force as representations of the magnetic power; not merely in the points of quality and direction, but also in quantity. The necessity I was under of a more frequent use of the term in some recent researches (2149. &c.), has led me to believe that the time has arrived, when the idea conveyed by the phrase should be stated very clearly, and should also be carefully examined, that it may be ascertained how far it may be truly applied in representing magnetic conditions and phænomena; how far it may be useful in their elucidation; and, also, how far it may assist in leading the mind correctly on to further conceptions of the physical nature of the force, and the recognition of the possible effects, either new or old, which may be produced by it.

3071. A line of magnetic force may be defined as that line which is described by a very small magnetic needle, when it is so moved in either direction correspondent to its length, that the needle is constantly a tangent to the line of motion; or it is that line along which, if a transverse wire be moved in either direction, there is no tendency to the formation of any current in the wire, whilst if moved in any other direction there is such a tendency; or it is that line which coincides with the direction of the magnecrystallic axis of a crystal of bismuth, which is carried in either direction along it. The direction of these lines about and amongst magnets and electric currents, is easily represented and understood, in a general manner, by the ordinary use of iron filings.

[1] Philosophical Transactions, 1852, p. 1.

3072. These lines have not merely a determinate direction, recognizable as above (3071.), but because they are related to a polar or antithetical power, have opposite qualities or conditions in opposite directions; these qualities, which have to be distinguished and identified, are made manifest to us, either by the position of the ends of the magnetic needle, or by the direction of the current induced in the moving wire.

3073. A point equally important to the definition of these lines is, that they represent a determinate and unchanging amount of force. Though, therefore, their forms, as they exist between two or more centres or sources of magnetic power, may vary very greatly, and also the space through which they may be traced, yet the sum of power contained in any one section of a given portion of the lines is exactly equal to the sum of power in any other section of the same lines, however altered in form, or however convergent or divergent they may be at the second place. The experimental proof of this character of the lines will be given hereafter (3109. &c.).

3074. Now it appears to me that these lines may be employed with great advantage to represent the nature, condition, direction and comparative amount of the magnetic forces; and that in many cases they have, to the physical reasoner at least, a superiority over that method which represents the forces as concentrated in centres of action, such as the poles of magnets or needles; or some other methods, as, for instance, that which considers north or south magnetisms as fluids diffused over the ends or amongst the particles of a bar. No doubt, any of these methods which does not assume too much, will, with a faithful application, give true results; and so they all ought to give the same results as far as they can respectively be applied. But some may, by their very nature, be applicable to a far greater extent, and give far more varied results, than others. For just as either geometry or analysis may be employed to solve correctly a particular problem, though one has far more power and capability, generally speaking, than the other; or just as either the idea of the reflexion of images, or that of the reverberation of sounds may be used to represent certain physical forces and conditions; so may the idea of the attractions and repulsions of centres, or that of the disposition of magnetic fluids, or that of lines of force, be applied in the consideration of magnetic phænomena. It is the occasional and more frequent use of the latter which I at present wish to advocate.

3075. I desire to restrict the meaning of the term *line of force*, so that it shall imply no more than the condition of the force in any given place, as to strength and direction; and not to include (at present) any idea of the nature of the physical cause of the phænomena; or be tied up with, or in any way dependent on, such an idea. Still, there is no impropriety in endeavouring to conceive the method in which the physical forces are either excited, or exist, or are transmitted; nor, when these by experiment and comparison are ascertained in any given degree, in representing them by any method which we adopt to represent the mere forces, provided no error is thereby introduced. On the contrary, when the natural truth and the conventional representation of it most closely agree, then are we most advanced in our knowledge. The emission and the æther theories present such cases in relation to light. The idea of a fluid or of two fluids is the same for electricity; and there the further idea of a current has been raised, which indeed has such hold on the mind as occasionally to embarrass the science as respects the true character of the physical agencies, and may be doing so, even now, to a degree which we at present little suspect. The same is the case with the idea of a magnetic fluid or fluids, or with the assumption of magnetic centres of action of which the resultants are at the poles. How the magnetic force is transferred through bodies or through space we know not:—whether the result is merely action at a distance, as in the case of gravity; or by some intermediate agency, as in the cases of light, heat, the electric current, and (as I believe) static electric action. The idea of magnetic fluids, as applied by some, or of magnetic centres of action, does not include that of the latter kind of transmission, but the idea of lines of force does. Nevertheless, because a particular method of representing the forces does not include such a mode of transmission, the latter is not therefore disproved; and that method of representation which harmonizes with it may be the most true to nature. The general conclusion of philosophers seems to be, that such cases are by far the most numerous, and for my own part, considering the relation of a vacuum to the magnetic force and the general character of magnetic phænomena external to the magnet, I am more inclined to the notion that in the transmission of the force there is such an action, external to the magnet, than that the effects are merely attraction and repulsion at a distance. Such an action may be a function of the æther; for it is not at all unlikely that, if there be an æther, it should have other uses than simply the conveyance of radiations (2591. 2787.). Perhaps when we are more clearly instructed in this matter, we shall see the source of the contradictions which are supposed to exist between the results of Coulomb, Harris and other philosophers, and find that they are not contradictions in reality, but mere differences in degree, dependent upon partial or imperfect views of the phænomena and their causes.

3076. Lines of magnetic force may be recognized, either by their action on a magnetic needle, or on a conducting body moving across them. Each of these actions may be employed also to indicate, either the direction of the line, or the force exerted at any given point in it, and this they do with advantages for the one method or the other under particular circumstances. The actions are however very different in their nature. The needle shows its results by attractions and repulsions; the moving conductor or wire shows it by the production of a current of electricity. The latter is an effect entirely unlike that produced on the needle, and due to a different action of the forces; so that it gives a view and a result of properties of the lines of force, such as the attractions and repulsions of the needle could never show. For this and other reasons I propose to develope and apply the method by a moving conductor on the present occasion.

3077. The general principles of the development of an electric current in a wire moving under the influence of magnetic forces, were given on a former occasion, in the First and Second Series of these Researches (36. &c.); it will therefore be unnecessary to do more than to call attention, at this time, to the special character of its indications as compared to those of a magnetic needle, and to show how it becomes a peculiar and important addition to it, in the illustration of magnetic action.

3078. The moving wire produces its greatest effect and indication, not when passing from stronger to weaker places, or the reverse, but when moving in places of equal action, *i. e.* transversely across the lines of force (217.).

3079. It determines the direction of the polarity by an effect entirely independent of pointing or such like results of attraction or repulsion; *i. e.* by the direction of the electric current produced in it during the motion [1].

3080. The principle can be applied to the examination of the forces *within* numerous solid bodies, as the metals, as well as outside in the air. It is not often embarrassed by the difference of the surrounding media, and can be used in fluids, gases or a vacuum with equal facility. Hence it can penetrate and be employed where the needle is forbidden; and in other cases where the needle might be resorted to, though greatly embarrassed by the media around it, the moving wire may be used with an immediate result (3142.).

3081. The method can even be applied with equal facility to the interior of a magnet (3116.), a place utterly inaccessible to the magnetic needle.

3082. The moving wire can be made to sum up or give the resultant at once of the magnetic action at many different places, *i. e.* the action due to an area or section of the lines of force, and so supply experimental comparisons which the needle could not give, except with very great labour, and then imperfectly. Whether the wire moves directly or obliquely across the lines of force, in one direction or another, it sums up, with the same accuracy in principle, the amount of the forces represented by the lines it has crossed (3113.).

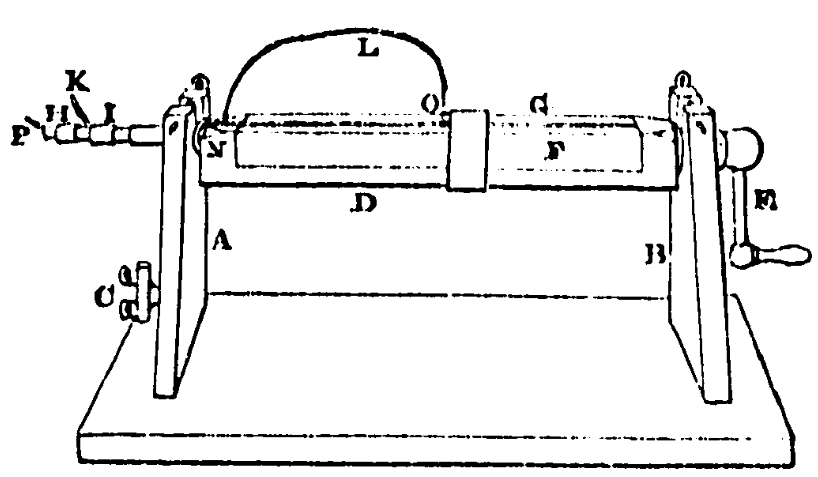
3083. So a moving wire may be accepted as a correct philosophical indication of the presence of magnetic force. Illustrations of the capabilities already referred to, will arise and be pointed out in the present paper; and though its sensibility does not as yet approach to that of the magnetic needle, still, there is no doubt that it may be very greatly increased. The diversity of its possible arrangements, and the great advantage of that diversity, is already very manifest to myself. Though both it and the needle depend for their results upon essential characters and

[1] A natural standard of this polarity may be obtained, by referring to the lines of force of the earth, in the northern hemisphere, thus:—if a person with arms extended move forward in these latitudes, then the direction of the electric current, which would tend to be produced in a wire represented by the arms, would be from the right-hand through the arm and body towards the left.

qualities of the magnetic force, yet those which are influential, and, therefore indicated, in the one case, are very different from those which are active in the other; I mean, as far as we have been able as yet to refer directly the effects to essential characters : and this difference may, hereafter, enable the wire to give a new insight into the nature of the magnetic force; and so it may, finally, bear upon inquiries, such as whether magnetic polarity is axial or dependent upon transverse lateral conditions; whether the transmission of the force is after the manner of a vibration or current, or simply action at a distance; and the many other questions that arise in the minds of those who are pursuing this branch of knowledge.

3084. I will proceed to take the case of a simple bar magnet, employing it in illustration of what has been said respecting the lines of force and the moving conductor, and also for the purpose of ascertaining how these lines of force are disposed, both without and within the magnet itself, upon which they are dependent or to which they belong. For this purpose the following apparatus was employed. Let fig. 1 represent a wooden stand, of which the base is a board 17·5 inches in length, and 6 inches in breadth, and 0·8 of an inch in thickness : these dimensions will serve as a scale for the other parts.

Fig. 1.

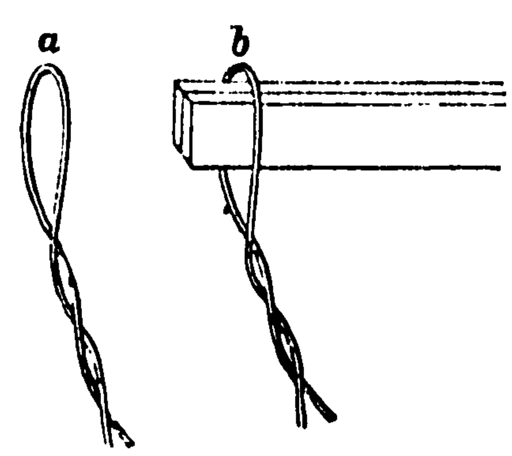


A B are two wooden uprights; D is an axis of wood having two long depressions cut into it, for the purpose of carrying the two bar magnets F and G. The wood is not cut away quite across the axis, but is left in the middle, so that the magnets are about $\frac{1}{13}$th of an inch apart. From O towards the support A, it is removed, however, as low down as the axis of revolution, so as to form a notch between the two magnets when they are in their places; and by further removal of the wood, this notch is continued on to the end of the axis at P. This notch, or opening, is intended to receive a wire, which can be carried down the axis of rotation, and then passing out between the two magnets, anywhere between O and N, can be returned towards the end P on the outside. The magnets are so placed, that the central line of their compound system coincides with the axis of rotation; E being a handle by which rotation, when required, is given. H and I are two copper rings, slipping tightly on to the axis, by which communication is to be made between a wire adjusted so as to revolve with the magnets, and the fixed ends of wires proceeding from a galvanometer. Thus, let P L represent a covered wire; which being led along the bottom of the notch in the axis of the apparatus, and passing out at the equatorial parts of the magnets, returns into the notch again near N, and terminates at K. When the form of the wire loop is determined and given to it, then a little piece of soft wood is placed between the wires in the notch at K, of such thickness, that when the ring I is put into its place, it shall press upon the upper wire, the piece of wood, and the lower wire, and keep all tightly fixed together, and at the same time leave the two wires effectually separated. The second ring, H, is then put into its place on the axis, and the introduction of a small wedge of wood, at the end of the axis, serves to press the end P into close and perfect contact with the ring H, and keep all in order. So the wire is free to revolve with the magnets, and the rings H and I are its virtual terminations. Two clips, as at C, hold the ends of the galvanometer wire (also of copper); and the latter are made to press against the rings by their elasticity, and give an effectual contact bearing, which generates no current, either by difference of nature or by friction, during the revolution of the axis.

3085. The two magnets are bars, each 12 inches long, 1 inch broad, and 0·4 of an inch thick. They weigh each 19 ounces, and are of such a strength as to lift each other end to end and no more. When the two are adjusted in their place, it is with the similar poles together, so that they shall act as one magnet, with a division down the middle: they are retained in their place by tying, or, at times, by a ring of copper which slips tightly over them and the axis.

3086. The galvanometer is a very delicate instrument made by Rhumkorff (2651.). It was placed about 6 feet from the magnet apparatus, and was not affected by any revolution of the latter. The wires, connecting it with the magnets, were of copper, 0·04 of an inch in diameter, and in their whole length about 25 feet. The length of the wire in the galvanometer I do not know; its diameter was $\frac{1}{133}$th of an inch. The condition of the galvanometer, wires, and magnets, was such, that when the bend of the wires was formed into a loop, and that carried once over the pole of the united magnets, as from *a* to *b*, fig. 2, the galvanometer needle was deflected two degrees or more. The vibration of the needle was slow, and it was easy therefore to reiterate this action five or six times, or oftener, breaking and making contact with the galvanometer at right intervals, so as to combine the effect of like induced currents; and then a deflection of 10° or 15° on either side of zero could be readily obtained. The arrangement, therefore, was sufficiently sensible for first experiments; and though the resistance opposed by the thin long galvanometer wire to feeble currents was considerable, yet it would always be the same, and would not interfere with results, where the final effect was equal to 0°, nor in those where the consequences were shown, not by absolute measurement, but by comparative differences.

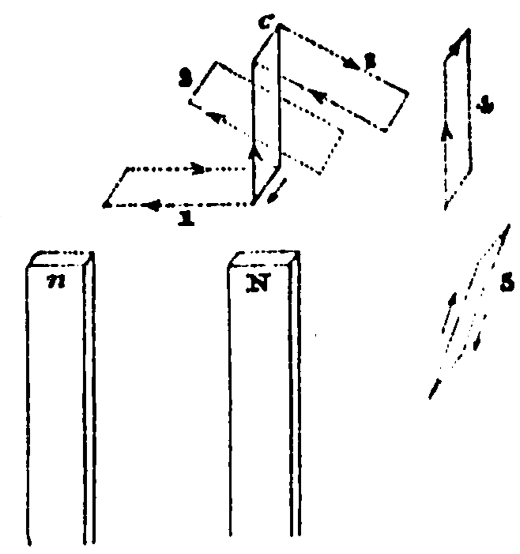
Fig. 2.



3087. The first practical result produced by the apparatus described, in respect of magneto-electric induction generally, is, that a piece of metal or conducting matter which moves across lines of magnetic force, has, or tends to have, a current of electricity produced in it. A more restricted and precise expression of the full effect is the following. If a continuous circuit of conducting matter be traced out, or conceived of, either in a solid or fluid mass of metal or conducting matter, or in wires or bars of metal arranged in non-conducting matter or space; which being moved, crosses lines of magnetic force, or being still, is by the translation of a magnet crossed by such lines of force; and further, if, by inequality of angular motion, or by contrary motion of different parts of the circuit, or by inequality of the motion in the same direction, one part crosses either more or fewer lines than the other; then a current will exist round it, due to the differential relation of the two or more intersecting parts during the time of the motion: the direction of which current will be determined (with lines having a given direction of polarity) by the direction of the intersection, combined with the relative amount of the intersection in the two or more efficient and determining (or intersecting) parts of the circuit.

3088. Thus, if fig. 3 represent a magnetic pole N, and over it a circuit, formed of metal, which may be of any shape, and which is at first in the position *c*; then if that circuit be moved in one direction into the position 1; or in the contrary direction into position 2; or by a double direction of motion into position 3; or by translation into position 4; or into position 5; or any position between the first and these or any resembling them; or, if the first position *c* being retained, the pole move to, or towards, the position *n*; then, an electric current will be produced in the circuit, having in every case the same direction, being that which is marked in the figure by arrows. Reverse motions will give currents in the reverse direction (256. &c.).

Fig. 3.



3089. The general principles of the production of electrical currents by magnetic induction have been formerly given (27. &c.)[1], and the law of the direction of the current in relation to the lines of force, stated (114. 3079. note). But the full meaning of the above description can only be appreciated hereafter, when the experimental results, which supply a larger knowledge of the relations of the current to the *lines of force*, have been described.

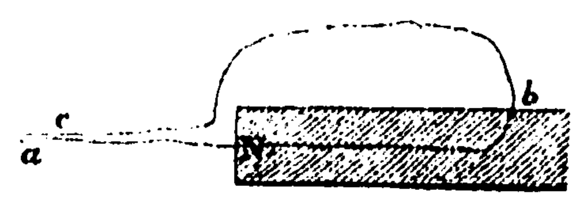
3090. When *lines of force* are spoken of as crossing a conducting circuit (3087.), it must be considered as effected by the *translation* of a magnet. No mere rotation of a bar magnet on its axis, produces any induction effect on circuits exterior to it; for then, the conditions above described (3088.) are not fulfilled. The system of power about the magnet must not be considered as necessarily revolving with the magnet, any more than the rays of light which emanate from the sun are supposed to revolve with the

[1] Philosophical Transactions, 1832, p. 131, &c.

sun. The magnet may even, in certain cases (3097.), be considered as revolving amongst its own forces, and producing a full electric effect, sensible at the galvanometer.

3091. In the first instance the wire was carried down the axis of the magnet to the middle distance, then led out at the equatorial part, and returned on the outside; fig. 4 will represent such a disposition. Supposing the magnet and wire to revolve once, it is evident that the wire *a* may be considered as passing in at the axis of the magnet, and returning from *b* across the lines of force external to the magnet, to the axis again at *c*; and that in one revolution, the wire from *b* to *c* has intersected once, all the lines of force emanating from the N end of the magnet. In other words, whatever course the wire may take from *b* to *c*, the whole system of lines belonging to the magnet has been *once* crossed by the wire. In order to have a correct notion of the relation of the result, we will suppose a person standing at the handle E, fig. 1 (3084.), and looking along the magnets, the magnets being fixed, and the wire loop from *b* to *c* turned over toward the left-hand into a horizontal plane; then, if that loop be moved over towards the right-hand, the magnet remaining stationary, it will be equivalent to a *direct* revolution (according to the hands of a watch or clock) of 180°, and will produce a feeble current in a given direction at the galvanometer. If it be carried back 180° in the reverse direction, it will produce a corresponding current in the reverse direction to the former. If the wire be held in a vertical, or any other plane, so that it may be considered as fixed, and the magnet be rotated through half a revolution, it will also produce a current; and if rotated in the contrary direction, will produce a contrary current; but as to the *direction* of the currents, that produced by the *direct* revolution of the wire is the same as that produced by the *reverse* revolution of the magnet; and that produced by the *reverse* revolution of the wire is the same as that produced by the *direct* revolution of the magnet. A more precise reference of the direction of the current to the particular pole employed, and the direction of the revolution of the wire or magnet, is not at

Fig. 4.

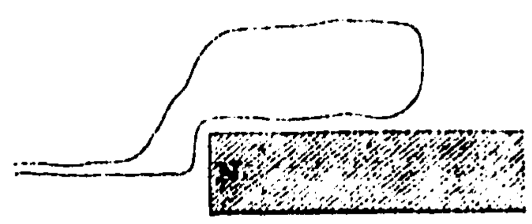


present necessary; but if required is obtained at once by reference to fig. 3 (3088.), or to the general law (114. 3079. note).

3092. The magnet and loop being rotated together in either direction, no trace of an electric current was produced. In this case the effect, if any, could be greatly exalted, because the rotation could be continued for 10, 20, or any number of revolutions without derangement, and it was easy to make thirty revolutions or more within the time of the swing of the galvanometer needle in one direction. It was also easy, if any effect were produced, to accumulate it upon the galvanometer by reversing the rotation at the due time. But no amount of revolution of the magnet and wire together could produce any effect.

3093. The loop was then taken out of the axis of the magnet, but attached to it by a piece of pasteboard, so that all should be fixed together and revolve with the same angular velocity, fig. 5; but whatever the shape or disposition of the loop, whether large or small, near or distant, open or shut, in one plane, or contorted into various planes; whatever the shape or condition, or place, provided it moved altogether with the magnet, no current was produced.

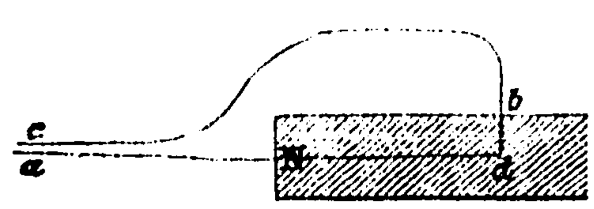
Fig. 5.



3094. Furthermore, when the loop was out of the magnets, and by expedients of arrangement, was retained immoveable, whilst the magnet revolved, no amount of rotation of the magnet (unaccompanied by translation of place) produced any degree of current through the loop.

3095. The loop of wire was then made of two parts; the portion *c*, fig. 6, on the outside of the magnet, was fixed at *b*, and the portion *a*, being a separate piece, was carried along the axis until it came in contact with the former at *d*; the revolution of one part was thus permitted either with or without the other, yet preserving always metallic contact and a complete circuit for the induced current. In this case, when the external wire and the magnet were fixed, no current was produced by any amount of revolution of the wire *a* on its axis. Neither was any current produced when the magnet and wire, *c d*, were revolved to-

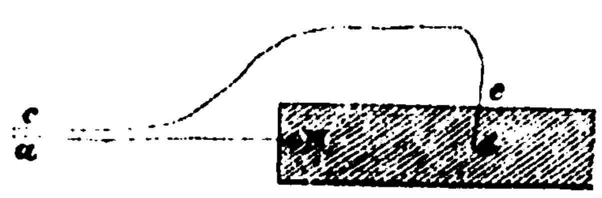
Fig. 6.



gether, whether the wire *a* revolved with them or not. When the magnet was revolved without the external part of *c d*, or the latter revolved without the magnet, then currents were produced as before (3091.).

3096. The magnet was now included in the circuit, in the following manner. The wire *a*, fig. 7, was placed in metallic contact on both sides of the interval between the magnets at N (or the pole), and the part *c* was brought into contact with the centre at *d*.

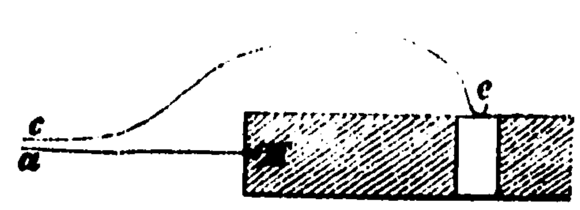
Fig. 7.



The result was in everything the same as when the wire *a* was continued up to *d*, *i. e.* no amount of revolution of the magnet and part *c* together could produce any electric current. When *c* was made to terminate at *e* or the equatorial part of the magnet, the result was precisely the same. Also, when *c* terminated at *e*, the part *a* of the wire was continued to the centre at *d*, and there the contact perfected, but the result was still the same. No difference, therefore, was produced, by the use between N and *d*, or *d* and *e*, of the parts of the magnet in place of an insulated copper wire, for the completion of the circuit in which the induced current was to travel. No rotation of the part *a* produced any effect, wherever it was made to terminate.

3097. In order to obtain the power of rotating the magnet without the external part of the wire, a copper ring was fixed round, and in contact with it at the equatorial part, and the wire *c*, fig. 8, made to bear by spring pressure against this ring, and also against the ring H on the axis, fig. 1 (3084.); the circuit was examined, and found complete. Now when the wire *c e* was fixed and the magnet rotated, a current was produced, and that to the same amount for the same number of revolutions, whether the part of the wire *a* terminated at N, or was continued on to the centre of the magnet, or was insulated from the magnet and continued up to the copper ring *e*. When the wire, by expedients, which though rough were sufficient, was made to revolve whilst the magnet was still, currents in the contrary direction were produced, in accordance with the effect before described

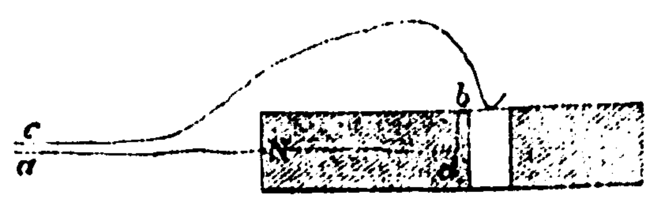
Fig. 8.



(3091.); and the results when the wire and magnet rotated together (3092.), show that these are in amount exactly equal to the former. When the inner and the outer wires were both motionless, and the magnet only revolved, a current in the full proportion was produced, and that, whether the axial wire *a* made contact at the pole of the magnet or in the centre.

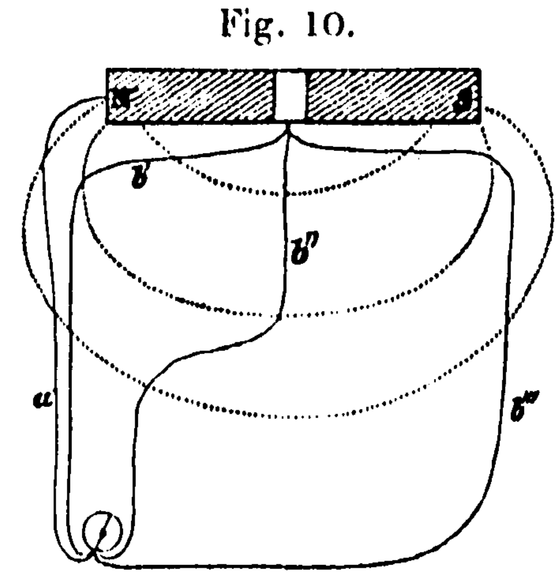
3098. Another arrangement of the magnet and wires was of the following kind. A radial insulated wire was fixed in the middle of the magnets, from the centre *d*, fig. 9, to the circumference *b*, being connected there with the equatorial ring (3097.); an axial wire touched this radial wire at the centre and passed out at the pole; the external part of the circuit, pressing on the ring at the equator, proceeded on the outside over the pole to form the communication as before. In the case where the magnet was revolved without the axial and the external wire, the full and proper current was produced; the small wire, *d b*, being, however, the only part in which this current could be generated by the motion; for it replaced, under these circumstances, the body of the magnet employed on the former occasion (3097.).

Fig. 9.



3099. The external part of the wire instead of being carried back over that pole of the magnet at which the axial wire entered, was continued away over the other pole, and so round by a long circuit to the galvanometer; still the revolution of the magnet, under any of the described circumstances, produced exactly the same results as before. It will be evident by inspection of fig. 10, that, however the wires are carried away, the general result will, according to the assumed principles of action, be the same; for if *a* be the axial wire, and *b′*, *b″*, *b‴* the equatorial wire, represented in three different positions, whatever magnetic lines of force pass across the latter wire in one position, will also pass across it in the other, or in *any* other position which can be given to it. The distance of the wire at the place of

Fig. 10.



intersection with the lines of force, has been shown, by the experiments (3093.), to be unimportant.

3100. Whilst considering the condition of the forces of a magnet, it may be admitted, that the two magnets used in the experimental investigations described, act truly as one central magnet. We have only to conceive smaller similar magnets to be introduced to fill up the narrow space not occupied by the wire, and then the complete magnet would be realized:—or it may be viewed as a magnet once perfect, which has had certain parts removed; and we know that neither of these changes would disturb the general disposition of the forces. In and around the bar magnet the forces are distributed in the simplest and most regular manner. Supposing the bar removed from other magnetic influences, then its power must be considered as extending to any distance, according to the recognized law; but, adopting the representative idea of *lines of force* (3074.), any wire or line proceeding from a point in the magnetic equator of the bar, over one of the poles, so as to pass through the magnetic axis, and so on to a point on the opposite side of the magnetic equator, must intersect *all* the lines in the plane through which it passes, whether its course be over the one pole or the other. So also a wire proceeding from the end of the magnet at the magnetic axis, to a point at the magnetic equator, must intersect curves equal to half those of a great plane, however small or great the length of the wire may be; and though by its tortuous course it may pass out of one plane into another on its way to the equator.

3101. Further, if such a wire as that last described be revolved once round the end of the magnet to which it is related, a slipping contact at the equator being permitted for the purpose, it will intersect *all* the lines of force during the revolution; and that, whether the polar contact is absolutely coincident with the magnetic axis, or is anywhere else at the end of the bar, provided it remain for the time unchanged. All this is true, though the magnet may be subject, by induction at a distance, to other magnets or bodies, and may be exerting part of its force on them, so as to make the distribution of its power very irregular as compared to the case of the independent bar (3084.), or may have an irregular or contorted shape, even up to the horseshoe form. It is evident, indeed, that if a wire have one of its ends applied to *any* point on the surface of a magnet, and the other end to a point in the magnetic equator, and the latter be slipped once round the magnetic equator, and the loop of wire be made to pass over either pole, so as at last to resume its first position, it will in the course of its journey have intersected *once* every line of force belonging to the magnet.

3102. A wire from pole to pole which passes close to the equator, of course intersects half the external lines of force in a great plane, twice, in opposite directions as regards the polarity; and, therefore, when revolved round the magnet, has no electric current induced in it. If it do not touch at the equator, still, whatever lines it intersects, are twice intersected, and so the same equilibrium is preserved. If the magnet rotate under the wire, it acts the part of the central rotating wire already referred to (3095.); or if any course for the electric current other than a right line is assumed in it, that course is subject to the law of neutrality above stated, as will be seen by reference to the internal condition of the magnet itself (3117.). Hence the reason why no currents are produced, under any circumstances of motion, by the application of such conducting circuits to the magnet. I may further observe, in reference to the intersection of the lines of force, that if a wire ring, a little larger in diameter than the magnet, be held edgeways at one of the poles, so that the lines of force there shall be in its plane, and be then turned 90° and carried over the pole to the equator (3088.), it will intersect *once* all the lines of the magnet, except the very few which will remain unintersected at the equator.

3103. Whilst endeavouring to establish experimentally the definite amount of the power represented by the *lines of force*, it is necessary to take certain precautions, or the results will be in error. For instance, ten revolutions of the wire about the magnet, or of the magnet within the fixed wire (3097.), ought to give a constant deflection at the galvanometer, and yet without any change in the position of the wire the results may at different times differ very much from each other; being at one time 9°, and at another only 4° or 5°. I found this to be due to difference of velocity within certain limits, and to be explained and guarded against as follows.

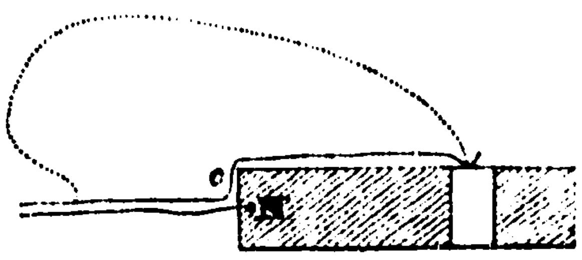
3104. If a wire move across lines of force slowly, a feeble electric current is produced in it, continuing for the time of the motion; if it move across the same lines quickly, a stronger current is produced for a shorter time. The effect of the current which deflects a galvanometer needle, is opposed by the action of the earth, which tends to return the needle to zero. A continuous weak current, therefore, cannot deflect it so far as a continuous stronger current. If the currents be limited in duration, the same effect will occur unless the time of the swing of the needle to one side be not considerably more than the time of either of the currents. If the time of the needle-swing be ten, and the time of ten quick rotations be six, then all the effect of the induced current is exerted in swinging the needle; but if the time of ten slow rotations be twelve or fifteen, then part of the current produced is not recognized by the extent of the vibration, but only by its holding the needle out awhile, at the extremity of a smaller arc of declination. Therefore, when quick and slow velocity was compared, and, indeed, in every case of comparative rotations of the wire and magnet, only that number of rotations was taken which could be well included within the time of the needle's journey to one side;—when the needle, therefore, was seen to travel on to its extreme distance after the rotation and the inducing current had ceased. If the needle began to return the instant the motion was over, such an experiment was rejected for purposes of comparison. When these precautions were attended to, and velocities of revolution taken, which occupied times from one-third to three-fourths of that required for the swing of the needle, then the same number of revolutions (ten) gave the same amount of deflection, namely, $9^{\circ}{\cdot}5$, with my apparatus, though the time of revolution varied as 1 : 2, or even in a higher degree.

3105. Another cause of difference produced by varying velocity, is the diminution of the action of the current on the needle, as the angle which the latter forms with the convolutions of the coil increases. Hence a constant current produces more effect on the deflection of the needle for the first moments of time than afterwards. This effect, however, was scarcely sensible for swinging deflections of 9° or 10°, produced by currents which were over before the needle had moved through 4° or 5°.

3106. It has already been shown, that it is a matter of indifference whether the wire revolve in one direction or the magnet in the other (3091.); and this is still further proved by the cases where the magnet and the wire revolve together (3092.); for then the currents which tend to form are exactly equal and opposed to each other, whatever the position of the wire may be. As the immobility of the needle is a point more easily ascertained than the extent of an arc, indicated only for a moment, and as the rotations of the magnet and wire conjointly can be made rapid and continuous, the proof in such cases is very satisfactory.

3107. Proceeding to experiment upon the effect of the *distance* of the wire *c*, fig. 11, from the magnet, the wire was made to vary, so that sometimes it was not more than 8 inches long (being of copper and 0·04 of an inch in diameter), and only half an inch from the magnet, whilst at other times it was 6 or 8 feet long and extended to a great distance. The deflection due to ten revolutions of the magnet was observed, and the average of several observations, for each position of the wire, taken: these were very close (with the precautions before described) for the same position; and the averages for different positions agreed perfectly together, being 9°·5. I endeavoured to repeat these experiments on distance by moving the wire and preserving the magnet stationary in the manner before described (3091.); they were not so striking because time would only allow of smaller deflections being obtained (3104.), but the same number of journeys through an arc of 180° gave the same deflection at the galvanometer, whether the course of the wire was close to the magnet or far off; and the deflection agreed with those obtained when the magnet was rotating and the wire at rest.

Fig. 11.



3108. As to *velocity* of motion; when the magnet was rotating and the wire placed at *different distances*, then ten revolutions of the magnet produced the same deflection of the needle, whether the motion was *quicker* or *slower*, and whatever the distance of the wire, provided the precautions before described were attended to (3104.). That the same would be true if the wire were moving and the magnet still, is shown by this; that whatever the velocity with which the wire and magnet revolve together, and whatever their distance apart, they exactly neutralize and equal each other (3096.).

3109. From these results the following conclusions may be drawn. The *amount* of magnetic force, as shown by its effect in evolving electric currents, is determinate for the same lines of force, whatever the distance of the point or plane, at which their power is exerted, is from the magnet. Or it is the same in any two, or more, sections of the same lines of force, whatever their form or their distance from the seat of the power may be. This is shown by the results with the magnet and the wire, when both are in the circuit (3108.); and also by the wire loop revolving with the magnet (3092.); where the tendency of currents to form in the two parts oppose and exactly neutralize or compensate each other.

3110. In the latter case very varying sections outside of the magnet may be compared to each other; thus, the wire may be conceived of as passing (or be actually formed so as to intersect) lines of force near the pole, and then, being continued *along* a line of force until over the equator, may be directed so as to intersect the same lines of force in the contrary direction, and then return along a line of force to its commencement; and so two surface sections may be compared. It is manifest that every loop forming a complete circuit, which is in a great plane passing through the axis of the magnet, must have precisely the same lines of force passing into and passing out of it, though they may, so to say, be expanded in one part and compressed in another; or (speaking in the language of radiation) be more intense in one part and less intense in the other. It is also as manifest, that, if the loop be not in one plane, still, on making one complete revolution, either with or without the magnet, it will have intersected in its two opposite parts an exactly equal amount of lines of force. Hence the comparison of any one section of a given amount of lines of force with any other section is rendered, experimentally, very extensive.

3111. Such results prove, that, under the circumstances, there is no loss, or destruction, or evanescence, or latent state of the magnetic power by distance.

3112. Also that convergence or divergence of the lines of force causes no difference in their amount.

3113. That obliquity of intersection causes no difference. It is easy so to shape the loop (3110.), that it shall intersect the lines of force directly across at both places of intersection, or directly at one and obliquely at the other, or obliquely in any degree at both; and yet the result is always the same (3093.).

3114. It is also evident, by the results of the rotation of the wire and magnet (3097. 3106.), that when a wire is moving amongst equal lines (or in a field of equal magnetic force), and with an uniform motion, then the current of electricity produced is proportionate to the *time*; and also to the *velocity* of motion.

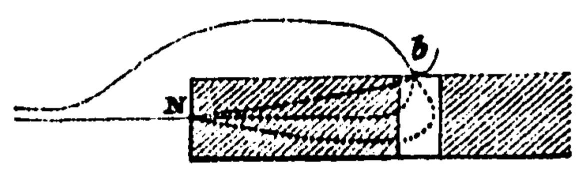
3115. They also prove, generally, that the quantity of electricity thrown into a current is directly as the amount of curves intersected.

3116. In addition to these results, this method of investigation gives much insight into the internal condition of the magnet, and the manner in which the lines of force (which represent truly all that we are acquainted with of the peculiar action of the magnet) either terminate at its exterior, or at any assumed points, to be called poles; or are continued and disposed of within. For this purpose, let us consider the external loop (3093.) of fig. 5. When revolving with the magnet no current is produced, because the lines of force which are intersected on the one part, are again intersected in an opposing direction on the other (3110.). But if one part of the loop be taken down the axis of the magnet, and the wire then pass out at the equator (3091.), still the same absence of effect is produced; and yet it is evident that, external to the magnet, every part of the wire passes through lines of force, which conspire together to produce a current; for all the external lines of force are then intersected by that wire in one revolution (3101.). We must therefore look to the part of the wire *within* the magnet, for a power equal to that capable of being exerted externally, and we find it in that small portion which represents a radius at the central and equatorial parts. When, in fact, the axial part of the wire was rotated it produced no effect (3095.); when the axial, the inner radial, and the external parts were revolved together, they produced no effect; when the external wire alone was revolved, *directly*, it produced a current (3091.); and when the internal radius wire alone (being insulated from the magnet) revolved, *directly*, it also produced a current (3095. 3098.) in the contrary direction to the former; and the two were exactly equal in power; for when both portions of the wire moved together *directly*, they perfectly compensated each other (3095.). This radius wire may be replaced by the magnet itself (3096. 3118.).

3117. So, by this test there exists lines of force within the magnet, of the same *nature* as those without. What is more, they are exactly equal in *amount* to those without. They have a relation in *direction* to those without; and in fact are continuations of them, absolutely unchanged in their nature, so far as the experimental test can be applied to them. Every line of force therefore, at whatever distance it may be taken from the magnet, must be considered as a closed circuit, passing in some part of its course through the magnet, and having an equal amount of force in every part of its course.

3118. When the axial part of the wire is dismissed and the magnet employed in its place, so as to be included in the circuit, it is easy to see how it acts the part of the conductor. For suppose the wire itself to be continued from N to *b*, fig. 12, by any of the three paths indicated by dotted lines, the effect is the same in all the cases, both by experiment (3093.) and by principle (3100.). For whatever the form of the path, it will in one revolution intersect the same amount of lines of force within the magnet, as are intersected in the contrary direction by the part of the wire outside the magnet; and when the magnet is employed to complete the circuit in place of the internal wire, then its substance produces precisely the same result; for direction and every other circumstance which influences the result remains the same: one conductor has simply been substituted for another. The great mass of the magnet might be supposed able to do something more than the thin wire, but the reason why it only equals it in effect will be seen hereafter (3137.) And as the axial wire, in revolving, does nothing but conduct (3095.), all the effect being produced by that part which represents a radius between the axis and the equator (3098.); so the magnet, rerevolving as a cylinder, is as to its mass like the revolving wire; with the exception of so much of it as represents a radius connecting together the two points at the pole or axis and at the equator, where communication with the wire is completed. As was shown long ago (220.), if a cylinder magnet be revolved, and the ends of the galvanometer wires *a c* be applied to the extremities of its axis, no current is evolved; but if *a* be applied

Fig. 12.



to one end, it matters not which, and *c* be applied at the equator or any other part on the surface of the cylinder, a current always in the same direction for the same rotation will be produced.

3119. Further to prove these points, the magnets were cut in half through the equatorial plane, and then, either a disc of copper placed there, or a wire radius only, or the magnets brought together again: and these three arrangements were used in succession to complete the circuit from the axial wire (3095.) to a fixed wire at the surface of the equator. Whichever was employed the current produced was the same, both in direction and amount. If the cylinder magnet above described (3118.) be terminated at the ends by attached discs of silver or copper, the wires applied to their surfaces, as they revolve with the magnet, produce precisely the same currents as to direction as if applied to the surface of the magnet itself (218. 219.).

3120. In this striking disposition of the forces of a magnet, as exhibited by the moving wire, it exactly resembles an electro-magnetic helix, both as to the direction of the lines of force in closed circuits, and in their equal sum within and without. No doubt, the magnet is the most heterogeneous in its nature, being composed, as we are well-aware, of parts which differ much in the degree of their magnetic development; so much so, that some of the internal portions appear frequently to act as keepers or submagnets to the parts which are further from the centre, and so, for the time, to form complete circuits, or something equivalent to them, within. But these make no part of the resultant of force externally, and it is only that resultant which is sensible to us in any way; either by the action on a needle, or other magnets, or soft iron, or the moving wire. So also the power which is manifest *within* the magnet by its effect on the moving mass, is still only that same resultant; being equal to, and by polarity and other qualities, identical with it. No doubt, there are cases, as upon the approach of a keeper to the poles, or the approximation of other magnets, either in favourable or adverse positions, when more external force is developed, or it may be a portion apparently thrown inwards and so the external force diminished. But in these cases, that which remains externally existent, corresponds precisely to that which is the resultant internally; for when either the same, or contrary poles, of a powerful horseshoe magnet were placed within an inch and a half of the poles of the bar-magnets, prepared to rotate with the attached wires (3092.), as before described, still, upon their revolution, not the slightest action at the galvanometer was perceived; the forces within the magnet and those without perfectly compensating each other.

3121. The definite character of the forces of an invariable magnet, at whatever distance they are observed from the magnet, has been already insisted upon (3109.). How much more strikingly does that point come forth now, that, being able to observe within the magnet, we find the same definite character there; every section of the forces, whether within or without the magnet, being exactly of the same amount! The power of a magnet may therefore be easily represented by the effects of *any* section of its lines of force; and as the currents induced by two different magnets may easily be conducted through one wire, or be, in other ways, compared to each other, so facilities may thus arise for the establishment of a standard amongst magnets.

3122. On the other hand, the use of the idea of *lines of force*, which I recommend, to represent the true and real magnetic forces, makes it very desirable that we should find a unit of such force, if it can be attainable, by any experimental arrangement, just as one desires to have a unit for rays of light or heat. It does not seem to me improbable that further research will supply the means of establishing a standard of this kind. In the mean time, for the enlargement of the utility of the idea in relation to the magnetic force, and to indicate its conditions graphically, lines may be employed as representing these units in any given case. I have so employed them in former series of these Researches (2807. 2821. 2831. 2874. &c.), where the direction of the *line of force* is shown at once, and the relative amount of force, or of lines of force in a given space, indicated by their concentration or separation, *i. e.* by their number in that space. Such a use of unit lines involves, I believe, no error either in the direction of the polarity or in the amount of force indicated at any given spot included in the diagrams.

3123. The currents produced in wires, when they cross lines of magnetic force, are so feeble in intensity (though abundant enough in quantity, as many results show), that a fine wire galvanometer must of necessity offer great obstruction to their passage. Therefore, before entering upon further experimental inquiries, I had another galvanometer constructed, in which the needles belonging to that made by Rhumkorff were employed, but the coil was replaced by a single convolution of very stout wire. The wire was of copper, 0·2 of an inch in diameter. It passed horizontally under the lower needle, then, as nearly as might be, between that and the upper needle, over the upper, and then again between that and the lower needle, fig. 13, and was afterwards attached to the stand, and continued for 19 or 20 feet outside of the glass cover. Such a wire had abundant conducting power; and though it passed but once round each needle, gave a deflection many times greater than that belonging to the former galvanometer. Thus when the ends of the nineteen feet of wire were soldered together, so as to form one loop or circuit, the passage of the wire once between the poles of a horseshoe magnet (3124.), caused a deflection, or rather swing of the needle of above 90°. I have had a more perfect instrument, of the same kind, constructed, in which the conducting coil was cut out of plates of copper, so as to form a square band 0·2 of an inch in thickness, which passed twice round the vibration plane of each needle, as represented, fig. 14. The length of metal around the needles was 24 inches, and the galvanometer was very sensitive, but the experiments to be described were made chiefly with the former instrument.

Fig. 13.

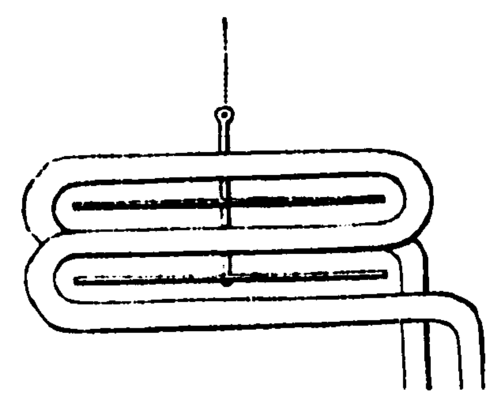
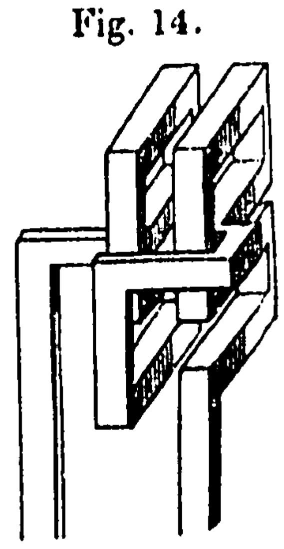
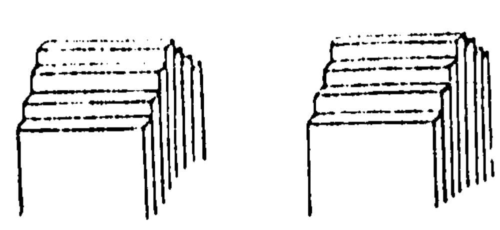


Fig. 14.



3124. It was necessary, first, to ascertain the effect of certain circumstances upon this simple galvanometer, as to their modification of its indications. The magnet to be used was a compound horseshoe instrument, weighing 16 lbs., and able to support 40 lbs. by the keeper or submagnet. It is some years since it was magnetized, and it is therefore, probably, in a nearly constant state as to power. The poles have the form delineated, fig. 15. Their distance apart is 1·375 inch, and the distance

Fig. 15.



downwards, from their summit to the bottom or equator of the magnet, is 8·5 inches. The galvanometer stood in the prolongation of the magnetic axis, *i. e.* the line from pole to pole, and whether it were 6 or only 3 feet distant, was hardly at all affected in the time of its vibration, being adjusted so nearly astatic as to require about ten seconds to swing to the right or to the left.

3125. On passing the wire across the magnetic field, as just described (3123.), but with different velocities, effects different in degree were obtained at the galvanometer, for the reasons formerly given (3104. 3106.). The quickest velocity gave the greatest result, equal at times to 140°, whilst a very slow motion gave only 30° or 40°. Still with moderately quick velocities the effects were nearly alike, and by operating with the same velocity, and taking the average of several observations, a very uniform result could be obtained.

3126. On cutting the wire across, and then putting the ends together in various ways, it was found that great care was requisite in making contact, in this or in similar cases. Thus, to press the ends lightly together was not sufficient; they required to be well and recently cleaned and pressed closely into contact. Junctions effected by soldering or dipping into cups of mercury were still better, when made with care, and were employed at the galvanometer and elsewhere as often as possible.

3127. To ascertain generally the obstruction caused by the interposition of thin wires, 28 inches of copper wire, 0·045 of an inch in diameter, was introduced into the circuit at a part away from the magnet, with excellent junctions. The oscillation or swing, which before was 140° or more, was now reduced to 40°. On taking out the wire and replacing it by another, also of copper, but only 19·5 inches in length, and 0·0135 in diameter, the deflection was reduced to 7° or 8°.

3128. For a rough comparison of the power of this magnet and the former bar magnets (3085.), by the present galvanometer, the thick wire was bent into a loop (3086.), and the two bar magnets, with like ends together, passed quickly through it up to the equatorial part; the deflection was about 30°. Such a passage intersected nearly all the lines of force of the bar magnets. A similar motion of the magnets close to, but outside of, the loop, produced no effect at the galvanometer.

3129. In respect of the alteration of the lines of force, either in position or in total amount, by bringing the poles of the horseshoe magnet (3124.) much nearer together, the following experiments were made. The distance between the poles is 1·375 inch; by placing a cube of soft iron, 0·8 of an inch in the side, within this space, it was diminished to 0·575, and thus, virtually, the distance apart much lessened, and, as was afterwards shown experimentally (3130.), the external power of the magnet concentrated there. Then, whilst the cube was in place, the thick wire of 0·2 of an inch in diameter, was arranged so as to pass across the magnetic axis or place of strongest action, and fixed; after which the iron cube was alternately removed and again restored, and the effects observed. Feeble electric currents were produced at these times; but whether the cube was put into its place from below, or above or the sides, the current produced was always in the same direction; and when it was removed the current produced was in the reverse direction. If the cube were carried up to, by, and away from, the magnetic axis in one motion, then there was no effect at the galvanometer. On the other hand, when the wire was carried across the magnetic field as described (3123.), so as to intersect all the lines of force in one movement, and sum up their power at the galvanometer, then there was no difference in the result, whether the iron cube was in its place or not; showing, as far as this apparatus could indicate, that the sum of power in the section of all the lines of force external to the magnet, was the same under both circumstances, though the distribution of it was different.

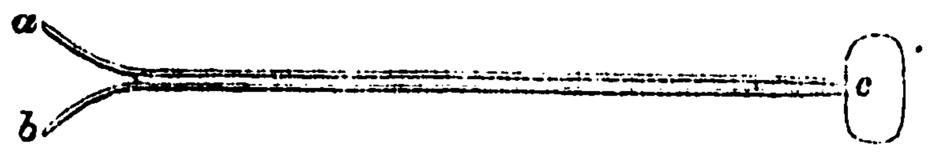
3130. The very action produced by the cube, when in and out of place (3129.), upon the forces which affected the stationary wire, was a proof of the difference of distribution at different times.

3131. A block of bismuth, employed in place of the iron cube, had no sensible effect upon the wire whether it were still or moving.

3132. This galvanometer was first employed for a repetition of all the former experiments with the bar magnets (3091. &c.). The results were absolutely the same, except that the amount of the deviation produced, when deviation was a result, was larger than in the former cases.

3133. For the comparison of different thicknesses of the same metal, I took copper wires in lengths of 10·5 inches, and different diameters, and bending them into loops of a form and size such as would admit them to pass with facility over a pole of the horseshoe magnet, soldered them to the ends of two conducting rods, made of copper wire 0·2 of an inch in diameter and 35 inches in length each, which were fixed on opposite sides of a narrow slip of wood. The whole arrangement is seen in fig. 16;

Fig. 16.



the terminations *a b* dip into the mercurial cups of the galvanometer, the parts at *c* are brought so close together as to touch, except for the intervention of a piece of card, and thus the parts from *c* to *a b* are thrown out of action, except as mere conductors, whilst the loop, being made to descend over one magnetic pole, intersects very nearly the whole of the magnetic curves, and always the same proportion.

3134. The former magnet was too powerful for comparative experiments, therefore a smaller one was employed, consisting of five plates, weighing 8 lbs., and able to carry 21 lbs. easily at the keeper. The poles were 1·2 inch apart and an inch thick each, in the direction of the magnetic axis. If less magnetic power were required, an adjustment was easily made, by applying the keeper to the side upon both limbs, the magnetic communication being effected either nearer to the poles, or nearer to the equator or bend, as less or more power was required. The descent of the loop between the poles is then best regulated by causing the conductor wires to bear ultimately against a stopping-block.

3135. The effect of a quick and a slow motion was found to be the same as before (3104. 3105.). Such velocities as the hand could impart were very effectual, and gave results of very considerable uniformity when quick motions were employed.

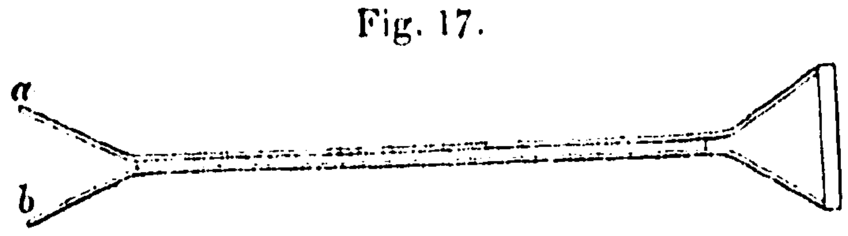
3136. Three different loops were compared together, consisting of copper wire, the diameters of which were 0·2, 0·1 and 0·05 of an inch, or as 4, 2 and 1; their sectional areas or masses therefore were as 16, 4 and 1. Ten or twelve observations were made with each loop; the results were near together, and the average for each loop, being the extent of the swing declination on one side from zero, is as follows:—

Copper wire of $\frac{1}{20}$th of an inch in thickness . .	16°·00
Copper wire of $\frac{1}{10}$th of an inch in thickness . .	44·40
Copper wire of $\frac{1}{5}$th of an inch in thickness . . .	57·37

Now though the thicker wires produced the largest effect, the results were evidently not at all in proportion to the masses of the wires; the smaller having greatly the advantage in that respect. On the other hand, when four of the smaller wires were placed side by side, so as to form one loop equal in mass to the second loop, it gave the same result as that loop, being of the same power.

3137. The disproportion of the *difference* of these three wires is evidently a consequence of the relative difference of the mere conducting part of the circuit. To compare accurately the effect of the lines of force on wires of different diameters moving across them, these diameters should continue to, and through the galvanometer (205.), otherwise the thin wire current has an advantage given to it in the conducting part, which the thick wire current has not. Hence the reason why a thin wire galvanometer, such as that before described (3086.), gives results which are alike, for thick or thin wire loops, or for fasciculi of few or many wires. To enlarge the comparison, I soldered on to two pairs of conductors, the dimensions of those described (3133.), two cylinders of copper, each 5·5 inches long, but one was only 0·2 of an inch thick and the other 0·7, or twelve times the mass of the first, fig. 17. They were then passed in suc-

Fig. 17.



cession between the poles of the magnet, and gave results very nearly alike. If there was any difference, the effect was highest with the smallest cylinder; and this may very well be; for as the magnetic field was not equal in force, but most intense in the magnetic axis, so it is evident, that whilst one part of the large cylinder, in passing across, was at the axis, other parts were in places of less intense force and action, and so a return current may have existed in them, which could not occur to the same extent in a cylinder little more than a fourth of the diameter of the former, and which, at the same time, had an outlet for the currents equal to its own diameter, through the conducting wires. A similar relation of mass occurs in the case where the body of the magnet itself in revolving, does no more than a small radial wire within it (3118.).

3138. The influence of this lateral conduction (3137.), in cases of magneto-electric conduction, must be well understood; otherwise, in the application of the principles to investigation, errors will frequently creep in. Their effect may be shown in the following instances:—a loop of four wires, 0·048 of an inch in diameter (3136.), was passed over the pole of the magnet, and produced a certain result of deflection or swing; when the wires were separated two and two, so as to be half or three-quarters of an inch apart, and when, therefore, in moving across the magnetic field, one pair went before the others, the effect was less, for the reason already given in the case of the copper cylinder (3137.). When three wires were allowed to go by together, but one taken aside a couple of inches, the effect fell very much; and when that fourth one was cut across to prevent the return current in it, the effect of the three rose at the galvanometer very greatly, almost equalling the effect of the four when together.

3139. A loop was constructed of seventy-six equal fine copper wires, each 10·5 inches long and 0·0125 of an inch in diameter, and its effect observed when more and more of the wires were cut away. As it is the comparison of the smaller numbers of wires, one with the other, that is of most value, I will give the averages of each number for several observations, in the reverse order in which they were obtained; and I introduce the results with larger numbers of wires only for the general purpose of showing how the effect passes into that with the cylinder of copper (3137.), the galvanometer conductors always being of the same length and thickness.

1 wire produced an average swing of . .	8°·3
2 wires produced an average swing of . .	15·3
3 wires produced an average swing of . .	21·8
4 wires produced an average swing of . .	27·9
5 wires produced an average swing of . .	34·4

6 wires produced an average swing of	. .	37°·8
8 wires produced an average swing of	. .	50·1
12 wires produced an average swing of	. .	65·1
16 wires produced an average swing of	. .	80·5
26 wires produced an average swing of	. .	118·0

36 almost swung the needle round.
46 stronger than the last.
56 swung the needle quite round.
66 a little stronger.
76 stronger: swung the needle freely round the circle.

Each time that the needle passed 180°, it was returned, that the torsion force might remain the same for every case.

3140. When the loop of four equal wires (3136.) was employed, so arranged that, in respect of the part which passed between the poles, they should be close together in one plane, it made no difference in the result, whether that plane was perpendicular to the magnetic axis or parallel to it; *i. e.* whether the wires in moving, formed a band which moved edgeways or flat ways; the results were the same as with the four wires close together, so as to represent, as far as they could, a round or square wire.

3141. From all these results it may be concluded, that the current or amount of electricity evolved in the wire moving amongst the lines of force, is not, simply, as the space occupied by its breadth correspondent to the direction of the line of force, which has relation to the *polarity* of the power, nor by that width or dimension of it which includes the number or *amount* of the lines of force, and which, corresponding to the direction of the motion, has relation to the *equatorial* condition of the lines; but is jointly as the compound ratio of the two, or as the mass of the moving wire. The power acts just as well on the interior portions of the wire as on the exterior or superficial portions, and a central particle, surrounded on all sides by copper, is just in the same relation to the force as those which, being superficial, have air next them on one side.

3142. By immersing the poles of the magnet in different media, and then making comparative experiments with the same copper wire loop (3145.), it was found that the amount of the induced current was the same in air, water, alcohol and oil of turpentine. The experiments in air were repeated between those with the liquids, so as to give a very consistent and safe result as to the equality of action in all the cases.

3143. The effect of *variation of substance* was the next subject which seemed to me important to bring under investigation, because it has a direct relation to the amount of force exerted, or ready to be exerted, within solid bodies, at any distance from the magnet, in situations and under circumstances where it was absolutely impossible to apply the vibrations of a magnetic needle, or any other form of the effects of attractive and repulsive forces. The interior of such bodies as iron, copper, bismuth, mercury, &c., including the most paramagnetic and the most diamagnetic, seemed, in this way, open to experimental investigation, both as to the amount of lines of force traversing them under various circumstances, and also as to the direction of the lines or their polarity.

3144. In an early series of these Researches[1], experiments bearing upon this subject are described (205-213.). Wires of different metals were moved across the lines of force of a magnet, and the result arrived at was, that the currents induced in these different bodies were proportional to their electro-conduction power (202. 213.).

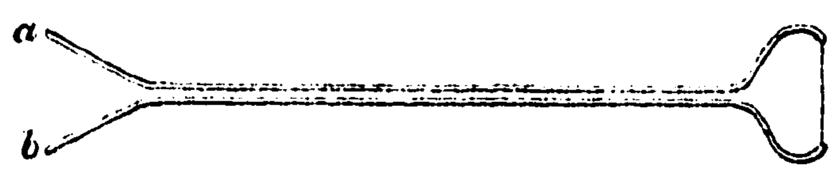
3145. The thick wire galvanometer (3123.), with its good and short conducting communications, promised however better results, and therefore loops like those already described of copper wire (3133.), were prepared with wires of different metals, all of the same diameter, namely, 0·04 of an inch, being only $\frac{1}{23}$th of the substance of the conducting and galvanometer wire. The metals were copper, silver, iron, tin, lead, platinum, zinc. Under these circumstances the substance concerned in the excitement of the current is made to vary, whilst the conducting part of the system is very good and remains the same. The results with these loops were as follows, being the average of from six to ten experiments for each loop:—

Copper . .	63°·0	Iron . . .	18°·0
Silver. . .	61·9	Platinum .	16·9
Zinc . . .	31·5	Lead . . .	12·1
Tin . . .	19·1		

[1] Philosophical Transactions, 1832, pp. 179-182.

3146. In order to dismiss, as much as possible, the obstruction caused by bad conducting power, and bring out any difference that might exist between paramagnetic and diamagnetic metals, three metals were selected, namely, tin, iron and lead in wires, as before, of 0·04 of an inch diameter; but the length was restricted to 3 inches, instead of extending to 10·5 inches, and the rest of the loop was made up of the conducting copper wire of 0·2 in diameter, as in fig. 18. Of course, the effect of the whole loop

Fig. 18.



is a mixed effect, being partly due to the power represented by the lines intersected by the thick copper portion, and partly by those intersected by the three inches of special wire passing between the poles. But as the great amount of force is concentrated within a space not more than an inch and a half or 2 inches in extent (as is seen on carrying any of the loops across the magnetic axis), and as even that could be made still more concentrated by using the iron cube (3129.), and so bringing the poles virtually nearer to each other, it was hoped that the chief effect would be there, and so any peculiar difference existing between iron on the one hand and tin and lead on the other, be rendered manifest, especially as the resistance to conduction was greatly diminished by shortening the wires from 10·5 to 3 inches.

3147. The many experiments made with each metal were very close together. The average of the results for the three metals was as follows:—

	c
Tin	37·1
Iron	34·8
Lead	25·4

The proportions, and therefore the results, are almost identical with those obtained before (3145.).

3148. When lead and copper, arranged at the bar magnets (3084. 3085.), had been compared in former experiments with each other by the fine wire galvanometer, the results for both had been the *same*. But then the two wires used were short, and far thicker than the wires of the galvanometer or of the conducting circuit, and were therefore limited in the production of their peculiar action, by those circumstances of mass already described (3137.). To show that that was the case, I now, with the thick wire galvanometer, employed two equal loops of copper and iron wire, 0·2 of an inch in thickness, fig. 16 (3133.), passing them equably over the pole of the small horse-shoe magnet, reduced by the keeper (3134.). The results were very consistent, and the mean of them was, for

Copper	41°·7
Iron	33·7

3149. Here, therefore, the difference between copper and iron is not so great as that of 1 to 1·24; whilst when the conductors, not concerned in the excitement, were very good, and able, comparatively, to carry on to the galvanometer nearly all the effect of the excitement, it was as great as 1 to 3·5, the difference being in the latter case above tenfold what it is in the former.

3150. To raise the effect dependent upon the mass in relation to that of the conducting wires to a still higher degree, I had a cylinder of iron, 5·5 inches in length and 0·7 of an inch in diameter, soldered on to the ends of conducting wires, so as to be in all respect like that of copper before described (3137.). In this case the iron not only rose up to the copper in effect, but even surpassed it; the results being for copper 35°·66, and for iron 38°·32. Thus, under these circumstances of mass, the difference between iron and copper disappears. The apparent inferiority of copper is probably due to the lateral discharge, which before reduced the effect of a cylinder below that of a thick wire (3137.). The iron being a worse conductor in itself, and having equally good conductors in the prolongation of the circuit as when it was employed as wire, would, I think, have proportionately less lateral discharge in it than the copper.

3151. For a comparison, both as regards the particular substance and the mass, I attached a similar cylinder of bismuth to conductors. Its effect, with the same magnet and force, was 23°; a very high proportion in relation to the copper, and no doubt due to its mass. If it could have been compared as a wire, only 0·04 in diameter (3145.), it would probably have appeared almost indifferent (3127.) [1].

[1] When bismuth is soldered into the circuit, it requires to be left a long

3152. So the current of electricity excited in different substances, moving across lines of magnetic force, appears to be directly as the conducting power of the substance. It appears to have no particular reference to the magnetic character of the body, for iron comes between tin and platinum, presenting no other distinction than that due to conducting power, and differing far less from them, than they do from other metals not magnetic.

3153. The amount of *lines of force* (and of the force represented by them) appears, therefore, to be equal for equal spaces occupied and traversed by tin, iron, and platina under the circumstances; for the difference in result is in no proportion to the ordinary magnetic difference, and only as the conducting power. This agrees with the conclusion before arrived at, that, for air, water, bismuth, oxygen, nitrogen, or a vacuum, the lines of force are the same in amount, except as they are more or less concentrated in the substance across which they pass (2807.), according as it is more or less competent to conduct (2797.), or transmit the magnetic force.

3154. Such a conclusion as that just arrived at, brings on the question of what is *magnetic polarity*, and how is it to be defined? For my own part, I should understand the term to mean, the opposite and antithetical actions which are manifested at the opposite ends, or the opposite sides, of a limited (or unlimited) portion of a line of force (2835.). The line of dip of the earth, or a part of it, may again be referred to as the natural case; and a free needle above or below the part, or a wire moving across it (3076. 3079.), will give the direction of the polarity. If we refer to an entirely different and artificial source as the electro-magnetic helix, the same meaning and description will apply.

3155. If the term *polarity* have any meaning, which has reference to experimental facts and not to hypotheses only, beyond that included in the above description, I am not aware that it

time before it is used for experiments, and should then be covered up, and the loop handled with great care; otherwise thermo-currents are produced. For an hour or two after soldering it generates electrical currents, which appear at the galvanometer very irregularly, being probably due to internal molecular changes, which occur from time to time until the whole has acquired a permanent state of equilibrium.

has ever been distinctly and clearly expressed. It may be so, for I dare not venture to say that I recollect all I have read, or even all the conclusions I myself have at different times come to. But if it neither have, nor should have, any other meaning, then the question arises, is it correctly exhibited or indicated in every case by attractions and repulsions, *i. e.* by such like mutual actions of particular bodies on each other under the magnetic influence? A weak solution of protosulphate of iron, if surrounded by water, will, in the magnetic field, point axially; if in a stronger solution than itself, it will point equatorially (2357. 2366. 2422.). The same is true with stronger cases. We cannot doubt it would be true even up to iron, nickel, and cobalt, if we could render these bodies fluid in turn without altering their paramagnetic power, or if we had the command of magnets and of paramagnetic and diamagnetic media, stronger or weaker at pleasure. But in the case of the solutions, we cannot suppose that the weaker has one polarity in the stronger solution and another in the water. The lines of force across the magnetic field have the same general polarity in all the cases, and would be shown experimentally to have it, by the moving wire (3076.), though not by the attractions and repulsions.

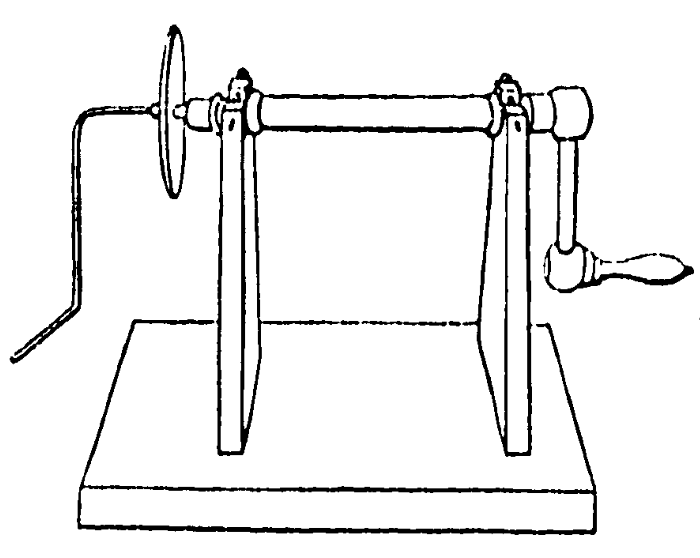
3156. Here, therefore, we have a *difference* in the two modes of experimental indication; not merely as to the method, but as to the nature of the results, and the very principles which are concerned in their production. Hence the value I think of the moving wire as an investigator; for it leads us into inquiries which touch upon the very nature of the magnetic force. There is no doubt that the needle gives true experimental indications; but it is not so sure that we always interpret them correctly. To assume that pointing is always the direct effect of attractive and repulsive forces acting in couples (as in the cases in question, or as in bismuth crystals), is to shut out ideas, in relation to magnetism, which are already applied in the theories of the nature of light and electricity; and the shutting out of such ideas *may be* an obstruction to the advancement of truth and a defence of wrong assumptions and error.

3157. What is the idea of polarity in a field of *equal force?* (whether it be occupied by air or by a mass of soft iron?). A magnetic needle, or an oblong piece of iron, would not show it in the air or elsewhere, except by disturbing the equal arrangement of the force and rendering it unequal; for on that the pointing of the needle or the iron, or the motions of either towards the walls of the magnetic field, if limited (2828.), would depend. A crystal of bismuth in showing this polarity by position (2464. 2839.), does it without much altering the distribution of the force, and the alteration which does take place is in the contrary direction to that effected by iron (2807.), for it expands the lines of force. It seems readily possible that a magnecrystal might exist, which, when in its stable position, should neither cause the convergence nor divergence of the lines of force within it. It need only be neutral in relation to space or any surrounding medium in that direction, and diamagnetic in its relation in the transverse direction, and the conditions would be fulfilled.

3158. But though an ordinary magnetic needle cannot show polarity in a field of equal force[1], having no reference to it, and in fact ignoring such a condition of things, a moving wire makes it manifest instantly, and also shows the full amount of magnetic power to which such polarity belongs; and this it does without disturbing the distribution of the power, as far as we comprehend or understand distribution, when thinking of magnetic needles. At least such at present appears to me to be the case, from the consideration of the action of thin and thick wires (3141.) and wires of different substances (3153.).

3159. As an experimentalist, I feel bound to let experiment guide me into any train of thought which it may justify; being satisfied that experiment, like analysis, must lead to strict truth if rightly interpreted; and believing also, that it is in its nature far more suggestive of new trains of thought and new conditions of natural power. In order to extend its indications, and vary the form in which the principle of the moving wire may be applied, I had an apparatus constructed, fig. 19, consisting of a wooden axis, one extremity

Fig. 19.



[1] One could easily imagine hypothetically a needle that should do so.

of which was terminated by a copper screw, intended to receive and carry one or more discs of metal that might be screwed on to it. This end projected so far beyond the support, that such discs could be partly introduced between the poles of a horseshoe magnet, so as when revolving, to move across the lines of force at their most intense place of action; and, whilst the magnet and the apparatus continued fixed, to revolve continuously across the same lines of force. One of the galvanometer wires was pointed, and so held as to bear into and against the surface of a cup-shaped cavity at the end of the axial screw; and the other was applied by the hand, or so fixed as to bear by a rounded part against the rim of the disc, at that point which was furthest within the poles of the magnet.

3160. Discs of metal were prepared for this apparatus, each 2·5 inches in diameter, and of different thicknesses and material. When a disc of copper was fixed on the axis, and adjusted in association with the large horseshoe magnet (3159.), as described above, three, or even two revolutions of it, would deflect the needle of the thick wire galvanometer through a swing of 30°. In this apparatus, the most effectual part of the portion of the disc which is at any moment passing across the magnetic axis, is that which is near the circumference; for it has the greatest velocity, consequently moves through more space, and that in a part where the lines of force are most concentrated.

3161. The contact at the end of the axle should always be carefully watched and made good. The degree of pressure on the edge of the disc should not be too slight; otherwise the contact, under the circumstances of the motion, is not sufficient to carry forward the same constant proportion of current generated. Neither should it be made at the angles of the disc edge; if a grating or cutting friction occur, an electric current is generated by it. With a smooth hard friction of copper wire against the copper disc there is very little evolution of current. When the copper wire presses against the edge of an iron disc there is far more. In either case, however, the effect may be eliminated or compensated; for, in whichever direction the disc is revolved *without* the magnet, the deviation of the needle, if any be produced, remains the same; whereas, when the magnet is in place, the deviations produced by it are in the reversed direction for reversed revolutions. Hence, if an equal number of revolutions be made in the two directions, and the unequal deflections in opposite directions be noted, the half of their sum will give nearly the amount of deflection which would have occurred if no current had been exerted by friction at the edge, *i. e.* provided the deflections have not been through large arcs. These effects of friction are no doubt objections to the principle in this form; still the results are, as it appears to me, valuable in relation to copper and iron, and are as follows.

3162. A copper disc, 0·05 of an inch in thickness, gave a swing deflection for two revolutions, which, being the average of several experiments, $=20^{\circ}\cdot8$. A second copper disc, of 0·1 of an inch in thickness, gave an average deflection of $27^{\circ}\cdot8$. A third copper disc, of 0·2 in thickness, gave a deflection of $26^{\circ}\cdot5$. Here, therefore, not only has the thickness (with these conditions of contact) been attained for the maximum effect, but even surpassed (3137.). Then an *iron* disc of 0·05 in thickness, was placed on the axle, and gave, as its mean result, a deflection of $15^{\circ}\cdot4$. Another iron disc of four times the thickness (or 0·2) gave a deflection only of 14°. So here also, as before, the thickness of maximum effect had been surpassed.

3163. The two discs of copper and iron of 0·2 in thickness each, which had produced separately the respective deviations of $26^{\circ}\cdot5$ and 14°, were then both fixed on the axle, being separated from mutual contact in respect of their mass, by a disc of paper, though both were of course in contact at the centre of motion with the copper axle, by means of which the electric communication was perfected. In arranging their place between the poles of the magnet, the iron was placed at mid-distance, and therefore the copper a little on one side. When the copper disc was brought into the circuit, it, by two revolutions, gave an average deviation of $23^{\circ}\cdot4$; and when the iron disc was in the circuit, the deviation produced by it was $11^{\circ}\cdot91$. Here, therefore, the proportions were nearly the same, when the two discs were subject at the same moment to the magnetic power, as when they were examined separately. Both have fallen a little, but not in any manner which seems to indicate that the iron has had any peculiar influence in altering or affecting the lines of force passing across the magnetic field. The effect which has taken place, appears to be one due to the action of the collateral mass of conducting matter.

3164. If the direction of the electric current induced by the magnetic force in the moving metal be taken as the true indication of polarity, and, I think, it cannot be denied that it represents that character of the force, which the term polarity is intended to express, and is unchangeably associated with that character; then these results show that the polarity of the lines of force within the iron is the same with that within the copper, when both are submitted in like manner to the magnetic force. In association with the former and new results with *bismuth* (2431. 3151. 3168.), and numerous other phænomena, the same conclusion may be drawn as to the lines of force within that substance, for the effects are the same with regard to the production of a current in it; and so further evidence is added to that which I have given, tending to show that bismuth is not polarized in the reverse direction as iron or a magnet (2429. 2640.). By reference to the phænomena presented by the relative actions of paramagnetic and diamagnetic substances, the same conclusions may be drawn with respect to all bodies and to space itself (2787. &c.).

3165. That the iron disc affects the disposition of the *lines of force*, is no doubt true, and the extent to which this is done is easily seen, by fixing a small magnetic needle, about 0·1 or 0·05 of an inch in length, across the middle of a piece of stretched thread as an axis, and then bringing it into the magnetic field and near the edges of the stationary disc. The lines of force will be seen (3071. 3076.) gathering in upon the iron at and near its edge, but only for a very little distance from it in any direction: the effect is that which I have considered proper to a paramagnetic body (2807.). Elsewhere, the lines of force go with the same direction across the magnetic field where the iron is, as where it is not; and it is to me a proved fact, proved by the numerous results given, that a section of the lines of force taken across the magnetic field through the air, close to the iron, is exactly equal in amount of force to a section taken across parallel to and through the iron disc (3163.). All iron under induction must have just as much force, *i. e.* lines of force in its internal parts, as is equivalent to the lines which fall on to, and are continued through and out of it; and the same is true, as it appears to me, of any other paramagnetic or diamagnetic substance whatever. The same is true *for the magnet itself*; for a section through the magnet has been shown to be exactly equal to a section anywhere through the outer lines of force (3121.), and these sections may be taken at the surface of the magnet, where they may be considered as either in the air or in the magnet indifferently; and therefore alike in size, shape, power, polarity, and every other point.

3166. I have used the phrase *conduction polarity* on a former occasion (2818. 2835.), but so limited, that it could lead to no mistake of my meaning, either then or now. It requires no words to show how it is included in the higher and general expression of the direction or polarity of the lines of force.

3167. Some other results with the disc apparatus (3159.) were obtained, which it may be useful to describe here. *Tin* was formed into a disc of 0·1 in thickness, and 2·5 inches in diameter. The effect of the friction of the copper conductor at its edge, was a feeble current, the reverse of that produced in the cases of copper and iron (3161.); but the current produced by the revolution, and dependent on the polarity of the lines of force, was the same as before. It produced a swing deflection of 14°·9 for two revolutions of the disc.

3168. A disc of *bismuth* produced far too strong a current by friction against the copper conductor, to allow of any useful result in its simple state. A ring of copper foil was therefore formed, and being placed tightly on the bismuth disc, was wedged up by plates of clean copper foil, so as to produce a clean hard contact; imperfect, no doubt, but as general as could be made under the circumstances. When this disc was rotated in the one direction, it gave a deflection in the same direction as if a copper or iron disc had been used; when rotated the other way, the deflection was little or nothing. This difference is due to the united influence of the rotation effect and the friction effect in the one case, and their opposition in the other; but the results show that the lines of force are in the same direction through bismuth, when between the magnetic poles, as they are through copper and iron. The induced current is small, both because of the bad conducting power of the bismuth and the imperfect contact at the edge. When the same copper rim was placed on the copper disc, it reduced the deflection of the needle from 26°·5 to 9°·34.

3169. In illustration of the effect produced by those parts of the disc, which, not being in the place of greatest action, are conducting back those currents formed by the radial parts in the place of maximum effect, I had a wooden disc constructed, 0·2 in thickness and 2·5 inches in diameter, the centre of which was copper, for the purpose of attachment and electrical connection, and the outer edge a ring of copper not more than $\frac{1}{20}$th of an inch in thickness. The two were connected by a single copper wire radius, in thickness 0·056 of an inch, which, as the disc revolved, was of course carried across and through the magnetic field. It gave a deflection of 14°. The copper disc of 0·05 thickness, gave only an average of 28°. Now, though the matter of the copper ring round the wood will cause part of the current, yet the chief portion must be due to the copper radius, which, at the effectual part near the edge (3160.), is not more than the $\frac{1}{140}$th part of the full copper disc; and this indicates how much of the electricity put in motion there by the magnetic force must be returned back in short circuits in the other parts of the disc.

3170. The disc apparatus shows well the dependence of the induced current upon the *intersection* of the lines of force (3082. 3113.). If the disc be so arranged as to stand edgeways to the magnetic poles, and in the plane of the magnetic axis, so that it shall be *parallel* to the lines of force which pass by and through it, then no revolution of it, with the most powerful magnet, produces the slightest signs of a current at the galvanometer.

3171. The relation of the induced current to the electro-conducting power of the substance, amongst the metals (3152.), leads to the presumption that with other bodies, as water, wax, glass, &c., it is absent, only in consequence of the great deficiency of conducting power. I thought that processes analogous to those employed with the metals, might in such non-conductors as shell-lac, sulphur, &c., yield some results of static electricity (181. 192.); and have made many experiments with this view in the intense magnetic field, but without any distinct result.

3172. All the results described are those obtained with *moving metals*. But mere motion would not generate a relation, which had not a foundation in the existence of some previous state; and therefore the *quiescent* metals must be in some relation to the active centre of force, and that not necessarily dependent on their paramagnetic or diamagnetic condition, because a metal at zero in that respect, would have an electric current generated in it as well as the others. The relation is not as the attractions or repulsions of the metals, and therefore not magnetic in the common sense of the word; but according to some other function of the power. Iron, copper, and bismuth are very different in the former sense, but when moving across the lines of force, give the same general result, modified only by electro-conducting power.

3173. If such a condition be hereafter verified by experiment and the idea of an electrotonic state (60. 242. 1114. 1661. 1729.) be revived and established, then, such bodies as water, oil, resin, &c., will probably be included in the same state; for the non-conducting condition, which prevents the formation of a current in them, does not militate against the existence of that condition which is prior to the effect of motion. A piece of copper, which cannot have the current, because it is not in a circuit (3087.), and a piece of lac, which cannot, because it is a non-conductor of electricity, may have peculiar but analogous states when moving across a field of magnetic power.

3174. On bringing this paper to a close, I cannot refrain from again expressing my conviction of the truthfulness of the representation, which the idea of lines of force affords in regard to magnetic action. All the points which are experimentally established with regard to that action, *i. e.* all that is not hypothetical, appear to be well and truly represented by it. Whatever idea we employ to represent the power, ought ultimately to include electric forces, for the two are so related that one expression ought to serve for both. In this respect, the idea of lines of force appears to me to have advantages over the method of representing magnetic forces by centres of action. In a straight wire, for instance, carrying an electric current, it is apparently impossible to represent the magnetic forces by centres of action, whereas the lines of force simply and truly represent them. The study of these lines have, at different times, been greatly influential in leading me to various results, which I think prove their utility as well as fertility. Thus, the law of magneto-electric induction (114.); the earth's inductive action (149. 161. 171.); the relation of magnetism and light (2146. and note); diamagnetic action and its law (2243.), and magnecrystallic action (2454.), are cases of this kind: and a similar influence of them, over my mind, will be seen in the further instances of the polarity of diamagnetic bodies (2640.); the relation of magnetic curves and the evolved electric currents (243.); the explication of Arago's phænomenon (81.), and the distinction between that and ordinary magnetism (243. 245.); the relation of electric and magnetic forces (1709.); the views regarding magnetic conduction (2797.), and atmospheric magnetism (2847.). I have been so accustomed, indeed, to employ them, and especially in my last Researches, that I may, unwittingly, have become prejudiced in their favour, and ceased to be a clear-sighted judge. Still, I have always endeavoured to make experiment the test and controller of theory and opinion; but neither by that nor by close cross examination in principle, have I been made aware of any error involved in their use.

3175. Whilst writing this paper I perceive, that, in the late Series of these Researches, Nos. XXV. XXVI. XXVII., I have sometimes used the term *lines of force* so vaguely, as to leave the reader doubtful whether I intended it as a merely representative idea of the forces, or as the description of the path along which the power was continuously exerted. What I have said in the beginning of this paper (3075.) will render that matter clear. I have as yet found no reason to wish any part of those papers altered, except these doubtful expressions: but that will be rectified if it be understood, that, wherever the expression *line of force* is taken simply to represent the disposition of the forces, it shall have the fullness of that meaning; but that wherever it may seem to represent the idea of the *physical mode* of transmission of the force, it expresses in that respect the opinion to which I incline at present. The opinion may be erroneous, and yet *all* that relates or refers to the disposition of the force will remain the same.

3176. The value of the moving wire or conductor, as an examiner of the magnetic forces, appears to me very great, because it touches the physics of the subject in a manner altogether different to the magnetic needle. It not only gives its indications upon a different principle and in a different manner, but in the mutual action of it and the source of power, it affects the power differently. The wire when quiescent does not sensibly disturb the arrangement of the force in the magnetic field; the needle when present does. When the wire is moving it does not sensibly disturb the forces external to it, unless perhaps in large masses, as in the discs (3163.), or when time is concerned (1730.), *i. e.* it does not disturb the disposition of the whole force, or the arrangement of the lines of force; a field of equal magnetic power is still equal to anything but the moving wire, whilst the wire moves across or through it. The moving wire also indicates quantity of force, independent of tension (2870.); it shows that the quantity within a magnet and that outside is the same, though the tension be very different. In addition to these advantageous points, the principle is available within magnets, and paramagnetic and diamagnetic bodies, so as to have an application beyond that of the needle, and thus give experimental evidence, of a nature not otherwise attainable.

Royal Institution,
October 9, 1851.

TWENTY-NINTH SERIES[1].

§ 35. *On the employment of the Induced Magneto-electric Current as a test and measure of Magnetic Forces.*

Received December 31, 1851,—Read March 25 and April 1, 1852.

3177. THE proposition which I have made to use the induced magneto-electric current as an experimental indication of the presence, direction and amount of magnetic forces (3074.), makes it requisite that I should also clearly demonstrate the principles and develope the practice necessary for such a purpose; and especially that I should prove that the amount of current induced is precisely proportionate to the amount of lines of magnetic force intersected by the moving wire, in which the electric current is generated and appears (3082. 3109.). The proof already given is, I think, sufficient for those who may repeat the experiments; but in order to accumulate evidence, as is indeed but proper in the first announcement of such a proposition, I proceeded to experiment with the magnetic power of the earth, which presents us with a field of action, not rapidly varying in force with the distance, as in the case of small magnets, but one which for a given place may be considered as uniform in power and direction; for if a room be cleared of all common magnets, then the terrestrial lines of magnetic force which pass through it, have one common direction, being that of the dip, as indicated by a free needle or other means, and are in every part in equal proportion or quantity, *i. e.* have equal power. Now the force being the same everywhere, the proportion of it to the current evolved in the moving wire is then perhaps more simply and directly determined, than in the case where, a small magnet being employed, the force rapidly changes in amount with the distance.

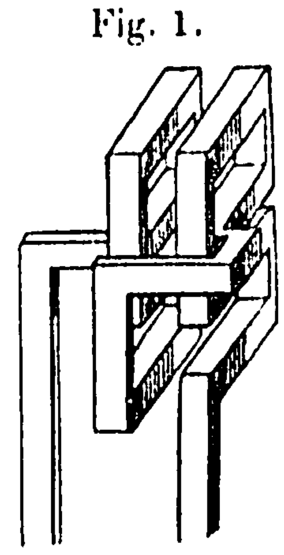
[1] Philosophical Transactions, 1852, p. 137.

¶ i. *Galvanometer.*

3178. For such experimental results as I now propose to give, I must refer to the galvanometer employed and the precautions requisite for its proper use. The instrument has been already described in principle (3123.), and a figure of the conductor which surrounds the needles, given. This conductor may be considered as a square copper bar, 0·2 of an inch in thickness, which passes twice round the plane of vibration of each of the needles forming the astatic combination, and then is continued outwards and terminates in two descending portions, which are intended to dip into cups of mercury. As both the needles are within the convolutions of this bar, an indicating bristle or fine wire of copper is fixed parallel to, and above them upon the same axis, and this, in travelling over the usual graduated circle, shows the place and the extent of vibration or swing of the needles below. The suspension is by cocoon silk, and in other respects the instrument is like a good ordinary galvanometer.

3179. It is highly important that the bar of copper about the needles should be perfectly clean. The vertical zero plane should, according to the construction, be midway between the two vertical coils of the bar, fig. 1; instead of which the needle at first pointed to the one side or the other, being evidently attracted by the upright portions of the bar. I at first feared that the copper was magnetic, but on cleaning the surface carefully with fine sand-paper, I was able to remove this effect, due no doubt to iron communicated by handling or the use of tools, and the needle then stood truly in a plane equidistant from the two coils, when that plane corresponded with the magnetic meridian.

Fig. 1.



3180. The connexions for this galvanometer (3123. 3133.) were all of copper rod or wire 0·2 of an inch in diameter; but even with wires of this thickness the extent of the conductors should not be made more than is necessary; for the increase from 6 to 8, 10 or 12 feet in length, makes a considerable difference at the galvanometer, when electric currents, low in intensity, are to be measured. It is most beautiful to observe in such cases the application of Ohm's law of currents to the effects produced. When the connexions were extended to a distance, straight lengths of wire with dropping ends were provided, and these by dipping into cups of mercury completed the connexion and circuit. The cups consisted of cavities turned in flat pieces of wood. The ends of the connecting rods and of the galvanometer bar were first tinned, and then amalgamated; after which their contact with the mercury was both ready and certain. Even where connexion had to be made by contact of the solid substances, I found it very convenient and certain to tin and amalgamate the ends of the conductors, wiping off the excess of mercury. The surfaces thus prepared are always ready for a good and perfect contact.

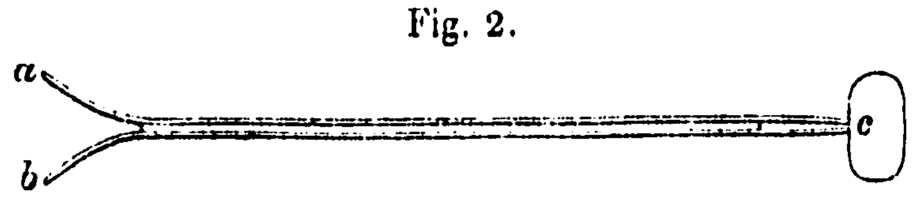
3181. When the needle has taken up its position under the earth's influence, and the copper coil is adjusted to it, the needle ought to stand at true zero, and appears so to do. When that is really the case, equal forces applied in succession on opposite sides of the needle (by two contrary currents through the coil for instance) ought to deflect the needle *equally* on both sides, and they do so. But sometimes, when the needle appears to stand at zero, it may not be truly in the magnetic meridian; for a little torsion in the suspension thread, even though it be only 10° or 15° (for an indifferent needle), and quite insensible to the eye looking at the magnetic needle, does deflect it, and then the force which opposes the swing of the needle, and which stops and returns the needle towards zero (being due both to the torsion and the earth's force), is not equal on the two sides, and the consequence is, that the extent of swing in the two directions is not equal for equal powers, but is greater on one side than the other.

3182. I have not yet seen a galvanometer which has an adjustment for the torsion of the suspending filament. Also, there may be other causes, as the presence about a room, in its walls and other places, of unknown masses of iron, which may render the forces on opposite sides of the instrument zero unequal in a slight degree; for these reasons it is better to make *double observations*. All the phænomena we have to deal with, present effects in two contrary directions. If a loop pass over the pole of a magnet (3133.), it produces a swing in one direction; if it be taken away, the swing is in the other direction; if the rectangles and rings to be described (3192.) be rotated one way, they produce one current; if the contrary way, the other and contrary current is produced. I have therefore, always, in measuring the power of a pole or the effect of a revolving intersecting wire, made many observations in both directions, either alternately or irregularly; have then ascertained the average of those on the one side, and also on the other (which have differed in different cases from $\frac{1}{50}$th to $\frac{1}{300}$th part); and have then taken the mean of these averages as the expression of the power of the induced electric current, or of the magnetic forces inducing it.

3183. Care must be taken as to the position of the instrument and apparatus connected with it, in relation to a fire or sources of different temperatures, that parts which can generate thermo-currents may not become warmed or cooled in different degrees. The instrument is exceedingly sensible to thermo-electric currents; the accidental falling of a sun-beam upon one of two connecting mercury cups for a few moments disturbed the indications and rendered them useless for some time.

3184. In order to ascertain practically, *i. e.* experimentally, the comparative value of degrees in different parts of the scale or graduation of this instrument and so to render it a measurer, the following trials were made. A loop like that before described (3133.), fig. 2, was connected with the galvanometer by

Fig. 2.



communications which removed the loop 9 feet from the instrument, and it was then fixed. A compound bar-magnet consisting of two plates, each 12 inches long, 1 inch broad, and 0·5 in thickness, was selected of such strength as to lift a bunch of clean iron filings, averaging 45 grains at either extremity. Blocks were arranged at the loop, so that this magnet, held in a vertical position, could have one end passed downwards through the loop until the latter coincided with the equator of the magnet (3191.); after which it could be quickly removed and the same operation be repeated at pleasure. When the magnet was thus moved, the loop being unconnected (at one of the mercury cups) with the galvanometer, there was no sensible change of place in the needles; the direct influence of the magnet at this distance of 9 feet being too small for such an effect.

3185. It must be well understood, that, in all the observations made with this instrument, the *swing* is observed and counted as the effect produced, unless otherwise expressed. A constant current in an instrument will give a constant and continued deflection, but such is not the case here. The currents observed are for short periods, and they give, as it were, a blow or push to the needle, the effect of which, in swinging the needle, continues to increase the extent of the deflection long after the current is over. Nevertheless the extent of the swing is dependent on the electricity which passed in that brief current; and, as the experiments seem to indicate, is simply proportional to it, whether the electricity pass in a longer or a shorter time (3104.), and notwithstanding the comparative variability of the current in strength during the time of its continuance.

3186. The compound bar being introduced once into the loop and left there, the swing at the galvanometer was observed and found to be 16°; the galvanometer needle was then brought to zero, and the bar removed, which gave a reverse current and swing, and this also was 16°. Many alternations, as before described, gave 16° as the mean result, *i. e.* the result of one intersection of the lines of force of this magnet (3102.). In order to comprehend the manner in which the effect of two or more intersections of these lines of force were added together, it should be remembered that a swing of the needle from right to left occupied some time (13 seconds); so that one is able to introduce the magnet into the loop, then break the electric circuit by raising one end of the communicating wire out of the mercury, remove the magnet, which by this motion does nothing, restore the mercury contact, and reintroduce the magnet into the loop, before a tenth part of the time has passed, during which the needles, urged by the first impulse, would swing. In this way two impulses could be added together, and their joint effect on the needle observed; and, indeed, by practice, three and even four impulses could be given within the needful time, *i. e.* within one-half or two-thirds of the time of the full swing; but of course the latter impulses would have less power upon the needles, because these would be more or less oblique to the current in the copper coil at the time when the impulses were given. There can be no doubt, that, as regarded the currents induced in the loop by the magnet, they would be equal on every introduction of the same magnet.

3187. Proceeding in this way I obtained results for one, two, three, and even four introductions with the same magnet.

One introduction	15°
Two introductions	31·25
Three introductions	46·87
Four introductions	58·50

Here the approximation to 1, 2, 3, 4 cannot escape observation [1]; and I may remark, that, whilst observing the place attained at the end of a swing which is retained only for an instant, some degree of error must creep in; and that that error must be greatest, in the first number, where it falls altogether upon the unit of comparison, than in the other observations, where only one-half or one-third of it is added to a half or a third of the whole result. Thus, if we halve the arc for two introductions of the pole, it gives $15^{\circ}{\cdot}625$; if we take the third of that for three introductions, it gives $15^{\circ}{\cdot}61$;—numbers which are almost identical, so that if the first number was increased by only $0^{\circ}{\cdot}6$, the proportion would be as 1, 2 and 3. The reason why the fourth, which is $14^{\circ}{\cdot}625$, is less, may perhaps be referred to the cause already assigned, namely, the declination distance of the needle from the coil when that impulse was given (3186.).

3188. In order to avoid in some degree this case, and to compare the degrees at the beginning of the scale, which are most important for the comparison of future experiments with one

[1] See note to (3189.)

$\sin\frac{15}{2} = \sin 7^{\circ}\,30' = {\cdot}130526$ $\quad {\cdot}130526$

$\sin\frac{31{\cdot}25}{2} = \sin 15{\cdot}625 = \sin 15\ 37{\cdot}5 = {\cdot}269200$ $\quad \frac{269200}{2} = {\cdot}134600$

$\sin\frac{46{\cdot}87}{2} = \sin 23{\cdot}435 = \sin 23\ 26{\cdot}1 = {\cdot}3976818$ $\quad \frac{3976818}{3} = {\cdot}1328606$

$\sin\frac{58{\cdot}50}{2} = \sin 29{\cdot}25 = \sin 27\ 15 = {\cdot}4886212$ $\quad \frac{4886212}{4} = {\cdot}1221553$

another, I took only one of the bars of the compound magnet employed above (3184.). The results were as follows:—

One introduction	8°
Two introductions	15·75
Three introductions	23·87
Four introductions	31·66

which numbers are very closely as 1, 2, 3 and 4. If we divide as before, we have 8°, 7°·87, 7°·95, 7°·91; so that if only 0·09 be subtracted from the first observation, or 8°, it leaves that simple result[1].

3189. Hence it appears, that in this mode of applying and measuring the magnetic powers, the number of degrees of swing deflection are for small arcs nearly proportional to the magnetic force which has been brought into action on the moving wire[2].

3190. I have found the needles very constant in their strength for days and weeks together. By care, the constancy of their state for a day is easily secured, and that is all that is required in comparative experiments. Those which I have in use weigh

[1] $\sin\frac{8}{2} = \sin 4^{\circ} = \cdot0697565 \qquad \cdot0697565$

$\sin\frac{15\cdot75}{2} = \sin 7\cdot875 = \sin 7^{\circ}\, 52'\cdot5 = \cdot1370123 \qquad \frac{1370123}{2} = \cdot0685061$

$\sin\frac{23\cdot87}{2} = \sin 11\cdot935 = \sin 11\ 56\cdot1 = \cdot2068019 \qquad \frac{2068019}{3} = \cdot0689340$

$\sin\frac{31\cdot66}{2} = \sin 15\cdot83 = \sin 15\ 49\cdot8 = \cdot2727840 \qquad \frac{2727840}{4} = \cdot0681960$

[2] Mr. Christie has recalled my attention to a paper in the Philosophical Transactions, 1833, p. 95, in which he has investigated, at p. 111, &c., the effect of what may be called magneto-electric impulses in deflecting the magnetic needle. He found that the velocity of the projection of the needle, which is a measure of the force acting upon it at the instant of its moving, will be proportional to the sine of half the arc of swing. My statement, therefore, would as a general expression be erroneous; but for small arcs the results as given by it are not far from the truth. The error does not interfere with the general reasoning and conclusions of the paper; and as the numbers are the results of experiment, which, though made with a first and therefore rough apparatus, were still made with some care, and are expressed simply as deflections, I prefer their appearance as they are rather than in an altered state. Mr. Christie has been so kind as to give me the true expression of force for many of the cases, and I have inserted the results as foot-notes where the cases occur.—Jan. 26, 1852.

with their axis and indicating wire 9 grains; and when out of the copper coil vibrate to and fro once in 26 seconds.

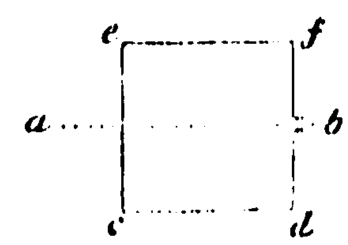
3191. With this instrument thus examined, I repeated most of the experiments with loops formerly described (3133. &c.), with the same results as before. It was also ascertained that the equator of a regular bar-magnet was the place at which the loop should be arrested, to produce the maximum action; and that if it came short of, or passed beyond that place, the final result was less. Employing a magnet 12 inches long, when the loop passed

2·3 inches over the pole the deflection was	. . .	5°·91
4·1 inches over the pole the deflection was	. . .	7·50
5·1 inches over the pole the deflection was	. . .	7·74
6·1 inches over the pole the deflection was	. . .	8·16
8·0 inches over the pole the deflection was	. . .	7·75
9·0 inches over the pole the deflection was	. . .	6·50

¶ ii. *Revolving Rectangles and Rings*[1].

3192. The form of moving wire which I have adopted for experiments with the magnetic forces of the earth (3177.), is either that of a rectangle or a ring. If a wire rectangle (fig. 3) be placed in a plane, perpendicular to the dip and then turned once round the axis *a b*, the two parts *c d* and *e f* will twice intersect the lines of magnetic force within the area *c e d f*. In the first 180° of revolution the contrary direction in which the two parts *c d* and *e f* intersect those lines, will cause them to conspire in producing one current, tending to run round the rectangle (161) in a given direction; in the following 180° of revolution they will combine in their effect to produce a contrary current; so that if the first current is from

Fig. 3.

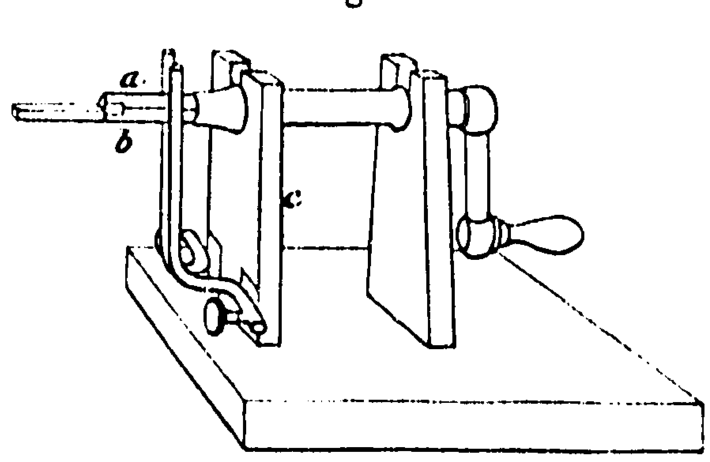


[1] A friend has pointed out to me that in July 1832, Nobili made experiments with rotating rings or spirals subject to the earth's magnetic influence; they were subsequent to and consequent upon my own experiments upon swinging wires (171. 148.) and revolving globes (160.) of January 1832; but he extended the considerations to the *thickness* of the wire; the *diameter* of the spirals and the *number* of the spirals dependent upon the *length* of the wire. The results (tabulated) will be found in vol. i. page 244, &c. of the Florence edition of his Mémoires.—March 1, 1852.

d by *c e* and *f* to *d* again, the second will be from *d* by *f e* and *c* to *d*. If the rectangle, instead of being closed, be open at *b*, and the ends there produced be connected with a commutator, which changes sides when the rectangle comes into the plane perpendicular to the dip, *i. e.* at every half revolution, then these successive currents can be gathered up and sent on to the galvanometer to be measured. The parts *c e* and *d f* of the rectangle may be looked upon simply as conductors; for as they do not in their motion intersect any of the lines of force, so they do not tend to produce any current.

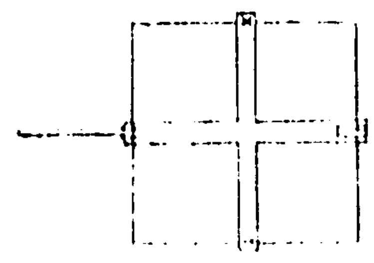
3193. The apparatus which carries these rectangles, and is also the commutator for changing the induced currents, consists of two uprights, fixed on a wooden stand, and carrying above a wooden horizontal axle, one end of which is furnished with a handle, whilst the other projects, and is shaped as in fig. 4. It may there be seen, that two semicylindrical plates of copper *a b* are fixed on the axle, forming a cylinder round it, except that they do not touch each other at their edges, which therefore leave two lines of separation on opposite sides of the axle. Two strong copper rods, 0·2 of an inch in diameter, are fixed to the lower part of the upright *c*, terminating there in sockets with screws for the purpose of receiving the ends of the rods proceeding from the galvanometer cups (3180.): in the other direction the rods rise up parallel to each other, and being perfectly straight, press strongly against the curved plates of the commutator on opposite sides; the consequence is, that, whenever in the rotation of the axle, the lines of separation between the commutator plates arrive at and pass the horizontal plane, their contact with these bearing rods is changed, and consequently the direction of the current proceeding from these plates to the rods, and so on to the galvanometer, is changed also. The other or outer ends of the commutator plates are tinned, for the purpose of being connected by soldering to the ends of any rectangle or ring which is to be subjected to experiment.

Fig. 4.



3194. The rectangle itself is tied on to a slight wooden cross (fig. 5), which has a socket on one arm that slides on to and over the part of the wooden axle projecting beyond the commutator plates, so that it shall revolve with the axle. A small copper rod forms a continuation of that part of the frame which occupies the place of axle, and the end of this rod enters into a hole in a separate upright, serving to support and steady the rectangle and its frame. The frames are of two or three sizes, so as to receive rectangles of 12 inches in the side, or even larger, up to 36 inches square. The rectangle is adjusted in its place, so that it shall be in the horizontal plane when the division between the commutator plates is in the same plane, and then its extremities are soldered to the two commutator plates, one to each. It is now evident, that when dealing with the lines of force of the earth, or any other lines, the axle has only to be turned until the upright copper rods touch on each side at the separation of the commutator plates, and then the instrument adjusted in position, so that the plane of the ring or rectangle is perpendicular to the direction of the lines of force which are to be examined, and then any revolution of the commutator and intersecting wire will produce the maximum current which such wire and such magnetic force can produce. The lines of terrestrial magnetic force are inclined at an angle of 69° to the horizontal plane. As, however, only comparative results were required, the instrument was, in all the ensuing experiments, placed in the horizontal plane, with the axis of rotation perpendicular to the plane of the magnetic meridian; under which circumstances no cause of error or variation was introduced into the results. As no extra magnet was employed, the commutator was placed within 3 feet of the galvanometer, so that two pieces of copper wire 3 feet long and 0·2 of an inch in thickness, sufficed to complete the communication. One end of each of these dipped into the galvanometer mercury cups, the other ends were tinned, amalgamated, introduced into the sockets of the commutator rods (3193.), and secured by the pinching screw (fig. 4).

Fig. 5.



3195. When a given length of wire is to be disposed of in the form best suited to produce the maximum effect, then the circumstances to be considered are contrary for the case of a loop to be employed with a small magnet (39. 3184.), and a rectangle or other formed loop to be employed with the lines of terrestrial force. In the case of the small magnet, *all* the lines of force belonging to it are inclosed by the loop; and if the wire is so long that it can be formed into a loop of two or more convolutions, and yet pass over the pole, then twice or many times the electricity will be evolved that a single loop can produce (36.). In the case of the earth's force, the contrary result is true; for as in circles, squares, similar rectangles, &c. the areas inclosed are as the squares of the periphery, and the lines of force intersected are as the areas, it is much better to arrange a given wire in one simple circuit than in two or more convolutions. Twelve feet of wire in one square intersects in one revolution the lines of force passing through an area of nine square feet, whilst if arranged in a triple circuit, about a square of one foot area, it will only intersect the lines due to that area; and it is thrice as advantageous to intersect the lines within nine square feet once, as it is to intersect those of one square foot three times.

3196. A square was prepared, containing 4 feet in length of copper wire 0·05 of an inch in diameter; it inclosed one square foot of area, and was mounted on the commutator and connected in the manner already described (3194.). Six revolutions of it produced a swing deflection of 14° or 15°, and twelve quick revolutions were possible within the required time (3104.). The results of *quick* and *slow* revolutions were first compared. Six slow revolutions gave as the average of several experiments 15°·5 swing. Six moderate revolutions gave also an average of 15°·5; six quick revolutions gave an average of 15°·66. At another time twelve moderate revolutions gave an average of 28°·75, and twelve quick revolutions gave an average of 31°·33 swing. As before explained (3186.), the probable reason why the quick revolutions gave a larger result than the moderate or slow revolutions is, that in slow time the later revolutions are performed at a period when the needle is so far from parallel with the copper coil of the galvanometer, that the impulses due to them are less effectually exerted. Hence a small or moderate number of revolutions and a quick motion is best. The difference in the extreme case is less than might have been expected, and shows that there is no practical objection in this respect to the method proposed of experimenting with the lines of magnetic force.

3197. In order to obtain for the present an expression of the power of the earth's magnetic force by this rectangle, observations were made on both sides of zero, as already recommended (3182.). Nine moderately quick direct revolutions (*i. e.* as the hands of the clock) gave as the average of many experiments 23°·87, and nine reverse revolutions gave 23°·37; the mean of these is 23°·62 for the nine revolutions of the rectangle, and therefore 2°·624 per revolution. Now the six quick revolutions (3196.) gave 15°·66, which is 2°·61 per revolution, and the twelve quick revolutions gave 31°·33, which is also 2°·61 per revolution; and these results of 2°·624, 2°·61, and 2°·61, are very much in accordance, and give great confidence in this method of investigating magnetic forces[1].

3198. A rectangle was prepared of the same length (4 feet) of the same wire, but the sides were respectively 8 and 16 inches (fig. 6), so that when revolving the intersecting parts should be only 8 inches in length instead of 12. The area of the rectangle was necessarily 128 square inches instead of 144. This rectangle showed the same difference of quick and slow rotations as before (3196.). When nine direct revolutions were made, the result was 20°·87 swing. Nine reverse revolutions gave an average of 20°·25 swing; the mean is 20°·56, or 2°·284 per revolution. A third rectangle was prepared of the same length and kind of wire, the sides of which were respectively 8 and 16 inches long (fig. 7), but now so revolved that the intersecting parts were 16 inches, or twice as long as before; the area of the rectangle remained the same, *i. e.* 128 inches. The like effect of slow and quick revolutions appeared as in the former cases (3196. 3198.). Nine direct revolutions gave as the average effect 20°·75; and nine reverse revolutions produced 21°·375; the mean is 21°·06, or 2°·34 per revolution.

Fig. 6.

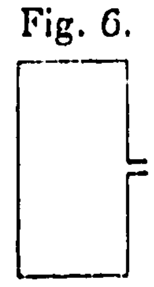
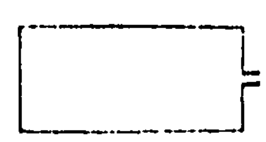


Fig. 7.



[1] $\sin\frac{15\cdot66}{2} = \sin 7^{\circ}\cdot83 = \sin 7^{\circ}\,49'\cdot8 = \cdot1362343 \qquad \frac{1362343}{6} = \cdot0227057$

$\sin\frac{23\cdot62}{2} = \sin 11\cdot81 = \sin 11\;48\cdot6 = \cdot2047069 \qquad \frac{2047069}{9} = \cdot0227474$

$\sin\frac{31\cdot33}{2} = \sin 15\cdot665 = \sin 15\;40 = \cdot2700403 \qquad \frac{2700403}{12} = \cdot0225034$

3199. Now $2^\circ{\cdot}34$ is so near to $2^\circ{\cdot}284$, that they may in the present state of the investigation be considered the same. The little difference that is evident, was, I suspect, occasioned by centrifugal power throwing out the middle of the longer intersecting parts during the revolution. The coincidence of the numbers shows, that the variation in the arrangement of the rectangle and in the length of the parts of the wires intersecting the lines of magnetic force, have had no influence in altering the result, which, being dependent alone on the number of lines of force intersected, is the same for both; for the area of the rectangles is the same. This is still further shown by comparing the results with those obtained with the square. The area in that case was 144 square inches, and the effect per revolution $2^\circ{\cdot}61$. With the long rectangles the area is 128 square inches, and the mean of the two results is $2^\circ{\cdot}312$ per revolution. Now 144 square inches is to 128 square inches as $2^\circ{\cdot}61$ is to $2^\circ{\cdot}32$; a result so near to $2^\circ{\cdot}312$ that it may be here considered as the same; proving that the electric current induced is directly as the lines of magnetic force intersected by the moving wire[1].

3200. It may also be perceived that no difference is produced when the lines of force are chiefly disposed in the direction of the motion of the wire, or else, chiefly in the direction of the length of the wire; *i. e.* no alterations are occasioned by variations in the *velocity* of the motion, or of the length of the wire, provided the amount of lines of magnetic force intersected remains the same.

3201. Having a square on the frame 12 inches in the side but consisting of copper wire 0·1 of an inch in thickness, I obtained the average result of many observations for one, two, three, four and five revolutions of the wire.

[1] Oblong rectangles of 128 square inches area give a mean of $20^\circ{\cdot}81$ (3198.). The rectangle of 144 square inches gave a mean of $23^\circ{\cdot}62$ (3197.).

$$\sin\frac{20{\cdot}81}{2} = \sin 10^\circ{\cdot}405 = \sin 10^\circ\,24'{\cdot}3 = {\cdot}1806049$$

$$\sin\frac{23{\cdot}62}{2} = \sin 11{\cdot}81 \;= \sin 11\;48{\cdot}6 = {\cdot}2047069$$

$$\frac{128}{144} = \frac{8}{9} \qquad \begin{array}{l} {\cdot}1806049 \times 9 = 1{\cdot}6254441 \\ {\cdot}2047069 \times 8 = 1{\cdot}6376552 \end{array} \qquad \text{Or thus:}\; \begin{array}{l} \dfrac{{\cdot}1806049}{8} = {\cdot}0225756 \\ \dfrac{{\cdot}2047069}{9} = {\cdot}0227452 \end{array}$$

One revolution gave 7° equal to 7° per revolution.
Two revolutions gave 13·875 equal to 6·937 per revolution.
Three revolutions gave 21·075 equal to 7·025 per revolution.
Four revolutions gave 28·637 equal to 7·159 per revolution.
Five revolutions gave 37·637 equal to 7·527 per revolution.

These results are exceedingly close upon each other, especially for the first 30°, and confirm several of the conclusions before drawn (3189. 3199.) as to the indications of the instrument, the amount of the curves, &c.[1]

3202. At another time I compared the effect of equable revolutions with other revolutions very irregular in their rates, the motion being sometimes even backwards and continually differing in degree by fits and starts, yet always so that within the proper time a certain number of revolutions should have been completed. The rectangle was of wire 0·2 of an inch thick; the mean of many experiments, which were closely alike in their results, gave for two smooth, equable revolutions, 17°·5, and also for two irregular uncertain revolutions the same amount of 17°·5.

3203. The relation of the current produced to the mass of the wire was then examined; a relation, which has been investigated on a former occasion by loops and small magnets (3133.).[2] For the present purpose two other equal squares were prepared, each a foot in the side, but the copper wire of which they consisted was respectively 0·1 and 0·2 of an inch in diameter; so that

		Differences.	
1 $\sin\frac{7}{2} = \sin 3^\circ{\cdot}30$	$= {\cdot}0610485$		${\cdot}0610485$
		${\cdot}0597381$	
$\sin\frac{13{\cdot}875}{2} = \sin 6{\cdot}9375 = \sin 6^\circ\, 56'{\cdot}25$	$= {\cdot}1207866$		$\frac{{\cdot}1207866}{2} = {\cdot}0603933$
		${\cdot}0620924$	
$\sin\frac{21{\cdot}075}{2} = \sin 10{\cdot}5375 = \sin 10\; 32{\cdot}25$	$= {\cdot}1828790$		$\frac{{\cdot}1828790}{3} = {\cdot}0609596$
		${\cdot}0644329$	
$\sin\frac{28{\cdot}637}{2} = \sin 14{\cdot}3185 = \sin 14\; 19{\cdot}11$	$= {\cdot}2473119$		$\frac{{\cdot}2473119}{4} = {\cdot}0618279$
		${\cdot}0752595$	
$\sin\frac{37{\cdot}637}{2} = \sin 18{\cdot}8185 = \sin 18\; 49{\cdot}11$	$= {\cdot}3225714$		$\frac{{\cdot}3225714}{5} = {\cdot}0645142$

2 See a corresponding investigation by Christie. Philosophical Transactions, 1833, p. 120.

with the former rectangle they formed a series of three, having the same size, shape and area, but the masses of the moving wire increasing in the proportion of one, four and sixteen. When the rectangle of 0·1 wire was employed, six direct revolutions gave an average result of 41°·75, and six to the left gave 46°·25; the mean of the two is 44°, and this divided by six gives 7°·33 as the deflection per revolution. Again, three direct revolutions gave 20°·12, and three reverse revolutions 23°·1; the mean being 21°·61, and the deflection per revolution 7°·20. This is very close to the former result with six revolutions, namely 7°·33, and is a large increase upon the effect of the rectangle of wire 0·05 in diameter, namely 2°·61; nevertheless, it is not as 4 : 1; nor could such a result be expected, inasmuch as the mass of the chief conductor remained the same (3137.). When the results are compared with those made with like wires in the form of loops, they are found to be exceedingly close; in that case the results were as 16° to 44°·4 (3136.), which would accord with a ratio in the present case of 2°·61 to 7°·26; and it is as 2°·61 to 7°·242, almost identical.

3204. The average of the direct and reverse revolutions is seen above to differ considerably, *i. e.* up to 4° and 5° in the higher case. This does not indicate any error in principle, but results simply from the circumstance, that when the needles were quiescent in the galvanometer, they stood a little on one side of zero (3182.). I did not wish to adjust the instrument at the time, as I was watching for spontaneous alterations of the zero place, and prefer giving the numbers as they came out in the investigation, to any pen-and-ink correction of the notes.

3205. The third square of 0·2 wire gave such large swings, that I employed only a small number of revolutions. Three direct revolutions gave an average of 25°·58; three reverse revolutions gave 28°·5; the mean is 27°·04, and the amount per revolution 9°·01. Again, two direct revolutions gave 17°·5; two reverse revolutions gave 18°; the mean is 17°·75, and the amount per revolution 8°·87; the mean of the two final results is 8°·94, and is again an increase on the effect produced by the preceding rectangle of wire, only half the diameter of the present. This thickness of wire was also employed formerly as a loop (3136.); and if we compare the results then obtained with the present results, it is remarkable how near they approach to each other; a circumstance which leads to great confidence in the principles and practice of both forms of examination. When wires having masses in the proportion of 1, 4 and 16 were employed as loops, the currents indicated by the galvanometer were as 1·00, 2·77, and 3·58; now that they are employed as rectangles subject to the earth's magnetic power, they are as 1·00, 2·78, and 3·45[1].

3206. I formed a square, 12 inches in the side, of four convolutions of copper wire 0·05 of an inch in diameter; the single wire which formed it was consequently 16 feet long. Such a rectangle will, in revolving, intersect the same number of lines of magnetic force as the former rectangle made with wire 0·1 in diameter (3203.): there will also be the same mass of wire intersecting the lines, but, as a conductor, the first wire has in respect of diameter, only one-fourth the conducting power of the second; and then, to increase the obstruction, it is four times as long. Six direct revolutions gave an average result of 20°·6, and six reverse revolutions 19°·7; the mean is 20°·15, and the proportion per revolution 3°·36. With the other rectangle having equal area and mass, but a single wire (3203.), the result per revolution was 7°·26; being above, though near upon twice as much as in the present case. Hence for such an excellent conducting galvanometer as that described (3123. 3178.), the moving wire had better be as one single thick wire rather than as many convolutions of a thin one. If it be, under all variations of circumstances, the same wire for the same area, then, of course, two or more convolutions are better than one.

[1] $\sin \frac{27{\cdot}04}{2} = \sin 13^\circ{\cdot}52 = \sin 13^\circ\ 31'{\cdot}2 = {\cdot}2337848 \qquad \frac{{\cdot}2337848}{2} =$ ·0779283. The square 12 inches side, of wire 0·05 in diameter, gave for six revolutions (3196. 3197.) ·0227057 as $\sin \frac{1}{2}$ A for one revolution. A like square of wire 0·10 in diameter gave for five revolutions (3021.) $\frac{{\cdot}3225714}{2} =$ ·06451428 as $\sin \frac{1}{2}$ A for one revolution. A like square of wire 0·20 in diameter gave ·0779283 as $\sin \frac{1}{2}$ A for one revolution.

$$\frac{{\cdot}06451428}{{\cdot}0227057} = 2{\cdot}841. \qquad \frac{{\cdot}0779283}{{\cdot}0227057} = 3{\cdot}432.$$

3207. It was to be expected, however, that the thin wire rectangle would produce a current of more *intensity* than that in the thick wire, though less in quantity; and to prove this point experimentally, I connected the two rectangles in succession with Rhumkorff's galvanometer (3086.), having wire only 1-135th of an inch in diameter. That of the single thick wire now gave only 1°·66 of swing for twelve revolutions of the rectangle, or 0°·138 per revolution; whilst the other of four convolutions of thin wire gave for twelve revolutions 7°·33, or 0°·61 per revolution. Now the needles of the two instruments were not very different in weight and other circumstances, so that without pretending to an accurate comparison, we may still perceive an immense falling-off in both cases, due to the obstruction of the fine wire in the Rhumkorff's galvanometer; for the thick wire it is from 7°·26 to 0°·138, and for the thin wire from 3°·36 to 0°·610. Still the thin wire rectangle has lost far less proportionately in power than the other; and by this galvanometer is above four times greater in effect than the rectangle of thicker wire. Of the thick wire effect less than a *fiftieth* passes the fine wire galvanometer, all the rest is stopped; of the fine wire effect more than ten times this proportion, or between a fourth and a fifth (because of the higher intensity of the current), surmounts the obstruction presented by the instrument. The quantity of electricity which really passes through the fine wire galvanometer is of course far less than in the proportion indicated above. The thick wire coil makes at the utmost four convolutions about the needles, whereas in the fine wire coil there are probably four hundred or more; so that the electricity which really travels forward as a current, is probably not a hundredth part of that which would be required to give an equal deflection in the thick wire galvanometer. Such a circumstance does not disturb the considerations with respect to the relative intensity of the magneto-electric currents from the two rectangles, which have been stated above.

3208. A large square was now constructed of copper wire 0°·2 of an inch in diameter. The square was 36 inches in the side, and therefore consisted of 12 feet of wire, and inclosed an area of 9 square feet; it was attached to the commutator by expedients, which, though sufficient for the present, were not accurate in the adjustments. It produced a fine effect upon the thick wire galvanometer (3178.); for one revolution caused a swing deflection of 80° or more; and when its rotation was continuous the needles were permanently deflected 40° or 50°. It was very interesting to see how, when this rectangle commenced its motion from the horizontal plane, the current increased in its intensity and then diminished again, the needles showing, that whilst the first 10° or 20° of revolution were being passed, there was very little power exerted on them; but that when it was towards, or near the 90°, the power was great; the wires then intersecting the lines of force nearly at right angles, and therefore, with an equal velocity, crossing the greatest number in a given time. It was also very interesting, by the same indications, to see the two chief impulses (3192.) given in one revolution of the rectangle. Being large and massive in proportion to the former wires, more time was required for a rotation than before, and the point of *time* or *velocity* of rotation became more essential. One rotation in a second was as much as I could well produce. A speed somewhat less than this was easy, convenient and quick enough; it gave for a single revolution near 80°, whilst a revolution with one-half or one-third the velocity, or less, gave only 60°, 50°, or even smaller amounts of deflection.

3209. Observations were now made on the measurement of one rotation having an easy quick velocity. The average of fifteen observations to the right, which came very near to each other, was 78°·846; the average of seventeen similar observations to the left was 78°·382; and the mean of these results, or 78°·614, I believe to be a good first expression for this rectangle. On measuring the distances across after this result, I found that in one direction, *i. e.* across between the intersecting portions of wire, it was rather less than 36 inches; having therefore corrected this error, I repeated the observations and obtained the result of 81°·44. The difference of 2°·83, I believe to be a true result of the alteration and increase of the area on making it more accurately 9 square feet; and it is to me an evidence of the sensibility and certainty of the instrument.

3210. As the two impulses upon the needles in one revolution (3208.) are here sensibly apart in time, and as the needle has as evidently and necessarily left its first place before the second impulse is impressed upon it, so, that second impulse cannot be so effectual as the first. I therefore observed the results with half a revolution, and obtained a mean of 41°·37 for the effect. This number evidently belongs to the first of the two impulses of one revolution; and if we subtract it from 81°·44, it gives 40°·07 as the value of the second impulse under the changed place of the needle. This difference of the two impulses of one revolution, namely 41°·37 and 40°·07, is in perfect accordance with the results that were to be expected.

3211. The square of this same copper wire, 0·2 in thickness, employed on a former occasion (3205.), had an area of one square foot, so that then the lines of force affected or affecting the moving wire, were one-ninth part of what they are in the present case: the effect then was 8°·94 per revolution. If, in comparing these cases, we take the ninth part of 81°·44, it gives 9°·04; a number so near the former, that we may consider the two rectangles as proving the same result, and at the same time the truth of the statement, that the magneto-electric current evolved is as the amount of lines of force intersected. A ninth part of the result with the large rectangle (78°·614), before its area was corrected, is 8°·734; so that the one is above and the other below the amount of the 12-inch rectangle. As that was not very carefully adjusted, nor indeed any of the arrangements made as yet with extreme accuracy, I have little doubt that with accurately adjusted rectangles the results would be strictly proportional to the areas[1].

3212. The moving wire, in place of being formed into a rectangle, may be adjusted as a ring; and then the advantage is obtained of the largest area which a given length of wire can in-

[1] The 9 square feet rectangle gave 81°·44 sin $\frac{81·44}{2}$ = sin 40·72 = sin 40° 43′·2 = ·6523630: or taking 41°·37 for the half revolution for $\frac{1}{2}$A (3210.) sin 41°·37 = sin 41° 22′·2 = ·6609190, which divided by nine give ·073435 as the force per square foot. The 1 square foot rectangle of like wire (3205.) gave ·07714, or ·07793 as the force of one revolution; the first of which is ·00370 more than $\frac{1}{9}$ of the measure of the effect of the large square; the difference being about $\frac{1}{29}$ of ·07714, or the whole force of one revoluiton.

close, and therefore for a uniform wire, the obstruction to the induced current, as respects its conduction, is the least. Small rings of one or several convolutions will probably be very valuable in the examination of small and local magnets under different circumstances. One consisting of ten spirals of copper wire 0·032 of an inch in diameter, containing 49 inches, in a ring about 1·5 inch in diameter, gave but small results under the earth's influence; but when brought near a horseshoe magnet told, in its effects for every difference in distance or in position. A single ring 4 inches in diameter, being made of a convolution of copper wire 0·2 in thickness, was employed with the earth's magnetic force as before; it gave as the average of six revolutions many times repeated 5°·995, or 0°·999 per revolution. For twelve revolutions it gave a mean of 12°·375 or 1°·031 per revolution [1]; the mean of the two results with such different numbers of revolutions being 1°. Another ring, consisting of 26 convolutions of copper wire 0·04 of an inch in diameter, was constructed and had a mean diameter of 3·6 or 3·7 inches; it contained 300 inches in length of wire. So the masses of the metal in the two rings are nearly the same, but the latter wire is singly only 1-25th of the mass of the former. It gave for twelve revolutions a mean of 6°·25, or 0°·52 per revolution. With the earth's power and the thick wire galvanometer, it gave therefore little more than half the result of the single thick wire ring. We know from former considerations (3206.), that if the 300 inches had been made into one single ring, it would have given a very high effect compared to the present.

3213. The application of the principle of the moving wire in the form of a revolving rectangle, makes the investigation of *conducting* power, and the results produced by difference in the nature of the *substance*, or in diameter, *i. e. mass*, or in *length*, very easy; and the obstruction offered by those parts, which moving not across but parallel to the lines of force (3071.), have no exciting action but perform the part of conductors merely, might be greatly removed by making them massive. They might be made to shift upon the axle so as to bear adjustment

[1] $\sin\frac{5^\circ{\cdot}995}{2} = \sin 2^\circ{\cdot}9975 = \sin 2^\circ\, 59'{\cdot}85 =$ ·0522925

$\sin\frac{12{\cdot}375}{2} = \sin 6{\cdot}1875 = \sin 6\; 11{\cdot}25 = {\cdot}1077825 \qquad \frac{{\cdot}1077825}{2} = {\cdot}0538912$

for different lengths of wires, and the commutator might in fact be made to a large extent a general instrument.

3214. In looking forward to further applications of the principle of the moving wire, it does not seem at all unlikely that by increased delicacy and perfection of the instrument, by increased velocity, by continued motion for a time in one direction and then reversal of the revolution with the reversal of the direction of the swing, &c., it may be applied with advantage hereafter to the investigation of the earth's magnetic force in different latitudes and places. To obtain the maximum effect, the axis of rotation must be perpendicular to the lines of force, *i. e.* the dip. It would even be possible to search for the *direction* of the lines of force, or the dip, by making the axis of rotation variable about the line of dip, adjusting it in two directions until there was no action at the galvanometer, and then observing the position of the axis; a double commutator would be required corresponding to the lines of adjustment, but that is an instrument of very simple construction.

§ 36. *On the amount and general disposition of the Forces of a Magnet when associated with other magnets.*

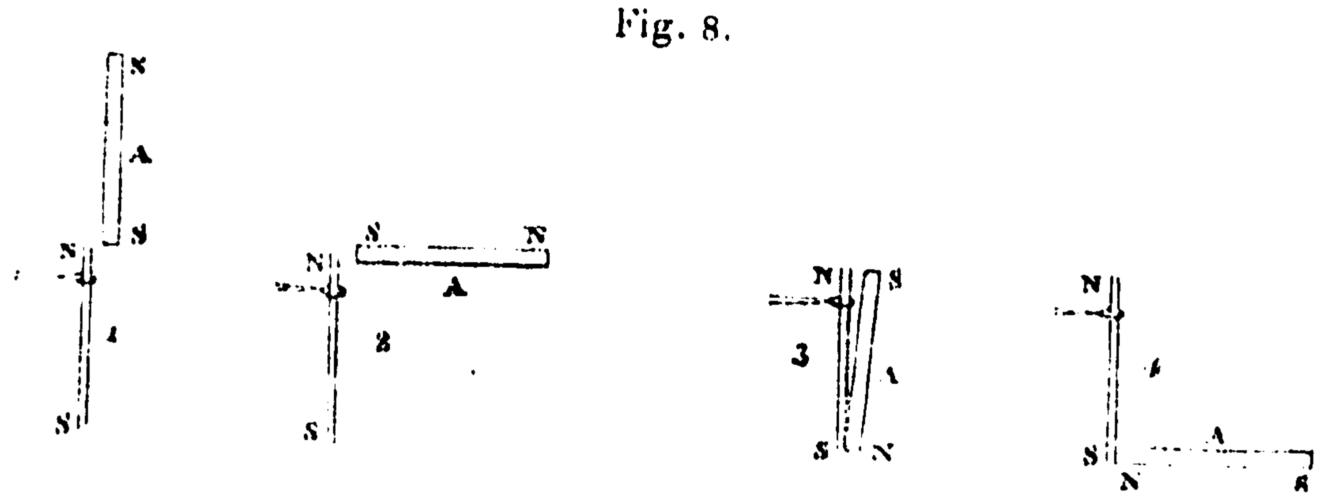
3215. Prior to further progress in the experimental development by a moving wire of the disposition of the lines of magnetic force pertaining to a magnet, or of the physical nature of this power and its possible mode of action at a distance, it became quite essential to know what change, if any, took place in the amount of force possessed by a perfect magnet, when subjected to other magnets in favourable or adverse positions; and how the forces combined together, or were disposed of, *i. e.* generally, and in relation to the principle already asserted and I think proved, that the power is in every case definite under those different conditions. The representation of the magnetic power by *lines of force* (3074.), and the employment of the moving wire as a test of the force (3076.), will I think assist much in this investigation.

3216. For such a purpose an ordinary magnet is a very irregular and imperfect source of power. It not only, when magnetized to a given degree, is apt by slight circumstances to have its magnetic power diminished or exalted, in a manner which may be considered for the time, permanent; but if placed in adverse or favourable relations to other magnets, frequently admits of a considerable temporary diminution or increase of its power externally, which change disappears as soon as it is removed from the neighbourhood of the dominant magnet. These changes produce corresponding effects upon the moving wire, and they render any magnet subject to them unfit for investigation in relation to definite power. Unchangeable magnets are, therefore, required, and these are best obtained, as is well known, by selecting good steel for the bars, and then making them exceedingly hard; I therefore procured some plates of thin steel twelve inches long and one inch broad, and making them as hard as I could, afterwards magnetized them very carefully and regularly, by two powerful steel bar-magnets, shook them together in different and adverse positions for a little while, and then examined the direction of the forces by iron filings. Small cracks and irregularities were in this way detected in several of them; but two which were very regular in the disposition of their forces were selected for further experiment, and may be distinguished as the subjected magnets D and E.

3217. These two magnets were examined by the moving loop precisely in the manner before described (3133.), *i. e.* by passing the loop over one of the poles, observing the swing, removing it, and again observing the swing and taking an average of many results; the process was performed over both poles at different times. The loop contained 7·25 inches in length of copper wire 0·1 of an inch in diameter, and was of course employed in all the following comparative experiments; the distance of the loop and magnets from the galvanometer was 9 feet. For one passage over the pole either on or off, *i. e.* for one intersection of the lines of force of the magnet D, the galvanometer deflection was 8°·36. For one intersection of the lines of force of the other bar E, the deflection was 8°·78. The two bars were then placed side by side with like poles together, and afterwards used as one magnet; their conjoined power was 16°·3, being only 0°·84 less than the sum of the powers of the two when estimated separately. This indicates that the component magnets do affect, and in this position reduce, each other somewhat; but it also shows how small the effect is as compared with ordinary magnets (3222.).

3218. The compound magnet D E (3217.) was now subjected to the close action of another magnet, sometimes under adverse, and at other times under favourable conditions; and was examined by the loop as to the sum of its power (not the direction) under these circumstances. For this purpose it was fixed, and another magnet A brought near, and at times in contact with it, in the positions indicated by the figure 8; the loop in each case

Fig. 8.



being applied many times to D E, that a correct average of its power might be procured. The dominant magnet A was much the stronger of the two, having the power indicated by a swing deflection of 25°·74.

3219. When the relative position of the magnets was as at 1, then the power of D E was 16°·37; when as at 2, the power was 16°·4; when as at 3, it was 18°·75; and when as at 4, it was 17°·18. All these positions are such as would tend to raise, by induction, the power of the magnet D E, and they do raise it above its first value, which was 16°·3; but it is seen at once how little the first and second positions elevate it; and even the third, which presents the most favourable conditions, only increases the power 2°·45, which falls again in the fourth position.

3220. Then the dominant magnet A was placed in the same positions, but with the ends reversed, so as to exert an adverse or depressing influence; and now the results with D E were as follows:—

Position 1	15°·37
Position 2	15·68
Position 3	15·37
Position 4	16·06

All these are a little below the original force of D E, or 16°·3, as they ought to be, and show how slightly this hard bar-magnet is affected.

3221. A soft iron bar, now applied in the first, second and third positions instead of the magnet A, raised D E to the following values respectively, 16°·24, 16°·43, and 18°.

3222. When an ordinary bar-magnet was employed instead of the hard magnet D E, great changes took place. Thus a bar B, corresponding to bar A in size and general character, was employed in place of the hard magnet. Alone, B had a power of 14°·83, but when associated adversely with A, as in position 3 (3218.), its power fell to 7°·87, being reduced nearly one-half. This loss was chiefly due to a coercion internally, and not to a permanent destruction of the state of magnet B; for when A was removed, B rose again to 13°·06. When B was laid for a few moments favourably on A and then removed, it was found that the latter had been raised to a permanent external action of 15°·25.

3223. A very hard steel bar 6 inches long, 0·5 broad and 0·1 in thickness, given to me by Dr. Scoresby, was magnetized and then found, by the use of the loop, to have a value at my galvanometer of 6°·88 (3189.). It was submitted in position 2 to a compound bar-magnet like D E, having a power of 11°·73, or almost twice its own force, but whether in the adverse or the favourable position, its power was not sensibly altered. When submitted in like manner to a 12-inch bar-magnet having a force of 40°·21, it was raised to 7°·53, or lowered to 5°·87, but here the dominant magnet had nearly six times the power of the one affected.

3224. The variability of soft steel magnets, both in respect of their *absolute* degree of excitation or charge, and also of the disposition of the force externally and internally, when their degree of excitation may for the time be considered as the same, is made very manifest by this mode of examination; and the results agree well with our former knowledge in this respect. It is equally manifest, that hard and invariable magnets are requisite for a correct and close investigation of the disposition and characters of the magnetic force. A common soft bar-magnet may be considered as an assemblage of hard and soft parts, disposed in a manner utterly uncertain; of which some parts take a much higher charge than others, and change less under the influence of external magnets; whilst, because of the presence of other parts within, acting as the keeper or submagnet, they may seem to undergo far greater changes than they really do. Hence the value of these hard and comparatively unchangeable magnets which Scoresby describes.

3225. From these and such results, it appears to me, that with perfect, unchangeable magnets, and using the term *line of force* as a mere representant of the force as before defined (3071. 3072.), the following useful conclusions may be drawn.

3226. Lines of force of different magnets in favourable positions to each other coalesce.

3227. There is no increase of the total force of the lines by this coalescence; the section between the two associated poles gives the same sum of power as that of the section of the lines of the invariable magnet when it is alone (3217.). Under these circumstances there is, I think, no doubt that the external and internal forces of the same magnet have the same relation and are equivalent to each other, as was determined in a former part of these Researches (3117.); and that therefore the equatorial section, which represents the sum of forces or lines of forces passing through the magnet, remains also unchanged (3232.).

3228. In this case the analogy with two or more voltaic batteries associated end to end in one circuit is perfect. Probably some effect, correspondent to *intensity* in the case of the batteries, will be found to exist amongst the magnets.

3229. The increase of power upon a magnetic needle, or piece of soft iron placed between two opposite, favourable poles, is caused by concentration upon it of the lines which before were diffused, and not by the addition of the power represented by the lines of force of one pole to that of the lines of force of the other. There is no more power represented by all the lines of force than before; and a line of force is not more powerful because it coalesces with a line of force of another magnet. In this respect the analogy with the voltaic pile is also perfect.

3230. A line of magnetic force being considered as a closed circuit (3117.), passes in its course through *both* the magnets, which are for the time placed so as to act on each other favourably, *i. e.* whose lines coincide and coalesce. Coalescence is not the addition of one line of force to another *in power*, but their union in one common circuit.

3231. A line of force may pass through many magnets before its circuit is complete; and these many magnets coincide as a case with that of a single magnet. If a thin bar-magnet 12 inches long be examined by filings (3235.), it will be found to present the well-known beautiful system of forces, perfectly simple in its arrangement. If it be broken in half, without being separated, and again examined, the manner in which, from the destruction of the continuity, the transmission of the force at the equator is interfered with, and many of the lines, which before were within are made to appear externally there, is at once evident, Plate III. fig. 6. Of those lines, which thus become external, some return back to the pole which is nearest to the new place, at which the lines issue into the air, making their circuit through only one of the halves of the magnet; whilst others proceed onward by paths more or less curved into the second half of the magnet, keeping generally the direction or polarity which they had whilst within the magnet, and complete their circuit through the two. Gradually separating the two halves, and continuing to examine the course of the lines of force, it is beautiful to observe how more and more of the lines which issue from the two new terminations, turn back to the original extremities of the bar, fig. 7, and how the portion which makes a common circuit through the two halves diminishes, until the halves are entirely removed from each other's influence, and then become two separate and independent magnets. The same process may be repeated until there are many magnets in place of one.

3232. All this time the amount of lines of force is the same if the fragments of the bar preserve their full state of magnetism; *i. e.* the sum of lines of force in the equator of *either* of the new magnets is equal to the sum of lines of force in the equator of the original unbroken bar. I took a steel bar 12 inches long, 1 inch broad and 0·05 of an inch thick, made it very hard, and magnetized it to saturation by the use of soft iron cores and a helix; its power was 6°·9. I broke it into two pieces nearly in the middle, and found the power of these respectively 5°·94 and 5°·89; indicating a fall not more than was to be expected considering the saturated state of the original magnet. When these halves were placed side by side, with like poles together as a compound magnet, they had a joint power of 11°·06, which, though it shows a mutual quelling influence, is not much below the sum of their powers ascertained separately. All this is in perfect harmony with the voltaic battery, where lines of dynamic electric force are concerned. If, as is well known, we separate a battery of 20 pair of plates into two batteries of 10 pair, or 4 batteries of 5 pair, each of the smaller batteries can supply as much dynamic electricity as the original battery, provided no sensible obstruction be thrown into the course of the lines, *i. e.* the path of the current.

3233. When magnets are placed in an adverse position, as neither could add power to the other in the former case, so now each retains its own power; and the lines of magnetic force represent this condition accurately. Two magnets placed end to end with like poles together are in this relation; so also are they if placed with like poles together side by side. In the latter case the two acting as one compound magnet, give a system of lines of force equal to the sum of the two separately (3232.), minus the portion which, as in imperfect magnets, is either directed inwards by the softer parts or ceases to be excited altogether.

§ 37. *Delineation of Lines of Magnetic Force by iron filings.*

3234. It would be a voluntary and unnecessary abandonment of most valuable aid, if an experimentalist, who chooses to consider magnetic power as represented by lines of magnetic force, were to deny himself the use of iron filings. By their employment he may make many conditions of the power, even in complicated cases, visible to the eye at once; may trace the varying direction of the lines of force and determine the relative polarity; may observe in which direction the power is increasing or diminishing; and in complex systems may determine the neutral points or places where there is neither polarity nor power, even when they occur in the midst of powerful magnets. By their use probable results may be seen at once, and many a valuable suggestion gained for future leading experiments.

3235. Nothing is simpler than to lay a magnet upon a table, place a flat piece of paper over it, and then sprinkling iron filings on the paper, to observe the forms they assume. Nevertheless, to obtain the best and most generally useful results, a few particular instructions may be desirable. The table on which the magnet is laid should be quite horizontal and steady.

Means should be taken, by the use of thin boards or laths, or otherwise, to block up round the magnet, so that the paper which is laid over it should be level. The paper should be without any cockle or bend, and perfectly flat, that the filings may be free to assume the position which the magnet tends to give them. I have found well-made cartridge or thin drawing-paper good for the purpose. It should not be too smooth in ordinary cases, or the filings, when slightly agitated, move too freely towards the magnet. With very weak or distant magnets I have found silvered paper sometimes useful. The filings should be clean, *i. e.* free from much dirt or oxide; the latter forms the lines but does not give good delineations. The filings should be distributed over the paper by means of a sieve more or less fine, their quantity being partly a matter of taste. It is to be remembered, however, that the filings disturb in some degree the conditions of the magnetic power where they are present, and that in the case of small magnets, as needles, a large proportion of them should be avoided. Large and also fine filings are equally useful in turn, when the object is to preserve the forms obtained. For the distribution of the latter it is better to use a fine sieve with the ordinary filings than to separate the filings first: a better distribution on the paper is obtained. The filings being sifted evenly on the paper, the latter should be tapped very lightly by a small piece of wood, as a pen-holder; the taps being applied wherever the particles are not sufficiently arranged. The taps must be perpendicularly downwards, not obliquely, so that the particles, whilst they have the liberty of motion, for an instant are not driven out of their places, and the paper should be held down firmly at one corner, so as not to shift right or left; the lines are instantly formed, especially with fine filings.

3236. The designs thus obtained may be fixed in the following manner, and then form very valuable records of the disposition of the forces in any given case. By turning up two corners of the paper on which the filings rest, they may be used as handles to raise the paper upwards from the magnet, to be deposited on a flat board or other plane surface. A solution of one part of gum in three or four of water having been prepared, a coat of this is to be applied equably by a broad camel-hair pencil, to a piece of cartridge paper, so as to make it fairly wet, but not to float it, and after wafting it through the air once or twice to break the bubbles, it is to be laid carefully over the filings, then covered with ten or twelve folds of equable soft paper, a board placed over the paper, and a half-hundred weight on the board for thirty or forty seconds. Or else, and for large designs it is a better process, whilst the papers are held so that they shall not shift on each other, the hand should be applied so as to rub with moderate pressure over all the surface equably and in one direction. If, after that, the paper be taken up, all the filings will be found to adhere to it with very little injury to the forms of the lines delineated; and when dry they are firmly fixed. If a little solution of the red ferroprussiate of potassa and a small proportion of tartaric acid be added to the gum-water, a yellow tint is given to the paper, which is not unpleasant; but besides that, prussian blue is formed under every particle of iron; and then when the filings are purposely or otherwise displaced, the design still remains recorded. When the designs are to be preserved in blue only, the gum may be dispensed with and the red ferroprussiate solution only be used.

3237. It must be well understood that these forms give no indication by their appearance of the relative strength of the magnetic force at different places, inasmuch as the appearance of the lines depends greatly upon the quantity of filings and the amount of tapping; but the direction and forms of the lines are well given, and these indicate, in a considerable degree, the direction in which the forces increase and diminish.

3238. Plate III. fig. 1, shows the forms assumed about a bar-magnet. On using a little electro-magnet and varying the strength of the current passed through it, I could not find that a variation in the strength of the magnet produced any alteration in the forms of the lines of force external to it. Fig. 2 shows the lines over a pole, and fig. 3 those between contrary poles. The latter accord with the magnetic curves, as determined and described by Dr. Roget and others, with the assumption of the poles as centres of force. The difference between them and those belonging to a continuous magnet, shown in fig. 1, is evident. Figs. 4, 5 show the lines produced by short magnets. In the latter case the magnet was a steel disc about one inch in diameter and 0·05 in thickness. Fig. 6 shows the result when a bar-magnet is broken in half, but not separated. Fig. 7 shows the development of the lines externally at the two new ends as the halves are more and more separated (3231.). Figs. 8, 9 and 10 present the results, with the two halves or new magnets in different positions. Figs. 11, 12, 13 and 14 show the results with disc magnets. Fig. 15 shows the condition of a system of magnetic forces when it is inclosed by a larger one, and is contrary to it. Fig. 16 shows the coalescence of the lines of force (3226.) when the magnets are so placed that the polarities are in accordance.

3239. Fig. 17 exhibits the lines of force round a vertical wire carrying a current of electricity. Whether the wire was thick or thin appeared to make no difference as to the intensity of the forces, the current remaining the same. Fig. 18 represents the lines round two like currents when within mutual influence. Fig. 19 shows the result when a third current is introduced in the contrary direction. Fig. 20 presents the transition to a helix of three convolutions. Fig. 21 indicates the direction of the lines within and outside the end of a cylindrical helix, on a plane through its axis. Fig. 22 presents the effect when a very small soft iron core is within the helix.

3240. Figs. 23 and 24 give an experimental illustration of the principles which I have adopted in relation to atmospheric magnetism and the general cause of the daily variations, &c. (2864. 2917.). A hemisphere of pure nickel presented to me by Dr. Percy, was supported with its flat face uppermost, and a large ring arranged round it to carry paper, which, resting both on the ring and the nickel, could then have iron filings sprinkled and arranged in form on it. The end of a bar-magnet in the same horizontal plane was adjusted about 2 inches from the nickel, and thus the forms of the lines of force associated with this pole could be determined over the place of the nickel hemisphere, under different circumstances, or even when it was removed. When the nickel was away, the forms of the lines of force were as in fig. 23; when the nickel was there, they were as in fig. 24. The application of a spirit-lamp to the nickel when in its place, raised its temperature to such a degree (above 600° Fahr.) that it lost its ordinary magnetic condition; and then the forms of the lines of force, as shown by filings, were the same as if the nickel was away. Removing the lamp, I was able to obtain the disposition of filings on successive pieces of paper, and as many as four results, like fig. 23, could be procured before the temperature had sunk so much as to cause the production of lines of force corresponding to fig. 24.

3241. These are exactly the same results with nickel as those I have assumed for the oxygen of the atmosphere. The change in the forms of the lines about the cooling nickel in this experiment are the same changes as those I have figured in the type globe of cooling air (2865. 2874.). Both nickel and oxygen are paramagnetic bodies, and change in the *same direction* by heating and cooling; and as the period of change with oxygen extends through degrees above and below common temperature (2861.), so inflections of the lines of force passing through the atmosphere, correspondent to those of the heating and cooling nickel, *must* take place to some extent. It is seen in the nickel results, that lines of force entirely outside of it do not for that reason continue an undeviating course, but are curved to and fro in consequence of the disposition of other lines within the nickel; a result, which, without reference to either one view or another of the physical action of the magnetic force, must be as true in the oxygen case as in the nickel case, because of the definite character of the magnetic force, whether represented by centres of action or by lines of power.

3242. Whether the amount of the deflection in the case of the atmosphere corresponds with the facts registered by observers, is a question which cannot be answered, I suppose, until we know the effect of very low temperatures upon the magnetic force of the atmosphere. In the nickel experiment the deflection is in places 30° or 40°; in nature the effect to be accounted for is not more than 13 or 14 minutes.

Royal Institution,
December 20, 1851.

On the Lines of Magnetic Force[1].

THAT beautiful system of power which is made manifest in the magnet, and which appears to be chiefly developed in the two extremities, thence called ordinarily the magnetic poles, is usually rendered evident to us in the case of a particular magnet by the attractive or repulsive effect of these parts on the corresponding parts of another magnet; and these actions have been employed, both to indicate the direction in which the magnetic force is exerted and also the amount of the force at different distances. Thus, if the attraction be referred to, it may be observed either upon another magnet or upon a piece of soft iron; and the law which results, for effects beyond a certain distance, is, that the force is inversely as the square of the distance. When the distances of the acting bodies from each other is small, then this law does not hold, either for the surface of the magnets or for any given point within them.

It is proposed to employ a new method, founded upon a property of the magnetic forces different from that producing attraction or repulsion, for the purpose of ascertaining the direction, intensity, and amount of these forces,—not to the displacement of the former method, but to be used in conjunction with it; and which may be highly influential in the further development of the nature of this power, inasmuch as the principle of action, though different, is not less magnetic than attraction and repulsion, not less strict, and the results not less definite.

The term *line of magnetic force* is intended to express simply the direction of the force in any given place, and not any physical idea or notion of the manner in which the force may be there exerted; as by actions at a distance, or pulsations, or waves, or a current, or what not. A line of magnetic force may be defined to be that line which is described by a very small magnetic needle, when it is so moved in either direction correspondent to its length, that the needle is constantly a tangent to the line of motion; or, it is that line along which, if a transverse wire be moved in either direction, there is no tendency

[1] Royal Institution Proceedings, 23rd January, 1852.

to the formation of an electric current in the wire, whilst if moved in any other direction there is such a tendency. The direction of these lines about and between ordinary magnets is easily represented in a general manner by the well-known use of iron filings.

The proposed method of recognizing and taking account of these lines of force, is to collect and measure the electricity set into motion in the moving transverse wire; a process entirely different in its nature and action to that founded on the use of a magnetic needle. That it may be advantageously employed, excellent conductors are required; and therefore those proceeding from the moving wire to the galvanometer were of copper 0·2 of an inch in thickness, and as short as was convenient. The galvanometer, also, instead of including many hundred convolutions of a long fine wire, consisted only of about 48 or 50 inches of such wire as that described above, disposed in two double coils about the astatic needle: and that used in the careful research contained only 20 inches in length of a copper bar 0·2 of an inch square. These galvanometers showed effects 30, 40 or 50 times greater than those constructed with fine wire; so abundant is the quantity of electricity produced by the intersections of the lines of magnetic force, though very low in intensity.

The lines of force already described will, if observed by iron filings or a magnetic needle or otherwise, be found to start off from one end of a bar-magnet, and after describing curves of different magnitudes through the surrounding space, to return to and set on at the other end of the magnet; and these forces being regular, it is evident that if a ring, a little larger than the magnet, be carried from a distance towards the magnet and over one end until it has arrived at the equatorial part, it will have intersected *once* all the external lines of force of that magnet. Such rings were soldered on to fitly shaped conductors connected with the galvanometer, and the deflections of the needle observed for one, two, or more such motions or intersections of the lines of force: it has been found that when every precaution was taken, and the results at the galvanometer carefully observed, the effect there was sensibly proportionate for small or moderate arcs to the number of times the loop or ring had passed over the pole. In this way, not only could the definite actions of the intersecting wire be observed and established, but also one magnet could be compared to another: wires of different thickness and of different substances could be compared; and the sections also described by the wire in its journey could be varied. When the wire was the same in length, diameter, and substance, no matter what its course was across the lines of force, whether direct or oblique, near to or far from the poles of the magnet, the result was the same.

A compound bar-magnet was so fitted up that it could revolve on its axis, and a broad circular copper ring was fixed on it at the middle distance or equator, so as to give a cylindrical exterior at that place. A copper wire being made fast to this ring within, then proceeded to the middle of the magnet, and afterwards along its axis and out at one end. A second wire touched, by a spring contact, the outside of the copper ring, and was then continued outwards six inches, after which it rose and finally turned over the upper pole towards the first wire, and was attached to a cylinder insulated from but moving round it. This cylinder and the wire passing through it were connected with the galvanometer so that the circuit was complete; but that circuit had its course down the middle of the magnet, then outwards at the equator, and back again on the outside, and whilst always perfect, allowed the magnet to be rotated without the external part of the circuit, or the latter without the magnet, or both together. When the magnet and external wire were revolved together, as one arrangement fixed in its parts, there was no effect at the galvanometer, however long the rotation was continued. When the magnet with the internal wire made four revolutions, as the hand of a watch, the outer conductor being still, the galvanometer needle was deflected 35° or 40° in one direction: when the magnet was still, and the outer wire made four revolutions as the hands of a watch, the galvanometer needle was deflected as much as before in the *contrary direction*: and in the more careful experiments the amount of deflection for four revolutions was precisely the same, whatever the course of the external wire, either close to or far off from the pole of the magnet. Thus it was shown, that when the magnet and the wire revolved in the same direction, contrary currents of electricity, exactly equal to each other, tended to be produced; that those outside resulted from the intersection by the outer wire of the lines of magnetic force external to the magnet; that wherever this intersection was made the result was the same; and that there were corresponding lines of force within the magnet, exactly equal in force or amount to those without, but in the contrary direction. That in fact every line of magnetic force is a closed curve, which in some part of its course passes through the magnet to which it belongs. The vicinity of other magnets to this magnet made no difference in the effect, provided they were not moved during the experiments; and in this manner the non-interference of such magnets with that under investigation was fully established.

In the foregoing cases the lines of force, belonging as they did to small systems, rapidly varied in intensity according to their distance from the magnet, by what may be called their divergence. The earth, on the contrary, presents us, within the limits of one experiment at any one time, a field of equal force. The dipping needle indicates the direction or polarity of this force; and if we work in a plane perpendicular to the dip, then the number or amount of the lines of force experimented with will be in proportion to the area which our apparatus may include. Wires were therefore formed into parallelograms, inclosing areas of various extent, as one square foot, or nine square feet, or any other proportion, and being fixed upon axes equidistant from two of the sides could have these axes adjusted perpendicular to the line of dip and could then be revolved. A commutator was employed, and associated both with the galvanometer and the parallelograms, so that the upper part of the revolving wire always sent the current induced in it in the same direction. Here it was found that rotation in one direction gave one electric current; that rotation in the reverse direction gave the contrary current; that the effect at the galvanometer was proportionate to the number of rotations with the same rectangle; that with different sized rectangles of the same wire the effect was proportionate to the area of the rectangle, *i. e.* the number of curves intersected, &c.

All these and other results are more fully stated and proved in papers sent to the Royal Society[1]. The general conclusions are, that the magnetic lines of force may be easily recognized and taken account of by the moving wire, both as to *direction* and

[1] See page 328, &c.

intensity, within metals, iron or magnets, as well as in the space around; and that the wire sums up the action of many lines in one result:—that the lines of forces well represent the *nature*, *condition*, *direction*, and *amount* of the magnetic forces:—that the effect is directly as the number of lines of force intersected, whether the intersection be direct or oblique:—that in a field of equal force, it is directly as the *velocity*; or as the *length* of the moving wire; or as the *mass* of the wire:—that the external power of an unchangeable magnet is *definite* yet illimitable in extent; and that any section of all the lines of force is equal to any other section:—that the lines of force within the magnet are equal to those without; and also that they are continuous with those without, the lines of force being closed curves.

On the Physical Character of the Lines of Magnetic Force [1].

Note.—The following paper contains so much of a speculative and hypothetical nature, that I have thought it more fitted for the pages of the Philosophical Magazine than those of the Philosophical Transactions. Still it is so connected with, and dependent upon former researches, that I have continued the system and series of paragraph numbers from them to it. I beg, therefore, to inform the reader, that those in the body of the text refer chiefly to papers already published, or ordered for publication, in the Philosophical Transactions; and that they are not quite essential to him in the reading of the present paper, unless he is led to a serious consideration of its contents. The paper, as is evident, follows Series xxviii. and xxix., now printing in the Philosophical Transactions, and depends much for its experimental support upon the more strict results and conclusions contained in them.

3243. I have recently been engaged in describing and defining the lines of magnetic force (3070.), *i. e.* those lines which are indicated in a general manner by the disposition of iron filings or small magnetic needles, around or between magnets; and I have shown, I hope satisfactorily, how these lines may be taken as exact representants of the magnetic power, both as to disposition and amount; also how they may be recognized by a moving wire in a manner altogether different in principle from the indications given by a magnetic needle, and in numerous cases with great and peculiar advantages. The definition then given had no reference to the physical nature of the force at the place of action, and will apply with equal accuracy whatever that may be; and this being very thoroughly understood, I am now about to leave the strict line of reasoning for a time, and enter upon a few speculations respecting the physical character of the lines of force, and the manner in which they may be supposed to be continued through space. We are obliged to enter into such speculations with regard to numerous natural powers, and, in-

[1] Philosophical Magazine for June 1852.

deed, that of gravity is the only instance where they are apparently shut out.

3244. It is not to be supposed for a moment that speculations of this kind are useless, or necessarily hurtful, in natural philosophy. They should ever be held as doubtful, and liable to error and to change; but they are wonderful aids in the hands of the experimentalist and mathematician. For not only are they useful in rendering the vague idea more clear for the time, giving it something like a definite shape, that it may be submitted to experiment and calculation; but they lead on, by deduction and correction, to the discovery of new phænomena, and so cause an increase and advance of real physical truth, which, unlike the hypothesis that led to it, becomes fundamental knowledge not subject to change. Who is not aware of the remarkable progress in the development of the nature of light and radiation in modern times, and the extent to which that progress has been aided by the hypotheses both of emission and undulation? Such considerations form my excuse for entering now and then upon speculations; but though I value them highly when cautiously advanced, I consider it as an essential character of a sound mind to hold them in doubt; scarcely giving them the character of opinions, but esteeming them merely as probabilities and possibilities, and making a very broad distinction between them and the facts and laws of nature.

3245. In the numerous cases of force acting at a distance, the philosopher has gradually learned that it is by no means sufficient to rest satisfied with the mere fact, and has therefore directed his attention to the manner in which the force is transmitted across the intervening space; and even when he can learn nothing sure of the manner, he is still able to make clear distinctions in different cases, by what may be called the affections of the lines of power; and thus, by these and other means, to make distinctions in the nature of the lines of force of different kinds of power as compared with each other, and therefore between the powers to which they belong. In the action of gravity, for instance, the line of force is a straight line as far as we can test it by the resultant phænomena. It cannot be deflected, or even affected, in its course. Neither is the action in one line at all influenced, either in direction or amount, by a like action in another line; *i. e.* one particle gravitating toward another particle has exactly the same amount of force in the same direction, whether it gravitates to that one alone or towards myriads of other like particles, exerting in the latter case upon each one of them a force equal to that which it can exert upon the single one when alone: the results of course can combine, but the direction and amount of force between any two given particles remain unchanged. So gravity presents us with the simplest case of attraction; and appearing to have no relation to any physical process by which the power of the particles is carried on between them, seems to be a pure case of attraction or action at a distance, and offers therefore the simplest type of the cases which may be like it in that respect. My object is to consider how far magnetism is such an action at a distance; or how far it may partake of the nature of other powers, the lines of which depend, for the communication of force, upon intermediate physical agencies (3075.).

3246. There is one question in relation to gravity, which, if we could ascertain or touch it, would greatly enlighten us. It is, whether gravitation requires *time*. If it did, it would show undeniably that a physical agency existed in the course of the line of force. It seems equally impossible to prove or disprove this point; since there is no capability of suspending, changing, or annihilating the power (gravity), or annihilating the matter in which the power resides.

3247. When we turn to radiation phænomena, then we obtain the highest proof, that though nothing ponderable passes, yet the lines of force have a physical existence independent, in a manner, of the body radiating, or of the body receiving the rays. They may be turned aside in their course, and then deviate from a straight into a bent or a curved line. They may be affected in their nature so as to be turned on their axis, or else to have different properties impressed on different sides. Their sum of power is limited; so that if the force, as it issues from its source, is directed on to or determined upon a given set of particles, or in a given direction, it cannot be in any degree directed upon other particles, or into another direction, without being proportionately removed from the first. The lines have no dependence upon a second or reacting body, as in gravitation; and they require time for their propagation. In all these things they are in marked contrast with the lines of gravitating force.

3248. When we turn to the electric force, we are presented with a very remarkable general condition intermediate between the conditions of the two former cases. The power (and its lines) here requires the *presence* of two or more acting particles or masses, as in the case of gravity; and cannot exist with one only, as in the case of light. But though two particles are requisite, they must be in an *antithetical* condition in respect of each other, and not, as in the case of gravity, alike in relation to the force. The power is now dual; there it was simple. Requiring two or more particles like gravity, it is unlike gravity in that the power is limited. One electro-particle cannot affect a second, third and fourth, as much as it does the first; to act upon the latter its power must be proportionately removed from the former, and this limitation appears to exist as a necessity in the dual character of the force; for the two states, or places, or directions of force must be equal to each other.

3249. With the electric force we have both the static and dynamic state. I use these words merely as names, without pretending to have a clear notion of the physical condition which they seem meaningly to imply. Whether there are two fluids or one, or any fluid of electricity, or such a thing as may be rightly called a current, I do not know; still there are well-established electric conditions and effects which the words *static, dynamic,* and *current* are generally employed to express; and with this reservation they express them as well as any other. The lines of force of the *static* condition of electricity are present in all cases of induction. They terminate at the surfaces of the conductors under induction, or at the particles of non-conductors, which, being electrified, are in that condition. They are subject to inflection in their course (1215. 1230.), and may be compressed or rarefied by bodies of different inductive capacities (1252. 1277.); but they are in those cases affected by the intervening matter; and it is not certain how the line of electric force would exist in relation to a perfect vacuum, *i. e.* whether it would be a straight line, as that of gravity is assumed to be, or curved in such a manner as to show something like physical existence separate from the mere distant actions of the surfaces or particles bounding or terminating the induction. No condition of *quality* or *polarity* has as yet been discovered in the line of static electric force; nor has any relation of *time* been established in respect of it.

3250. The lines of force of dynamic electricity are either limited in their extent, as in the lowering by discharge, or otherwise of the inductive condition of static electricity; or endless and continuous, as closed curves in the case of a voltaic circuit. Being definite in their amount for a given source, they can still be expanded, contracted, and deflected almost to any extent, according to the nature and size of the media through which they pass, and to which they have a direct relation. It is probable that matter is always essentially present; but the hypothetical æther may perhaps be admitted here as well as elsewhere. No condition of quality or polarity has as yet been recognized in them. In respect of *time*, it has been found, in the case of a Leyden discharge, that time is necessary even with the best conductors; indeed there is reason to think it is as necessary there as in the cases dependent on bad conducting media, as, for instance, in the lightning flash.

3251. Three great distinctions at least may be taken among these cases of the exertion of force at a distance; that of gravitation, where propagation of the force by physical lines through the intermediate space is not supposed to exist; that of radiation, where the propagation does exist, and where the propagating line or ray, once produced, has existence independent either of its source, or termination; and that of electricity, where the propagating process has intermediate existence, like a ray, but at the same time depends upon both extremities of the line of force, or upon conditions (as in the connected voltaic pile) equivalent to such extremities. Magnetic action at a distance has to be compared with these. It may be unlike any of them; for who shall say we are aware of all the physical methods or forms under which force is communicated? It has been assumed, however, by some, to be a pure case of force at a distance, and so like that of gravity; whilst others have considered it as better represented by the idea of streams of power. The question at present appears to be, whether the lines of magnetic force have or have not a physical existence; and if they have, whether such physical existence has a static or dynamic form (3075. 3156. 3172. 3173.).

3252. The lines of magnetic force have not as yet been affected in their *qualities*, *i.e.* nothing analogous to the polarization of a ray of light or heat has been impressed on them. A relation between them and the rays of light when polarized has been discovered (2146.)[1]; but it is not of such a nature as to give proof as yet, either that the lines of magnetic force have a separate existence, or that they have not; though I think the facts are in favour of the former supposition. The investigation is an open one, and very important.

3253. No relation of *time* to the lines of magnetic force has as yet been discovered. That iron requires *time* for its magnetization is well known. Plücker says the same is the case for bismuth, but I have not been able to obtain the effect showing this result. If that were the case, then mere space with its æther ought to have a similar relation, for it comes between bismuth and iron (2787.); and such a result would go far to show that the lines of magnetic force have a separate physical existence. At present such results as we have cannot be accepted as in any degree proving the point of *time*; though if that point were proved, they would most probably come under it. It may be as well to state, that in the case also of the moving wire or conductor (125. 3076.), time is required[2]. There seems no hope of touching the investigation by any method like those we are able to apply to a ray of light, or to the current of the Leyden discharge; but the mere statement of the problem may help towards its solution.

3254. If an action in *curved* lines or directions could be proved to exist in the case of the lines of magnetic force, it would also prove their physical existence external to the magnet on which they might depend; just as the same proof applies in the case of static electric induction[3]. But the simple disposition of the lines, as they are shown by iron particles, cannot as yet be brought in proof of such a curvature, because they may be dependent upon the presence of these particles and their mutual action on each other and the magnets; and it is possible that attractions and repulsions in right lines might produce the same arrangement. The results therefore obtained by the moving wire (3076. 3176.)[4], are more likely to supply data fitted to elucidate this point, when they are extended, and the true magnetic relation of the moving wire to the space which it occupies is fully ascertained.

[1] Philosophical Transactions, 1846, p. 1.
[2] Experimental Researches, 8vo edition, vol. ii. pp. 191, 195.
[3] Philosophical Transactions, 1838, p. 16. [4] Ibid. 1852.

3255. The *amount* of the lines of magnetic force, or the force which they represent, is clearly limited, and therefore quite unlike the force of gravity in that respect (3245); and this is true, even though the force of a magnet in free space must be conceived of as extending to incalculable distances. This limitation in amount of force appears to be intimately dependent upon the dual nature of the power, and is accompanied by a displacement or removability of it from one object to another, utterly unlike anything which occurs in gravitation. The lines of force abutting on one end or pole of a magnet may be changed in their direction almost at pleasure (3238.), though the original seats of their further parts may otherwise remain the same. For, by bringing fresh terminals of power into presence, a new disposition of the force upon them may be occasioned; but though these may be made, either in part or entirely, to receive the external power, and thus alter its direction, no change in the amount of the force is thus produced. And this is the case in strict experiments, whether the new bodies introduced are soft iron or magnets (3218. 3223.)[1]. In this respect, therefore, the lines of magnetic force and of electric force agree. Results of this kind are well shown in some recent experiments on the effect of iron, when passing by a copper wire in the magnetic field of a horseshoe magnet (3129. 3130.), and also by the action of iron and magnets on each other (3218. 3223.).

3256. It is evident, I think, that the experimental data are as yet insufficient for a full comparison of the various lines of power. They do not enable us to conclude, with much assurance, whether the magnetic lines of force are analogous to those of gravitation, or direct actions at a distance; or whether, having a physical existence, they are more like in their nature to those of electric induction or the electric current. I incline at present to the latter view more than to the former, and will proceed to certain considerations bearing on the question, with a view to the further and future elucidation of the subject.

3257. I think I have understood that the mathematical expression of the laws of magnetic action at a distance is the same as that of the laws of static electric actions; and it has been

[1] Philosophical Transactions, 1852.

assumed at times that the supposition of north and south magnetisms, spread over the poles or respective ends of a magnet, would account for all its external actions on other magnets or bodies. In either the static or dynamic view, or in any other view like them, the exertion of the magnetic forces outwards, at the poles or ends of the magnet, must be an essential condition. Then, with a given bar-magnet, can these forces exist without a mutual relation of the two, or else a relation to contrary magnetic forces of equal amount originating in other sources? I do not believe they can; because, as I have shown in recent researches, the sum of the lines of force is equal for any section across them taken anywhere externally between the poles (3109.). Besides that, there are many other experimental facts which show the relation and connexion of the forces at one pole to those at the other [1]; and there is also the analogy with static electrical induction, where the one electricity cannot exist without relation to, equality with, and dependence on the other. Every dual power appears subject to this law as a law of necessity. If the opposite magnetic forces could be independent of each other, then it is evident that *a charge* with one magnetism only is possible; but such a possibility is negatived by every known experiment and fact.

3258. But supposing this necessary relation, which constitutes polarity, to exist, then how is it sustained or permitted in the case of an independent bar-magnet situated in free space? It appears to me, that the outer forces at the poles can only have relation to each other by *curved* lines of force through the surrounding space; and I cannot conceive curved lines of force without the conditions of a physical existence in that intermediate space. If they exist, it is not by a succession of particles, as in the case of static electric induction (1215. 1231.), but by the condition of space free from such material particles. A magnet placed in the middle of the best vacuum we can produce, and whether that vacuum be formed in a space previously occupied by paramagnetic or diamagnetic bodies, acts as well upon a needle as if it were surrounded by air, water or glass;

[1] The manner in which a large powerful magnet deranges, overpowers, and even inverts the magnetism of a smaller magnet, when it is brought near it in different directions without touching it, presents a number of such cases.

and therefore these lines exist in such a vacuum as well as where there is matter.

3259. It may perhaps be said that there is no proof of any outer lines of force, in the case of a magnet, except when the objects employed experimentally to show these lines, as a magnetic needle, soft iron, a moving wire, or a crystal of bismuth, are present; that these bodies, in fact, cause and develope the lines; just as in the case of gravity no idea of a line of gravitating force, in respect of a particle of matter by itself, can be formed: the idea exists only when a second particle is concerned. We are dealing, however, with a dual power; and we know that we cannot call into action, by magnetic induction upon soft iron or by electric currents, or otherwise, one magnetism without the other. Supposing, therefore, a bar of soft iron, or another bar-magnet, when brought end on and near to the first magnet, did by that approach develope the external force, the power which then only would become external should produce a corresponding external force of the contrary kind at the opposite extremity, or should not. If the first case occurs, it should be accompanied by the development of lines of force equivalent to it *within* the magnet. But I think we know, now, that in a very hard and perfect magnet there is no change of this kind (3223.). The outer and the inner lines of force remain the same in amount, whether the secondary magnet or the soft iron is present or away. It is the *disposition* only of the outer lines that is changed; their sum, and therefore their existence, remains the same. If the second case occurs, then the magnet, if broken in half under induction, should present in its fragments cases of absolute magnetic charge, or charge with one magnetism only (3257. 3261.).

3260. Or if it be imagined for a moment, that the two polarities of the bar-magnet are in relation to each other, but that whilst there is no external object to be acted upon they are related to each other through the magnet itself (an idea very difficult to conceive after the experimental demonstration of the course of the lines as closed curves (3117. 3230.)), still it would follow, that upon the forces being determined externally, a change in the sum of force both within and without the magnet should be caused. We can now, however, take cognizance of both these portions of force; and it appears that, with a good magnet, whether alone or under the influence of soft iron or other magnets of fourfold strength, the sum of forces without (3223.), and therefore also within (3117. 3121.) the magnet remains the same.

3261. If the northness and southness be considered so far independent of each other as to be compared to two fluids diffused over the two ends of the magnet (like the two electricities over a polarized conductor), then breaking the magnet in half ought to leave the two parts, one absolutely or differentially north in character, and the other south. Each should not be both north and south in equality of proportion, considering only the external force. But this never happens. If it be said that the new fracture renders manifest, externally, two new poles, opposite in kind but equal in force (which is the fact), because of the necessity of the case, then the same necessity exists also for the dependence and relation of the original poles of the original magnet, no matter what or where the first source of the power may be. But in that case the *curved lines* of force between the poles of the original magnet follow as a consequence; and the curvature of these lines appears to me to indicate their physical existence.

3262. If the magnetic poles in a bar-magnet be supposed to exert some kind of power internally, backward, as if they were centres of force, both within and without the magnet, by which they are able, upon the breaking of the magnet, to develope the contrary poles and their force, then that power cannot be the identical portion which is at the same time exerted externally; and if not the same, then when the magnet is broken, the two halves ought to have a degree of north or south charge. They ought not to be determinate magnets having equipotential poles. But they are so; and we may *break a hard magnet in half* whilst opposed to another powerful magnet which ought most to disturb the forces, and yet the broken halves are perfect magnets, equivalent in their polarities, just as if, when they were made by breaking, the dominant magnet was away. The power at the old poles is neither increased nor diminished, but remains in amount and in polar direction unchanged.

3263. Falling back, therefore, upon the case of a hard, well-made and well-charged straight bar-magnet, subject only to its own powers, it appears to me that we must either deny the joint external relation of the poles, and consider them as having no mutual tendency towards or action upon each other, or else admit that there is such an action exerted in or transmitted on through *curved* lines. To deny such an action, would be to set up a distinction between the action of the north end of a bar upon its south end, and its action upon the south end of other magnets, which, in the face of all the old experiments, and the new ones made with the moving wire (3076.), it appears to me impossible to admit. To acknowledge the action in curved lines, seems to me to imply at once that the lines have a physical existence. It may be a vibration of the hypothetical æther, or a state or tension of that æther equivalent to either a dynamic or a static condition; or it may be some *other state*, which though difficult to conceive, may be equally distinct from the supposed non-existence of the line of gravitating force, and the independent and separate existence of the line of radiant force (3251.)[1]. Still the existence of the state does not appear to me to be mere assumption or hypothesis, but to follow in some degree as a consequence of the known condition of the force concerned, and the facts dependent on it.

3264. I have not referred in the foregoing considerations to the view I have recently supported by experimental evidence, that the lines of force, considered simply as representants of the magnetic power (3117.), are closed curves, passing in one part of their course through the magnet, and in the other part through the space around it. These lines are identical in their nature, qualities and amount, both within the magnet and without. If to these lines, as formerly defined (3071.), we add the idea of physical existence, and then reconsider such of the cases which have just been mentioned as come under the new idea, it will be seen at once that the probability of curved external lines of force, and therefore of the physical existence of the lines, is as great, and even far greater, than before. For now no back action in the magnet could be supposed; and the external relation and dependence of the polarities (3257. 3263.) would, if it were possible, be even more necessary than before. Such a view would tend to give, but not necessarily, a dynamic form to the idea of magnetic force; and its close relation to dynamic electricity is well known (3265.) This I will proceed to examine; but before

[1] See Euler's views of the disposition of the magnetic force; also of the magnetic fluid, or æther and its streams. Letters, vol. ii. letters 62, 63.

doing so, will again look for a moment at static electric induction, as a case of the dual powers in mutual dependence by curved lines of force, but with these lines terminated, and not existing as closed circuits. An electric conductor polarized by induction, or an insulated, unconnected, rectilineal, voltaic battery presents such a case, and resembles a magnet in the disposition of the external lines of force. But the sustaining action (as regards the induction) being dependent upon the necessary relation of the opposite dual conditions of the force, is external to the conductor, or the battery; and in such a case, if the conductor or battery be separated in the middle, no charge appears there, nor any origin of new lines of inductive force. This is, no doubt, a consequence of the fact, that the lines of static inductive force are not continued internally; and, at the same time, a cause why the two divided portions remain in opposite states or absolutely charged. In the magnet such a division *does* develope new external lines of force; which being equal in amount to those dependent on the original poles, shows that the lines of force are continuous through the body of the magnet, and with that continuity gives the necessary reason why no absolute charge of northness or southness is found in the two halves.

3265. The well-known relation of the electric and magnetic forces may be thus stated. Let two rings, in planes at right angles to each other, represent them, as in Plate IV fig. 1. If a current of electricity be sent round the ring E in the direction marked, then lines of magnetic force will be produced, correspondent to the polarity indicated by a supposed magnetic needle placed at NS, or in any other part of the ring M to which such a needle may be supposed to be shifted. As these rings represent the lines of electro-dynamic force and of magnetic force respectively, they will serve for a standard of comparison. I have elsewhere called the electric current, or the line of electro-dynamic force, "an axis of power having contrary forces exactly equal in amount in contrary directions" (517.). The line of magnetic force may be described in *precisely the same terms*; and these two axes of power, considered as right lines, are perpendicular to each other; with this additional condition, which determines their mutual direction, that they are separated by a right line perpendicular to both. The meaning of the words above, when applied to the electric current, is precise, and does not imply that the forces are contrary *because* they are in reverse directions, but are *contrary in nature*; the turning one round, end for end, would not at all make it resemble the other; a consideration which may have influence with those who admit electric fluids, and endeavour to decide whether there are one or two electricities.

3266. When these two axes of power are compared, they have some remarkable correspondences, especially in relation to their position at right angles to each other. As a physical fact, Ampère[1] and Davy[2] have shown, that an electric current tends to elongate itself; and, so far, that may be considered as marking a character of the *electric* axis of power. When a free magnetic needle near the end of a bar-magnet first points and then tends to approach it, I see in the action a character of the contrary kind in the *magnetic* axis of power; for the lines of magnetic force, which, according to my recent researches, are common to the magnet and the needle (3230.), are shortened, first by the motion of the needle when it points, and again by the action which causes the needle to approach the magnet. I think I may say, that all the other actions of a magnet upon magnets, or soft iron, or other paramagnetic and diamagnetic bodies, are in harmony with the same effect and conclusions.

3267. Again:—like electric currents, or lines of force, or axes of power, when placed side by side, attract each other. This is well known and well seen, when wires carrying such currents are placed parallel to each other. But like magnetic axes of power or lines of force repel each other: the parallel case to that of the electric currents is given, by placing two magnetic needles side by side with like poles in the same direction; and by the use of iron filings, numerous pictorial representations (3234.) of the same general result may be obtained.

3268. Now these effects are not merely *contrasts* continued through two or more different relations, but they are contrasts which *coincide* when the position of the two axes of power at right angles to each other are considered (1659. 3265.). The tendency to *elongate* in the electric current, and the tendency to *lateral* separation of the magnetic lines of force which surround that current, are both tendencies in the same direction, though they seem like contrasts, when the two axes are considered out

[1] *Ann. de Chim.* 1822, vol. xxi. p. 47. [2] Phil. Trans. 1823, p. 153.

of their relation of mutual position; and this, with other considerations to be immediately referred to, probably points to the intimate physical relation, and it may be, to the oneness of condition of that which is apparently two powers or forms of power, electric and magnetic. In that case many other relations, of which the following are some forms, will merge in the same result. Thus, unlike magnetic lines, when end on, repel each other, as when similar poles are face to face; and unlike electric currents, if placed in the same relation, stop each other; or if raised in intensity, when thus made static, repel each other. Like electric currents or lines of force, when end on to each other, coalesce; like magnetic lines of force similarly placed do so too (3266. 3295.). Like electric currents, end to end, do not add their sums; but whilst there is no change in quantity, the intensity is increased. Like magnetic lines of force, similarly placed do not increase each other, for the power then also remains the same (3218.): perhaps some effect correspondent to the gain of intensity in the former case may be produced, but there is none as yet distinctly recognised. Like electric currents, side by side, add their quantities together; a case supplied either by uniting several batteries by their like ends, or comparing a large plate battery with a small one. Like magnetic lines of force do the same (3232.).

3269. The mutual relation of the magnetic lines of force and the electric axis of power has been known ever since the time of Œrsted and Ampère. This, with such considerations as I have endeavoured to advance, enables us to form a guess or judgement, with a certain degree of probability, respecting the nature of the lines of magnetic force. I incline to the opinion that they have a physical existence correspondent to that of their analogue, the electric lines; and having that notion, am further carried on to consider whether they have a probable dynamic condition, analogous to that of the electric axis to which they are so closely and, perhaps, inevitably related, in which case the idea of magnetic currents would arise; or whether they consist in a state of tension (of the æther?) round the electric axis, and may therefore be considered as static in their nature. Again and again the idea of an *electro-tonic* state (60. 1114. 1661. 1729. 1733.) has been forced on my mind; such a state would coincide and become identified with that which would then constitute the physical lines of magnetic force. Another consideration tends in the same direction. I formerly remarked that the magnetic equivalent to *static* electricity was not known; for if the undeveloped state of electric force correspond to the like undeveloped condition of magnetic force, and if the electric current or axis of electric power correspond to the lines of magnetic force or axis of magnetic power, then there is no known magnetic condition which corresponds to the static state of the electric power (1734.). Now assuming that the physical lines of magnetic force are currents, it is very unlikely that such a link should be naturally absent; more unlikely, I think, than that the magnetic condition should depend upon a state of tension; the more especially as under the latter supposition, the lines of magnetic power would have a physical existence as positively as in the former case, and the curved condition of the lines, which seems to me such a necessary admission, according to the natural facts, would become a possibility.

3270. The considerations which arise during the contemplation of the phænomena and laws that are made manifest in the mutual action of magnets, currents of electricity, and *moving conductors* (3084. &c.), are, I think, altogether in favour of the physical existence of the lines of magnetic force. When only a single magnet is employed in such cases, and the use of iron or paramagnetic bodies is dismissed, then there is no effect of attraction or repulsion or any ordinary magnetic result produced The phænomena may all very fairly be looked upon as purely electrical, for they are such in character; and if they coincide with magnetic actions (which is no doubt the case), it is probably because the two actions are one. But being considered as electrical actions, they convey a different idea of the condition of the field where they occur, to that involved in the thought of magnetic action at a distance. When a copper wire is placed in the neighbourhood of a bar-magnet, it does not, as far as we are aware (by the evidence of a magnetic needle or other means), disturb in the least degree the disposition of the magnetic forces, either in itself or in surrounding space. When it is moved across the lines of force, a current of electricity is developed in it, or tends to be developed; and there is every reason to believe, that if we could employ a perfect conductor, and obtain a perfect result, it would be the full equivalent to the force, electric or magnetic, which is exerted in the place occupied by the conductor. But, as I have elsewhere observed (3172.), this current, having its full and equivalent relation to the magnetic force, can hardly be conceived of as having its entire foundation in the mere fact of motion. The motion of an external body, otherwise physically indifferent, and having no relation to the magnet, could not beget a physical relation such as that which the moving wire presents. There must, I think, be a previous state, a state of tension or a static state, as regards the wire, which, when motion is superadded, produces the dynamic state or current of electricity. This state is sufficient to constitute and give a physical existence to the lines of magnetic force, and permit the occurrence of curvature or its equivalent external relation of poles, and also the various other conditions, which I conceive are incompatible with mere action at a distance, and which yet do exist amongst magnetic phænomena.

3271. All the phænomena of the moving wire seem to me to show the physical existence of an atmosphere of power about a magnet, which, as the power is antithetical, and marked in its direction by the lines of magnetic force, may be considered as disposed in sphondyloids, determined by the lines, or rather shells of force[1]. As the wire intersects the lines within a given sphondyloid external to the magnet, a current of electricity is generated, and that current is definite and the same for any or every intersection of the given sphondyloid. At the same time, whether the wire be quiescent or in motion, it does not cause derangement, or expansion, or contraction of the lines of force; the state

[1] The lines of magnetic force have been already defined (3071.). They have also been traced, as I think, and shown to be closed curves passing in one part of their course through the magnet to which they belong, and in the other part through space (3117.). If, in the case of a straight bar-magnet, any one of these lines, E, be considered as revolving round the axis of the magnet, it will describe a surface; and as the line itself is a closed curve, the surface will form a tube round the axis and inclose a solid form. Another line of force, F, will produce a similar result. The sphondyloid body may be either that contained by the surface of revolution of E, or that contained between the two surfaces of E and F, and which, for the sake of brevity, I have (by the advice of a kind friend) called simply the *sphondyloid*. The parts of the solid described, which are within and without the magnet, are in power equivalent to each other. When it is needful to speak of them separately, they are easily distinguished as the inner and outer sphondyloids; the surface of the magnet being then part of the bounding surface.

of the power in the neighbouring or other parts of the sphondyloid remaining sensibly the same (3176.).

3272. The old experiment of a wire when carrying an electric current[1] moving round a magnetic pole, or of a current being produced in the same wire when it is carried per force round the same pole (114.), shows the electrical dependence of the magnet and the wire, both when the current is employed from the first, and when it is generated by the motion. It coincides in principle with the results already quoted, and it includes, experimentally, all currents of electricity, whatever the medium in which they occur, even up to that due to the discharge of the Leyden jar or that between the electrodes of the voltaic battery. I think it also indicates the state of magnetic or electric tension in the surrounding space, not only when that space is occupied by metal or a wire, but also by air and other bodies; for whatever be the state in one case, it is probably general and therefore common to all (3173.).

3273. I will now venture for a time to assume the physical existence of the external lines of magnetic force, for the purpose of considering how the idea will accord with the general phænomena of magnetism. The magnet is evidently the sustaining power, and in respect of its internal condition or that of its particles, there is no idea put forth to represent it which at all approaches in probability and beauty to that of Ampère (1659.). Its analogy with the helix is wonderful; nevertheless there is, as yet, a striking experimental distinction between them; for whereas an unchangeable magnet can never raise up a piece of soft iron to a state more than equal to its own, as measured by the moving wire (3219.), a helix carrying a current can develope in an iron core magnetic lines of force, of a hundred or more times as much power as that possessed by itself, when measured by the same means. In every point of view, therefore, the magnet deserves the utmost exertions of the philosopher for the development of its nature, both as a magnet and also as a source of electricity, that we may become acquainted with the great law under which the apparent anomaly may disappear, and by which all these various phænomena presented to us shall become *one*.

3274. The physical lines of force, in passing out of the magnet into space, present a great variety of conditions as to form

[1] Experimental Researches, 8vo edition, vol. ii. p. 127.

(3238.). At times their refraction is very sudden, leaving the magnet at right, or obtuse, or acute angles, as in the case of a hard well-charged bar-magnet, fig. 2; in other cases the change of form of the line in passing from the magnet into space is more gradual, as in the circular plate or globe-magnet, figs. 3, 4, 5. Here the form of the magnet as the source of the lines has much to do with the result; but I think the condition and relation of the surrounding medium has an essential and evident influence, in a manner I will endeavour to point out presently. Again, this refraction of the lines is affected by the relative difference of the nature of the magnet and the medium or space around it; as the difference is greater, and therefore the transition is more sudden, so the line of force is more instantaneously bent. In the case of the earth, both the nature of its substance and also its form, tend to make the refractions of the line of force at its surface very gradual; and accordingly the line of dip does not sensibly vary under ordinary circumstances, at the same place, whether it be observed upon the surface or above or below it.

3275. Though the physical lines of force of a magnet may, and must be considered as extending to infinite distance around it as long as the magnet is absolutely alone (3110.), yet they may be condensed and compressed into a very small local space, by the influence of other systems of magnetic power. This is indicated by fig. 6. I have no doubt, after the experimental results given in Series XXVIII. respecting definite magnetic action (3109.), that the sphondyloid representing the total power, which in the experiment that supplied the figure had a sectional area of not two square inches in surface, would have equal power upon the moving wire, with that infinite sphondyloid which would exist if the small magnet were in free space.

3276. The magnet, with its surrounding sphondyloid of power, may be considered as analogous in its condition to a voltaic battery immersed in water or any other electrolyte; or to a gymnotus (1773. 1784.) or torpedo, at the moment when these creatures, at their own will, fill the surrounding fluid with lines of electric force. I think the analogy with the voltaic battery so placed, is closer than with any case of *static* electric induction, because in the former instance the physical lines of electric force may be traced both through the battery and its surrounding medium, for they form continuous curves like those I have imagined within and without the magnet. The direction of these lines of electric force may be traced, experimentally, many ways. A magnetic needle freely suspended in the fluid will show them in and near to the battery, by standing at right angles to the course of the lines. Two wires from a galvanometer will show them; for if the line joining the two ends in the fluid be at right angles to the lines of electric force (or the currents), there will be no action at the galvanometer; but if oblique or parallel to these lines, there will be deflection. A plate, or wire, or ball of metal in the fluid will show the direction, provided any electrolytic action can go on against it, as when a little acetate of lead is present in the medium, for then the electrolysis will be a maximum in the direction of the current or line of force, and nothing at all in the direction at right angles to it. The same ball will disturb and inflect the lines of electric force in the surrounding fluid, just as I have considered the case to be with paramagnetic bodies amongst magnetic lines of force (2806. 2821. 2874.). No one I think will doubt that as long as the battery is in the fluid, and has its extremities in communication by the fluid, lines of electric force having a physical existence occur in every part of it, and the fluid surrounding it.

3277. I conceive that when a magnet is in free space, there is such a medium (magnetically speaking) around it. That a vacuum has its own magnetic relations of attraction and repulsion is manifest from former experimental results (2787.); and these place the vacuum in relation to material bodies, not at either extremity of the list, but in the *midst* of them, as, for instance, between gold and platina (2399.), having other bodies on either side of it. What that surrounding magnetic medium, deprived of all material substance, may be, I cannot tell, perhaps the æther. I incline to consider this outer medium as *essential* to the magnet; that it is that which relates the external polarities to each other by curved lines of power; and that these must be so related as a matter of necessity. Just as in the case of the battery above, there is no line of force, either in or out of the battery, if this relation be cut off by removing or intercepting the conducting medium;—or in that of static electric induction, which is impossible un'il this related state be allowed (1169.)[1];—

[1] Philosophical Magazine, March 1843; or Experimental Researches, 8vo, vol. ii. p. 279.

so I conceive, that without this external mutually related condition of the poles, or a related condition of them to other poles sustained and rendered possible in like manner, a magnet could not exist; an absolute northness or southness, or an unrelated northness or southness, being as impossible as an absolute or an unrelated state of positive or negative electricity (1178.).

3278. In this view of a magnet, the medium or space around it is as essential as the magnet itself, being a part of the true and complete magnetic system. There are numerous experimental results which show us that the relation of the lines to the surrounding space can be varied by occupying it with different substances; just as the relation of a ray of light to the space through which it passes can be varied by the presence of different bodies made to occupy that space, or as the lines of electric force are affected by the media through which either induction or conduction takes place. This variation in regard to the magnetic power may be considered as depending upon the aptitude which the surrounding space has to effect the mutual relation of the two external polarities, or to carry onwards the physical line of force; and I have on a former occasion in some degree considered it and its consequences, using the phrase *magnetic conduction* to represent the physical effect (2797.) produced by the presence either of paramagnetic or diamagnetic bodies.

3279. When, for instance, a piece of cold iron (3129.) or nickel (3240.) is introduced into the magnetic field, previously occupied by air or being even mere space, there is a concentration of lines of force on to it, and more power is transmitted through the space thus occupied than if the paramagnetic body were not there. The lines of force therefore converge on to or diverge from it, giving what I have called conduction polarity (2818.); and this is the whole effect produced as regards the amount of the power; for not the slightest addition to, or diminution of, that external to the magnet is made (3218. 3223.). A new disposition of the force arises; for some passes now where it did not pass before, being removed from places where it was previously transmitted. Supposing that the magnet was inclosed in a surrounding solid mass of iron, then the effect of its superior conducting power would be to cause a great contraction inwards of the sphere of external action, and of the various sphondyloids, which we may suppose to be identified in different parts of it. A magnetic needle, if it could be introduced into the iron medium, would indicate extreme diminution, if not apparent annihilation, of the external power of this magnet; but the moving wire would show that it was there present to its full extent (3152. 3162.) in a very concentrated condition, just as it shows it in the very body of a magnet (3116.); and the power within the magnet, it being a hard and perfect one, would remain the same.

3280. The reason why a magnetic needle would fail as a correct indicator of the amount of power present in a given space is, that when perfect, it, because of the necessary condition of hardness, cannot carry on through its mass more lines of force than it can excite (3223.). But because of the coalescence of like lines of force end on (3226.), such a needle, when surrounded by a bad magnetic conductor, determines on to itself many of the lines which would otherwise pass elsewhere, has a high magnetic polarity, and is affected in proportion; every experiment, as far as I can perceive, tending to show that the attractions and repulsions are merely consequences of the tendency which the lines of physical magnetic force have to shorten themselves (3266.). So when the magnetic needle is surrounded by a medium gradually increasing in conducting power, it seems to show less and less force in its neighbourhood, though in reality the force is increasing there more and more. We can easily conceive a very hard and feebly charged magnetic needle surrounded by a medium, as soft iron, better than itself in conducting power, *i. e.* carrying on by conduction more lines of force than the needle could determine or carry on by its state of charge (3298.). In that case I conceive it would, if free to move, point feebly in the iron, because of the coalescence of the lines of force, but would be repelled bodily from the chief magnet, in analogy with the action on a diamagnetic body. As I have before stated, the principle of the moving wire can be applied successfully in those cases where that of the magnetic needle fails (3155.).

3281. If other paramagnetic bodies than iron be considered in their relation to the surrounding space, then their effects may be assumed as proportionate to the conducting power. If the surrounding medium were hard steel, the contraction of the sphondyloid of power would be much less than with iron; and the effects, in respect of the magnetic needle, would occur in a limited degree. If a solution of protosulphate of iron were used, the effect would occur in a very much less degree. If a solution were prepared and adjusted so as to have no paramagnetic or diamagnetic relation (2422.), it would be the same to the lines of force as free space. If a diamagnetic body were employed, as water, glass, bismuth or phosphorus, the extent of action of the sphondyloids would expand (3279.); and a magnetic needle would appear to increase in intensity of action, though placed in a region having a smaller amount of magnetic force passing across it than before (3155.). Whether in any of these cases, even in that of iron, the body acting as a conductor has a state induced upon its particles for the time like that of a magnet in the corresponding state, is a question which I put upon a former occasion (2833.); but I leave its full investigation and decision for a future time.

3282. The circumstances dependent upon the shape and size of magnets appear to accord singularly well with the view I am putting forth of the action of the surrounding medium. If there be a function in that medium equivalent to conduction, involving differences of conduction in different cases, that of necessity implies also reaction or resistance. The differences could not exist without. The analogous case is presented to us in every part by the electric force. When, therefore, a magnet, in place of being a bar, is made into a horseshoe form, we see at once that the lines of force and the sphondyloids are greatly distorted or removed from their former regularity; that a line of maximum force from pole to pole grows up as the horseshoe form is more completely given; that the power gathers in, or accumulates about this line, just because the badly conducting medium, *i. e.* the space or air between the poles, is shortened. A bent voltaic battery in its surrounding medium (3276.), or a gymnotus curved at the moment of its peculiar action (1785.), present exactly the like result.

3283. The manner in which the keeper or sub-magnet, when in place, reduces the power of the magnet in the space or air around, is evident. It is the substitution of an excellent conductor for a poor one; far more of the power of the magnet is transmitted through it than through the same space before, and less, therefore, in other places. If a horseshoe magnet be charged to saturation with its keeper on, and its power be then ascertained, removing the keeper will cause the power to fall. This will be (according to the hypothesis) because the iron keeper could, by its conduction, sustain higher external conditions of the magnetic force, and therefore the *magnet* could take up and sustain a higher condition of charge. The case passes into that of a steel ring magnet, which being magnetized, shows no external signs of power, because the lines of force of one part are continued on by every other part of the ring; and yet when broken exhibits strong polarity and external action, because then the lines, which, being determined at a given point, were before carried on through the continuous magnet, have now to be carried on and continued through the surrounding space.

3284. These results, again, pass into the fact, easily verified partially, that if soft iron surround a magnet, being in contact with its poles, that magnet may receive a much higher charge than it can take, being surrounded with a lower paramagnetic substance, as air: also another fact, that when masses of soft iron are at the ends of a magnet, the latter can receive and keep a higher charge than without them; for these masses carry on the physical lines of force, and deliver them to a body of surrounding space; which is either widened, and therefore increased in the direction across the lines of force, or shortened in that direction parallel to them, or both; and both are circumstances which facilitate the conduction from pole to pole, and the relation of the external lines to the lines of force *within* the magnet. In the same way the armature of a natural loadstone is useful. All these effects and expedients accord with the view, that the space or medium external to the magnet is as important to its existence as the body of the magnet itself.

3285. Magnets, whether large or small, may be supersaturated, and then they fall in power when left to themselves; quickly at first if strongly supersaturated, and more slowly afterwards. This, upon the hypothesis, would be accounted for by considering the surrounding medium as unable, by its feeble magneto-conducting power, to sustain the higher state of charge. If the conducting power were increased sufficiently, then the magnet would not be supersaturated, and its power would not fall. Thus, if a magnet were surrounded by iron, it might easily be made to assume and retain a state of charge, which, if the iron were suddenly replaced by air, would instantly fall. Indeed, magnets can only be supersaturated by placing them for the time under the dominion of other sources of magnetic power, or of other more favourable surrounding media than that in which they manifest themselves as supersaturated.

3286. The well-known result, that small bar-magnets are far stronger in proportion to their size than larger similar magnets, harmonizes and *sustains* that view of the action of the external medium which has now been given. A sewing-needle can be magnetized far more strongly than a bar twelve inches long and an inch in diameter; and the reason under the view taken is, that the excited system in the magnet (correspondent to the voltaic battery in the analogy quoted (3276.)) is better sustained by the necessary conjoint action of the surrounding medium in the case of the small magnet. For as the imperfect magneto-conducting power of that medium (or the consequent state of tension into which it is thrown) acts back upon the magnet (3282.), so the smaller the sum of exciting force in the centre of the magnetic sphondyloids, the better able will the surrounding medium be to do its part in sustaining the resultant of force. It is very manifest, that if the twelve-inch bar be conceived of as subdivided into sewing-needles, and these be separated from each other, the whole amount of exciting force acts upon, and is carried onwards in closed magnetic curves, by a very much larger amount of external surrounding medium than when they are all accumulated in the single bar.

3287. The results which have been observed in the relation of *length* and *thickness* of a bar-magnet, harmonize with the view of the office of the external medium now urged. If we take a small, well-proportioned, saturated magnet, as a sewing-needle; alone, it has, as just stated, such relation to the surrounding space as to have its high condition sustained; if we place a second like magnetic needle by the side of the first, the surrounding space of the two is scarcely enlarged, it is not at all improved in conducting character, and yet it has to sustain double the internal exciting magnetic force exerted when there was one needle only (3232.); this must react back upon the magnets, and cause a reduction of their power. The addition of a third needle repeats the effect; and if we conceive that successive needles are added until the bundle is an inch thick, we have a result which will illustrate the effect of a thickness too large, and disproportionate to the length.

3288. On the other hand, if we assume two such needles similarly placed in a right line at a distance from each other, each has its surrounding system of curves occupying a certain amount of space; if brought together by unlike poles, they form a magnet of double the length; the external lines of force coalesce (3226.), those at the faces of contact nearly disappear; those which proceed from the extreme poles coalesce externally, and form one large outer system of force, the lines of which have a greater length than the corresponding lines of either of the two original needles. Still, by the supposition that the magnets are perfectly hard and invariable, the exciting force within remains, or tends to remain the same (3227.) in quantity, there is nothing to increase it. The increase in length, therefore, of the external circuit, which acts as a resisting medium upon the internal action, will tend to diminish the force of the whole system. Such would be the case if a voltaic battery surrounded by distilled water, as the analogous illustration (3276.), could be elongated in the water, and so its poles be removed further apart; and though in the case of magnets previously charged, some effect equivalent to intensity of excitement may be produced by conjoining several together end on, yet the diminished sustentation of power externally appears to follow as a consequence of the increased distance of the extreme poles, or external, mutually dependent parts. Static electric induction also supplies a correspondent and illustrative case.

3289. The usual case in which the influence of length and thickness becomes evident, is not, however, always or often that of the juxtaposition of magnets already as highly charged as they can be, but rather that of a bar about to be charged. If two bars, alike in steel, hardness, &c., one an inch long and the tenth of an inch in diameter, and the other of the same length but five-tenths of an inch in diameter, be magnetized to supersaturation, the latter, though it contains twenty-five times the steel of the former, will not retain twenty-five times the power, for the reason already given (3287.); the surrounding medium not being able to sustain external lines of force to that amount. But if a third bar, two inches long and also five-tenths in diameter, be magnetized at the same time, it can receive much more power than the second one. A natural reason for this presents itself by the hypothesis; for the limitation of power in the two cases is not in the magnets themselves, but in the external medium. The shorter magnet has contact and connexion with that medium by a certain amount of surface; and just what power the medium outside that surface can support, the magnet will retain. Make the magnet as long again, and there is far more contact and relation with the surrounding medium than before; and therefore the power which the magnet can retain is greater. If there were such limited points of resulting action in the magnet as is often understood by the word *poles*, then such a result could hardly be the case, on my view of the physical actions. But such poles do not exist. Every part of the surface of the magnet, so to say, is pouring forth externally lines of magnetic force, as may be seen in figs. 2, 3, 4, 5 (3274.). The larger the magnet, to a certain extent, and the larger the amount of external conducting medium in contact with it, the more freely is this transmission made. If the second magnet, being an inch long, be conceived to be charged to its full amount, and then, whilst in free space, could have half an inch of iron added to its length at each end, we see and know that many of the lines of force originally issuing from that part of its surface still left in contact with the air at the equatorial part, would now move internally towards the ends, and issue at a part of the soft iron surface; indicating the manner in which the tension would be relieved by this better conducting medium at the ends, and by increased surface of contact with the surrounding bad conductor of air or space. The thick, short magnet could evidently excite and carry on physical lines of magnetic power far more numerous than those which the space about it can receive and convey from pole to pole; and the increase in the length of the magnet may go on advantageously, until the increasing sum of power, sustainable by the increasing medium in the circuit, is equal to that which the magnet can sustain or transmit internally; for all the lines of power, wherever they issue from the magnet, have to pass through its equator; and in this way the equator or thickness of the magnet becomes related to its length. So the advantageous increase in length of the bar is limited by the increasing resistance within, and especially at the equator of the bar; and the increase in breadth, by the increasing resistance (for increasing powers) of the external surrounding medium (3287.).

3290. It is very interesting to observe the results obtained when an attempt is made to magnetize, regularly, a thin steel wire, about 15 or 20 inches in length, and 0·05 of an inch in diameter. It can hardly be effected by bars; and when the wire is afterwards examined by filings (3234.), it is found to have irregular and consecutive poles, which vary as the magnetization is repeated with the same wire, as if they broke out suddenly by a rupture of something like unstable equilibrium; the effects apparently being chiefly referable to the cause now assigned. Again, when a magnet is made out of a thin, hard, steel plate, whose length is ten or twelve times its width, it is well known how the lines of force issue from it in greatest abundance at the extreme angles, and then at the edges; and how a spot on the face gives exit to a much smaller number of lines than a like spot on the edge, at the same distance from the magnetic equator. Iron filings show such results readily, and so also do the vibrations of a magnetic needle, and likewise the revolutions of a wire ring (3212.). Now this state of the plate-magnet is precisely that which would be expected from the hypotheses of the necessary and dependent state of the magnet on the medium surrounding it.

3291. The mutual dependence of a magnet and the external medium, assumed in the view now put forth, bears upon, and may probably explain, numerous observations of the apparently superficial character of the magnetism of iron and magnets in different cases. If a hard steel bar be magnetized by touch of other magnets, both the vicinity of the superficial parts of the bar to the exciting magnet in the first instance, and afterwards to the surrounding sustaining medium, will tend to cause the magnetism to be superficial in the bar. If a small magnet or a horseshoe bar be surrounded by a thick shell of iron as its external medium, the inner surface of the iron, or that nearest to the magnet, with its neighbouring parts, will convey on more power than the parts further away. If a thick iron core be placed in a helix carrying a feeble or moderate electric current, it is the part of the core nearest to the helix which becomes most highly charged. Probably many other like results may appear, or be hereafter devised, and may greatly help to assist the discussion of the question of physical lines of force now under consideration.

3292. When, in place of considering the medium external to a magnet as homogeneous or equal in magnetic power, we make it variable in different parts, then the effects in it appear to me still to be in perfect accordance with the notion of physical lines of magnetic force, which, being present externally, are definite in direction and amount. The series of substances at our command which affect the surrounding space in this respect, do not present a great choice of successive steps; but having iron, nickel and cobalt, very high as paramagnetic bodies, we then possess hard steel, as very far beneath them; next, perhaps, oxides of iron, and so on by solutions of the magnetic metals to oxygen, water, glass, bismuth and phosphorus, in the diamagnetic direction. Taking the magnetic force of the earth as supplying the source of power, and placing a globe of iron or nickel in the air, we see by the pointing of a small magnetic needle (or in another case, by the use of iron filings (3240.)), the deflected course of the lines of force as they enter into and pass out of the sphere, consequent upon the conducting power of the paramagnetic body. These have been described in their forms in another place (3238.). If we take a large bar-magnet, and place a piece of soft iron, about half the width of the magnet, and three or four times as long as it is wide, end on to, and about its own width from one pole, and covering that with paper, then observe the forms of the lines of force by iron filings; it will be seen how beautifully those issuing from the magnet converge, by fine inflections, on to the iron, entering by a comparatively small surface, and how they pass out in far more diffuse streams by a much larger surface at the further part of the bar, fig. 7. If we take several pieces of iron, cubes for instance, then the lines of force which are altogether outside of them, may be seen undergoing successive undulations in contrary directions, fig. 8. Yet in all these cases of the globe, bar and cubes, I, at least, am satisfied that a section across the same lines of force in any part of their course, however or whichever way deflected, would yield the same amount of effect (3109. 3218.); at the same time this effect of deflection is not only consistent with, but absolutely suggests the idea of a physical line of force.

3293. Then the manner in which the power disappears in such cases to an ordinary magnetic needle is perfectly consistent. A little needle held by the side of the soft bar described above (3292.), indicates much less magnetic power than if the iron were away. If held in a hole made in the iron, it is almost indifferent to the magnet; yet what power remains shows that the lines through the air in the hole are in the same general direction as those through the neighbouring iron. These effects are perfectly well known, no doubt; and my object is only to show that they are consistent with, and support the idea of external media having magnetic conducting power. But these apparent destructions of power, and even far more anomalous cases (2868. 3155.), are fully accounted for by the hypothesis; and the force absolutely unaffected in amount is found, experimentally, by the moving wire. I have had occasion before to refer to the modification of the magnetic force (in relation to the magnetic needle), where, its absolute quantity being the same, it passes across better or worse conductors, and I have temporarily used the words *quantity* and *intensity* (2866. 2868. 2870.). I would, however, rather not attempt to limit or define these or such like terms now, however much they may be wanted, but wait until what is at present little more than suggestion, may have been canvassed, and if true in itself, may have received assurance from the opinions or testimony of others.

3294. The association of magnet with magnet, and all the effects then produced (3218.), are in harmony, as far as I can perceive, with the idea of a physical line of magnetic force. If the magnets are all free to move, they set to each other, and then tend to approach; the great result being, that the lines from all the sources tend to coalesce, to pass through the best conductors, and to contract in length. When there are several magnets in presence and in restrained conditions, the lines of force, which they present by filings, are most varied and beautiful (3238.); but all are easily read and understood by the principles I have set forth. As the power is definite in amount, its removability from place to place, according to the changing disposition of the magnets, or the introduction of better or worse conductors into the surrounding media, becomes a perfectly simple result.

3295. As magnets may be looked upon as the habitations of bundles of lines of force, they probably show us the tendencies of the physical lines of force where they occur in the space around; just as electric currents, when conducted by solid wires, or when passing, as the Leyden or the voltaic spark, through air or a vacuum, are alike in their essential relations. In that case, the repulsion of magnets when placed side by side, indicates the lateral tendency of separation of lines of magnetic force (3267.). The effect, however, must be considered in relation to the simultaneous gathering up of the terrestrial lines of force in the surrounding space upon each magnet, and also the tendency of each magnet to secure its own independent external medium. The effect coincides with, and passes into that of the lateral repulsion of balls of iron in a previously equal magnetic field (2814.); which again, by a consideration of the action in two directions, *i. e.* parallel to and across the magnetic axis, links the phænomena of separation with those of attraction.

3296. When speaking of magnets, in illustration of the question under consideration, I mean magnets perfect in their kind, *i. e.* such as are very hard and hold their charge, so that there shall be neither internal reaction of discharge or development (3224.), nor any external change, except what may depend upon such absolute and permanent loss of exciting power as is consequent upon an over-ruling change of the external relations. Heterogeneous magnets, which might allow of irregular variations of power, are out of present consideration.

3297. With regard to the great point under consideration, it is simply, whether the lines of magnetic force have a *physical existence* or not? Such a point may be investigated, perhaps even satisfactorily, without our being able to go into the further questions of how they account for magnetic attraction and repulsion, or even by what condition of space, æther or matter, these lines consist. If the extremities of a straight bar-magnet, or if the polarities of a circular plate of steel (3274.), are in magnetic relation to each other externally (3257.), then I think the existence of *curved* lines of magnetic force must be conceded (3258. 3263.)[1]; and if that be granted, then I think that the physical nature of the lines must be granted also. If the external relation of the poles or polarity is denied, then, as it appears to me, the internal relation must be denied also; and with it a vast number of old and new facts (3070. &c.) will be left without

[1] See for a case of curved lines the inclosed and compressed system of forces belonging to the central circular magnet, fig. 6 (3275.).

either theory, hypothesis, or even a vague supposition to explain them.

3298. Perhaps both magnetic attraction and repulsion, in all forms and cases, resolve themselves into the differential action (2757.) of the magnets and substances which occupy space, and modify its magnetic power. A magnet first originates lines of magnetic force; and then, if present with another magnet, offers in one position a very free conduction of the new lines, like a paramagnetic body; or if restrained in the contrary position, resists their passage, and resembles a highly diamagnetic substance. So, then, a source of magnetic lines being present, and also magnets or other bodies affecting and varying the conducting power of space, those bodies which can convey onwards the most force, may tend, by differential actions, with the others present, to take up the position in which they can do so the most freely, whether it is by pointing or by approximation; the best conductor passing to the place of strongest action (2757.), whilst the worst retreats from it, and so the effects both of attraction and repulsion be produced. The tendency of the lines of magnetic force to shorten (3266. 3280.) would be consistent with such a notion. The result would occur whether the physical lines of force were supposed to consist in a dynamic or a static state (3269.).

3299. Having applied the term *line of magnetic force* to an abstract idea, which I believe represents accurately the nature, condition, direction, and comparative amount of the magnetic forces, without reference to any physical condition of the force, I have now applied the term *physical line of force* to include the further idea of their physical nature. The first set of lines I *affirm* upon the evidence of strict experiment (3071. &c.). The second set of lines I advocate, chiefly with a view of stating the question of their existence; and though I should not have raised the argument unless I had thought it both important, and likely to be answered ultimately in the affirmative, I still hold the opinion with some hesitation, with as much, indeed, as accompanies any conclusion I endeavour to draw respecting points in the very depths of science, as, for instance, regarding one, two, or no electric fluids; or the real nature of a ray of light, or the nature of attraction, even that of gravity itself, or the general nature of matter.

Royal Institution, March 6, 1852.

On the Physical Lines of Magnetic Force.

[Royal Institution Proceedings, June 11, 1852.]

On a former occasion [1] certain lines about a bar-magnet were described and defined (being those which are depicted to the eye by the use of iron filings sprinkled in the neighbourhood of the magnet), and were recommended as expressing accurately the nature, condition, direction, and amount of the force in any given region either within or outside of the bar. At that time the lines were considered in the abstract. Without departing from or unsettling anything then said, the inquiry is now entered upon of the possible and probable *physical existence* of such lines. Those who wish to reconsider the different points belonging to these parts of magnetic science may refer to two papers in the first part of the Phil. Trans. for 1852 [2] for data concerning the *representative* lines of force, and to a paper in the Phil. Mag. 4th Series, 1852, vol. iii. p. 401, for the argument respecting the *physical* lines of force [3].

Many powers act manifestly at a distance; their physical nature is incomprehensible to us: still we may learn much that is real and positive about them, and amongst other things something of the condition of the space between the body acting and that acted upon, or between the two mutually acting bodies. Such powers are presented to us by the phænomena of gravity, light, electricity, magnetism, &c. These when examined will be found to present remarkable differences in relation to their respective lines of forces; and at the same time that they establish the existence of real physical lines in some cases, will facilitate the consideration of the question as applied especially to magnetism.

When two bodies, *a*, *b*, gravitate towards each other, the line in which they act is a straight line, for such is the line which either would follow if free to move. The attractive force is not altered, either in *direction* or *amount*, if a third body is made to act by gravitation or otherwise upon either or both of the two first. A balanced cylinder of brass gravitates to the earth with a weight exactly the same, whether it is left like a pendulum freely to hang towards it, or whether it is drawn aside by other

[1] See page 402. [2] See page 328. [3] See page 407.

attractions or by tension, whatever the amount of the latter may be. A new gravitating force may be exerted upon *a*, but that does not in the least affect the amount of power which it exerts towards *b*. We have no evidence that *time* enters in any way into the exercise of this power, whatever the distance between the acting bodies, as that from the sun to the earth, or from star to star. We can hardly conceive of this force in one particle by itself; it is when two or more are present that we comprehend it: yet in gaining this idea we perceive no difference in the character of the power in the different particles; all of the same kind are *equal, mutual*, and *alike*. In the case of gravitation, no effect which sustains the idea of an independent or physical line of force is presented to us; and as far as we at present know, the line of gravitation is merely an ideal line representing the direction in which the power is exerted.

Take the Sun in relation to another force which it exerts upon the earth, namely its illuminating or warming power. In this case rays (which are lines of force) pass across the intermediate space; but then we may affect these lines by different media applied to them in their course. We may alter their direction either by reflection or refraction; we may make them pursue curved or angular courses. We may cut them off at their origin and then search for and find them before they have attained their object. They have a relation to *time*, and occupy 8 minutes in coming from the sun to the earth: so that they may exist independently either of their source or their final home, and have in fact a clear distinct physical existence. They are in extreme contrast with the lines of gravitating power in this respect; as they are also in respect of their condition at their terminations. The two bodies terminating a line of gravitating force are alike in their actions in every respect, and so the line joining them has like relations in both directions. The two bodies at the terminals of a ray are utterly unlike in action; one is a source, the other a destroyer of the line; and the line itself has the relation of a stream flowing in one direction. In these two cases of gravity and radiation, the difference between an abstract and a physical line of force is immediately manifest.

Turning to the case of Static Electricity we find here attractions (and other actions) at a distance as in the former cases; but when we come to compare the attraction with that of gravity, very striking distinctions are presented which immediately affect the question of a physical line of force. In the first place, when we examine the bodies bounding or terminating the lines of attraction, we find them as before, mutually and equally concerned in the action; but they are not alike: on the contrary, though each is endued with a force which speaking generally is of the like nature, still they are in such contrast that their actions on a third body in a state like either of them are precisely the reverse of each other,—what the one attracts the other repels; and the force makes itself evident as one of those manifestations of power endued with a dual and antithetical condition. Now with all such dual powers, attraction cannot occur unless the two conditions of force are present and in face of each other through the lines of force. Another essential limitation is that these two conditions must be exactly equal in amount, not merely to produce the effects of attraction, but in every other case; for it is impossible so to arrange things that there shall be present or be evolved more electric power of the one kind than of the other. Another limitation is that they must be in physical relation to each other; and that when a positive and a negative electrified surface are thus associated, we cannot cut off this relation except by transferring the forces of these surfaces to equal amounts of the contrary forces provided elsewhere. Another limitation is that the power is definite in amount. If a ball *a* be charged with 10 of positive electricity, it may be made to act with that amount of power on another ball *b* charged with 10 of negative electricity; but if 5 of its power be taken up by a third ball *c* charged with negative electricity, then it can only act with 5 of power on ball *a*, and that ball must find or evolve 5 of positive power elsewhere: this is quite unlike what occurs with gravity, a power that presents us with nothing dual in its character. Finally, the electric force acts in curved lines. If a ball be electrified positively and insulated in the air, and a round metallic plate be placed about 12 or 15 inches off, facing it and uninsulated, the latter will be found, by the necessity mentioned above, in a negative condition; but it is not negative only on the side facing the ball, but on the other or outer face also, as may be shown by a carrier applied there, or by a strip of gold or silver leaf hung against that outer face. Now the power affecting this face does not pass through the uninsulated plate, for the thinnest gold leaf is able to stop the inductive action, but round the edges of the face, and therefore acts in curved lines. All these points indicate the existence of physical lines of electric force:—the absolutely essential relation of positive and negative surfaces to each other, and their dependence on each other contrasted with the known mobility of the forces, admit of no other conclusion. The action also in curved lines must depend upon a physical line of force. And there is a third important character of the force leading to the same result, namely its affection by media having different specific inductive capacities.

When we pass to Dynamic Electricity the evidence of physical lines of force is far more patent. A voltaic battery having its extremities connected by a conducting medium, has what has been expressively called a current of force running round the circuit, but this current is an axis of power having equal and contrary forces in opposite directions. It consists of lines of force which are compressed or expanded according to the transverse action of the conductor, which changes in direction with the form of the conductor, which are found in every part of the conductor, and can be taken out from any place by channels properly appointed for the purpose; and nobody doubts that they are physical lines of force.

Finally as regards a Magnet, which is the object of the present discourse. A magnet presents a system of forces perfect in itself, and able, therefore, to exist by its own mutual relations. It has the dual and antithetic character belonging to both static and dynamic electricity; and this is made manifest by what are called its polarities, *i. e.* by the opposite powers of like kind found at and towards its extremities. These powers are found to be absolutely equal to each other; one cannot be changed in any degree as to amount without an equal change of the other; and this is true when the opposite polarities of a magnet are not related to each other, but to the polarities of other magnets. The polarities, or the *northness* and *southness* of a magnet are not only related to each other, through or within the magnet itself, but they are also related externally to opposite polarities (in the manner of static electric induction), or they cannot exist; and this external relation involves and necessitates an exactly equal amount of the new opposite polarities to which those of the magnet are related. So that if the force of a magnet *a* is related to that of another magnet *b*, it cannot act on a third magnet *c* without being taken off from *b*, to an amount proportional to its action on *c*. The lines of magnetic force are shown by the moving wire to exist both within and outside of the magnet; also they are shown to be closed curves passing in one part of their course through the magnet; and the amount of those within the magnet at its equator is exactly equal in force to the amount in any section including the whole of those on the outside. The lines of force outside a magnet can be affected in their direction by the use of various media placed in their course. A magnet can in no way be procured having only one magnetism, or even the smallest excess of northness or southness one over the other. When the polarities of a magnet are not related externally to the forces of other magnets, then they are related to each other: *i. e.* the northness and southness of an isolated magnet are externally dependent on and sustained by each other.

Now all these facts, and many more, point to the existence of physical lines of force external to the magnets as well as within. They exist in curved as well as in straight lines; for if we conceive of an isolated straight bar-magnet, or more especially of a round disc of steel magnetized regularly, so that its magnetic axis shall be in one diameter, it is evident that the polarities must be related to each other externally by curved lines of force; for no straight line can at the same time touch two points having northness and southness. Curved lines of force can, as I think, only consist with physical lines of force.

The phænomena exhibited by the moving wire confirm the same conclusion. As the wire moves across the lines of force, a current of electricity passes or tends to pass through it, there being no such current before the wire is moved. The wire when quiescent has no such current, and when it moves it need not pass into places where the magnetic force is greater or less. It may travel in such a course that if a magnetic needle were carried through the same course it would be entirely unaffected magnetically, *i. e.* it would be a matter of absolute indifference to the needle whether it were moving or still. Matters may be so arranged that the wire when still shall have the same diamagnetic force as the medium surrounding the magnet, and so in no way cause disturbance of the lines of force passing through both;

and yet when the wire moves, a current of electricity shall be generated in it. The mere fact of motion cannot have produced this current: there must have been a state or condition around the magnet and sustained by it, within the range of which the wire was placed; and this state shows the physical constitution of the lines of magnetic force.

What this state is, or upon what it depends, cannot as yet be declared. It may depend upon the æther, as a ray of light does, and an association has already been shown between light and magnetism. It may depend upon a state of tension, or a state of vibration, or perhaps some other state analogous to the electric current, to which the magnetic forces are so intimately related. Whether it of necessity requires matter for its sustentation will depend upon what is understood by the term matter. If that is to be confined to ponderable or gravitating substances, then matter is not essential to the physical lines of magnetic force any more than to a ray of light or heat; but if in the assumption of an æther we admit it to be a species of matter, then the lines of force may depend upon some function of it. Experimentally mere space is magnetic; but then the idea of such mere space must include that of the æther, when one is talking on that belief; or if hereafter any other conception of the state or condition of space rise up, it must be admitted into the view of that, which just now in relation to experiment is called mere space. On the other hand it is, I think, an ascertained fact, that ponderable matter is not essential to the existence of physical lines of magnetic force.

On the Magnetic Relations and Characters of the Metals[1].

In two former communications to the Philosophical Magazine[2] respecting the magnetic characters of metals generally and the temperatures at which they respectively lost or gained the powers of magnetic induction, I said that iron and nickel were the only metals which had this power, and that I could not find it in pure cobalt. In this I was in error; cobalt has this property in common with iron and nickel, as others have said. I have sought for the piece of cobalt on which I experimented and believed to be pure, but cannot find it, and am now unable to ascertain the cause of my error, though not too late to correct it.

By favour of Dr. Percy and Mr. Askin I have recently experimented with two fine pieces of pure cobalt prepared by the latter, both being well-fused and perfectly clean masses. This metal becomes strongly magnetic by induction, either of a magnet or an electro-current, and can easily be made to lift more than its own weight. Like soft iron and nickel, it does not retain its magnetism when the inducing influence is removed.

It was to me a point of great interest to ascertain whether, and at what temperature, cobalt would lose this power and become as the unmagnetic metals. To my surprise I found this to be very high, not merely much higher than with nickel, but far above that required for iron or steel, and nearly approaching the temperature of melted copper. That for iron is a moderate red heat, and that for nickel the temperature of boiling oil only. As the temperature rises, the magnetic force of the cobalt continues, apparently undiminished, to a certain degree of heat, and then suddenly ceases; and it comes on as suddenly in descending from still higher temperatures.

The oxide of cobalt procured by burning the metal cobalt on charcoal by a jet of oxygen was obtained as a fused globule and was not magnetic.

The oxide of nickel being heated and cooled is not magnetic, but whilst heating care must be taken that it is not reduced. If heated in the flame of a spirit-lamp, especially at the lower part,

[1] Philosophical Magazine, July 1845.

[2] 1836, vol. viii. p. 177, and 1839, vol. xiv. p. 161; or Exp. Res. vol. ii. pp. 217, 223.

it will often appear to be not magnetic, and then as the temperature falls will become magnetic; but this is caused by the reduction of a little of the oxide at the edges or elsewhere in the mass to the metallic state, and its exhibition of the properties of the *metal* at temperatures above and below the magnetic point.

Manganese.—A piece of manganese prepared by Mr. Thomson and considered pure was put into my hands. It contained a trace of iron; it was very slightly magnetic, and probably only in consequence of the little iron present. Before a jet of oxygen or charcoal, it burnt with sparkles somewhat in the manner of iron, and produced an oxide which could be obtained either as a porous white mass or as a dense compact brown mass, translucent in small slices; but in neither state was the oxide magnetic.

I then cooled[1] manganese, chromium, and many other metals and bodies to the lowest temperature which I could obtain by a mixture of æther and solid carbonic acid placed *in vacuo*[2], the temperature being then not higher than 156° F. below 0°, but not one of them assumed the magnetic state. The following is a list of the substances:—

Platinum.	Speculum metal.
Gold.	Plumbago.
Silver.	Gas-retort carbon.
Palladium.	Kish.
Copper.	Orpiment.
Tin.	Realgar.
Lead.	Sulphuret of antimony.
Cadmium.	... bismuth.
Zinc.	... copper.
Rhodium.	... iron.
Manganese.	... lead.
Chromium.	... silver.
Titanium.	... tin.
Iridium and Osmium.	Carbonate of iron (native).
Antimony.	Prussian blue.
Arsenic.	Crystallized sulphate of iron.
Bismuth.	Calomel.
Fusible metal.	Chloride of silver.

[1] Philosophical Magazine, 1839, vol. xiv. p. 162; or Exp. Res. vol. ii. p. 224.
[2] Philosophical Transactions, 1845, pp. 157, 158.

Chloride of lead.
Protoxide of arsenic.
... antimony.
... lead.
Oxide of bismuth.
... tin (native).
... manganese (native).

Thus it appears that only iron, nickel and cobalt are magnetic, or can be made magnetic amongst metals after the manner of iron; but the addition of cobalt, and still more, the very high temperature required to take away this property from it, increases the probability[1] that all the metals form a common class in respect of this property; and that it is only a difference of temperature which distinguishes these three from the rest, just as it also in a similar respect distinguishes them from each other.

In connexion with the effect of heat it may be remarked (and perhaps has been remarked already), that, assuming an elevated temperature for the internal parts of the earth, then it is evident that at a very moderate distance from the surface, as compared with the earth's diameter, the substances composing the earth must be destitute of such magnetic power as is possessed by a magnet; and, at a distance somewhat greater, none of them can retain even that power which soft iron has of becoming magnetic by induction. In such case, whether the earth be considered as magnetic of itself, like a loadstone, or rendered magnetic by induction under the influence of external magnetic masses, as the sun for instance; still it can only be in its crust that the magnetic power could be developed. Assuming with Ampère, that the magnetism of the earth is due to electric currents circulating around it parallel to the equator, then of course the above observations regarding the effects of heat would not apply.

Royal Institution,
June 7, 1845.

[1] Philosophical Magazine, 1836, vol. viii. p. 177; or Exp. Res. ii. 217.

Thoughts on Ray-vibrations[1].

To Richard Phillips, Esq.

DEAR SIR,

AT your request I will endeavour to convey to you a notion of that which I ventured to say at the close of the last Friday-evening Meeting, incidental to the account I gave of Wheatstone's electro-magnetic chronoscope; but from first to last understand that I merely threw out as matter for speculation, the vague impressions of my mind, for I gave nothing as the result of sufficient consideration, or as the settled conviction, or even probable conclusion at which I had arrived.

The point intended to be set forth for the consideration of the hearers was, whether it was not possible that the vibrations which in a certain theory are assumed to account for radiation and radiant phænomena may not occur in the lines of force which connect particles, and consequently masses of matter together; a notion which, as far as it is admitted, will dispense with the æther, which, in another view, is supposed to be the medium in which these vibrations take place.

You are aware of the speculation[2] which I some time since uttered respecting that view of the nature of matter which considers its ultimate atoms as centres of force, and not as so many little bodies surrounded by forces, the bodies being considered in the abstract as independent of the forces and capable of existing without them. In the latter view, these little particles have a definite form and a certain limited size; in the former view such is not the case, for that which represents size may be considered as extending to any distance to which the lines of force of the particle extend: the particle indeed is supposed to exist only by these forces, and where they are it is. The consideration of matter under this view gradually led me to look at the lines of force as being perhaps the seat of the vibrations of radiant phænomena.

Another consideration bearing conjointly on the hypothetical view both of matter and radiation, arises from the comparison

[1] Philosophical Magazine, S. 3, vol. xxviii. No. 188, May 1846.

[2] Philosophical Magazine, 1844, vol. xxiv. p. 136; or Exp. Res. ii. 284.

of the velocities with which the radiant action and certain powers of matter are transmitted. The velocity of light through space is about 190,000 miles in a second; the velocity of electricity is, by the experiments of Wheatstone, shown to be as great as this, if not greater: the light is supposed to be transmitted by vibrations through an æther which is, so to speak, destitute of gravitation, but infinite in elasticity; the electricity is transmitted through a small metallic wire, and is often viewed as transmitted by vibrations also. That the electric transference depends on the forces or powers of the matter of the wire can hardly be doubted, when we consider the different conductibility of the various metallic and other bodies; the means of affecting it by heat or cold; the way in which conducting bodies by combination enter into the constitution of non-conducting substances, and the contrary; and the actual existence of one elementary body, carbon, both in the conducting and non-conducting state. The power of electric conduction (being a transmission of force equal in velocity to that of light) appears to be tied up in and dependent upon the properties of the matter, and is, as it were, existent in them.

I suppose we may compare together the matter of the æther and ordinary matter (as, for instance, the copper of the wire through which the electricity is conducted), and consider them as alike in their essential constitution; *i. e.* either as both composed of little nuclei, considered in the abstract as matter, and of force or power associated with these nuclei, or else both consisting of mere centres of force, according to Boscovich's theory and the view put forth in my speculation; for there is no reason to assume that the nuclei are more requisite in the one case than in the other. It is true that the copper gravitates and the æther does not, and that therefore the copper is ponderable and the æther is not; but that cannot indicate the presence of nuclei in the copper more than in the æther, for of all the powers of matter gravitation is the one in which the force extends to the greatest possible distance from the supposed nucleus, being infinite in relation to the size of the latter, and reducing that nucleus to a mere centre of force. The smallest atom of matter on the earth acts directly on the smallest atom of matter in the sun, though they are 95,000,000 of miles apart; further, atoms which, to our knowledge, are at least nineteen times that distance, and indeed, in cometary masses, far more, are in a similar way tied together by the lines of force extending from and belonging to each. What is there in the condition of the particles of the supposed æther, if there be even only *one* such particle between us and the sun, that can in subtilty and extent compare to this?

Let us not be confused by the *ponderability* and *gravitation* of heavy matter, as if they proved the presence of the abstract nuclei; these are due not to the nuclei, but to the force superadded to them, if the nuclei exist at all; and, if the *æther* particles be without this force, which according to the assumption is the case, then they are more material, in the abstract sense, than the matter of this our globe; for matter, according to the assumption, being made up of nuclei and force, the æther particles have in this respect proportionately more of the nucleus and less of the force.

On the other hand, the infinite elasticity assumed as belonging to the particles of the æther, is as striking and positive a force of it as gravity is of ponderable particles, and produces in its way effects as great; in witness whereof we have all the varieties of radiant agency as exhibited in luminous, calorific, and actinic phænomena.

Perhaps I am in error in thinking the idea generally formed of the æther is that its nuclei are almost infinitely small, and that such force as it has, namely its elasticity, is almost infinitely intense. But if such be the received notion, what then is left in the æther but force or centres of force? As gravitation and solidity do not belong to it, perhaps many may admit this conclusion; but what are gravitation and solidity? certainly not the weight and contact of the abstract nuclei. The one is the consequence of an *attractive* force, which can act at distances as great as the mind of man can estimate or conceive; and the other is the consequence of a *repulsive* force, which forbids for ever the contact or touch of any two nuclei; so that these powers or properties should not in any degree lead those persons who conceive of the æther as a thing consisting of force only, to think any otherwise of ponderable matter, except that it has more and other *forces* associated with it than the æther has.

In experimental philosophy we can, by the phænomena presented, recognize various kinds of lines of force; thus there are the lines of gravitating force, those of electro-static induction, those of magnetic action, and others partaking of a dynamic character might be perhaps included. The lines of electric and magnetic action are by many considered as exerted through space like the lines of gravitating force. For my own part, I incline to believe that when there are intervening particles of matter (being themselves only centres of force), they take part in carrying on the force through the line, but that when there are none, the line proceeds through space[1]. Whatever the view adopted respecting them may be, we can, at all events, affect these lines of force in a manner which may be conceived as partaking of the nature of a shake or lateral vibration. For suppose two bodies, A B, distant from each other and under mutual action, and therefore connected by lines of force, and let us fix our attention upon one resultant of force having an invariable direction as regards space; if one of the bodies move in the least degree right or left, or if its power be shifted for a moment within the mass (neither of these cases being difficult to realize if A and B be either electric or magnetic bodies), then an effect equivalent to a lateral disturbance will take place in the resultant upon which we are fixing our attention; for, either it will increase in force whilst the neighbouring resultants are diminishing, or it will fall in force as they are increasing.

It may be asked, what lines of force are there in nature which are fitted to convey such an action and supply for the vibrating theory the place of the æther? I do not pretend to answer this question with any confidence; all I can say is, that I do not perceive in any part of space, whether (to use the common phrase) vacant or filled with matter, anything but forces and the lines in which they are exerted. The lines of weight or gravitating force are, certainly, extensive enough to answer in this respect any demand made upon them by radiant phænomena; and so, probably, are the lines of magnetic force: and then who can forget that Mossotti has shown that gravitation, aggregation, electric force, and electro-chemical action may all have one common connection or origin; and so, in their actions at a distance, may have in common that infinite scope which some of these actions are known to possess?

[1] Experimental Researches in Electricity, pars. 1161, 1613, 1663, 1710, 1729, 1735, 2443.

The view which I am so bold as to put forth considers, therefore, radiation as a high species of vibration in the lines of force which are known to connect particles and also masses of matter together. It endeavours to dismiss the æther, but not the vibrations. The kind of vibration which, I believe, can alone account for the wonderful, varied, and beautiful phænomena of polarization, is not the same as that which occurs on the surface of disturbed water, or the waves of sound in gases or liquids, for the vibrations in these cases are direct, or to and from the centre of action, whereas the former are lateral. It seems to me, that the resultant of two or more lines of force is in an apt condition for that action which may be considered as equivalent to a *lateral* vibration; whereas a uniform medium, like the æther, does not appear apt, or more apt than air or water.

The occurrence of a change at one end of a line of force easily suggests a consequent change at the other. The propagation of light, and therefore probably of all radiant action, occupies *time*; and, that a vibration of the line of force should account for the phænomena of radiation, it is necessary that such vibration should occupy time also. I am not aware whether there are any data by which it has been, or could be ascertained whether such a power as gravitation acts without occupying time, or whether lines of force being already in existence, such a lateral disturbance of them at one end as I have suggested above, would require time, or must of necessity be felt instantly at the other end.

As to that condition of the lines of force which represents the assumed high elasticity of the æther, it cannot in this respect be deficient: the question here seems rather to be, whether the lines are sluggish enough in their action to render them equivalent to the æther in respect of the time known experimentally to be occupied in the transmission of radiant force.

The æther is assumed as pervading all bodies as well as space: in the view now set forth, it is the forces of the atomic centres which pervade (and make) all bodies, and also penetrate all space. As regards space, the difference is, that the æther presents successive parts or centres of action, and the present supposition only lines of action; as regards matter, the difference is, that the æther lies between the particles and so carries on the vibrations, whilst as respects the supposition, it is by the lines of force between the centres of the particles that the vibration is continued. As to the difference in intensity of action within matter under the two views, I suppose it will be very difficult to draw any conclusion, for when we take the simplest state of common matter and that which most nearly causes it to approximate to the condition of the æther, namely the state of rare gas, how soon do we find in its elasticity and the mutual repulsion of its particles, a departure from the law, that the action is inversely as the square of the distance!

And now, my dear Phillips, I must conclude. I do not think I should have allowed these notions to have escaped from me, had I not been led unawares, and without previous consideration, by the circumstances of the evening on which I had to appear suddenly and occupy the place of another. Now that I have put them on paper, I feel that I ought to have kept them much longer for study, consideration, and, perhaps, final rejection; and it is only because they are sure to go abroad in one way or another, in consequence of their utterance on that evening, that I give them a shape, if shape it may be called, in this reply to your inquiry. One thing is certain, that any hypothetical view of radiation which is likely to be received or retained as satisfactory, must not much longer comprehend alone certain phænomena of light, but must include those of heat and of actinic influence also, and even the conjoined phænomena of sensible heat and chemical power produced by them. In this respect, a view, which is in some degree founded upon the ordinary forces of matter, may perhaps find a little consideration amongst the other views that will probably arise. I think it likely that I have made many mistakes in the preceding pages, for even to myself, my ideas on this point appear only as the shadow of a speculation, or as one of those impressions on the mind which are allowable for a time as guides to thought and research. He who labours in experimental inquiries knows how numerous these are, and how often their apparent fitness and beauty vanish before the progress and development of real natural truth.

I am, my dear Phillips,
Ever truly yours,
M. FARADAY.

Royal Institution,
April 15, 1846.

On the Magnetic Affection of Light, and on the Distinction between the Ferromagnetic and Diamagnetic Conditions of Matter[1].

When a ray of polarized light and lines of magnetic force pass simultaneously and parallel to each other through a transparent solid or liquid medium not possessing forces of double refraction, the ray is rotated according to a simple law of action, which I have expressed in the last part of the Philosophical Transactions[2] (2160. 2161.). When such a ray passes through certain specimens of rock-crystal, oil of turpentine, &c., it is also rotated according to a natural law well-known, without any reference to magnetic force. A very striking distinction exists between these two cases of rotation, though they at first appear to be the same; for the former rotation is dependent in its direction upon the lines of magnetic force, and not upon the position of the observer or the course of the ray of light, whereas the latter is dependent upon the position of the observer or the course of the ray.

Upon consideration it appeared that the peculiar character of the magnetic rotation might be made available in exalting the final effect of the magnetic force upon the ray, and also in demonstrating many important points in a more marked manner and higher degree than had yet been possible; and upon referring the idea to experiment, it was found to be true. The following pages contain some of the results.

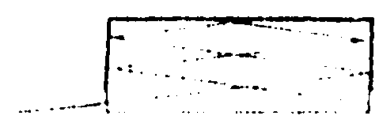
A parallelopiped of heavy glass 0·7 of an inch square and 2·5 inches long, had the two ends polished and silvered. The silvering was then removed from a space about 0·1 of an inch wide along one of the edges of one end, and also from a corresponding space on the other end, except that the parts cleared were on the contrary sides of the parallelopiped; so that each end was furnished with a good plane reflector, but these overlapped each other (fig. 2). In consequence of this arrangement, a ray of light could be transmitted diagonally across the length of the piece of glass; or the ray, after entering at one end, could be reflected two or more times within the glass and then allowed to pass out.

[1] Phil. Mag. Sept. and Oct. 1846.

[2] 1846, part i. pp. 4, 5. [Phil. Mag. vol. xxviii. pp. 298, 299.]

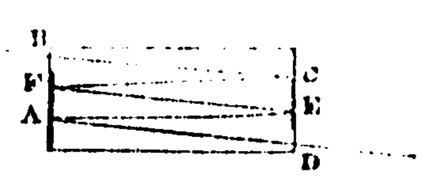
A similar piece of heavy glass was silvered at the two ends and one side of the prism; and the silvering was then removed at the ends for the space of 0·1 of an inch from those edges which were the furthest from the silvered side (fig. 1). A ray of light passing in at the unsilvered part of one end with a certain degree of obliquity, could be reflected at the other end, then at the side, and again at the first end, passing thus three times along the glass and finally out at the second end. At other inclinations the ray would pass five, seven, nine, eleven, or a greater number of times along the glass before it issued forth on its course through the air to the eye of the observer.

Fig. 1.



Either of these pieces of glass could produce the desired result of repeated reflexions within, but the first form was found most convenient in use. When a strong light was employed, it was not difficult to follow the series of images produced by successive reflexion up to the ninth or tenth image, these corresponding of course to a transit of the ray seventeen or nineteen times along the substance of the glass. A little change of position of the silvered glass between the Nichol's prisms used as the polarising and analysing apparatus, was sufficient to bring any one of these images into view, the glass being at the same time under the full influence of the electro-magnet, or the helix, employed to generate lines of magnetic force. A further advantage is gained if the ends of the piece of glass are not quite parallel to each other, the sides proceeding from the edges where the ray enters and issues forth being in a very slight degree different in length. This arrangement causes the series of reflected images to open out if seen at one end and to close up if seen at the other, and thus the observation of a particular image or the simultaneous comparison of two or more images, is favoured.

Fig. 2.



On considering the effect of this arrangement, it is evident that if ABCD represent a trough of solution of sugar, or any other body having the ordinary rotating influence over a polarized ray, then a ray sent in at D and passing out at A would be rotated to a certain amount. But if, instead of proceeding onwards at A, it were reflected by the surface A F to E, and were there observed, it would be found to have received no rotation, for the effect produced in going from D to A would be exactly compensated by its return from A to E. Or if the reflexions were made more numerous and recurred at E F and C, so that the ray should traverse the body five times, still an amount of rotation equal only to that which its passage once along the substance could effect would be finally produced.

Such would not be the case if A B C D were a diamagnetic, rotating the ray by means of magnetic force; for then, whichever way the ray was passing, it would still be rotated in the *same* direction in relation to the lines of force. So if observed issuing forth at A, it would have an amount of rotation (which we may call right-handed) equal to what one transit across the diamagnetic could produce; if observed at E, it would have an amount of left-handed rotation double the amount of the first or unit quantity; if observed at F, it would have three times the first amount of right-handed rotation; if observed at C, four times the amount of left-handed rotation; and at B would possess five times the original amount of right-handed rotation.

This was confirmed by the result of an experiment. The great magnet described in the Philosophical Transactions[1] (2247.) was employed, and the parallelopiped of glass (fig. 2) submitted to its powers; the direct ray, or that producing the first image of the luminous object, acquired a right-handed rotation equal to 12°. Moving the glass a little the second image was brought into view, or that produced by the ray which had traversed the glass three times, and its rotation was 36°. The third image was then observed, and the rotation of the ray producing it was 60°, as nearly as my rough apparatus could measure angular quantities. The same general results were obtained with the second piece of glass described.

The experiment proves in a very striking manner, that whichever way the ray of light is passing through the interposed body, the direction of its rotation depends essentially and alone upon the direction of the lines of magnetic force.

It also proves and makes manifest in a manner not to be mis-

[1] 1846, p. 22. Phil. Mag. vol. xxviii. p. 398.

taken, the difference in this respect between the magnetic rotation of the ray and that produced by quartz, sugar, oil of turpentine, and such bodies.

Either by independent or by conjoint observations of the different images, it proves that the effect is proportionate to the length of ray submitted to the magnetic force (2163.); for the unit length and multiples of the unit length may be observed at once, the intensity of the magnetic force and other circumstances remaining unchanged.

It permits the attainment of a far greater degree of accuracy in the measurement of the amount of rotation of a given ray, or in the estimation of the comparative degrees of rotation of the different coloured rays.

The form of the arrangement makes a short piece of any given diamagnetic, as a crystal, &c., sufficient for an experiment, which would not suffice if the ray were passed but once through it.

It allows of the concentration of the magnetic force by an approximation of the poles, when a magnet is used, so as to exalt the effect; or to render a weak magnet equivalent to a stronger one, so that even good ordinary magnets may now be made available. Or if a helix be used, a much shorter and weaker one than that which before was necessary, may now be employed.

Having ascertained the great advantage which this form of apparatus possessed for the examination of many substances which would give no sensible results by the process I formerly described, I proceeded to apply it to the cases of air and some doubly refracting bodies (2237.). For this purpose I made the faces of the magnetic poles reflectors, by applying to each a polished plate of steel; and as the poles were moveable, their reflecting surfaces could be placed at any distance and in any position required, the substance experimented on being between them.

Air.—I could obtain no signs of action upon the ray when air was between the magnetic poles, even with the fourth, fifth and sixth images.

Rock-crystal.—The cubes of this substance formerly described (2178.) were submitted to examination; but I could detect no trace of action on the ray of light when passing through them, although they were 0·75 of an inch in the side, and the ray was observed after passing seven, and even nine times across them. The cubes were examined in all directions.

Iceland Spar.—A cube of this substance (2179.) was examined in the same manner, but I obtained no effect.

Heavy glass presented the expected phænomena easily and well.

Failing to procure any positive result in these trials, either with air or with doubly refracting crystals, I silvered the latter in the manner that had been employed for the heavy glass, that the magnetic poles might be brought as close as possible; still no evidence of any magnetic action on the ray could be observed.

A natural six-sided prism of rock-crystal, 2·3 inches in length, was polished and silvered at the ends: no magnetic effect upon the light could be observed with this crystal with either the first, second or third image.

M. E. Becquerel thinks that he has observed an effect produced in doubly refracting crystalline bodies; and it is probable that his apparatus is far more delicate for the observance of optical changes than mine. In that case, if combined with the method founded on repeated transits of the ray, it perhaps would produce very distinct results; but the latter process alone has not as yet given any evidence of the action sought after.

Certain indications led me to look with interest for any possible effect which the crossing of the reflected rays might produce in the arrangement of reflectors and glass represented in fig. 1; but I could find no difference of action between it and the other arrangement, fig. 2, in which no such crossing occurred.

Near the close of last year I sent to the Royal Society two papers On the Magnetic Condition of all matter[1] (2243. 2343.), in which I believe that I proved the existence of a magnetic action new to our knowledge; antithetical in its nature to the magnetism manifested by iron in any of its forms or conditions, strong or weak, or to that magnetism which iron could, in any quantity or under any circumstances, produce. Further, that

[1] Philosophical Transactions, 1846, p. 21, or Phil. Mag. vol. xxviii. 1846.

all bodies not magnetic as iron, were magnetic according to this new mode of action; and that as *attraction* by the magnet marked the magnetic condition of iron, however small its quantity, or whatever its state might be, so *repulsion* was the distinctive characteristic of all those bodies which were naturally fitted to acquire the new state, and develope this new form of power.

M. Becquerel has sent a note to the Academy of Sciences[1], in which he states certain results of his own much anterior to mine, due to ordinary magnetic action, and in which the position of the substances was *across* the magnetic axis. I need not quote the whole, but will select the following words at the end:—"From these facts it results that the magnetic effects produced in steel or in soft iron by the influence of a magnet, differ from those which occur in all bodies, in this; that in the first the distribution of magnetism is always in the direction of the length, whilst in tritoxide of iron, wood, gumlac, &c., it occurs generally in a transverse direction, especially when they are formed into needles. This difference of effect is due to the circumstance, that the magnetism being very weak in these bodies, we may neglect the reaction of the particles of the bodies on themselves." These words, and the time of their publication (January 1846), sufficiently show that M. Becquerel does not admit the new form or condition of magnetic action, which I supposed I had previously demonstrated.

M. E. Becquerel, in a communication to the Academy of the date of June 1846[2], after confirming the action on light which I had announced, touches the question of magnetic action on all bodies; and at this late period is still of the same opinion as his father. I need quote only a few brief lines here and there to show this state of his mind. After speaking of Coulomb's results, and of his father's in 1827, he says, "These experiments have been made nearly twenty years, and nevertheless M. Faraday has announced anew the phænomenon of transverse direction, has made of the bodies which place themselves thus a new class of substances, and has named them diamagnetic substances. . . . . I ask myself how, under these circumstances, one can push the spirit of classification thus far, when one may give to the same substance all the va-

[1] *Comptes Rendus*, 1846, p. 147. [2] *Ibid.* 1846, p. 595.

rious positions of longitudinal, transverse or oblique. In fact, place the two extremities of two strong magnets opposite each other, at 0·08 or 0·12 of an inch apart, and at about 0·04 from their surface, suspend by means of cocoon silk, a small needle of wood or of copper, about 0·04 in diameter and 1·9 or 2·3 inches long, it will place itself transversely. If the needle be cut in two and again divided, &c., we shall end by having a fragment, which will place itself in the line of the poles. It is simply a phænomenon of the resultant forces; for we may give to one and the same substance these different positions, according to its form, by modifying the distance of the poles." Then, speaking of the small quantity of iron which he found in certain specimens, and of his former statement, that these substances behaved as a mixture of inert matter and magnetic particles, or as ferruginous mixtures, he says, "all these conclusions still subsist at this day in all their generality."

These conclusions from two profound philosophers, so well able in every respect to judge any question arising on such a subject as magnetism, made me anxious on two accounts; for first, I thought it possible I might really be in error respecting the broad and general principle of magnetic action, which I supposed I had discovered; and next, that if right on that point, I must have been sadly deficient in describing my results not to have conveyed a better impression to the minds of those so competent to receive and understand. I therefore, for my own sake, entered into an examination of this point; for though I am nearly indifferent to the fate of any speculative or hypothetical view I may venture to send forth, I am far from being so as regards the correctness of any announcement I may make of a law of action or a new fact; and having carefully experimented on one or two of the cases of transverse position assumed by certain bodies magnetic, as iron, I now give the general result.

Some good uniform peroxide of iron (being one of the substances which M. Becquerel experimented with) was prepared and introduced regularly into a thin glass tube, 0·25 of an inch in diameter and 1·4 inch long; it was then suspended by a long filament of cocoon silk, and could be brought into any position relative to one of the poles of a strong electro-magnet, which could be made to assume various forms by the use of terminal pieces of iron. As peroxide of iron can occasionally receive and retain a feeble magnetic state, it is necessary the experimenter should be aware of the possibility, and guard against its effect in producing irregular results.

The pole of the magnet was in the first place a cone, of which the base was 1·5 inch in diameter, its axis being in a horizontal line. The cylinder of peroxide of iron was advanced towards the cone with its centre of suspension in a line with the axis. When within the influence of the magnet, and transverse to the axial line, it retained that position; but this was a position of unstable equilibrium; for if the cylinder became oblique to the axial line on either side, then the end nearest to the cone approached towards it, being attracted, and at last went up to and remained against it. But whether directly across the axial line, and so in the position of unstable equilibrium, or in any other position, the centre of gravity of the whole was always *attracted*; an effect easily appreciated with an electro-magnet by interrupting and renewing the exciting current.

As a contrast with diamagnetic bodies, I will state, that if a similar cylinder of phosphorus, bismuth, or heavy glass be placed in precisely the same circumstances, then the transverse position is a position of *stable* equilibrium: if the cylinder be moved from it, it returns by vibrations into it; and during the whole time the centre of gravity of the mass is *repelled*.

A square end was now given to the pole of the magnet, the face opposed to the cylinder of peroxide being 1·75 inch wide and of an equal height. The axial line is that which passed horizontally from the middle of this face, and took its course through the centre of gravity of the cylinder, which was also its centre of rotation. When the cylinder was at any distance less than 0·3 of an inch from the face of the pole, it stood parallel to the face, and therefore transverse to the axial line: being moved out of this position, it resumed it, so that the position was one of stable equilibrium. At distances a little greater this became a position of unstable equilibrium, and two positions of stable equilibrium were found equally inclined on the opposite side of the transverse position, becoming more and more oblique to it as the distance was increased. Both the transverse positions and the oblique positions were easily referable to the concentration of the lines of magnetic force at the edges of the square end of the magnet. Effects due to the same cause have already been pointed out (2298. 2299. 2384.)[1].

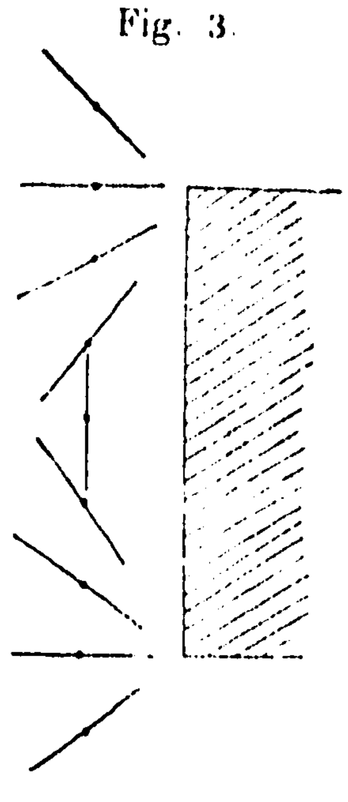
In every position of the cylinder of peroxide before this magnetic pole, the peroxide, as a mass, was *attracted.*

By using another termination of iron, the end of the pole opposite the peroxide was enlarged in its horizontal dimensions to 2·5 inches. All the former phænomena recurred; but the distance between the face of the pole and the tube of peroxide could be increased to one inch or more, before the tube began to assume the oblique positions.

A third termination presented a face having 3·5 inches of horizontal extension: the phænomena were here precisely the same, but the distance could be increased to 1·75 inch before the cylinder ceased to be parallel to the face, and began to acquire an oblique position.

For the complete comprehension of this and other effects due to the form of the face of the pole, and the concentration of the lines of magnetic force passing through the air near the edges, I will indicate the positions assumed by the cylinder of peroxide, when its centre of suspension was preserved at a constant distance from the face of the pole, but was carried into different positions on one side or the other of the axial line. These are represented in the figure; by which it will be seen, that as the peroxide was carried to one side or the other of the axial line it became inclined to that line, in a manner and to a degree easily comprehensible by those who consider the concentration of magnetic force at the edges of the face. The same was the case with the former face of 2·5 inches. Either end of the cylinder of peroxide might be the end nearest to the face of the pole; but the centre of gravity of the cylinder was in every case *attracted* by the magnet.

Fig. 3.



Other cylinders of peroxide of iron of different diameters and lengths were employed; and when they were smaller in length

[1] Philosophical Transactions, 1846, pp. 32, 48.

than the opposed faces of the magnetic poles, the results were precisely the same.

A cylinder of paper, magnetic through the presence of iron, was used and produced the same results.

Having been led to think that the disintegration of the peroxide of iron had considerable influence over these phænomena, by obstructing and preventing the communication of magnetic induction from particle to particle, and was far more influential than the mere weakness of magnetic force, I took some substances, even more weak in magnetic power than the peroxide, and formed them into cylinders. The substances were solution of proto-sulphate of iron, muriate of cobalt and muriate of nickel, which were introduced into thin glass tubes 0·25 of an inch in diameter and 1·4 inch long. These, when suspended before the poles with their centres of suspension in the axial line, did not act as the peroxide of iron or the paper. They could indeed be kept in a position parallel to the face of the pole, but this was a position of unstable equilibrium; and when the least removed out of that position, the end nearest the magnet continued to approach until it came in contact, and then the whole remained unchanged. The action was precisely like that of a piece of iron, but very inferior in energy.

A saturated solution of proto-sulphate of iron was diluted with five times its volume of water, and still showed precisely the same phænomena as the stronger solution: yet its magnetic power was very far inferior to the magnetic force of the peroxide of iron, as was fully manifested by the degree of the attraction of the centre of gravity of the two portions of the substances. When the peroxide was under experiment, the suspension thread was drawn twice or even three times further from the perpendicular than when the solution was used.

If a piece of iron wire be substituted for the cylinder of peroxide or the magnetic solutions, it will not remain parallel to the face of the pole, or oblique and *not* touching it; for one end will always go up to the face of the pole; or if it be very short, and then by loading, or otherwise be prevented from coming in contact with the pole, one end will point towards the part of the pole face nearest to it. In this respect it is as the magnetic solution, and not as the peroxide: however weak the magnetic pole may be, if it have power to affect the iron wire at all, it will produce the *same* effect. Further, if the iron wire be rendered perfectly free from magnetism, by making it red-hot, either end may be made that which is nearest to the face of the pole.

A piece of hæmatite, separated, not by an iron tool but by an agate, or otherwise in a careful manner, pointed in the manner of iron, though of course not with the same power, *i. e.* it did not take up a stable position either parallel to the face of the magnet or inclined, but not touching it; for one end or the other always went up to and remained in contact with the metal of the pole. The hæmatite, being powdered and put into a small tube, acted in the same way as when whole.

A piece of bottle-glass tube, which was magnetic from the iron it contained, acted as the hæmatite, either when whole or powdered up and put into a flint-glass tube: it therefore was unlike the peroxide of iron.

Pure peroxide of manganese appeared to take a place between these bodies and the peroxide of iron. Generally speaking, the end nearest to the flat face of the pole went up to it and remained there; but when one end was opposite the edge of the face and the other end nearly opposite the middle, at the time the magnet was made active, the latter end, though nearer than the former, would recede, and the former end come up and remain in contact. If the latter end was still nearer, it would approach from the first; and, there was no place of stable equilibrium for the tube in which it remained parallel to the face, or nearly so, with neither end touching it.

A piece of thick platinum wire acted as the hæmatite or green glass, and not as the peroxide in Becquerel's experiment. Spongy platina pressed into a tube acted as the peroxide of manganese, giving like it in certain situations, the beginnings of an action like that of the peroxide of iron.

The centre of gravity of all these bodies was attracted by the magnet, whatever part of the face they were placed in juxtaposition with. In no case was anything like a repulsion shown.

Now I do not see how any difficulty can arise in referring all these results of position to the ordinary action of magnets upon matter ordinarily magnetic, as iron is. All depends upon the shape and size of the poles, and upon the state of unity or disintegration, more or less complete, of the magnetic matter; for matter which is much weaker in magnetic power than peroxide of iron, as the solution of nickel, or dilute solution of iron, or the metal platina, does not act as the peroxide, but as metallic iron. Still, in every case, the phænomena are phænomena of *attraction*; for not only is the centre of gravity of the whole mass attracted, but the parts of the cylinder of peroxide of iron, as well as of the other magnetic substances, are in those positions which the resultants of attractive force would necessarily give them. This is precisely the reverse of what takes place with diamagnetic bodies, for there the centre of gravity of the whole mass is *repelled*; and whatever form the mass may have, its parts take those positions which are most consistent with the direction and degree of the *repulsive* force.

All ambiguity and doubt may be removed in such cases as the above by the use of a single pole, either conical, wedge-shaped or round. It is true, that if a wire of iron, two or three inches long, be placed with its middle part near the apex of a conical or wedge-shaped pole, it will stand at right angles across the axis of the cone or wedge, vibrating like a balance-beam; and also that if a cylinder of bismuth, phosphorus or heavy glass be placed in the same position, it will take a similar position. But no magnetician who looks at the effect can for a moment confound the phænomena; for he will see at once that the iron as a mass is attracted, and the diamagnetic is, as a mass, repelled: and then, if for this observation of the latter phænomena he will take small spheres of iron, peroxide of iron, or solution of muriate of iron on the one hand, and of bismuth or phosphorus, copper or wood on the other, he will have the phænomena in the simplest state, and the fundamental fact will be before him; for the magnetics will be attracted, the diamagnetics will be repelled.

I cannot find any case of transverse position which does not enter into one or other of the two kinds referred to above; that is to say, which is not either a magnetic or a diamagnetic result. Even as regards the effect of ordinary magnetism in peroxide of iron and such like bodies, I see no reason to accept the statement of M. Becquerel, that the distribution of magnetism tends to take place especially in a transverse direction. It appears to me that the destruction of the continuity of the mass in these cases, combined with the degree of magnetic force in the substance, prevents the transmission of the force by induction or conduction from particle to particle, to the same extent as when the continuity exists, and so consecutive poles at short distances and in different directions are produced; and hence the reason why the solution of iron or nickel, or the metal platina, does not behave as the peroxide of iron, though weaker in magnetic force, but acts as metallic iron.

If it had not been for the remarkable relation of a vacuum, and with it of those attenuated forms of matter, air, gases and vapours, which I have for this very reason amongst others insisted on in the Experimental Researches (2432. &c.), it might have become questionable whether those bodies which I have called diamagnetics, were not acted upon strictly in the same manner as magnetic bodies; and the result, whether of attraction or repulsion, a consequence of a difference of degree only between the body observed and the medium surrounding it (2438. &c.). But I cannot help looking upon a vacuum as presenting a zero point in the phænomena of attraction and repulsion; and as magnetic bodies are attracted, and diamagnetic bodies repelled (2406. 2436.) by a magnet, when surrounded by and in relation to it, so I believe that these two conditions represent two antithetical forms of magnetic force. This is the conclusion I have set forth in my original papers, and notwithstanding my very great respect for the judgement of MM. Becquerel, it is that which by the facts I am encouraged still to maintain[1].

[1] I take the liberty in this note to refer to a similar point in the philosophy of static electricity. I have often been asked for the proof of an absolute natural zero between positive and negative electricity; and in reference to M. Peltier's views, that the earth is negative to the space around it, which in its turn is positive, have been told that if all parts of a portion of its plane surface were equally negative, we on that surface could not tell that it was not in a zero state. But such is not the case. A surface which is truly negative may appear, in comparison, to be positive to one still more highly negative; or a negative surface may seem to be in a zero state in relation to two other surfaces, one of which is more negative and the other less so, or even positive; but if referred to a true standard its real state is shown at once, and this standard is given by the inside of any metallic vessel, from which, by its shape or depth, external influence is excluded. Such a vessel always presents the same normal condition within, whatever charge its external surface may have; and by comparing the surface of the earth with the inside of such a vessel, which is easily done by the use of carriers, such as Coulomb employed, any one may tell for himself whether that surface is in a negative or a zero state.

When heavy glass is submitted to the action of a powerful electro-magnet, the maximum degree of rotation of the ray is not obtained at once, but requires a sensible interval of time (Experimental Researches, 2170.); this I have attributed to the gradual rising in intensity of the force of the magnet, and the corresponding rising of the effect of that force in the glass. M. E. Becquerel does not agree with me in the above explanation, but thinks it is due to this; that the particles of the diamagnetic itself require time to assume their new state. That they may require time is, I think, very probable. I do not know any state the acquiring of which does not need time. This time is however most probably exceedingly small in the instance of diamagnetic bodies; and that the phænomena referred to by me are not due to such a cause, is, I think, shown by two considerations. The first is, that the electro-magnet is well known by other kinds of proof, as the induction of currents, &c., to require time to develope its maximum force due to a given electric current. I have shown that the rotation of the ray must increase during the time the magnet is thus rising; and I found that the power to induce currents exists simultaneously with the increasing rotation. The next consideration is, that when the diamagnetic is submitted, not to the action of an electro-magnet, but of a helix, the rotation does not increase in the same gradual manner as before, but is instantly at a maximum (Experimental Researches, 2195.): hence my reasons for adhering to the explanation I gave in the papers themselves last year.

But on subjects so new as these, differences of opinion must inevitably arise upon many points; and it is better for the inquiries themselves that it should be so, for the facts in consequence receive a more close investigation. I therefore leave many points of difference between myself and others unnoticed for the present; believing that new and connected facts will rapidly accumulate, and that time, with his powerful aid, will in a very few years give both facts and opinions their right places.

Royal Institution,
August 11, 1846.

On the Diamagnetic conditions of Flame and Gases[1].

To Richard Taylor, Esq.

My dear Sir,

I lately received a paper from Professor Zantedeschi, published by him, and containing an account of the discovery, by P. Bancalari, of the magnetism (diamagnetism) of flame, and of the further experiments of Zantedeschi, by which he confirms the result, and shows that flame is repelled from the axial line joining two magnetic poles. I send you the paper that you may, if you estimate its importance as highly as I do, reprint it in the Philosophical Magazine; and I send also with it these further experimental confirmations and extensions of my own. As M. Zantedeschi has published his results, I have felt myself at liberty to work on the subject, which of course interested me very closely. Probably what I may describe will only come in confirmation of that which has been done already in Italy or elsewhere; and if so, I hope to stand excused; for a second witness to an important fact is by no means superfluous, and may in the present case help to induce others to enter actively into the new line of investigation presented by diamagnetic bodies generally.

I soon verified the chief result of the diamagnetic affection of flame, and scarcely know how I could have failed to observe the effect years ago. As I suppose I have obtained much more striking evidence than that referred to in Zantedeschi's paper, I will describe the shape and arrangement of the essential parts of my apparatus. The electro-magnet used was the powerful one described in the Experimental Researches (2247.). The two terminal pieces of iron forming the virtual magnetic poles were each 1·7 inch square and six inches long; but the ends were shaped to a form approaching that of a cone, of which the sides have an angle of about 100°, and the axis of which is horizontal and in the upper surface of the pieces of iron. The apex of each end was rounded; nearly a tenth of an inch of the cone being in this way removed. When these terminations are brought near

[1] Philosophical Magazine, S. 3, vol. xxxi., No. 210, December 1847.

to each other, they give a powerful effect in the magnetic field, and the axial line of magnetic force is of course horizontal, and on a level nearly with the upper surface of the bars. I have found this form exceedingly advantageous in a great variety of experiments.

When the flame of a wax taper was held near the axial line, but on one side or the other, about one-third of the flame rising above the level of the upper surface of the poles, as soon as the magnetic force was on, the flame was affected; and receded from the axial line, moving equatorially, until it took an inclined position, as if a gentle wind was causing its deflection from the upright position; an effect which ceased the instant the magnetism was removed.

The effect was not instantaneous, but rose gradually to a maximum. It ceased very quickly when the magnetism was removed. The progressive increase is due to the gradual production of currents in the air about the magnetic field, which tend to be, and are, formed on the assumption of the magnetic conditions, in the presence of the flame.

When the flame was placed so as to rise truly across the magnetic axis, the effect of the magnetism was to compress the flame between the points of the poles, making it recede in the direction of the axial line from the poles towards the middle transverse plane, and also to shorten the top of the flame. At the same time the top and sides of the compressed part burnt more vividly, because of two streams of air which set in from the poles on each side directly against the flame, and then passed out with it in the equatorial direction. But there was at the same time a repulsion or recession of the parts of the flame from the axial line; for those portions which were below did not ascend so quickly as before, and in ascending they also passed off in an inclined and equatorial direction.

On raising the flame a little more, the effect of the magnetic force was to increase the intensity of the results just described, and the flame actually became of a fish-tail shape, disposed across the magnetic axis.

If the flame was raised until about two-thirds of it were above the level of the axial line, and the poles approached so near to each other (about 0·3 of an inch) that they began to cool and compress the part of the flame at the axial line, yet without interfering with its rising freely between them; then, on rendering the magnet active, the flame became more and more compressed and shortened; and as the effects proceeded to a maximum, the top at last descended, and the flame no more rose between the magnetic poles, but spread out right and left on each side of the axial line, producing a double flame with two long tongues. This flame was very bright along the upper extended forked edge, being there invigorated by a current of air which *descended* from between the poles on to the flame at this part, and in fact drove it away in the equatorial direction.

When the magnet was thrown out of action, the flame resumed its ordinary upright form between the poles, at once; being depressed and redivided again by the renewal of the magnetic action.

When a small flame, only about one-third of an inch high, was placed between the poles, the magnetic force instantly flattened it into an equatorial disc.

If a ball of cotton about the size of a nut be bound up by wire, soaked in æther and inflamed, it will give a flame six or seven inches high. This large flame rises freely and naturally between the poles; but as soon as the magnet is rendered active, it divides and passes off in two flames, the one on one side, and the other on the other side of the axial line.

Such therefore is the general and very striking effect which may be produced on a flame by magnetic action, the important discovery of which we owe to P. Bancalari.

I verified the results obtained by M. Zantedeschi with different flames, and found that those produced by alcohol, æther, coal-gas, hydrogen, sulphur, phosphorus, and camphor were all affected in the same manner, though not apparently with equal strength. The brightest flames appeared to be most affected.

The chief results may be shown in a manner in some respects still more striking and instructive than those obtained with flame, by using a smoking taper. A taper made of wax, coloured green by verdigris, if suffered to burn upright for a minute and then blown out, will usually leave a wick with a spark of fire on the top. The subdued combustion will however still go on, even for an hour or more, sending up a thin dense stream of smoke, which, in a quiet atmosphere, will rise vertically for six or eight inches; and in a moving atmosphere will show every change of its motion, both as to direction and intensity. When the taper is held beneath the poles, so that the stream of smoke passes a little on one side of the axial line, the stream is scarcely affected by the power of the magnet, the taper being three or four inches below the poles; but if the taper be raised, so that the coal is not more than an inch below the axial line, the stream of smoke is much more affected, being bent outwards; and if it be brought still higher, there is a point at which the smoke leaves the taper-wick even in a horizontal direction, to go equatorially. If the taper be held so that the smoke-stream passes *through* the axial line, and then the distances be varied as before, there is little or no sensible effect when the wick is four inches below: but being raised, as soon as the *warm* part of the stream is between the poles, it tends to divide; and when the ignited wick is about an inch below the axial line, the smoke rises vertically in one column until about two-thirds of that distance is passed over, and then it divides, going right and left, leaving the space between the poles clear. As the taper is slowly raised, the division of the smoke descends, taking place lower down, until it occurs upon the wick, at the distance of 0·4 or 0·5 of an inch below the axial line. If the taper be raised still more, the magnetic effect is so great, as not only to divide the stream, but to make it descend on each side of the ignited wick, producing a form resembling that of the letter W; and at the same time the top of the burning wick is greatly brightened by the stream of air that is impelled downwards upon it. In these experiments the magnetic poles should be about 0·25 of an inch apart.

A burning piece of amadou, or the end of a splinter of wood, produced the same effect.

By means of a like small spark and stream of smoke, I have even rendered evident the power of an ordinary magnet. The magnet was a good one, and the poles were close to each other and conical in form.

Before leaving this description of the general phænomenon and proceeding to a consideration of the principles of magnetic action concerned in it, I may say that a single pole of the magnet produces similar effects upon flame and smoke, but that they are much less striking and observable.

Though the effect be so manifest in a flame, it is not, at first sight, evident what is the chief cause or causes of the result. The *heat* of the flame is the most apparent and probable condition; but there are other circumstances which may be equally or more influential. Chemical action is going on at the time:—solid matter, which is known to be diamagnetic, exists in several of the flames used:—and a great difference exists between the matter of the flame and the surrounding air. Now any or all of these circumstances of temperature, chemical action, solidity of part of the matter, and differential composition in respect to the surrounding air, may concur in producing or influencing the result.

I placed the wires of an electrometer, and also of a galvanometer, in various parts of the affected flame, but could not procure any indications of the evolution of electricity by any action on the instruments.

I examined the neighbourhood of the axial line as to the existence of any current in the air when there was no flame or heat there, using the visible fumes produced when little pellets of paper dipped in strong solutions of ammonia and muriatic acid were held near each other; and though I found that a stream of such smoke was feebly affected by the magnetic power, yet I was satisfied there was no current or motion in the common air, as such, between the poles. The smoke itself was feebly diamagnetic; due, I believe, to the solid particles in it.

But when flame or a glowing taper is used, strong currents are, under favourable circumstances, produced in the air. If the flame be between the poles, these currents take their course along the surface of the poles, which they leave at the opposite faces connected by the axial line, and passing parallel to the axial line, impinge on the opposite sides of the flame; and feeding the flame, they make part of it, and proceed out equatorially. If the flame be driven asunder by the force of these currents and retreat, the currents follow it; and so, when the flame is forked, the air which is between the poles forms a current which sets from the poles downwards and sideways towards the flame. I do not mean that the air in *every* case travels along the surface of the poles or along the axial lines, or even from between the poles; for in the case of the glowing taper, held half an inch or so beneath the axial line, it is the cool air which is next nearest to the taper, and (generally) between the taper and the axial line, that falls with most force upon it. In fact the movements of the parts of the air and flame are due to a differential action. We shall see presently that the air is diamagnetic as well as flame or hot smoke; *i.e.* that both tend, according to the general law which I have expressed in the Experimental Researches (2267. &c.), to move from stronger to weaker places of magnetic force, but that hot air and flame are more so than cold or cooler air: so, when flame and air, or air at different temperatures, exist at the same time within a space under the influence of magnetic forces, differing in intensity of action, the hotter particles will tend to pass from stronger to weaker places of action, to be replaced by the colder particles; the former therefore will have the effect of being repelled; and the currents that are set up are produced by this action, combined with the mechanical force or current possessed by the flame in its ordinary relation to the atmosphere.

It will be evident to you that I have considered flame only as a particular case of a general law. It is a most important and beautiful one, and it has given us the discovery of diamagnetism in gaseous bodies: but it is a complicated one, as I shall now proceed to show, by analysing some of its conditions and separating their effects.

For the purpose of examining the effect of heat alone in conducing to the diamagnetic condition of flame, a small helix of fine platina wire was attached to two stronger wires of copper, so that the helix could be placed in any given position as regarded the magnetic poles, and at the same time be ignited at pleasure by a voltaic battery. In this manner it was substituted for the burning taper, and gave a beautiful highly-heated current of air, unchanged in its chemical condition. When the helix was placed directly under the axial line, the hot air rose up between the poles freely, being rendered evident above by a thermometer, or by burning the finger, or even scorching paper; but as soon as the magnet was rendered active, the hot air divided into a double stream, and was found ascending on the two sides of the axial line; but a descending current was formed between the poles, flowing downwards towards the helix and the hot air, which rose and passed off sideways from it.

It is therefore perfectly manifest that hot air is diamagnetic in relation to, or more diamagnetic than, cold air; and, from this fact I concluded, that, by cooling the air below the natural temperature, I should cause it to approach the magnetic axis, or appear to be magnetic in relation to ordinary air. I had a little apparatus made, in which a vertical tube delivering air was passed through a vessel containing a frigorific mixture; the latter being so clothed with flannel that the external air should not be cooled, and so invade the whole of the magnetic field. The central current of cold air was directed downwards a little on one side of the axial line, and falling into a tube containing a delicate air-thermometer, there showed its effect. On rendering the magnet active, this effect however ceased, and the thermometer rose; but on bringing the latter under the axial line it again fell, showing that the cold current of air had been drawn inwards or attracted towards the axial line, *i. e.* had been rendered magnetic in relation to air at common temperatures, or less diamagnetic than it. The lower temperature was 0° F. The effect was but small; still it was distinct.

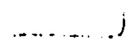
The effect of heat upon air, in so greatly increasing its diamagnetic condition, is very remarkable. It is not, I think, at all probable that the mere effect of expanding the air is the cause of the change in its condition, because one would be led to expect that a certain bulk of expanded air would be less sensible in its diamagnetic effects than an equal bulk of denser air; just as one would anticipate that a vacuum would present no magnetic or diamagnetic effects whatever, but be at the zero point between the two classes of bodies (Experimental Researches, 2423. 2424.). It is certainly true, that if the air were a body belonging to the magnetic class, then its expansion, being equivalent to dilution, would make it seem diamagnetic in relation to ordinary air (Experimental Researches, 2367. 2438.); but that, I think, is not likely to be the case, as will be seen by the results described further on in reference to oxygen and nitrogen.

If the power conferred by heat is a direct consequence, and proportionate to the temperature, then it gives a very remarkable character to gases and vapours, which, as we shall see hereafter, possess it in common. In my former experiments (Experimental Researches, 2359. 2397.) I heated various diamagnetic bodies, but could not perceive that their degree of magnetic force was at all increased or affected by the temperature given to them. I have again submitted small cylinders of copper and silver to the action of a single pole, at common temperatures and at a red heat, with the same result. If there was any effect of increased temperature, it was that of a very slight increase in the diamagnetic force, but I am not sure of the result. At present, therefore, the gaseous and vaporous bodies seem to be strikingly distinguished by the powerful effect which heat has in increasing their diamagnetic condition.

As all the experiments, whether on flame, smoke, or air, seemed to show that air had a distinct magnetic relation, which, though highly affected by temperature, still belonged to it at all temperatures; so it was a probable conclusion that other gaseous or vaporous bodies would be diamagnetic or magnetic, and that they would differ from each other even at common or equal temperatures. I proceeded therefore to examine them, delivering streams of each into the air, in the first instance, by fit apparatus and arrangements, and examining the course taken by these streams in passing across the magnetic field, the magnetic force being either induced or not at the time.

In delivering the various streams, I sometimes introduced the gases into a globe with a mouth and also a tubular spout, and then poured the gas out of the spout, upwards or downwards, according as it was lighter or heavier than air. At other times, as with muriatic acid or ammonia, I delivered the streams from the mouth of the retort. But as it is very important not to deluge the magnetic field with a quantity of invisible gas, I devised the following arrangement, which answered well for all the gases not soluble in water. A Woulf's bottle was chosen having three apertures at the top, *a*, *b* and *c*; a wide tube was fixed into aperture *a*, descending within the bottle to the bottom, and being open above and below; by this any water could be poured into the bottle and employed to displace the gas previously within it. Aperture *b* was closed by a stopper. Aperture *c* had an external tube, with a stop-cock fixed in it to conduct the gas to any place desired. To expel the gas and send it forward, a cistern of water was placed above the bottle, and its cock so plugged by a splinter of wood, that when full open it delivered only twelve cubic inches of fluid in a minute.

This stream of water being directed into aperture *a*, and the cock of tube *c* open, twelve cubic inches of any gas within the Woulf's bottle was delivered in a minute of time; and this I found an excellent proportion for our magnet and apparatus.

With respect to the delivery of this gas at the magnetic poles, a piece of glass tube bent into this shape  was held by a clamp on the stage of the magnet, so that it could easily be slipped backward and forward, or to one side, and so its vertical part be placed anywhere below the axial line. The aperture at this end was about the one-eighth of an inch internal diameter. In the horizontal part near the angle was placed a piece of bibulous paper, moistened with strong solution of muriatic acid (when necessary). The horizontal part of the tube was connected and disconnected in a moment, when necessary, with the tube *c* of the gas-bottle, by a short piece of vulcanized rubber tube. If the gas to be employed as a stream were heavier than the surrounding medium, then the glass tube, instead of having the form delineated above, was so bent as to deliver its stream downwards and over the axial line. In this manner currents of different gases could be delivered, perfectly steady and under perfect command.

The next point was to detect and trace the course of these streams. A little ammonia vapour, delivered near the magnetic field, did this in some degree, but was not satisfactory; for, in the first place, the little cloud of muriate of ammonia particles formed, is itself diamagnetic; and further, the tranquil condition of the air in the magnetic field was then too much disturbed. Catch-tubes were therefore arranged, consisting of tubes of thin glass about the size and length of a finger, open at both ends, and fixed upon little stands so that they could be adjusted either over or under the magnetic poles at pleasure. When they were over the poles, I generally had three at once; one over the axial line and one at each side. When they were under the poles, the lower end was turned up a little for the purpose of facilitating observation there.

The gas delivered at the poles, as already described, contained a little muriatic acid (obtained from the solution in the paper), but not enough to render it visible. To make it manifest up which catch-tube it passed, a little piece of bibulous paper, folded and bound round and suspended by a copper wire, was dipped in the solution of ammonia and hung in each of the tubes. It was then evident at once, by the visible fume formed at the top of one of the tubes, whether the gas delivered below passed up the one or the other tube, and which: and yet the gas was perfectly clear and transparent as it passed by the place of magnetic action.

In addition to these arrangements, I built up a sheltering chamber about the magnetic poles and field, to preserve the air undisturbed. This was about six inches long by four inches in width and height, and was easily made of thin plates of mica, which were put together or taken down in a moment. The chamber was frequently left more or less open at the top or bottom for the escape of gases, or the place of the catch-tubes. Its advantages were very great.

Air.—In the first place air was sent in under these arrangements, the stream being directed by the axial line. It made itself visible in the catch-tube above by the smoke produced; but whether the magnet was active or not, its course was the same; showing that, so far, the apparatus worked well, and did not of itself cause any erroneous indications.

Nitrogen.—This gas was sent from below upwards, and passed directly by the axial line into the catch-tube above; but when the magnet was made active, the stream was affected, and though not stopped in the middle catch-tube, part appeared in the side tubes. The jet was then arranged a little on one side of the axial line, so that, without the magnetic action, it still ascended and went up the middle catch-tube: then, when the magnetic action was brought on, it was clearly affected, and a great portion of it was sent to the side catch-tube. The nitrogen was, in fact, manifestly diamagnetic in relation to common air, when both were at the same temperature; but as four-fifths of the atmosphere consists of nitrogen, it seemed very evident, from the result, that nitrogen and oxygen must be very different from each other in their magnetic relations.

Oxygen.—A stream of oxygen was sent down through air between the poles. When there was no magnetic action it descended vertically, and when the magnetic action was on it appeared to do the same; at all events it did not pass off equatorially. But as there was reason, from the above experiments with nitrogen, to expect that oxygen would appear, not diamagnetic but magnetic in air; so the place of the stream was changed and made to be on one side of the axial line. In this case it fell perfectly well at first into a catch-tube placed beneath; but as soon as the magnet was rendered active, the stream was deflected, being drawn towards the axial line, and fell into another catch-tube placed there to receive it. So oxygen appears to be magnetic in common air. Whether it be really so, or only less diamagnetic than air (a mixture of oxygen and nitrogen), we shall be better able to consider hereafter.

Hydrogen.—This gas proved to be clearly and even strongly diamagnetic; for notwithstanding the powerful ascensive force which its stream has in the atmosphere, because of its small specific gravity, still it was well deflected and sent equatorially. Considering the lightness of the gas, one might have expected that it would have been drawn towards the axial line, as a stream of rarefied air (if it could exist) would be. Its diamagnetic state, therefore, shows in a striking point of view, that gases, like solids, have peculiar and distinctive degrees of diamagnetic force.

Carbonic acid.—This gas made a beautiful experiment. The stream was delivered downwards a little on one side of the axial line; a catch-tube was placed a little further out, so that the stream should fall clear of it as long as there was no activity in the magnet. But on rendering the magnet efficient, the stream left its vertical direction, passed equatorially, and fell into the catch-tube: and by looking horizontally, could be seen flowing out at its lower extremity like water, and falling away through the air. Again, the magnet was thrown out of action, and a glass with lime-water placed beneath the lower end of the catch-tube; no carbonic acid appeared there, though the fluid in the glass was continually stirred; but the instant the magnet was excited, the carbonic acid appeared in the catch-tube, fell into the glass and made the lime-water turbid. This gas therefore is diamagnetic in air.

Carbonic oxide.—This gas was carefully freed from carbonic acid before it was used. It was employed as a descending stream, and was apparently very diamagnetic: but it is to be remarked, that a substance which is so nearly the specific gravity of atmospheric air is easily dispersed right and left in it, and therefore that the facility of dispersion is not a correct indication of the diamagnetic force. By introducing a little ammonia into the mica chamber, it was, however, easily seen that carbonic oxide was driven away equatorially with considerable power; and I judge from the appearance, that it is more diamagnetic than carbonic acid.

Nitrous oxide.—This gas was moderately, but clearly, diamagnetic in air. Much interest belongs to this and the other compounds of nitrogen and oxygen, both because they contain the same elements as air, and because of the relations of nitrogen and oxygen separately.

Nitric oxide.—I tried this gas both as an up and down current, but could not determine its magnetic condition. What with the action of the oxygen of the air, the change of the nature of the substances, and the heat produced, there was so much incidental disturbance and so little effect due to magnetic influence, that I could not be sure of the result. On the whole it was very slightly diamagnetic; but so little, that the effect might be due to the smoke particles which served to render it visible.

Nitrous acid gas.—Difficult to observe, but I believe it is slightly magnetic in relation to air.

Olefiant gas was diamagnetic, and well so. The little difference in specific gravity of this gas and air, even creates a difficulty in following the course of the olefiant gas, unless it be watched for on every side.

Coal-gas.—The coal-gas of London is lighter than air, being only about two-thirds in weight of the latter. It is very well diamagnetic, and gives exceedingly good and distinct results.

Sulphurous acid gas is diamagnetic in air. It was generated in a small tube containing liquid sulphurous acid; this being connected, in place of the gas bottle, with the delivery-tube and mouthpiece by the vulcanized rubber tube. The presence or absence of the gas in the catch-tube was well shown by ammonia, and still better by litmus paper.

Muriatic acid.—The retort in which it was generated was connected, as just described, with the delivery-tube. The gas was very decidedly diamagnetic in air.

Hydriodic acid was also diamagnetic in air. When there was an abundant stream of gas, its entrance into and passage through the side catch-tube, on rendering the magnet active, was very striking. When there was less gas, the stream was dispersed equatorially in all directions, and less entered the tube.

Fluo-silicon.—Diamagnetic in air.

Ammonia.—This gas was evolved from materials in a retort, and tested in the catch-tube above by muriatic acid in the paper. It was well diamagnetic, corresponding in this respect with the character of its elements. It could also be very well indicated by reddened litmus paper held over the tubes.

Chlorine was sent from the Woulf's bottle apparatus, and proved to be decidedly diamagnetic in air. Either ammonia by its fumes, or litmus paper by its becoming bleached, served to indicate the entrance of the chlorine into the side catch-tube every time the magnet was rendered active.

Iodine.—A piece of glass tube was so shaped at its lower extremity as to form a chamber for the reception of iodine, which chamber had a prolonged mouth directed downwards so as to deliver the vapour formed within. On putting a little iodine into the chamber, then heating it, and especially the mouth part, by a spirit-lamp, and afterwards inclining the apparatus, abundance of the vapour of iodine was generated as the substance flowed on to the hotter parts, and passed in a good stream from the mouth downwards. This purple stream was diamagnetic in air, and could be seen flowing right and left from the axial line, when not too dense. If very dense and heavy, its gravity was such as to make it break through the axial line, notwithstanding the action of the magnet; still it was manifest that iodine is diamagnetic to air.

Bromine.—A little bromine was put into the horizontal part of the delivery-tube, and then air passed over it by the apparatus already described. So much bromine rose into vapour as to make the air of a yellow colour, and caused it to fall well in a stream by the axial line. A little ammonia delivered near the magnetic field showed that this stream was diamagnetic, and hence it may fairly be presumed that the pure vapour of bromine would be diamagnetic also.

Cyanogen.—Strongly diamagnetic in air.

Taking air as the standard of comparison, it is very striking to observe, that much as gases appear to differ one from another in the degree of their diamagnetic condition, there are very few that are not more diamagnetic than it; and when the investigation is carried forward into the relation of the two chief constituents of air, oxygen and nitrogen, it is still more striking to observe the very low condition of oxygen, which, in fact, is the cause of the comparatively low condition of air. Of all the vapours and gases yet tried, oxygen seems to be that which has the least diamagnetic force. It is as yet a question where it stands; for it may be as low as a vacuum, or may even pass to the magnetic side of it, and experiment does not as yet give an answer to the question. I believe it to be diamagnetic; and this belief is strengthened by the action of heat upon it, to be described hereafter; but it is exceedingly low in the scale, and far below chlorine, iodine, and such like bodies.

All the compounds of oxygen and nitrogen seem to show the influence of the presence of the oxygen. Nitrous acid seems to be less diamagnetic than air. Nitric oxide mingled with nitrous acid and warm, is about as air. Nitrous oxide is clearly diamagnetic in air, though it contains more oxygen: but it also contains more nitrogen than air, and is also denser than it, so that there is more matter present; still I think the results are in favour of the idea that oxygen is diamagnetic. By referring to the relation of carbonic oxide to carbonic acid, described further on, it will be seen that the addition of oxygen seems to make a body less diamagnetic. But the truth may be, not that oxygen is really magnetic, but that a compound body possesses a specific diamagnetic force, which is not the sum of the forces of its particles.

It is very difficult to form more than a mere guess at the relative degree of diamagnetic force possessed by different gaseous bodies when they are examined only in air, because of the many circumstances which tend to confuse the results. First, there is the invisibility of the gas which deprives us of the power of adjusting by sight so as to obtain the best effect: then, there is the difference of gravity; for if a gas ascend or descend in a rapid stream, it may seem less deflected than another flowing more slowly, though it be more diamagnetic; and as to gases nearly of the specific gravity of air, whether more or less diamagnetic, they are almost entirely dispersed in different directions, so that little only enters the catch-tube. Another modifying circumstance is the distance of the aperture delivering gas from the axial line, which, to obtain the maximum effect, ought to vary with the gravity of the gases and their diamagnetic force. Again, it is important that the magnetic field be not filled with the gas to be examined, and that generally speaking only a moderate stream be employed; which however must depend again upon the specific gravity.

The only correct way therefore of comparing two gases together is to experiment with them one in the other. For the experiments made with gases, in gases or in air, are differential, and similar in their nature with those made on a former occasion with solutions (Experimental Researches, 2362. &c.); I therefore changed the surrounding medium in a few experiments, substituting other gases for air; and first selected carbonic acid as a body easy to experiment with, and one that would, probably, be more powerfully than some other of the gases, diamagnetic (I speak as to the appearances or relative results only) in air.

I constructed a kind of tray or box, by folding up a doubled sheet of waxed paper; thus making a vessel 13 inches long, 5 inches wide, and 5 inches high. This was placed on the ends of the great magnet, and the terminal pieces of iron before described, placed in it. The box was covered over loosely by plates of mica, and formed a long square chamber in which were contained the magnetic poles and field. All the former arrangements in respect of the magnetic field, the delivery-tube, the catch-tubes, &c., were then made; and lastly, the box was filled with carbonic acid by a tube, which entered it at one corner; and was, from time to time, supplied with a fresh portion of gas, as the previous contents became diluted with gases or air. Everything answered perfectly, and the following results were easily obtained.

Air passed axially, being less diamagnetic than carbonic acid gas.

Oxygen passed to the magnetic axis, as was to be expected.

Nitrogen went equatorially, being therefore diamagnetic, even in carbonic acid.

Hydrogen, coal-gas, olefiant gas, muriatic acid and *ammonia* passed equatorially in carbonic acid, and were fairly diamagnetic in relation to it.

Carbonic oxide was very fairly diamagnetic in carbonic acid gas. Here the effect of oxygen seems to be very well illustrated. Equal volumes of carbonic oxide and carbonic acid contain equal quantities of carbon; but the former contains only half as much oxygen as the latter. Yet it is more diamagnetic than the latter; so, that, though an additional volume and quantity of oxygen, equal to that in the carbonic oxide, is in the carbonic acid added and compressed into it, it does not add to, but actually takes from, the diamagnetic force.

Nitrous oxide appears to be slightly diamagnetic in relation to carbonic acid; but nitric oxide gas was in the contrary relation and passed towards the axial line.

Hence it seems that carbonic acid, though more diamagnetic than air, is not far removed from it in that respect; and this position it probably holds because of the quantity of oxygen in it. The apparent place of nitrous oxide close to it appears, in a great measure, to depend on the same circumstance of oxygen entering largely into its composition. Still it is manifest that the action is not *directly* as the oxygen, for then common air would be more diamagnetic than either of them. It seems rather that the forces are modified, as in the cases also of iron and oxygen, and that each compound body has its peculiar but constant intensity of action.

In order to make similar experiments in light gases, the two terminal pieces of the magnet were raised, so that they might be covered by a French glass shade, which, with its stand, made a very good chamber about them. The pipe to supply and change the gaseous medium, and also that for bringing the gas under trial as a stream into the magnetic field, passed through holes made in the bottom of the stand. The different gases to be compared with those employed as media, were, except in the cases of ammonia and chlorine, mingled with a trace of muriatic acid, as before described. The gaseous media used were two, coal-gas and hydrogen. Whilst using coal-gas, I observed the direction of the currents of the other gases in it by bringing a little piece of paper, at the end of a wire and dipped in ammonia solution, near the stream. In the case of the hydrogen, I diffused a little ammonia through the whole of the gas in the first instance.

Air passed towards the axial line in coal-gas, but was not much affected.

Oxygen had the appearance of being strongly magnetic in coal-gas, passing with great impetuosity to the magnetic axis, and clinging about it; and if much muriate of ammonia fume were purposely formed at the time, it was carried by the oxygen to the magnetic field with such force as to hide the ends of the magnetic poles. If then the magnetic action were suspended for a moment, this cloud descended by its gravity; but being quite below the poles, if the magnet were again rendered active, the oxygen cloud immediately started up and took its former place. The attraction of iron filings to a magnetic pole is not more striking than the appearance presented by the oxygen under these circumstances.

Nitrogen.—Clearly diamagnetic in coal-gas.

Olefiant, carbonic oxide, and *carbonic acid* gases were all slightly, but more or less diamagnetic in the coal-gas.

On substituting hydrogen as the surrounding medium in place of coal-gas, more care was taken in the experiments. Each gas experimented upon was tried in it twice at least; first in the hydrogen of a previous experiment, and then in a new atmosphere of hydrogen.

Air.—Air passes axially in hydrogen when there is very little smoke in it: when there is much smoke in the stream the latter is either indifferent or tends to pass equatorially. I believe that air and hydrogen cannot be far from each other.

Nitrogen is strikingly diamagnetic in hydrogen.

Oxygen is as strikingly magnetic in relation to hydrogen. It presented the appearances already described as occurring in coal-gas; but as the jet delivered the descending stream of oxygen a little on one side of the axial line, its centrifugal power, in relation to the axial line, was so balanced by the centripetal power produced by the magnetic action, that the stream at first revolved in a regular ring round the axial line, and produced a cloud that continued to spin round it as long as the magnetic force was continued, but fell down to the bottom of the chamber when that force was removed.

Nitrous oxide.—This gas was clearly diamagnetic in the hydrogen, and gave rise to a very beautiful result in consequence of its following the oxygen; for at the beginning of the experiment, the little oxygen contained in the conducting tube passed axially; but the instant that was expelled, and the nitrous oxide issued forth, the stream changed its direction, and passed off diamagnetically in the most striking manner.

Nitric oxide.—This gas passed axially in hydrogen, and therefore is magnetic in relation to it.

Ammonia.—Diamagnetic in hydrogen.'

Carbonic oxide, carbonic acid, and *olefiant gases* were diamagnetic in hydrogen; the last most so, and the carbonic acid apparently the least.

Chlorine was slightly diamagnetic in hydrogen. It was clearly so; but the cloudy particles might conduce much to the small effect produced.

Muriatic acid gas.—I think it was a little diamagnetic in the hydrogen.

Notwithstanding the many disturbing causes which interfere with first and hasty experiments of this kind, and produce results which occasionally cross and contradict each other, still there are some very striking considerations which arise in comparing the gases with each other at the same temperature. Foremost amongst these is the place of oxygen; for of all the gaseous bodies yet tried it is the least diamagnetic, and seems in this respect to stand far apart from the rest of them. The condition of nitrogen, as being highly diamagnetic, is also important. The place of hydrogen, as being less diamagnetic than nitrogen, and of chlorine, which, instead of approaching to oxygen, is above hydrogen, and also of iodine, which is probably far above chlorine, are marked circumstances.

Air of course owes its place to the proportion and the individual diamagnetic character of the oxygen and nitrogen in it. The great difference existing between these two bodies in respect of magnetic relation, and the striking effect presented by oxygen in coal-gas and hydrogen, bodies not far removed from nitrogen in diamagnetic force, made me think it might not be impossible to separate air into its two chief constituents by magnetic force alone. I made an experiment for this purpose, but did not succeed; but I am not convinced that it cannot be done. For since we can actually distinguish certain gases, and especially these two by their magnetic properties, it does not seem impossible that sufficient power might cause their separation from a state of mixture.

In the course of these experiments I subjected several of the gases to heat, to ascertain whether they generally underwent the same exaltation of their diamagnetic power which occurred with common air (2854.). For this purpose a helix of platina wire was placed in the mouth of the delivering tube, which itself was placed below the magnetic axis between the poles. The helix could be raised to any temperature by a little voltaic battery, and any gas could be sent through it and upwards across the magnetic field by means of the Woulf's bottle apparatus already described. It was easy to ascertain whether the gas went directly up between the poles, or, when the magnet was active, left that direction and formed two equatorial side-streams, either by the sensation on the finger, or by a thermoscope formed of a spiral compound lamina of platinum and silver placed in a tube above. In every case the hot gas was diamagnetic in the air, and I think far more so than if the gas had been at common temperatures. The gases tried were as follows: oxygen, nitrogen, hydrogen, nitrous oxide, carbonic acid, muriatic acid, ammonia, coal-gas, olefiant gas.

But as in these experiments the surrounding air would, of necessity, mingle with the gas first heated, and so form, in fact, a part of the heated stream, I arranged the platinum helix so that I could heat it in a given gas, and thus compare the same gas at different temperatures with itself.

A stream of hot oxygen in cold oxygen was powerfully diamagnetic. The effect and its degree may be judged of by the following circumstances. When the platinum helix below the axial line was ignited, the effect of heat on the indicating compound spiral, placed in a tube over the axial line, was such as to cause its lower extremity to pass through one and a half revolutions, or 540°: when the magnetic force was rendered active, the spiral returned through all these degrees to its first position, as if the ignited helix below had been lowered to the common temperature or taken away; and yet in respect of it, nothing had been changed. On rendering the magnet inactive, the current of hot oxygen instantly resumed its perpendicular course and affected the thermoscope as before.

On experimenting with carbonic acid, it was found that hot carbonic acid was diamagnetic to cold carbonic acid; and the effects were apparently as great in amount as in oxygen.

On making the same arrangement in hydrogen, I failed to obtain any result regarding the relation of the hot and cold gas, for this reason :—that I could not, in any case, either with or without the magnetic action, obtain any signs of heat on the thermoscopic spiral above, even when the platinum helix, not more than an inch below it, was nearly white hot. This effect is, I think, greatly dependent upon the rapidity with which hydrogen is heated and cooled in comparison with other gases, and also upon the vicinity of the cold masses of iron forming the magnetic poles, between which the hot gas has to pass in its way upwards: and it is most probably connected with the fact observed by Mr. Grove of the difficulty of igniting a platinum wire in hydrogen.

When the igniting helix was placed in coal-gas, it was found that the hot gas was diamagnetic to that which was cold; as in all the other cases. Here, again, an effect like that which was observed in hydrogen occurred; for when there was no magnetic action, the ascending stream of hot coal-gas could cause the thermoscopic spiral to revolve through only 280° or 300°, in place of above 540°; through which it could pass when the surrounding gas was oxygen, air, or carbonic acid; and that even when the helix was at a higher temperature in the coal-gas than in any of these gases.

The proof is clear then that oxygen, carbonic acid, and coal-gas, are more diamagnetic hot than cold. The same is the case with air; and as air consists of four-fifths nitrogen and only one-fifth oxygen, and yet shows an effect of this kind as strongly as oxygen, it is manifest that nitrogen also has the same relation when hot and cold.

Of the other gases also I have no doubt; though to be quite certain, they ought to be tried in atmospheres of their own substance (2854.), or else in gases more diamagnetic at common temperatures than they. The olefiant and coal-gases in air easily bore the elevation of the helix to a full red heat, without inflaming when out of the exit-tube: the hydrogen required that the helix should be at a lower temperature. Muriatic acid and ammonia showed the division of the one stream into two, very beautifully, on holding blue and red litmus paper above.

There is another mode of observing the diamagnetic condition of flame, and experimenting with the various gases, which is sometimes useful, and should always be understood, lest it inadvertently might lead to confusion. I have a pair of terminal magnetic poles which are pierced in a horizontal direction, that a ray of light may pass through them. The opposed faces of these vertical poles are not, as in the former case, the rounded ends of cones; but, though rounded at the edges, may be considered as flat over an extent of surface an inch in diameter. The pierced passages are in the form of cones, the truncation of which in this flat surface is rather more than half an inch in diameter. When these poles were in their place, and from 0·3 to 0·4 of an inch apart, a taper flame, burning freely between them, was for a few moments unaffected by throwing the magnet into action; but then it suddenly changed its form, and extending itself axially, threw off two horizontal tongues, which entered the passages in the poles; and thus it continued as long as the magnetism continued, and no part of it passed equatorially.

On using a large flame made with the cotton ball and æther, two arms could be thrown off from the flame by the force of the magnetism, which passed in an equatorial direction, as before; and other two parts entered the passages in the magnetic poles, and actually issued out occasionally at their further extremities.

When the poles were about 0·25 of an inch apart, and the smoking taper was placed in the middle between them level with the centres of the passages, the effect was very good; for the smoke passed axially and issued out at the further ends of the pole passages.

Coal-gas delivered in the same place also passed axially, *i. e.* into the pole passages and parallel to the line joining them.

A little consideration easily leads to the true cause of these effects, and shows that they are not inconsistent with the former results. The law of all these actions is, that if a particle, placed amongst other particles, be more diamagnetic (or less magnetic) than they, and free to move, it will go from strong to weaker places of magnetic action; also, that particles less diamagnetic will go from weaker to stronger places of action. Now with the poles just described, the line or lines of maximum force are not coincident with the axis of the holes pierced in the poles, but lie in a circle having a diameter, probably, a little larger than the diameter of the holes; and the lines within that circle will be of lesser power, diminishing in force towards the centre. A hot particle therefore within that circle will be driven inwards, and being urged by successive portions of matter driven also inwards, will find its way out at the other ends of the passages, and therefore seem to go in an axial direction; whilst a hot particle outside of that circle of lines of maximum force will be driven outwards, and so, with others, will form the two tongues of flame which pass off in the equatorial direction. By bringing the glowing taper to different parts, the circle of lines of maximum magnetic intensity can be very beautifully traced; and by placing the taper inside or outside of that circle, the smoke could be made to pass axially or equatorially at pleasure.

I arranged an apparatus on this principle for trying the gases, but did not find it better than, or so good as, the one I have described.

Such are the results I have obtained in verifying and extending the discovery made by P. Bancalari. I would have pursued them much further, but my present state of health will not permit it: I therefore send them to you with, probably, many imperfections. It is now almost proved that many gaseous bodies are diamagnetic in their relations, and probably all will be found to be so. I say almost proved; for it is not, as yet, proved in fact. That many, and most, gaseous bodies are subject to magnetic force is proved; but the zero is not yet distinguished. Now, until it is distinguished, we cannot tell which gaseous bodies will rank as diamagnetic and which as magnetic; and also, whether there may not be some standing at zero. There is evidently no natural impossibility to some gases or vapours being magnetic, or that some should be neither magnetic nor diamagnetic. It is the province of experiment to decide such points; and the affirmative or negative may not be asserted before such proof is given, though it may, very philosophically, be believed.

For myself I have always believed that the zero was represented by a vacuum, and that no body really stood with it. But though I have only guarded myself from asserting more than I knew, Zantedeschi (and I think also De la Rive), with some others, seem to think that I have asserted the gases are *not* subject to magnetic action; whereas I only wished to say that I could not find that they were, and perhaps were not: I will therefore quote a few of my words from the Experimental Researches. Speaking of the preparation of a liquid medium at zero, I say, "Thus a *fluid* medium was obtained, which practically, as far as I could perceive, had every magnetic character and effect of a gas, *and even of a vacuum*, &c."—Experimental Researches, 2423. Again, at (2433.) I say, "At one time I looked to air and gases as the bodies which, allowing attenuation of their substance without addition, would permit of the observation of corresponding variations in their magnetic properties, but now all such power by rarefaction *appears* to be taken away." And further down at (2435.), "Whether the negative results obtained by the use of gases and vapours depend upon the *smaller quantity of matter* in a given volume, or whether they are the direct consequences of the altered physical condition of the substance, is a point of very great importance to the theory of magnetism. I have imagined in elucidation of the subject an experiment, &c., but expect to find great difficulty in carrying it into execution, &c." Happily P. Bancalari's discovery has now settled this matter for us in a most satisfactory manner. But where the true zero is, or that every body is more or less removed from it on one side or the other, is not, as yet, experimentally shown or proved.

I cannot conclude this letter without expressing a hope that since gases are shown to be magnetically affected, they will also shortly be found, when under magnetic influence, to have the power of affecting light (Experimental Researches, 2186. 2212.). Neither can I refrain from signalizing the very remarkable and direct relation between the forces of heat and magnetism which is presented in the experiments on flame, and heated air and gases. I did not find on a former occasion (Experimental Researches, 2397.) that solid diamagnetic bodies were sensibly affected by heat, but shall repeat the experiments and make more extensive ones, if the Italian philosophers have not already done so. In reference to the effect upon the diamagnetic gases, it may be observed, that, speaking generally, it is in the same direction as that of heat upon iron, nickel and cobalt; *i. e.* heat tends in the two sets of cases, either to the diminution of magnetic force, or the increase of diamagnetic force; but the results are too few to allow of any general conclusion as yet.

As air at different temperatures has different diamagnetic relations, and as the atmosphere is at different temperatures in the upper and lower strata, such conditions may have some general influence and effect upon its final motion and action, subject as it is continually to the magnetic influence of the earth.

I have for the sake of brevity frequently spoken in this letter of bodies as being magnetic or diamagnetic in relation one to another, but I trust that in all the cases no mistake of my meaning could arise from such use of the terms, or any vague notion arise respecting the clear distinction between the two classes, especially as my view of the true zero has been given only a page or two back.

I am, my dear Sir,
Yours, &c.,
M. Faraday.

Richard Taylor, Esq.,
Ed. Phil. Mag., &c. &c.

On the Motions presented by Flame when under the Electro-magnetic Influence. By Prof. Zantedeschi.

The most eminent philosophers have at all times maintained the universality of the magnetism of bodies[1]; and in our days Faraday is the only one who has placed the expansible fluids at the zero of the scale of action among magnetic and diamagnetic bodies. On the 21st of September 1847, at the Physical Section of the Ninth Italian Scientific Congress in Venice, Padre Bancalari, Professor of Physics in the Royal University of Genoa, read a memoir on the universality of magnetism; and the argument was considered by philosophers to be of such importance, that a desire arose to verify chiefly the action of magnetism on expansible fluids. It was announced by the Reporter Belli at the sitting of the 27th of September, that it had been proved in the presence of various philosophers that, on the interposition of a flame between the two poles of an electro-magnet, it was repulsed at the instant the electric current was closed, to return to the first position the instant it was broken. This discovery received well-merited applause in the sitting of the 28th of September, from the General Secretary and the Secretary of the Section of Physics. A wish was expressed by some to witness the experiment of Bancalari; and a Daniell's apparatus having been got ready, of ten elements eighteen centimeters each in dimension, I endeavoured to repeat the experiment in the Cabinet

[1] *Raccolta Fisico-Chimica Italiana*, t. iii. Dei corpi magnetici e diamagnetici.

of Physics of the Royal Imperial Lyceum of Venice; but I did not chance to see the asserted phænomenon. My temporary magnet had the power of sustaining above 48 kilogrms. weight; but as my principle is, that a negative argument never destroys a positive one, I for my further information requested the machinist Cobres to give me the particulars of the apparatus; Belli not having treated of these in his report, and they having escaped Prof. Zambra, the Secretary of the Section. I knew that the two pieces of soft iron, which constituted the interrupted anchor, were perforated in the axial direction. I suspected that the repulsion of the flame was not the immediate effect of the magnetism, but of two currents of air issuing from the apertures of the perforated keeper generated by a vorticose movement produced by the magnetism, as the celebrated Faraday had observed in liquids[1]; and I was confirmed in this suspicion by the negative experiment which I had instituted in Venice with solid pieces. On arriving in Turin, I communicated my doubts to the well-known mechanicians Jest, father and son, who to their professional abilities unite a rare courtesy. They soon furnished me in their laboratory with a Bunsen's apparatus, and constructed terminal pieces of soft iron forming the interrupted anchor, both solid and pierced, of a parallelopipedon and cylindric form, as I pointed out to them; and I have repeated the experiments in their company: the temporary magnet, made in the shape of a horseshoe, was formed of a cylinder of soft iron of the length of $0^{m}\cdot335$ and the diameter of $0^{m}\cdot015$; and its electro-magnetic spiral was formed of a copper wire 33^{m} long, and of a diameter of a millimeter and a third; the internal distance of the poles was $0^{m}\cdot027$; the two solid parallelopipedon contacts, forming the interrupted anchor, were $0^{m}\cdot04$ long; and of the sides $0^{m}\cdot011$ and $0^{m}\cdot006$: and the hollow terminal pieces were $0^{m}\cdot035$ long; and of the side $0^{m}\cdot009$. They were placed at a distance from one another of four to five millimeters, the magnet being kept in a vertical position with the poles turned upwards. In front of the interval of the separation of the contact pieces was placed the flame of a small candle, or of a little oil or alcohol lamp, so that it surmounted with its top by nearly a fourth the thickness of the contacts. The electric circuit was closed by copper wires, and the metallic unions

[1] *Raccolta*, cited above, t. ii. Relazione dell' influenza delle forze elettriche e magnetiche sulla luce ed il calorico.

were maintained both at the magnetic poles and at those of the pile by clamps: one of the wires therefore was divided into two equal parts, and the ends being dipped into a tumbler of mercury, allowed the closing and opening of the circuit at pleasure.

I have constantly observed repulsion in the act of closing the circle, which lasted the whole time that the magnetism was kept up; and, when in the act of opening the circle, I saw the flame return to its primitive position. Well satisfied with having in this manner confirmed this important fact which reflects honour on its discoverer, I applied myself to the study of the phænomenon, and I found—

I. *That this happens with contacts of both solid and hollow soft iron*; whereupon I abandoned my suspicion that the movement of the flame was attributable to currents of air; I convinced myself that it was an immediate action of the magnetism upon the flame,—a fact of the greatest importance to science.

II. *That the repulsion, when it is quite distinct and the flame quite pure, and terminated in a well-shaped top, is accompanied by depression*: repulsion and depression are simultaneously observed at the closing of the circle; the return of the flame and rising of the same, at the opening of the circle.

III. *That,* ceteris paribus, *the greatest effect takes place when the flame is touching the convex of the magnetic curves indicated by iron filings.*

IV. *That the action is null, or almost null, when the flame is placed in the centre of the interval which separates the two contacts.*

V. *That in the manifestation of the effects stated above, it is not necessary for the contacts to be entirely separated*: they may be placed at an angle and touch at two corners; the flame placed within the base of this triangle, generally manifests the two phænomena indicated.

VI. *That there is a certain mass of the contacts (or keeper pieces) which is the most efficacious: beyond a limit, which can be shown by experiment, increase of the mass causes a diminution of the effect*: from this I found the cause of my negative results, which I obtained in Venice in the first experiments that I made.

VII. *That the movements of the flame increase with the number of the pairs (of battery plates). With one pair the effect was* *not perceptible to me*[1]*: with two pairs the movements began to show themselves; with three pairs they became distinct, and increased with the increase of the number of pairs up to ten, which was the greatest that I employed in this experiment. The pairs were of the known ordinary size.*

On the repetition of the phænomena as above stated, the precaution was taken to cover the apparatus with a bell, which was open above and supported by two discs below, which left a free access to the air, by which to support the combustion: in this manner all agitation and danger of disturbance under the circumstances were avoided.

I must not forget, in concluding this article, to state that the celebrated Prof. Gazzaniga, starting from his numerous experiments, which demonstrate the influence of magnetism upon the same aëriform fluids, in a manner therefore different from that of Bancalari, was induced to consider the sun and all the other celestial bodies as so many enormous magnets; by which he established that attraction is merely an effect of the magnetism of the great celestial masses placed at an enormous distance,—an idea which reappeared in 1846 in Prussia, and in 1847 in France, as we see from the *Comptes Rendus* of the Royal Academy of Sciences at Paris. The mystery that attraction operates at a distance without intermedia would be removed in this case, and the phænomena of attraction would enter again into the class of those of common dynamics.

Dalla Gazz. Piem.,
Oct. 12, 1847, *No.* 242.

[1] Messrs. Jest prepared for me last evening an electro-magnet of a circular form interrupted by a prismatic section having an interval of two millimeters; and I had, without need of contact pieces, the phænomena distinct with a single element. The most conspicuous movements here appeared in the greater proximity of the flame to the section.

The complete apparatus, of a circular form, furnished with a glass bell with its accessories, is sold in Turin by Messrs. Jest, at the price of thirty francs, not including the electro-motor.

On the Use of Gutta Percha in Electrical Insulation[1].

MY DEAR PHILLIPS,

I HAVE lately found gutta percha very useful in electrical experiments; and therefore, that others may take advantage of its properties if they have occasion or are so inclined, give you this notice for insertion in the Philosophical Magazine. Its use depends upon the high insulating power which it possesses under ordinary conditions, and the manner in which it keeps this power in states of the atmosphere which make the surface of glass a good conductor. All gutta percha is not however equally good as it comes from the manufacturer's hands; but it does not seem difficult to bring it into the best state: I will describe the qualities of a proper specimen, and refer to the differences afterwards. A good piece of gutta percha will insulate as well as an equal piece of shell-lac, whether it be in the form of sheet, or rod, or filament; but being tough and flexible when cold, as well as soft when hot, it will serve better than shell-lac in many cases where the brittleness of the latter is an inconvenience. Thus it makes very good handles for carriers of electricity in experiments on induction, not being liable to fracture: in the form of thin band or string it makes an excellent insulating suspender: a piece of it in sheet makes a most convenient insulating basis for anything placed on it. It forms excellent insulating plugs for the stems of gold-leaf electrometers when they pass through sheltering tubes, and larger plugs supply good insulating feet for extemporary electrical arrangements: cylinders of it half an inch or more in diameter have great stiffness, and form excellent insulating pillars. In these and in many other ways its power as an insulator may be useful.

Because of its good insulation it is also an excellent substance for the excitement of negative electricity. It is hardly possible to take one of the soles sold by the shoemakers out of paper or into the hand, without exciting it to such a degree as to open the leaves of an electrometer one or more inches; or if it be unelectrified, the slightest passage over the hand or face, the clothes, or almost any other substance gives it an electric state. Some of the gutta percha is sold in very thin sheets, resembling

[1] Philosophical Magazine, March 1848, xxxii. 165.

in general appearance oiled silk; and if a strip of this be drawn through the fingers, it is so electric as to adhere to the hand or attract pieces of paper. The appearance is such as to suggest the making a thicker sheet of the substance into a plate electrical machine for the production of negative electricity.

Then as to inductive action through the substance, a sheet of it is soon converted into an excellent electrophorus; or it may be coated and used in place of a Leyden jar; or in any of the many other forms of apparatus dependent on inductive action.

I have said that all gutta percha is not in this good electrical condition. With respect to that which is not so (and which has constituted about one-half of that which, being obtained at the shops, has passed through my hands), it has either discharged an electrometer as a piece of paper or wood would do, or it has made it collapse greatly by touching, yet has on its removal been followed by a full opening of the leaves again: the latter effect I have been able to trace and refer to a conducting portion within the mass covered by a thin external non-conducting coat. When a piece which insulates well is cut, the surface exposed has a resinous lustre and a compact character that is very distinctive; whilst that which conducts has not the same degree of lustre, appears less translucent, and has more the aspect of a turbid solution solidified. I believe both moist steam-heat and water-baths are used in its preparation for commerce; and the difference of specimens depends probably upon the manner in which these are applied, and followed by the after process of rolling between hot cylinders. However, if a portion of that which conducts be warmed in a current of hot air, as over the glass of a low gas flame, and be stretched, doubled up, and kneaded for some time between the fingers, as if with the intention of dissipating the moisture within, it becomes as good an insulator as the best.

I have soaked a good piece in water for an hour; and on taking it out, wiping it, and exposing it to the air for a minute or two, found it insulate as well as ever. Another piece was soaked for four days and then wiped and tried: at first it was found lowered in insulating power; but after twelve hours' exposure to air under common circumstances it was as good as ever. I have not found that a week's exposure in a warm air cupboard of a piece that did not insulate made it much better:

a film on the outside became non-conducting; but if two fresh surfaces were exposed by cutting, and these were brought into contact with the electrometer and the finger, the inside portion was still found to conduct.

If the gutta percha in either the good or the bad condition (as to electrical service) be submitted to a gradually increasing temperature, at about 350° or 380°, it gives off a considerable proportion of water; being then cooled, the substance which remains has the general properties of gutta percha, and insulates well. The original gum is probably complicated, being a mixture of several things; and whether the water has existed in the substance as a hydrate, or is the result of a deeper change of one part or another of the gum, I am not prepared to say. All I desire in this note is to make known its use in the arrangement of extemporary or permanent electrical apparatus for the advantage of working philosophers, both juvenile and adult.

I am, dear Phillips,

Yours,

M. Faraday.

Royal Institution,
Feb. 9, 1848.

Observations on the Magnetic Force[1].

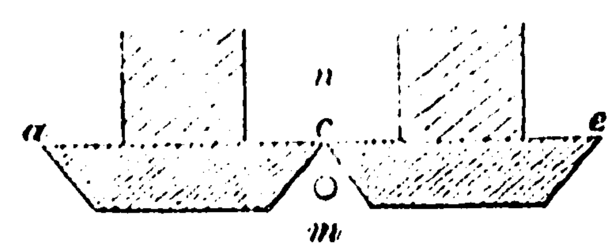
INASMUCH as the general considerations to be brought forward have respect to those great forces of the globe, exerted by it, both as a mass and through its particles, namely magnetism and gravitation, it is necessary briefly to recall certain relations and differences of the two which have been insisted upon on former occasions. Both can act at a distance, and doubtless at any distance; but whilst gravitation may be considered as simple and unpolar in its relations, magnetism is dual and polar. Hence *one* gravitating particle or system cannot be conceived to act by gravitation, as a particle or system, on itself; whereas a magnetic particle or system, because of the dual nature of its force, can have such a self-relation. Again, either polarity of the magnetic force can act either by attraction or repulsion; and not merely so, but the joint or *dual* action of a magnet can act also either by attraction or repulsion, as in the case of paramagnetic and diamagnetic bodies: the action of gravity is always that of attraction. As a further consequence of the difference in character of the powers, little or no doubt was entertained regarding the existence of physical lines of force[2] in the cases of dual powers, as electricity and magnetism; but in respect of gravitation the conclusion did not seem so sure. In reference to the growing magnetic relations of the sun and the earth, it is well to keep in mind Arago's idea, of the relative magnitude of the two; for, supposing that the centres of the two globes were made to coincide, the sun's body would not only extend as far as the moon, but nearly as far again, its bulk being about seven times that of a globe which should be girdled by the moon's orbit.

For the more careful study of the magnetic power, a torsion balance has been constructed of a particular kind. The torsion wire was of hard drawn platinum, 24 inches in length, and of such diameter that 28·5 inches weighed one grain. It was

[1] Proceedings of the Royal Institution, Jan. 21, 1853.

[2] Proceedings of the Royal Institution, June 11, 1852, p. 216 (see p. 438); also Phil. Mag. 4th Series, 1852, iii. p. 401, (see p. 407).

attached as usual to a torsion head and index. The horizontal beam was a small glass tube terminated at the object end by a glass hook. The objects to be submitted to the magnetic force were either cylinders of glass with a filament drawn out from each, so as to make a long stiff hook for suspension from the beam; or cylindrical bulbs of glass, of like shape, but larger size, formed out of glass tube; or other matters. The fine tubular extremities of the bulbs being opened, the way through was free from end to end; the bulbs could then be filled with any fluid or gas, sealed, and be re-submitted many times in succession to the magnetic force. The source of power employed was at first a large electro-magnet; but afterwards, in order to be certain of a constant power, and for the advantage of allowing any length of time for the observations the great magnet, constructed by M. Logeman upon the principles developed by Dr. Elias (and which, weighing above 100 lbs., could support 430 lbs. according to the report of the Great Exhibition Jury), was purchased by the Royal Institution and used in the inquiries. The magnet was so arranged that the axis of power was 5 inches below the level of the glass beam, the interval being traversed by the suspension filament or hook, spoken of above. The form and position of the terminations of soft iron are shown in plan by the diagram upon a scale of $\frac{1}{10}$, and also the place of the object. All this part is



enclosed in the box which belongs to and carries the torsion-balance, which box is governed by six screws fixed upon the magnet table; and as both the box and the table have lines and scales marked upon them, it is easy to adjust the former on the latter so that the beam shall be over and parallel to the line *a*, *e*, with the point of suspension over *c*; or, by moving the whole box parallel to itself towards *m*, to give the point of suspension any other distance from the angle *c*. As already said, the objects were constructed with a suspension filament of such length as to make them coincide in height with the angle in the magnetic field. When suspended on the beam, they were counterpoised by a ring or rings of lead on the further arm of the beam. These when required were moved along the beam until the latter was horizontal; and that state was ascertained by a double arm support, which sustained the beam when out of use, brought it into a steady state when moving, and delivered it into a condition of freedom when required. The motion of the box to the right or left, so as to place the object in the middle of the magnetic angle, was given by two of the screws before spoken of; the motion to the given distance from *c*, by the other four.

Supposing the distance from *c* towards *m* to be adjusted to 0·6 of an inch, when the beam was loaded above, and no object before the magnet (the beam having been of course previously adjusted to its normal position and the torsion index placed at zero), it then remained to determine the return of the beam to its place when the object had been suspended on it and repelled: this was done in the following manner. A small plane reflector is fixed on the beam, near its middle part, under the point of suspension; a small telescope associated with a divided scale is placed about 6 feet from the reflector, and in such a position that when the beam is in its right place, a given degree in the scale coincides with the fine wire in the telescope. Of course the scale appears to pass by the wire as the beam itself moves, and with a double angular velocity, because of the reflexion. As it is easy to read to the fiftieth and even to the hundredth of an inch in this way, and as each degree occupies apparently 2·4 inches with the radius of 6 feet, so an angular motion, or difference of $\frac{1}{240}$th of a degree could be observed; and as the radius of the arm of the beam carrying the object was 6 inches, such a quantity there would be less than $\frac{1}{2000}$th of an inch; *i.e.* the return of the beam to its first or normal position by the torsion force put on to counteract the repulsion, could be ascertained to within that amount. When an object was put on the adjusted beam, if diamagnetic it was repelled; and then, as the observer sat at the telescope, he, by means of a long handle, a wheel and pinion, put on torsion until the place of the beam was restored; and afterwards the amount of torsion read off on the graduated scale became the measure in degrees of the repulsive force exerted. At the time of real observations, the magnet, balance, and telescope were all fixed in a basement room, upon a stone floor. But it is unnecessary to describe here the numerous precautions required in relation to the time of an observation, the set of the suspension wire by a high torsion, the possible electricity of the object or beam by touch, the effect of feeble currents of air within the box, the shape of the object, the precaution against capillary action when fluids were employed as media, and other circumstances; or the use of certain stops, and the mode of procedure in the cases of paramagnetic action;—the object being at present to present only an intelligent view of the principles of action.

When a body is submitted to the power of a magnet, it is affected, as to the result, not merely by the magnet, but also by the medium surrounding it; and even if that medium be changed for a vacuum, the vacuum and the body still are in like relation to each other. In fact the result is always differential; any change in the medium changes the action on the object, and there are abundance of substances which when surrounded by air are repelled, and when by water, are attracted, upon the approach of a magnet. When a certain small glass cylinder weighing only 66 grains was submitted on the torsion-balance to the Logeman magnet surrounded by air, at the distance of 0·5 of an inch from the axial line, it required 15° of torsion to overcome the repulsive force and restore the object to its place. When a vessel of water was put into the magnetic field, and the experiment repeated, the cylinder being now in the water was attracted, and 54°·5 of torsion were required to overcome this attraction at the given distance of 0·5. If the vessel had contained a fluid exactly equal in diamagnetic power to the cylinder of glass, neither attraction nor repulsion would have been exerted on the latter, and therefore the torsion would have been 0°. Hence the three bodies, air, glass (the especial specimen), and water, have their relative force measured in relation to each other by the three experimental numbers 15°, 0°, and 54°·5. If other fluids are taken, as oil, æther, &c., and employed as the media surrounding the *same glass cylinder*, then the degrees of torsion obtained with each of them respectively, show its place in the magnetic series. It is the principle of the hydrometer or of Archimedes in respect of gravity applied in the case of the magnetic forces. If a different cylinder be employed of another size or substance, or at a different distance, the torsion numbers will be different, and the zero (given by the cylinder) also different;

but the media (with an exception to be made hereafter) will have the same relation to each other as in the former case. Therefore to bring all the experimental results into one common relation, a Centigrade scale has been adopted bounded by air and water at common temperatures, or 60° F. For this purpose every separate series of results made under exactly the same circumstances included air and water; and then all the results of one series were multiplied by such a number as would convert the difference between air and water into 100°: in this way the three results given above become 21°·6, 0°, and 78°·4. By such a process the magnetic intervals between the bodies are obtained on the Centigrade scale, but the true zero is not as yet determined. Either water, or air, or the glass, may be assumed as the zero, the intervals not being in any way dependent upon that point, but the results will then vary in expression thus:—

Air .	0°	21°·6	100°
Glass .	21·6	0	78·4
Water .	100	78·4	0

all above the zero being paramagnetic, and all below diamagnetic, in relation to it. I have adopted a vacuum as the zero in the table of results to be given hereafter.

In this manner it is evident that, upon principle, any solid, whatever its size, shape, or quality, may be included in the list, by its subjection to a magnet in air and in water, or in fluids already related to these: also that any fluids may be included, by the use of the *same* immersed solid body for them, air and water; and also that by using the same vessel, as for instance the same glass bulb, and filling it successively with various gases and fluids, including always air and water in each series, these included bodies may then have their results reduced and be entered upon the list. The following is a table of some substances estimated on the Centigrade scale, and though there are many points both of theory and practice yet to be wrought out, as regards the use of the torsion-balance described, so that the results can only as yet be recorded as approximations, even now, the average of three or four careful experiments gives an expression for any particular substance under the same conditions of distance, power, &c. near upon and often within a degree of the place assigned to it. The powers are expressed for a distance of 0·6 of an inch from the magnetic axis of the magnet as arranged and described, and, of course, for *equal volumes* of the bodies mentioned. The extreme decimal places must not be taken as correct, except as regards the record of the experiments: they are the results of calculation. Hydrogen, nitrogen, and perhaps some other of the bodies near zero, may ultimately turn out to be as a vacuum; it is evident that a very little oxygen would produce a difference, such as that which appears in nitrogen gas. The first solution of copper mentioned was colourless, and the second the same solution oxidized by simple agitation in a bottle with air, the copper, ammonia, and water being in both the same.

	°		°
Prot-ammo. of copper	134·23	Camphor . . .	82·59
Per-ammo. of copper	119·83	Camphine . .	82·96
Oxygen . . .	17·5	Linseed oil . .	85·56
Air	3·4	Olive oil . . .	85·6
Olefiant gas . .	0·6	Wax . . .	86·73
Nitrogen . . .	0·3	Nitric acid . .	87·96
Vacuum . . .	0·0	Water . . .	96·6
Carbonic acid gas .	0·0	Solution of ammonia	98·5
Hydrogen . . .	0·1	Bisulphide of carbon	99·64
Ammonia gas . .	0·5	Sat. sol. nitre . .	100·08
Cyanogen . . .	0·9	Sulphuric acid . .	104·47
A glass . . .	18·2	Sulphur . . .	118
Pure zinc . . .	74·6	Chloride of arsenic .	121·73
Æther . . .	75·3	Fused borate of lead .	136·6
Alcohol, absolute .	78·7	Phosphorus . .	
Oil of lemons . .	80	Bismuth . . .	1967·6

Plücker in his very valuable paper[1] has dealt with bodies which are amongst the highly paramagnetic substances, and his estimate of power is made for equal weights.

One great object in the construction of an instrument delicate as that described, was the investigation of certain points in the philosophy of magnetism; and amongst them especially that of the right application of the law of the inverse square of the distance as the universal law of magnetic action. Ordinary magnetic may action be divided into two kinds, that between

[1] Taylor's Scientific Memoirs, vol. v. pp. 713, 730.

magnets permanently magnetized and unchangeable in their condition, and that between bodies of which one is a permanent unchangeable magnet, and the other, having no magnetic state of its own, receives and retains its state only whilst in subjection to the first. The former kind of action appears in the most rigid and pure cases to be subject to that law; but it would be premature to assume beforehand, and without abundant sufficient evidence, that the same law applies in the second set of cases also; for a hasty assumption might be in opposition to the truth of nature, and therefore injurious to the progress of science, by the creation of a preconceived conclusion. We know not whether such bodies as oxygen, copper, water, bismuth, &c., owe their respective paramagnetic and diamagnetic relation to a greater or less facility of conduction in regard to the lines of magnetic force, or to something like a polarity of their particles or masses, or to some as yet unsuspected state; and there is little hope of our developing the true condition, and therefore the cause of magnetic action, if we assume beforehand the unproved law of action and reject the experiments that already bear upon it;—for Plücker has distinctly stated as the fact, that diamagnetic force increases more rapidly than magnetic force, when the power of the dominant magnet is increased; and such a fact is contrary to the law above enunciated. The following are further results in relation to this point.

When a body is submitted to the great unchanging Logeman magnet in air and in water, and the results are reduced to the Centigrade scale, the relation of the three substances remains the same for the same distance, but not for *different* distances. Thus, when a given cylinder of flint-glass was submitted to the magnet surrounded by air and by water, at the distance of 0·3 of an inch, as already described, it proved to be diamagnetic in relation to both; and when the results were corrected to the Centigrade scale, and water made zero, it was 9°·1 below, or on the diamagnetic side of water. At the distance 0·4 of an inch it was 10°·6 below water: at the distance of 0·7 it was 12°·1 below water. When a more diamagnetic body, as heavy glass, was employed, the same result in a higher degree was obtained; for at the distance of 0·3 it was 37°·8 below water, and at that of 0·8 it was 48°·6 beneath it. Bismuth presented a still more striking case, though, as the volume of the substance was necessarily small, equal confidence cannot be placed in the exactitude of the numbers. The results are given below for the three substances, air being always 100° and water 0°; the first column of figures for each substance contains the distance[1] in tenths of an inch from the axial line of the magnetic field, and the second, the place in Centigrade magnetic degrees below water.

Flint-Glass.		Heavy Glass.		Bismuth.	
0·3	9·1°	0·3	37·8°	0·6	1871°
0·4	10·6	0·4	38·6	1·0	2734
0·5	11·1	0·6	40·0	1·5	3626
0·6	11·2	0·8	48·6		
0·7	12·1	1·0	51·5		
		1·2	65·6		

The result here is, that the greater the distance of the diamagnetic body from the magnet, the more diamagnetic is it in relation to water, taking the interval between water and air as the standard: and it would further appear, if an opinion may be formed from so few experiments, that the more diamagnetic the body compared to air and water, the greater does this difference become. At first it was thought possible that the results might be due to some previous state induced upon the body, by its having been nearer to or further from the magnet; but it was found that whether the progress of the experiments was from small to large distances, or the reverse; or whether, at any given distance, the object was previous to the measurement held close up to the magnet or brought from a distance, the results were the same;—no evidence of a temporary induced state could in any of these ways be found.

It does not follow from the experiments, if they should be sustained by future researches, that it is the glass or the bismuth

[1] A given change of distance necessarily implies change in degree of force, and change in the forms of the lines of force; but it does not imply always the same amount of change. The forces are not the same at the same distance of 0·4 of an inch in opposite directions from the axial line towards *m* and *n* in the figure, page 498, nor at any other equal moderate distance; and though by increase and diminution of distance the change is in the same direction, it is not in the same proportion. By fitly arranged terminations, it may be made to alter with extreme rapidity in one direction, and with extreme slowness or not at all in another.

only that changes in relation to the other two bodies. It may be the oxygen of the air that alters, or the water, or more probably all these bodies; for if the result be a true and natural result in these cases, it is probably common to all substances. The great point is, that the three bodies concerned, air, water, and the subject of the experiment, alter in the degree of their magnetic relations to *each other*;—at different given distances from the magnet the ratio of their magnetic power does not, according to the experiments, remain the same; and if that result be confirmed, then it cannot be included by a law of action which is inversely as the square of the distance. A hydrometer floating in a fluid and subject to the gravity of the earth alone, would (other things being the same) stand at the same point, whether at the surface of the earth, or removed many diameters of the earth from it, because the action of gravity is inversely as the square of the distance; but if we suppose the substance of the hydrometer and the fluid to differ magnetically, as water and bismuth do, and the earth to act as a magnet instead of by gravity, then the hydrometer would, according to the experiments, stand at a different point for different distances, and if so could not be subject to the former law.

The cause of this variation in the ratio of the substances one to another, if it be finally proved, has still to be searched out. It may depend in some manner upon the *forms* of the lines of magnetic force, which are different at different distances; or not upon the forms of the lines, but the *amount* of power at the different distances; or not upon the mere amount, but on the circumstance that in every case the body submitted to experiment has lines of *different degrees of force* passing through different parts of it (for however different the magnetic or diamagnetic conditions of a body and the fluid surrounding it, they would not move at all in relation to each other in a field of equal force); but whatever be the cause, it will be a concomitant of magnetic actions; and therefore ought to be included in the results of any law by which it is supposed that these actions are governed.

It has not yet been noticed that these general results appear to be in direct opposition to those of Plücker, who finds that diamagnetic power increases more rapidly than magnetic power with increase of force. But such a circumstance, if both conclusions be accordant with facts, only shows that we have yet a great deal to learn about the physical nature of this force; and we must not shut our eyes to the first feeble glimpses of these things, because they are inconsistent on both sides with our assumed laws of action; but rather seize them, as hoping that they will give us the key to the truth of nature. Bodies when subject to the power of the magnet appear to acquire a new physical state, which varies with the distance or the power of the magnet. Each body may have its own rate of increase and decrease; and that may be such as to connect the extreme effect of Plücker, amongst paramagnetic bodies on the one hand, and the extreme effects amongst diamagnetic bodies now described, on the other; and when we understand all this rightly, we may see the apparent contradiction become harmony, though it may not conform to the law of the inverse square of the distance as we now try to apply it.

Plücker has already said, because of his observations regarding paramagnetic and diamagnetic force, that no correct list of magnetic substances can be given. The same consequence follows, though in a different direction, from what has now been stated, and hence the reservation before made (p. 501). Still the former table is given as an approximation, and it may be useful for a time. Before leaving this first account of recent experimental researches, it may be as well to state that they are felt to be imperfect and may perhaps even be overturned; but that, as such a result is not greatly anticipated, it was thought well to present them to the Members of the Royal Institution and the scientific world, if peradventure they might excite criticism and experimental examination, and so aid in advancing the cause of physical science.

On a former occasion[1] the existence of *physical* lines of force in relation to magnetism and electricity was inferred from the dual nature of these powers, and the necessity in all cases and at all times of a relation and dependence between the polarities of the magnet, or the positive and negative electrical surfaces. With respect to gravity a more hesitating opinion was expressed, because of the difficulty of observing facts having any relation to *time*, and because two gravitating particles or masses did not seem to have any necessary dependence on each other for the

[1] See p. 438.

existence or excitement of their mutual power[1]. A passage may now be quoted from Newton which has since been discovered in his works, and which, showing that he was an unhesitating believer in physical lines of gravitating force, must from its nature rank him amongst those who sustain the physical nature of the lines of magnetic and electrical force: it is as follows, in words written to Bentley[2]: "That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers."

[1] Philosophical Magazine, 4th Series, 1852, vol. iii. p. 403 (3246.).

[2] Newton's Works, Horsley's edition, 4to, 1783, vol. iv. p. 438, or the Third Letter to Bentley.

On Electric Induction—Associated cases of current and static effects[1].

Certain phænomena that have presented themselves in the course of the extraordinary expansion which the works of the Electric Telegraph Company have undergone, appeared to me to offer remarkable illustrations of some fundamental principles of electricity, and strong confirmation of the truthfulness of the view which I put forth sixteen years ago, respecting the mutually dependent nature of induction, conduction, and insulation (Exp. Res., 1318. &c.). I am deeply indebted to the Company, to the Gutta Percha works, and to Mr. Latimer Clarke, for the facts; and also for the opportunity both of seeing and showing them well.

Copper wire is perfectly covered with gutta percha at the Company's works, the metal and the covering being in every part regular and concentric. The covered wire is usually made into half-mile lengths, the necessary junctions being effected by twisting or binding, and ultimately, soldering; after which the place is covered with fine gutta percha, in such a manner as to make the coating as perfect there as elsewhere: the perfection of the whole operation is finally tried in the following striking manner, by Mr. Statham, the manager of the works. The half-mile coils are suspended from the sides of barges floating in a canal, so that the coils are immersed in the water, whilst the two ends of each coil rise into the air: as many as 200 coils are thus immersed at once, and when their ends are connected in series, one great length of 100 miles of submerged wire is produced, the two extremities of which can be brought into a room for experiment. An insulated voltaic battery of many pairs of zinc and copper, with dilute sulphuric acid, has one end connected with the earth, and the other, through a galvanometer, with either end of the submerged wire. Passing by the first effect, and continuing the contact, it is evident that the battery current can take advantage of the whole accumulated conduction or defective insulation in the 100 miles of gutta percha on the wire, and that whatever portion of electricity passes through to the

[1] Proceedings of the Royal Institution, Jan. 20, 1854.

water will be shown by the galvanometer. Now the battery is made one of intensity, in order to raise the character of the proof, and the galvanometer employed is of considerable delicacy; yet so high is the insulation that the deflection is not more than 5°. As another test of the perfect state of the wire, when the two ends of the battery are connected with the two ends of the wire, there is a powerful current of electricity shown by a much coarser instrument; but when any one junction in the course of the 100 miles is separated, the current is stopped, and the leak or deficiency of insulation rendered as small as before. The perfection and condition of the wire may be judged of by these facts.

The 100 miles, by means of which I saw the phænomena, were thus good as to insulation. The copper wire was $\frac{1}{16}$th of an inch in diameter:—the covered wire was $\frac{4}{16}$; some was a little less, being $\frac{7}{32}$ in diameter:—the gutta percha on the metal may therefore be considered as 0·1 of an inch in thickness. 100 miles of like covered wire in coils were heaped up on the floor of a dry warehouse and connected in one series, for comparison with that under water.

Consider now an insulated battery of 360 pairs of plates (4 × 3 inches) having one extremity to the earth; the water wire with both its insulated ends in the room, and a good earth-discharge wire ready for the requisite communications:—when the free battery end was placed in contact with the water wire and then removed, and, afterwards, a person touching the earth-discharge touched also the wire, he received a powerful shock. The shock was rather that of a voltaic than of a Leyden battery: it occupied *time*, and by quick tapping touches could be divided into numerous small shocks: I obtained as many as forty sensible shocks from one charge of the wire. If *time* were allowed to intervene between the charge and discharge of the wire, the shock was less; but it was sensible after 2, 3, or 4 minutes, or even a longer period.

When, after the wire had been in contact with the battery, it was placed in contact with a Statham's fuse, it ignited the fuse (or even six fuses in succession) vividly:—it could ignite the fuse 3 or 4 seconds after separation from the battery. When, having been in contact with the battery, it was separated and placed in contact with a galvanometer, it affected the instrument very powerfully: it acted on it, though less powerfully, after the lapse of 4 or 5 minutes, and even affected it sensibly 20 or 30 minutes after it had been separated from the battery. When the insulated galvanometer was permanently attached to the end of the water wire, and the battery pole was brought in contact with the free end of the instrument, it was most instructive to see the great rush of electricity into the wire; yet after that was over, though the contact was continued, the deflection was not more than 5°, so high was the insulation. Then separating the battery from the galvanometer, and touching the latter with the earth wire, it was just as striking to see the electricity rush out of the wire, holding for a time the magnet of the instrument in the reverse direction to that due to the ingress or charge.

These effects were produced equally well with either pole of the battery or with either end of the wire; and whether the electric condition was conferred and withdrawn at the same end, or at the opposite ends of the 100 miles, made no difference in the results. An intensity battery was required, for reasons which will be very evident in the sequel. That employed was able to decompose only a very small quantity of water in a given time. A Grove's battery of eight or ten pair of plates, which would have far surpassed it in this respect, would have had scarcely a sensible power in affecting the wire.

When the 100 miles of wire in the air were experimented with in like manner, not the slightest signs of any of these effects were produced. There is reason, from principle, to believe that an infinitesimal result is obtainable, but as compared to the water wire the action was nothing. Yet the wire was equally well and better insulated, and as regarded a constant current, it was an equally good conductor. This point was ascertained, by attaching the end of the water wire to one galvanometer, and the end of the air wire to another like instrument; the two other ends of the wires were fastened together, and to the earth contact; the two free galvanometer ends were fastened together, and to the free pole of the battery; in this manner the current was divided between the air and water wires, but the galvanometers were affected to precisely the same amount. To make the result more certain, these instruments were changed one for the other, but the deviations were still alike; so that the two wires conducted with equal facility.

The cause of the first results is, upon consideration, evident enough. In consequence of the perfection of the workmanship, a Leyden arrangement is produced upon a large scale; the copper wire becomes charged statically with that electricity which the pole of the battery connected with it can supply[1]; it acts by induction through the gutta percha (without which induction it could not itself become charged, Exp. Res. 1177.), producing the opposite state on the surface of the water touching the gutta percha, which forms the outer coating of this curious arrangement. The gutta percha across which the induction occurs, is only 0·1 of an inch thick, and the extent of the coating is enormous. The surface of the copper wire is nearly 8300 square feet, and the surface of the outer coating of water is four times that amount, or 33,000 square feet. Hence the striking character of the results. The intensity of the static charge acquired is only equal to the intensity at the pole of the battery whence it is derived; but its quantity is enormous, because of the immense extent of the Leyden arrangement; and hence when the wire is separated from the battery and the charge employed, it has all the powers of a considerable voltaic current, and gives results which the best ordinary electric machines and Leyden arrangements cannot as yet approach.

That the air wire produces none of these effects is simply because there is no outer coating correspondent to the water, or only one so far removed as to allow of no sensible induction, and therefore the inner wire cannot become charged. In the air wire of the warehouse, the floor, walls, and ceiling of the place constituted the outer coating, and this was at a considerable distance; and in any case could only affect the outside portions of the coils of wire. I understand that 100 miles of wire stretched in a line through the air, so as to have its whole extent opposed to earth, is equally inefficient in showing the effects, and there it must be the distance of the inductric and inducteous surfaces (1483.), combined with the lower specific inductive capacity of air, as compared with gutta percha, which causes the negative result. The phænomena altogether offer a beautiful case of the identity of static and dynamic electricity. The whole power of a considerable battery may in this way be worked off in separate portions, and measured out in units of static force, and yet be employed afterwards for any or every purpose of voltaic electricity.

I now proceed to further consequences of associated static and

[1] Davy, Elements of Chemical Philosophy, p. 154.

dynamic effects. Wires covered with gutta percha, and then enclosed in tubes of lead or of iron, or buried in the earth, or sunk in the sea, exhibit the same phænomena as those described; the like static inductive action being in all these cases permitted by the conditions. Such subterraneous wires exist between London and Manchester, and when they are all connected together so as to make one series, offer above 1500 miles; which, as the duplications return to London, can be observed by one experimenter at intervals of about 400 miles, by the introduction of galvanometers at these returns. This wire, or the half, or fourth of it, presented all the phænomena already described; the only difference was, that as the insulation was not so perfect, the charged condition fell more rapidly. Consider 750 miles of the wire in one length, a galvanometer *a* being at the beginning of the wire, a second galvanometer *b* in the middle, and a third *c* at the end: —these three galvanometers being in the room with the experimenter, and the third *c* perfectly connected with the earth. On bringing the pole of the battery into contact with the wire through the galvanometer *a*, that instrument was instantly affected; after a sensible time *b* was affected, and after a still longer time *c*: when the whole 1500 miles were included, it required two seconds for the electric stream to reach the last instrument. Again; all the instruments being deflected (of course not equally because of the electric leakage along the line), if the battery were cut off at *a*, that instrument instantly fell to zero; but *b* did not fall until a little while after; and *c* only after a still longer interval;—a current flowing on to the end of the wire whilst there was none flowing in at the beginning. Again; by a short touch of the battery pole against *a*, it could be deflected and could fall back into its neutral condition, before the electric power had reached *b*; which in its turn would be for an instant affected, and then left neutral before the power had reached *c*; a wave of force having been sent into the wire which gradually travelled along it, and made itself evident at successive intervals of time, in different parts of the wire. It was even possible, by adjusted touches of the battery, to have two simultaneous waves in the wire, following each other, so that at the same moment that *c* was affected by the first wave, *a* or *b* was affected by the second; and there is no doubt that by the multiplication of instruments and close attention, four or five waves might be obtained at once.

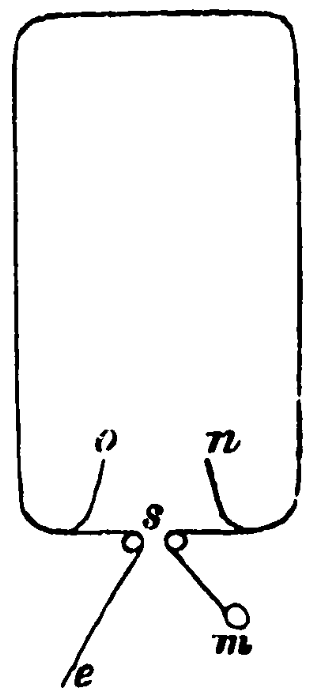
If after making and breaking battery contact at *a*, *a* be immediately connected with the earth, then additional interesting effects occur. Part of the electricity which is in the wire will return, and passing through *a* will deflect it in the reverse direction; so that currents will flow out of both extremities of the wire in opposite directions, whilst no current is going into it from any source. Or if *a* be quickly put to the battery and then to the earth, it will show a current first entering into the wire, and then returning out of the wire at the same place; no sensible part of it ever travelling on to *b* or *c*.

When an air wire of equal extent is experimented with in like manner, no such effects as these are perceived: or if, guided by principle, the arrangements are such as to be searching, they are perceived only in a very slight degree, and disappear in comparison with the former gross results. The effect at the end of the very long air wire (or *c*) is in the smallest degree behind the effect at galvanometer *a*; and the accumulation of a charge in the wire is not sensible.

All these results as to *time*, &c. evidently depend upon the same condition as that which produced the former effect of static charge, namely *lateral induction*; and are necessary consequences of the principles of conduction, insulation, and induction, three terms which in their meaning are inseparable from each other (Exp. Res. 1320. 1326[1]. 1338. 1561. &c.). If we put a plate of shell-lac upon a gold-leaf electrometer and a charged carrier (an insulated metal ball of two or three inches diameter) upon it, the electrometer is diverged; removing the carrier, this divergence

[1] 1326. All these considerations impress my mind strongly with the conviction, that insulation and ordinary conduction cannot be properly separated when we are examining into their nature; that is, into the general law or laws under which their phænomena are produced. They appear to me to consist in an action of contiguous particles, dependent on the forces developed in electrical excitement; these forces bring the particles into a state of tension or polarity, which constitutes both *induction* and *insulation*; and being in this state the contiguous particles have a power or capability of communicating these forces, one to the other, by which they are lowered and discharge occurs. Every body appears to discharge (444. 987); but the possession of this capability in a *greater* or *smaller degree* in different bodies, makes them better or worse conductors, worse or better insulators: and both *induction* and *conduction* appear to be the same in their principle and action (1320.), except that in the latter, an effect common to both is raised to the highest degree, whereas in the former, it occurs in the best cases, in only an almost insensible quantity.

instantly falls; this is *insulation* and *induction*: if we replace the shell-lac by metal, the carrier causes the leaves to diverge as before, but when removed, though after the shortest possible contact, the electroscope is left diverged; this is *conduction*. If we employ a plate of spermaceti instead of the metal, and repeat the experiment, we find the divergence partly falls and partly remains, because the spermaceti insulates and also conducts, doing both imperfectly; but the shell-lac also conducts, as is shown if time be allowed; and the metal also obstructs conduction, and therefore insulates, as is shown by simple arrangements. For if a copper wire, 74 feet in length and $\frac{1}{18}$th of an inch in diameter, be insulated in the air, having its end *m* a metal ball; its end *e* connected with the earth, and the parts near *m* and *e* brought within half an inch of each other, as at *s*; then an ordinary Leyden jar being charged sufficiently, its outside connected with *e* and its inside with *m*, will give a charge to the wire, which instead of travelling wholly through it, though it be so excellent a conductor, will pass in large proportion through the air at *s*, as a bright spark; for with such a length of wire, the resistance in it is accumulated until it becomes as much, or perhaps even more, than that of the air, for electricity of such high intensity.



Admitting that such and similar experiments show that conduction through a wire is preceded by the act of induction (1338.), then all the phænomena presented by the submerged or subterranean wires are explained; and in their explanation confirm, as I think, the principles given. After Mr. Wheatstone had, in 1834, measured the velocity of a wave of electricity through a copper wire, and given it as 288,000 miles in a second, I said, in 1838, upon the strength of these principles (1333.), "that the velocity of discharge through the *same wire* may be greatly varied, by attending to the circumstances which cause variations of discharge through spermaceti or sulphur. Thus, for instance, it must vary with the tension or intensity of the first urging force, which tension is charge and induction. So if the two ends of the wire in Professor Wheatstone's experiment were immediately connected with two large insulated metallic surfaces exposed to the air, so that the primary act of induction, after making the contact for discharge, might be in part removed from the internal portion of the wire at the first instant, and disposed for the moment on its surface jointly with the air and surrounding conductors, then I venture to anticipate that the middle spark would be more retarded than before: and if these two plates were the inner and outer coating of a large jar, or a Leyden battery, then the retardation of that spark would be still greater." Now this is precisely the case of the submerged or subterraneous wires, except that instead of carrying their surfaces towards the inducteous coatings (1483.), the latter are brought near the former; in both cases the induction consequent upon charge, instead of being exerted almost entirely at the moment within the wire, is to a very large extent determined externally; and so the discharge or conduction being caused by a lower tension, therefore requires a longer time. Hence the reason why, with 1500 miles of subterraneous wire, the wave was two seconds in passing from end to end; whilst with the same length of air wire, the time was almost inappreciable.

With these lights it is interesting to look at the measured velocities of electricity in wires of metal, as given by different experimenters.

	Miles per second.
[1] Wheatstone in 1834, with copper wire made it .	288,000
[1] Walker in America with telegraph iron wire . .	18,780
[1] O'Mitchell, ditto ditto . .	28,524
[1] Fizeau and Gonnelle (copper wire)	112,680
[1] Ditto (iron wire)	62,600
[2] A. B. G. (copper) London and Brussels Telegraph	2,700
[2] Ditto (copper) London and Edinburgh Telegraph	7,600

Here, the difference in copper is seen by the first and fifth result to be above a hundredfold. It is further remarked in Liebig's report of Fizeau's and Gonnelle's experiments, that the velocity is not proportional to the conductive capacity, and is independent of the thickness of the wire. All these circumstances and incompatibilities appear rapidly to vanish, as we recognise and take into consideration the lateral induction of the

[1] Liebig and Kopp's report, 1850 (translated), p. 168.
[2] Athenæum, 14th January, 1854, p. 54.

wire carrying the current. If the velocity of a brief electric discharge is to be ascertained in a given length of wire, the simple circumstances of the latter being twined round a frame in small space, or spread through the air through a large space, or adhering to walls, or lying on the ground, will make a difference in the results. And in regard to long circuits such as those described, their conducting power cannot be understood, whilst no reference is made to their lateral static induction, or to the conditions of intensity and quantity which then come into play; especially in the case of short or intermitting currents, for then static and dynamic are continually passing into each other.

It has already been said that the conducting power of the air and water wires is alike for a constant current. This is in perfect accordance with the principles and with the definite character of the electric force, whether in the static or current or transition state. When a voltaic current of a certain intensity is sent into a long water wire, connected at the further extremity with the earth, part of the force is in the first instance occupied in raising a lateral induction round the wire, ultimately equal in intensity at the near end to the intensity of the battery stream, and decreasing gradually to the earth end, where it becomes nothing. Whilst this induction is rising, that within the wire amongst its particles is beneath what it would otherwise be; but as soon as the first has attained its maximum state, then that in the wire becomes proportionate to the battery intensity, and therefore equals that in the air wire, in which the same state is (because of the absence of lateral induction) almost instantly attained. Then of course they discharge alike and therefore conduct alike.

A striking proof of the variation of the conduction of a wire by variation of its lateral static induction, is given in the experiment proposed sixteen years ago (1333.). If, using a constant charged jar, the interval *s*, page 514, be adjusted so that the spark shall freely pass there (though it would not if a little wider), whilst the short connecting wires *n* and *o* are insulated in the air, the experiment may be repeated twenty times without a single failure; but if after that, *n* and *o* be connected with the inside and outside of an insulated Leyden jar, as described, the spark will never pass across *s*, but all the charge will go round the whole of the long wire. Why is this? The quantity of electricity is the same, the wire is the same, its resistance is the same, and that of the air remains unaltered; but because the intensity is lowered, through the lateral induction momentarily allowed, it is never enough to strike across the air at *s*; and it is finally altogether occupied in the wire, which in a little longer time than before, effects the whole discharge. M. Fizeau has applied the same expedient to the primary voltaic currents of Rhumkorff's beautiful inducting apparatus, with great advantage. He thereby reduces the intensity of these currents at the moment when it would be very disadvantageous, and gives us a striking instance of the advantage of viewing static and dynamic phænomena as the result of the same laws.

Mr. Clarke arranged a Bains' printing telegraph with three pens, so that it gave beautiful illustrations and records of facts like those stated: the pens are iron wires, undei which a band of paper imbued with ferro-prussiate of potassa passes at a regular rate by clock-work; and thus regular lines of prussian blue are produced whenever a current is transmitted, and the time of the current is recorded. In the case to be described, the three lines were side by side, and about 0·1 of an inch apart. The pen *m* belonged to a circuit of only a few feet of wire, and a separate battery; it told whenever the contact key was put down by the finger; the pen *n* was at the earth end of the long air wire, and the pen *o* at the earth end of the long subterraneous wire; and by arrangement, the key could be made to throw the electricity of the chief battery into either of these wires, simultaneously with the passage of the short circuit current through pen *m*. When pens *m* and *n* were in action, the *m* record was a regular line of equal thickness, showing by its length the actual time during which the electricity flowed into the wires; and the *n* record was an equally regular line, parallel to, and of equal length with the former, but the least degree behind it; thus indicating that the long air wire conveyed its electric current almost instantaneously to the further end. But when pens *m* and *o* were in action, the *o* line did not begin until some time after the *m* line, and it continued after the *m* line had ceased, *i.e.* after the *o* battery was cut off. Furthermore, it was faint at first, grew up to a maximum of intensity, continued at that as long as battery contact was continued, and then gradually diminished to nothing. Thus the record *o* showed that the wave of powei took time in the water wire to reach the further extremity; by its first faintness, it showed that power was consumed in the exertion of lateral static induction along the wire; by the attainment of a maximum and the after equality, it showed when this induction had become proportionate to the intensity of the battery current; by its beginning to diminish, it showed when the battery current was cut off; and its prolongation and gradual diminution showed the time of the outflow of the static electricity laid up in the wire, and the consequent regular falling of the induction which had been as regularly raised.

With the pens *m* and *o* the conversion of an intermitting into a continuous current could be beautifully shown; the earth wire by the static induction which it permitted, acting in a manner analogous to the fly-wheel of a steam-engine, or the air-spring of a pump. Thus when the contact key was regularly but rapidly depressed and raised, the pen *m* made a series of short lines separated by intervals of equal length. After four or more of these had passed, then pen *o*, belonging to the subterraneous wire, began to make its mark, weak at first, then rising to a maximum, but always continuous. If the action of the contact key was less rapid, then alternate thickening and attenuations appeared in the *o* record; and if the introductions of the electric current at the one end of the earth wire were at still longer intervals, the records of action at the other end became entirely separated from each other;—all showing most beautifully, how the individual current or wave, once introduced into the wire, and never ceasing to go onward in its course, could be affected in its intensity, its time, and other circumstances, by its partial occupation in static induction.

By other arrangements of the pens *n* and *o*, the near end of the subterraneous wire could be connected with the earth immediately after separation from the battery; and then the back flow of the electricity, and the time and manner thereof, were beautifully recorded; but I must refrain from detailing results which have already been described in principle.

Many variations of these experiments have been made and may be devised. Thus the ends of the insulated battery have been attached to the ends of the long subterraneous wire, and then the two halves of the wire have given back opposite return currents when connected with the earth. In such a case the wire is positive and negative at the two extremities, being permanently sustained by its length and the battery, in the same condition which is given to the short wire for a moment by the Leyden discharge, p. 514; or, for an extreme but like case, to a filament of shell-lac having its extremities charged positive and negative. Coulomb pointed out the difference of long and short as to the insulating or conducting power of such filaments, and the like difference occurs with long and short metal wires.

The character of the phænomena described in this report, induces me to refer to the terms *intensity* and *quantity* as applied to electricity; terms which I have had such frequent occasion to employ. These terms, or equivalents for them, cannot be dispensed with by those who study both the static and the dynamic relations of electricity; every current where there is resistance, has the static element and induction involved in it, whilst every case of insulation has more or less of the dynamic element and conduction; and we have seen that with the same voltaic source, the same current in the same length of the same wire, gives a different result as the intensity is made to vary, with variations of the induction around the wire. The idea of intensity or the power of overcoming resistance, is as necessary to that of electricity, either static or current, as the idea of pressure is to steam in a boiler, or to air passing through apertures or tubes; and we must have language competent to express these conditions and these ideas. Furthermore, I have never found either of these terms lead to any mistakes regarding electrical action, or give rise to any false view of the character of electricity or its unity. I cannot find other terms of equally useful significance with these; or any which, conveying the same ideas, are not liable to such misuse as these may be subject to. It would be affectation, therefore, in me to search about for other words; and besides that, the present subject has shown me more than ever their great value and peculiar advantage in electrical language.

The fuse referred to in page 509, is of the following nature. Some copper wire was covered with sulphuretted gutta percha; after some months it was found that a film of sulphuret of copper was formed between the metal and the envelope; and further, that when half the gutta percha was cut away in any place, and then the copper wire removed for about $\frac{1}{4}$ of an inch, so as to remain connected only by the film of sulphuret adhering to the remaining gutta percha, an intensity battery could cause this sulphuret to enter into vivid ignition, and fire gunpowder with the utmost ease. Gunpowder was fired with certainty at the end of eight miles of single wire; and also through 100 miles of covered wire immersed in the canal, by the use of this fuse.

On Subterraneous Electro-Telegraph Wires[1].

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

A COMMUNICATION has been just brought to my notice on some remarkable phænomena presented by subterraneous electro-telegraph wires observed and described by M. Werner Siemens of Berlin, in a communication bearing date April 15, 1850. They are the same phænomena as those shown to me by Mr. Latimer Clarke, and used in my communication (inserted in your Magazine for March 1854, p. 197) as illustrations of the truth of my ancient views of the nature of insulation, induction and conduction. It is only justice that I should refer to them; and I think they are so interesting, that you will be willing to reprint the account, very slightly abbreviated, which I send you: the effects are produced with wires covered with gutta percha and laid in the earth.

"A very remarkable phænomenon is constantly observed on long, well-insulated telegraphic lines. Suppose one extremity, B, of the wire be insulated, and the other, A, be connected with one pole of a battery of which the other touches the earth; at the instant of communication a brief current is observed in the near parts of the wire in the same direction as the instantaneous current which would exist if the extremity B were connected with the earth; on lines of perfect insulation no trace of this current remains. Suddenly replacing, through the action of a commutator, the battery by an earth conductor, a second instantaneous current is obtained of an intensity nearly equal to the first, but in the inverse direction. Finally, breaking the communication of A with the battery and also the earth, so as to insulate this extremity, and uniting the end B at the same instant with the ground, an instantaneous current is observed nearly equal in intensity to the former, and this time in the same direction as the first, *i. e.* as the continuous current of the battery. This last experiment can only be made on a double subterranean line having the two extremities A and B at the same station. One might at first sight suppose these phænomena to be due to

[1] From the Philosophical Magazine for June 1854.

secondary polarities developed on the wire, but many facts oppose such a conclusion. 1. The phænomena are more striking as the wire is better insulated. 2. The currents are much more brief than those due to secondary polarities. 3. Their intensity is proportional to the force of the battery, and independent of the intensity of any derived current that may occur in consequence of imperfect insulation; it follows that the intensity of the instantaneous currents can greatly surpass the maximum intensity which secondary currents in the same circuit could acquire. 4. Finally, the intensity of the instantaneous currents is proportional to the length of the wire, whilst an inverse relation ought to occur if the currents were due to secondary polarities.

"The phænomena are easily comprehended if we recall the beautiful experiment by which Volta furnished the most striking proof of the identity of galvanism and electricity. He showed that on communicating one of the ends of his pile with the earth, and the other with the interior of a non-insulated Leyden battery, the battery was charged in an instant of time to a degree proportional to the force of the pile. At the same time an instantaneous current was observed in the conductor between the pile and the battery, which, according to Ritter, had all the properties of an ordinary current. Now it is evident that the subterraneous wire with its insulating covering may be assimilated exactly to an immense Leyden battery: the glass of the jars represents the gutta percha; the internal coating is the surface of the copper wire; the external surface is the moistened earth. To form an idea of the capacity of this new kind of battery, we have only to remember that the surface of the wire is equal to 7 square metres per kilometre. Making such a wire communicate by one of its ends with a pile, of which the other extremity is in contact with the earth, whilst the other extremity of the wire is insulated, must cause the wire to take a charge, of the same character and tension as that of the pole of the pile touched by it: —that is what came to pass in the first of the instantaneous currents described. In Volta's experiment, on breaking the communication between the pole and the battery and connecting the two coatings of the latter by a conductor, an ordinary discharge was obtained:—to this discharge correspond the two instantaneous currents which are observed in opposite directions at the two extremities of the charged wire, on communicating their extremities with the earth, to the exclusion of the pile. It will be understood, also, that the first instantaneous current, namely that which is connected with the charge of the wire, ought to be equally produced, though of a lower intensity, even when the other extremity of the wire is in communication with the earth. The instantaneous current then precedes the continuous current, or, if the statement be preferred, is added to it at the first moment. This instantaneous current has an intensity much greater than that of the continuous current; doubtless because in the act of charging the wire, the electricity in going to the different points of the wire passes through paths so much the shorter as the points to be charged are nearer to the pile." The above is from the *Annales de Chimie*, 1850, vol. xxix. p. 398, &c.

I am, Gentlemen,

Your very faithful Servant,

M. FARADAY.

Royal Institution,
April 28, 1854.

On Magnetic Hypotheses[1].

This discourse, the purpose of which was to direct the attention of the audience to the different hypothetical attempts made to account physically for the known properties of matter in relation to its magneto-electrical phænomena, followed on very naturally to that of Dr. Frankland on the 2nd instant, who then gave an account of the different views advanced by Davy, Ampère, and Berzelius, of the manner in which electricity might be associated with the atoms or molecules of matter, so as to account for their electro-chemical actions, and of the logical and experimental objections which stood in the way of each. Reference was first made to Coulomb's investigations of mutual magnetic actions; to the hypothesis advanced by him, that two magnetic fluids, associated with the matter of magnetic bodies, would account for all the phænomena; and to Poisson's profound mathematical investigation of the sufficiency of the hypothesis. Then Œrsted's discovery of the relation of common magnetism to currents of electricity was recalled to mind:—hence an enormous enlargement of the scope of magnetic force and of our knowledge of its actions; and hence Ampère's beautiful investigations, and his hypothesis (also sustained by the highest mathematical investigation),—that all magnetic phænomena are due to currents of electricity; and that in such bodies as magnets, iron, nickel, &c., the atoms or particles have naturally currents of electricity running round them in one direction, about what may be considered as their equatorial parts. After Œrsted's time, further experimental discoveries occurred; currents of electricity were found competent to induce collateral currents, and magnets proved able to produce like currents; thus showing the identity of action of magnets and currents in producing effects of a kind different to ordinary magnetic attractions and repulsions. Then diamagnetism was discovered, in which actions analogous to those of ordinary magnetism occurred, but with the antithesis of attraction for repulsion and repulsion for attraction; and these were so extensive, that whatever bodies were not magnetic proved to be diamagnetic; and thus *all* matter was brought under the dominion of that magnetic

[1] Royal Institution Proceedings, June 9th, 1854.

force, whose physical mode of action hypothesis endeavours to account for. As the hypothesis of Ampère could not account for diamagnetic action, some assumed that magnetic and electric force might, in diamagnetic matter, induce currents of electricity in the reverse direction to those in magnetic matter; or else might induce currents where before there were none; whereas in magnetic cases it was supposed they only constrained particle-currents to assume a particular direction, which before were in all directions. Weber stands eminent as a profound mathematician who has confirmed Ampère's investigations as far as they proceeded, and who has made an addition to his hypothetical views; namely, that there is electricity amongst the particles of matter, which is not thrown into the form of a current until the magnetic induction comes upon it, but which then assumes the character of current, having a direction the contrary to that of the currents which Ampère supposed to be always circulating round magnetic matter; and so these other matters are rendered diamagnetic.

De la Rive, who has recently most carefully examined the various hypotheses, and who as an experimentalist and discoverer has the highest right to enter into the consideration of these deep, searching, and difficult inquiries, after recalling the various phænomena which show that the powers concerned belong to the particles of matter and not to the masses merely (the former conferring them by association upon the latter), then distinguishes magnetic action into four kinds or modes—namely the ordinary, the diamagnetic, the induction of currents, and the rotation of a ray; and points out that any acceptable hypotheses ought to account for the *four* modes of action, and, it may be added, ought to agree with, if not account for, the phænomena of electro-chemical action also. De la Rive conceives that as regards these modes of action this hypothetical result may be obtained, and both Ampère's and Weber's views also retained in the following manner. All the atoms of matter are supposed to be endowed with electrical currents of a like kind, which move about them for ever, without diminution of their force or velocity, being essentially a part of their nature. The direction of these currents for each atom is through one determinate diameter, which may therefore be considered as the axis. Where they emerge from the body of the atom they divide in all directions, and running over every part of the surface converge towards the opposite end of the axis diameter, and there re-enter the atom to run ever through the same course. The converging and diverging points are as it were poles of force. Where the atoms of matter are close or numerous in a given space (and chemical considerations lead to the admission of such cases), the hypothesis then admits that several atoms may conjoin into a ring, so that their central or axial currents may run one into the other, and not return as before over the surface of each atom: these form the molecules of magnetic matter, and represent Ampère's hypothesis of molecular currents. Where the atoms, being fewer in a given space, are further apart, or where, being good conductors, the current runs as freely over the surface as through the axis, then they do not form like groups to the molecules of magnetic matter, but are still considered subject to a species of induction by the action of external magnets and currents; and so give rise to Weber's reverse currents. The induction of momentary currents and the rotation of a ray are considered by De la Rive as in conformity with such a supposition of the electric state of the atoms and particles of matter.

It would seem that the great variety of these hypotheses and their rapid succession was rather a proof of weakness in this department of physical knowledge than of strength, and that the large assumptions which were made in turn for each should ever be present to the mind. Even in the most perfect of them, *i. e.* De la Rive's, these assumptions are very considerable; for it is necessary to conceive of the molecules as being flat or disc-like bodies, however numerous the atoms of each may be; also that the atoms of one molecule do not interfere with or break up the disposition of those of another molecule; also that electro-chemical action may consist with such a constituted molecule; also that the motive force of each atom current is resident in the axis, and on the other hand that the passage of the current over the surface offers *resistance*; for unless there were a difference between the axial and the surface force in one direction or the other, the atoms would have no tendency to congregate in molecules. These remarks, however, are not to be understood in depreciation of hypothesis, or as objections to its right use. No discoverer could advance without it; and such exertions as those made by De la Rive, to bring into harmony thoughts which in their earlier forms were adverse to each other, are of the more value, because they are the exertions of a man who knows the value both of hypothesis and of laws, of theory and of fact, and has given proofs of the power of each by the productions of his own mind. Still, that mental reservation is to be advocated which keeps hypothesis in its right place and is ready to abandon it when it fails; and as examples Newton may be referred to, who (as is shown by his Letters to Bentley) had very strong convictions of the physical nature of the lines of gravitating force, yet in what he publicly advanced stopped short at the law of action of the force, and thence deduced his great results;—and also Arago, who, discovering the phænomena of magnetic rotation, yet not perceiving their physical cause, had that philosophic power of mind which enabled him to refrain from suggesting one.

On some Points of Magnetic Philosophy[1].

3300. Within the last three years I have been bold enough, though only as an experimentalist, to put forth new views of magnetic action in papers having for titles, "On Lines of Magnetic Force[2]," and "On Physical Lines of Magnetic Force[3]." The first paper was simply an attempt to give, for the use of experimentalists and others, a correct expression of the dual nature, amount, and direction of the magnetic power both within and outside of magnets, apart from any assumption regarding the character of the source of the power; that the mind, in reasoning forward towards new developments and discoveries, might be free from the bondage and deleterious influence of assumptions of such a nature (3075. 3243.). The second paper was a speculation respecting the possible physical nature of the force, as existing outside of the magnet as well as within it, and within what are called magnetic bodies, and was expressly described as being entirely hypothetical in its character (3243.).

3301. There are at present two, or rather three general hypotheses of the physical nature of magnetic action. First, that of æthers, carrying with it the idea of fluxes or currents, and this Euler has set forth in a simple manner to the unmathematical philosopher in his Letters[4];—in that hypothesis the magnetic fluid or æther is supposed to move in streams through magnets, and also the space and substances around them. Then there is the hypothesis of two magnetic fluids, which being present in all magnetic bodies, and accumulated at the poles of a magnet, exert attractions and repulsions upon portions of both fluids at a distance, and so cause the attractions and repulsions of the distant bodies containing them. Lastly, there is the hypothesis of Ampère, which assumes the existence of electrical currents round the particles of magnets, which currents, acting at a distance upon other particles having like currents, arranges them in the masses to which they belong, and so renders such masses

[1] From the Philosophical Magazine for February 1855.

[2] Phil. Trans. 1852, p. 25.

[3] Phil. Mag. 1852, June, p. 401.

[4] Euler's Letters, translated, 1802, vol. i. p. 214; vol. ii. pp. 240, 242. 244.

subject to the magnetic action. Each of these ideas is varied more or less by different philosophers, but the three distinct expressions of them which I have just given will suffice for my present purpose. My physico-hypothetical notion does not go so far in assumption as the second and third of these ideas, for it does not profess to say how the magnetic force is originated or sustained in a magnet; it falls in rather with the first view, yet does not assume so much. Accepting the magnet as a centre of power surrounded by lines of force, which, as representants of the power, are now justified by mathematical analysis (3302.), it views these lines as *physical* lines of power, essential both to the existence of the force within the magnet, and to its conveyance to, and exertion upon, magnetic bodies at a distance. Those who entertain in any degree the æther notion might consider these lines as currents, or progressive vibrations, or as stationary undulations, or as a state of tension. For many reasons they should be contemplated round a wire carrying an electric current, as well as when issuing from a magnetic pole.

3302. The attention of two very able men and eminent mathematicians has fallen upon my proposition to represent the magnetic power by lines of magnetic force; and it is to me a source of great gratification and much encouragement to find that they affirm the truthfulness and generality of the method of representation. Professor W. Thomson, in referring to a like view of lines of force applied to static electricity (1295. 1304.), and to Fourier's law of motion for heat, says that the lines of force give the same mathematical results as Coulomb's theory, and by more simple processes of analysis (if possible) than the latter[1]; and afterwards refers to the "strict foundation for an analogy on which the *conducting power of a magnetic medium for lines of force* may be spoken of[2]." Van Rees has published a mathematical paper on my lines of force in Dutch[3], which has been transferred into Poggendorff's *Annalen*[4], and of which I have only a very imperfect knowledge by translated abstracts. He objects, as I understand, to what I may call the physical part of my view as assigning no origin for the lines, and as not presenting the higher

[1] Phil. Mag. 1854, vol. viii. p. 53.

[2] Ibid. p. 56.

[3] Trans. Royal Acad. Sciences of Amsterdam, 1854, p. 17.

[4] Poggendorff's *Annalen*, 1853, vol. xc. p. 415.

principle conveyed by the idea of magnetic fluids or of electric currents: he says it does not displace the old theories, or render them superfluous; but I think I am right in believing, that, as far as the lines are taken to be representations of the power, he accepts them as correct representations, even to the full extent of the hypotheses, either of magnetic fluids or electric currents. It was always my intention to *avoid* substituting anything in place of these fluids or currents, that the mind might be delivered from the bondage of preconceived notions; but for those who desire an idea to rest upon, there is the old principle of the æthers.

3303. The encouragement I derive from this appreciation by mathematicians of the mode of figuring to one's self the magnetic forces by lines, emboldens me to dwell a little more upon the further point of the true but unknown natural magnetic action. Indeed, what we really want, is not a variety of different methods of representing the forces, but the one true physical signification of that which is rendered apparent to us by the phænomena, and the laws governing them. Of the two assumptions most usually entertained at present, magnetic fluids and electric currents, *one* must be wrong, perhaps *both* are; and I do not perceive that the mathematician, even though he may think that each contains a higher principle than any I have advanced, can tell the true from the false, or say that either is true. Neither of these views could have led the mind to the phænomena of diamagnetism, and I think not to the magnetic rotation of light; and I suppose that if the question of the possibility of diamagnetic phænomena could have been asked beforehand, a mathematician, guided by either hypothesis, must have denied that possibility. The notion that I have introduced complicates the matter still further, for it is inconsistent with either of the former views, so long as they depend exclusively upon action at a distance without intermediation; and yet in the form of lines of force it represents magnetic actions truly in all that is not hypothetical. So that there are now three fundamental notions, and *two* of them at least must be impossible, *i. e.* untrue.

3304. It is evident, therefore, that our physical views are very doubtful; and I think good would result from an endeavour to shake ourselves loose from such preconceptions as are contained in them, that we may contemplate for a time the force as much as possible in its purity. At present we cannot think of polarity without feeling ourselves drawn into one or the other of the two hypotheses of the origin of polar powers; and as mathematical considerations cannot give a decision, we feel as if the subject were in that same doubtful condition which hung over the conflicting theories of light prior to the researches of modern time; but as there the use of Wheatstone's reflector, combined with Arago's suggestion of a decisive experiment, and its realization by Leon Foucault, appear to have settled that question, so we may hope by a due exertion of judgement, united with experiment, to obtain a resolution of the magnetic difficulty also.

3305. If we could tell the *disposition* of the force of a magnet, first at the place of its origin, and next in the space around, we should then have attained to a very important position in the pursuit of our subject; and if we could do that, assuming little or nothing, then we should be in the very best condition for carrying the pursuit further. Supposing that we imagine the magnet a sort of sun (as there is every reason to believe that the sun is a magnet) polarized, with antithetical powers, ever filling all space around it with its curved beams, as either the sun or a candle fills space with luminous rays; and supposing that such a view takes equal position with either of the two former views in representing truly the disposition of the forces, and that mathematical considerations cannot at present decide which of the three views is either above or inferior to its co-rivals; it surely becomes necessary that physical reasoning should be brought to bear upon the subject as largely as possible. For if there be such physical lines of magnetic force as correspond (in having a real existence) to the rays of light, it does not seem so very impossible for experiment to touch *them*; and it must be very important to obtain an answer to the inquiry respecting their existence, especially as the answer is likely enough to be in the affirmative. I therefore purpose, without asserting anything regarding the physical hypothesis of the magnet more strongly than before (3299.), to call the attention of experimenters, in a somewhat desultory manner, to the subject again, both as respects the deficiency of the present physical views and the possible existence of lines of physical force, concentrating the observations I may have to make about a few points—as *polarity*, *duality*, &c., as occasion may best serve; and I am encouraged to make this endeavour by the following considerations. 1. The confirmation by mathematicians of the truthfulness of the abstract lines of force in representing the direction and amount of the magnetic power;—2. My own personal advantageous use of the lines on numerous occasions (3174.);—3. The close analogy of the magnetic force and the other dual powers, either in the static or dynamic state, and especially of the magnet with the voltaic battery or any other sustaining source of an electric current;—4. Euler's idea of magnetic æthers or circulating fluids; —5. The strong conviction expressed by Sir Isaac Newton, that even gravity cannot be carried on to produce a distant effect except by some interposed agent[1] fulfilling the conditions of a physical line of force;—6. The example of the conflict and final experimental settlement of the two theories of light.

3306. I believe that the use by me of the phrase "places of force" has been considered by some as objectionable, inasmuch as it would seem to anticipate the decision that there are physical lines of force. I will endeavour so to use it, if necessary, as not to imply the assertion. Nevertheless I may observe, that we use such a phrase in relation to a ray of light, even in those parts of the ray where it is not extinguished, and where therefore we have no better knowledge of it or its existence than in similar magnetic cases; and we also use the phrase when speaking of gravity in respect of places where no second body to gravitate upon is present, and where, when existing, it cannot, according to our present views, cause the gravitating force of the primary body, or even the determination of it, upon that particular place.

Magnetic polarity.

3307. The meaning of this phrase is rapidly becoming more and more uncertain. In the ordinary view, polarity does not

[1] Newton says, "That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial I have left to the consideration of my readers." See the third letter to Bentley.

necessarily touch much upon the idea of lines of physical force; yet in the one natural truth it must either be essential to, and identified with it, or else absolutely incompatible with, and opposed to it. Coulomb's view makes polarity to depend upon the resultant in direction of the action of two separated and distant portions of two magnetic fluids upon other like separated portions, which are either originally separate, as in a magnet, or are induced to separate, as in soft iron, by the action of the dominant magnet;—it is essential to this hypothesis that the polarity force of one name should repel polarity force of the same name and attract that of the other name. Ampère's view of polarity is, that there are no magnetic fluids, but that closed currents of electricity can exist round particles of matter (or round masses), and that the known experimental difference on the opposite sides of these currents, shown by attraction and repulsion of other currents, constitutes polarity. Ampère's view is modified (chiefly by addition) in various ways by Weber, De la Rive, Matteucci, and others. My view of polarity is founded upon the character in direction of the force itself, whatever the cause of that force may be, and asserts that when an electro-conducting body moving in a constant direction near or between bodies acting magnetically on themselves or each other, has a current in a constant direction produced in it, the magnetic polarity is the same; if the motion or the current be reversed, the contrary polarity is indicated. The indication is true either for the exterior or the interior of magnetic bodies whenever the electric current is produced, and depends upon the unknown but essential dual or antithetical nature of the force which we call magnetism (3154.).

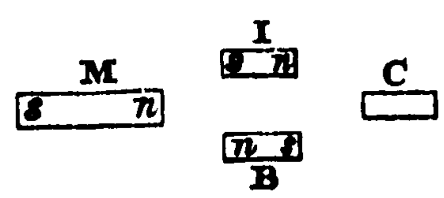
3308. The numerous meanings of the term polarity, and various interpretations of polarity indications at present current, show the increasing uncertainty of the idea and the word itself. Some consider that the mere set or attraction, or even repulsion, shown by a body when subject to a dominant magnet is sufficient to mark polarity, and I think it is as good a test as any more refined arrangement (2693.) when the old notion of polarity only is under consideration. Others require that two bodies under the power of a dominant magnet should by their actions show a mutual relation to each other before they can be considered as polar. Tyndall, without meaning to include any idea of the nature of the magnetic force, takes his type from soft iron, and considers that any body presenting the like or the antithetical phænomena which such iron would present under magnetic action, is in a like or antithetical state of polarity[1]. Thomson does not view two bodies which present these antithetical positions or phænomena as being necessarily the reverse of each other in what may be called their polar states[2], but, I think, looks more to differential action, and in that approaches towards the views held generally by E. Becquerel and myself. Matteucci considers that the whole mass of the polar body ought to be in dependence by its particles as a mass of iron is, and that a solution of iron and certain salts of iron have not poles, properly speaking, but that at the nearest points to the dominant pole there is the contrary magnetism to that of the pole, surrounded by the same magnetism as of the pole in the further part, the two ends of a bar of such matter between two dominant poles having no relation to each other[3]. Becquerel considers that polarity may in certain cases occur transverse to the length, and so produce results which others explain by reverse polarity. The views of very many parties always include the idea of the source of the polar action, whether that be supposed to depend on the accumulation of magnetic fluids at the chief poles of the dominant magnet, or the action of electric currents in a determinate position around its molecules; and such views are adhered to even when the polarity induced is of the reverse kind, as in bismuth, &c., to that of the inducing magnet. Others, like Weber, add to Ampère's hypothesis an idea of electricity, loose as regards the particles, though inseparably associated with the mass of the body under induction. Some, I think, make the polarity not altogether dependent upon the dominant magnet, but upon the neighbouring or surrounding substances; and I propose, if the physical lines of force should hereafter be justified, to make that which is commonly called polarity, in distinction from the true polarity (3307.), dependent upon the curvature of lines of force due to the better or worse magneto-conduction power of the substances presenting the usual polar phænomena (2818.).

3309. The views of polar action and of magnetism itself, as

[1] Athenæum, No. 1406, p. 1203.
[2] Ibid. column 3 at bottom.
[3] *Cours spécial sur l'induction, &c.*, p. 201.

formerly entertained, have been powerfully agitated by the discovery of diamagnetism. I was soon driven from my first supposition, that the N pole of a magnet induced like or N polarity in the near part of a piece of bismuth or phosphorus; but as that view has been sustained by very eminent men, who tie up with it the existence of magnetic fluids or closed electric currents as the source of magnetic power, it claims continued examination, for it will most likely be a touchstone and developer of real scientific truth, whichever way the arguments may prevail. To me the idea appears to involve, if not magnetic impossibilities, at least great contradiction and much confusion, some of which I proceed to state, but only with the desire of elucidating the general subject.

Fig. 1.



3310. If an ordinary magnet M, fig. 1, acting upon a piece of iron or other paramagnetic matter I, renders it polar by throwing its near end into the contrary or S state in the manner usually understood, and, acting upon a like piece of diamagnetic matter as bismuth B, renders it also polar, but with the near end in the same state; then B and I are for the time two magnets, and must act back upon the magnet M; or if they could be made able to retain their states after M is removed (and that is the case with I), would act as magnets upon a third piece of magnetic matter as C. When M acts upon I, it exerts its influence, according to the received theories, upon all the particles of the latter, bringing them into like polar position with itself, and these, consistently with the simple assumption, act also upon each other as particle magnets, and exalt the polarity of the whole mass in its two extremities. In like manner M should act upon B, polarizing the mass and all its particles; for the particles of the diamagnetic body B, even to the smallest, must be operated upon; and we know experimentally, that a tube filled with powdered bismuth acts as a bar of the metal does. But then, what is the mutual action of these bismuth particles on each other? for though all may be supposed to have a reverse polarity to that of M, they cannot in that case be reverse in respect of each other. All must have like polarity, and the N of one particle must be opposed to the S of the next particle in the polarity direction. That these particles act on each other, must be true, and Tyndall's results on the effect of compression have proved that by the right means, namely experiment. If they were supposed to have no such action on each other, it would be in contradiction to the essential nature of magnetic action, and there would remain no reason to think that the magnet itself could act on the particles, or the particles react on it. If they acted on each other as the magnet is supposed to act on them, *i. e.* to induce contrary poles, then the power of the magnet would be nullified, and the more effectually the nearer the particles were together; whereas Tyndall has shown that the bismuth magnetic condition is exalted by such vicinity of the particles, and hence we have a further right to conclude that they do act on, or influence each other, to the exaltation of the state of the mass. But if the N-ness of one particle corresponds to, and aids in sustaining and exalting, the S-ness of the next particle, the whole mass must have the same kind of force; so that, as a magnet, its polarity must have the same kind of polarity as that of the particles themselves. For whether a particle of bismuth be considered as acting upon a neighbouring particle or upon a distant particle of bismuth, or whether a mass of particles be considered as acting on the distant particle, the action in both cases must be precisely of the same kind.

3311. But why should a polarized particle of bismuth acting upon another particle of bismuth produce in it like polarity, and with a particle of iron produce a contrary polarity? or why should masses of bismuth and iron, when they act as magnets (3310.), produce such different effects? If such were the case, then the N pole of a paramagnetic body would induce an S pole on the near end of an iron rod, whilst the N pole of a diamagnetic body would produce a pole contrary to the former, *i. e.* an N pole at the same end of the iron rod in the same position and place. This would be to assume two kinds of magnetism, *i. e.* two north fluids (or electric currents) and two south; and the northness of bismuth would differ from the northness of iron as much as pole from pole. Still more, the northness of bismuth and the southness of iron would be found to have exactly like qualities in all points, and to differ in nothing but name; and the southness of bismuth and northness of iron would also prove to be absolutely alike. What is this, in fact, but to say they *are* the same? and why should we not accept the confirmation and unfailing proof that it is so, which is given to us experimentally by the moving wire? (3307. 3356.)

3312. If we employ a magnet as the originally inducing body (3310.), and entertain the idea of magnetic fluids accumulated at the poles, which act by their power of attracting each other, but repelling their like, then the inconsistency of supposing that the north fluid of a given pole can attract the north fluid of one body and the south fluid of another, or that the north and south fluids of the dominant magnet can attract one and the same fluid in bismuth and in iron, &c., is very manifest. Or if we act by a solenoid or a helix of copper wire carrying an electric current instead of a magnet, and find that analogous effects are produced, are we to admit at once that the electric currents in it, acting upon the assumed electric circuits round the particles of matter, sometimes attract them on the one side and sometimes on the other? or if such bodies as bismuth and platinum are put into such a helix, are we to allow that currents in opposite directions are induced in them by one and the same inducing condition? and that, too, when all the other phænomena, and there are many, point to a uniformity of action as to direction with a variation only in power.

Media.

3313. Let us now consider for a time the action of different *media*, and the evidence they give in respect of polarity. If a weak solution of protosulphate of iron [1], *m*, be put into a selected thin glass tube about an inch long, and one-third or one-fourth of an inch in diameter, and sealed up hermetically (2279.), and be then suspended horizontally between the magnetic poles in the air, it will point axially, and behave in other respects as iron; if, instead of air between the poles, a solution of the same kind as *m*, but a little stronger, *n*, be substituted, the solution in the tube will point equatorially, or as bismuth. A like solution somewhat weaker than *m*, to be called *l*, enclosed in a similar tube, will behave like bismuth in air but like iron in water. Now these are precisely the actions which have been attributed to polarity, and by which the assumed reverse polarities of paramagnetic and diamagnetic bodies have been considered as esta-

[1] Let *l* contain 4 grains, *m* 8 grains, *n* 16 grains, and *o* 32 grains, of crystallized protosulphate of iron in each cubic inch of water.

blished; but when examined, how will ideas of polarity apply to these cases, or they to it? The solution *l* points and acts like bismuth in air and like iron in water; are we then to conclude that it has reverse polarity in these cases? and if so, what are the reasons and causes for such a singular contrast in that which must be considered as dependent upon its internal or molecular state?

3314. In the first place, no want of magnetic continuity of parts can have anything to do with the inversion of the phænomena; for it has been shown sufficiently by former experiments[1], that such solutions are as magnetically continuous in character as iron itself.

3315. In the next place, I think it is impossible to say that the medium interposed between the magnet and the suspended cylinder of fluid can cut off, or in any way affect the direct force of the former on the latter, so as to change the direction of its internal polarity. Let the tube be filled with the solution *m*, then if it be surrounded by the solution *l*, it will point as iron; if the stronger solution *n* surround it, it will point as bismuth; and with sufficient care a succession of these fluids may be arranged as indicated in figs. 2, 3, where the outlines between the poles represent the forms of thin glass troughs, and the letters the solutions in them. In fig. 2 we see that the action on *m* is the same as that on *m'*, and the pointing of the two portions is the same, *i. e.* equatorial; neither has the action on *m* been altered by the power of the poles having to traverse *n*, *m'* and *n'*; and in fig. 3 we see, that, under like circumstances of the power, *m'* points as bismuth and *m* as iron, though they are the same solution with each other and with the former *m m'* solutions. No cutting off of power by the media could cause these changes;—repetitions of position in the first case, and inversions in the second. All that could be expected from any such interceptions would be perhaps diminutions of action, but not inversions of polarity; and every consideration indicates that all the portions of

Fig. 2.

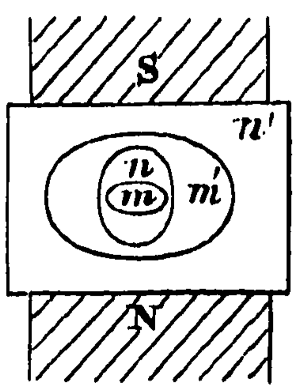
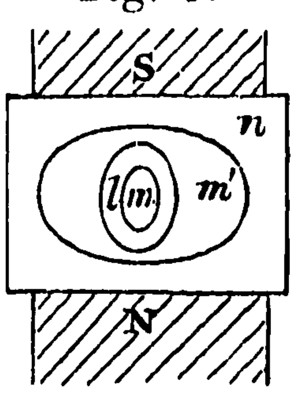


Fig. 3.



[1] Phil. Mag. 1846, vol. xxix. p. 254.

these solutions in the field at once have *like polarity, i. e.* like direction of force through them, and like internal condition; each solution in its complex arrangement being affected exactly in the same way and degree as if it filled the whole of the magnetic field, although in these particular arrangements it sometimes points like iron, and at other times like bismuth (2362. 2414.).

3316. These motions and pointings of the same or of different solutions, contain every action and indication which is supposed to distinguish the contrary polarities of paramagnetic and diamagnetic bodies from each other, and the solutions *l* and *m* in air repeat exactly the phænomena presented in air by phosphorus and platinum, which are respectively diamagnetic and paramagnetic substances. But we know that these actions are due to the differential result of the masses of the moving or setting solution and of that (or the air) surrounding it. No structural or internal polarity, having opposite directions, is necessary to account for them (2361. 2757.). If, therefore, it is still said that the solution *m* has one polarity in *l* and the reverse polarity in *n*, that would be to make the polarity depend upon the *mass* of *m* independently of its particles; for it can hardly be supposed that the particles of *m* are more affected by the influence upon them of the surrounding medium (itself under like inductive action only, and almost insensible as a magnet), than they are by the dominant magnet[1]. It would be also to make the polarity of *m* as much, or more, dependent upon the surrounding medium than upon the magnet itself;—and it would be, to make the masses of *m* and *l* and even their *form* the determining cause of the polarity; which would remove polarity altogether from dependence upon internal molecular condition, and, I think, destroy the last remains of the usual idea. For my own part, I cannot conceive that when a little sphere of *m* in the solution *l* is attracted upon the approach of a given magnetic pole, and repelled under the action of the same pole when it is in the solution *n*, its particles are in the two cases polar in two opposite

[1] If the polarity of the inner mass of solution is dependent upon that of the outer, and cannot be affected but through it, then why is not air and space admitted as being in effective magnetic relation to the bodies surrounded by them? How else could a distant body be acted upon by a magnet, if the inner solution of sulphate of iron is so acted on? Are we to assume one mode of action by contiguous masses or particles in one case, and another through distance in another case?

directions; or that if for a north magnetic pole it is the *near* side of the particles of *m* when in *l* that assume the south state, it is the *further* side which acquires the same state when the solution *l* is changed for *n*. Nor can I think that when the particles of *m* have the same polar state in both solutions, the whole, as a mass, can have the opposite states.

3317. These differential results run on in one uninterrupted course from the extreme of paramagnetic bodies to the extreme of diamagnetic bodies; and there is no substance within the series which, in association with those on each side of it, may not be made to present in itself the appearances and action which are considered as indicating the opposite polarities of iron and bismuth. How then is their case, in the one or the other condition, to be distinguished from the assumed polarity conditions of bismuth or of iron? —only, I think, by assuming other points which beg the whole question. In the first place, it must be, or is assumed, that no magnetic force exists in the space around a magnet when it is in a vacuum, it being denied that the power either crosses or reaches a locality in that space until some material substance, as the bismuth or iron, is there. It is assumed that the space is in a state of magnetic darkness (3305.), an assumption so large, considering the knowledge we have of natural powers, and especially of dual forces, that there is none larger in any part of magnetic or electric science, and is the very point which of all others should be held in doubt and pursued by experimental investigation. It is as if one should say, there is no light or form of light in the space between the sun and the earth because that space is invisible to the eye. Newton himself durst not make a like assumption even in the case of gravitation (3305.), but most carefully guards himself and warns others against it, and Euler[1] seems to follow him in this matter. Such an assumption, however, enables the parties who make it to dismiss the consideration of differential effects when bodies are placed in a vacuum, and to divide the bodies into the well-known double series of paramagnetic and diamagnetic substances. But in the second place, even then, those who assume the reverse polarity of diamagnetic bodies, must assume also that the state set up in them by induction is less favourable to either the exercise or the transmission of the magnetic force than the original unpo-

[1] Letters, &c. translated. Letter LXVIII., or pp. 260–262.

larized state of the bismuth; an assumption which is, I think, contrary to the natural action and final stable condition into which the physical forces tend to bring all bodies subject to them. That a magnet acting on a piece of iron should so determine and dispose of the forces as to make the magnet and iron mutually accordant in their action, I can conceive; but that it should throw the bismuth into a state which would make it repel the magnet, whereas if unaffected it should be so far favourable as to be at least indifferent, is what I cannot imagine to myself. In the third place, those who rest their ideas on *magnetic fluids*, must assume that in all diamagnetic cases, and in them only, the fundamental idea of their mutual action must not only be set aside but inverted, so that the hypothesis would be at war with itself; and those who assume that *electric currents* are the cause of magnetic effects, would have to give up the law of their inducing action (as far as we know it) in all cases of diamagnetism, at the very same moment when, if they approached the diamagnetic bismuth in the form of a spiral to the pole, they would have a current produced in it *according* to that law.

Time.

3318. I will venture another thought or two regarding the condition into which diamagnetic bodies are brought by the act of magnetic induction, in connexion with the point of *time*. It appears, as far as I remember, that all natural forces tend to produce a state of rest, except in cases where vital or organic powers are concerned; and that as in *life* the actions are for ever progressive, and have respect to a future rather than a present state (Paget), so all *inorganic* exertions of force tend to bring about a stable and permanent condition, having as the result a state of rest, *i. e.* a static condition of the powers.

3319. Applying this consideration to the case of bismuth in the magnetic field, it seems to me more like the truth of nature that the state assumed by the bismuth should be one more favourable to the final and static exercise of the power of the dominant magnet upon it, than that state belonging to the bismuth before it had suffered or undergone the induction; exactly as in soft iron we know that before it has acquired the state which a dominant magnet can induce upon it, it is not so favourable to the final static condition of the powers as it is afterwards.

Now it is very manifest, by numerous forms of experiment, that *time* enters as an element into ordinary magnetic and magneto-electric actions, and there is every reason to expect, into diamagnetic actions also; and it is also well known that we can take advantage of this time, and test the state of a piece of iron in the magnetic field before it has attained its finally induced state, and afterwards;—as, for instance, by placing it with a helix round it in the magnetic field and quickly connecting the helix *afterwards* with a galvanometer, when a current of electricity in such direction as to prove the truth of the statement will be obtained. In other forms of experiment, and with large pieces of iron, the time which can be so separated or snatched up during the act of progressive induction will amount to a minute or more. Supposing this could be done in any sensible degree with diamagnetic bodies, then the following considerations present themselves. A globe or bar of bismuth in the magnetic field may have its states, before and after induction, considered as separated by a moment of time; if the induction raises up a state of polarity the reverse of that of the magnet, then the bismuth ought to be more favourable to the determination of magnetic force upon it *before* the induction than *after*; whereas if, according to my view, the polarity is not reversed, but is the same as that of the magnet, the metal ought to be more favourable to the determination of magnetic force upon or through it *after* induction than *before*. Believing this to be an experiment which would settle the question of reverse polarity, and perhaps the existence or non-existence of physical lines of magnetic force, I have made many attempts in various ways, and especially by alternating motions of cylinders and balls of bismuth between soft iron magnetic poles furnished with helices, to obtain some results due to the *time* of induction, but have been as yet unable to succeed. I cannot doubt that time is concerned; but it seems to be so brief in period as to be inappreciable by the means I have employed.

3320. Professor Thomson has put this matter of time and polarity in another form. If a globe of bismuth be placed without friction in the middle of the magnetic field, it will not point or move because of its shape; but if it have reverse polarity, it will be in a state of unstable equilibrium; and if *time* be an element, then the ball, being once moved on its axis ever so little, would then have its polarity inclined to the magnetic axis, and would go on revolving for ever, producing a perpetual motion. I do not see how this consequence can be avoided, and therefore cannot admit the principles on which it rests. The idea of a perpetual motion produced by static forces is philosophically illogical and impossible, and so I think is the polarly opposed or adverse static condition to which I have already referred.

3321. It is not necessary here that I should refer to the manner in which my view of the lines of magnetic force meet these cases, for it has been done in former papers (2797. &c.); but I will call the attention of those who like to pursue the subject, to a true case of reverse polarity in the magnetic field (Experimental Researches, 3238, fig. 15), and there they will easily see and comprehend the beginning of the rotation of Professor Thomson's bismuth globe, and its continuance, if, as supposed, the polar state represented in the figure could be continually renewed.

3322. When the north pole of a magnet repels a piece of bismuth in a vacuum, or makes a bar of it set equatorially, and is found to produce like actions with many paramagnetic bodies when surrounded by media a little more paramagnetic than themselves, and with as many diamagnetic bodies when surrounded by media a little less diamagnetic, it would seem more cautious in the first instance to inquire how these latter motions take place, and how it is that parts, which with the paramagnetics have certainly been brought into a south condition by the north end of the pole, recede from it; and to apply these results in the first instance to those obtained with bismuth in a vacuum, before we assume a total change in principle, and yet an exceptional change as to substances, in the general law of magnetic polarity, without any cause assigned than, or any supporting facts beyond, the effect in question.

Curved lines of magnetic force—dependence of the dualities.

3323. The representative idea of lines of magnetic force which I entertain, includes in it the thought of the curvature of these lines, not as a merely convenient notion making the idea of the lines more manageable, but as one flowing from and suggested, if not proved, by the phænomena themselves. It is in this point of view that I proceed to consider it; and as the proof of the curvature is, in respect of *principle*, in the essential and necessary dependence of the two qualities or parts of a dual force upon each other (3324. &c.); and in respect of *experiment*, by the numerous results supplied during the mutual actions of magnets and magnetic bodies and the phænomena of moving conductors (3337. &c.), I will consider each in turn.

3324. There is no known case of one form or part of a dual power existing otherwise than with, and in dependence on, the other, which then exists simultaneously to an equivalent, *i. e.* an equal, degree. In static electricity, where supposed electric fluids are considered as being separated from each other, they are in equal amount (1177.), are ever related to each other (1681.), often by curved lines of force (1215.), and the existence of the one electricity without the other, or in the smallest degree of excess or deficiency, is absolutely impossible (1174.). In the voltaic battery, or in the electric current produced in any other way, as by thermo arrangements or inductions, the current in one part of the circuit is absolutely the same in amount and in dual character as in another; and in the insulated, unconnected voltaic battery, where the sustaining power is internal, not the slightest development of the forces, or of either of them, can occur until circuit is completed, or induction allowed at the extremities; for if, when there is no circuit, the induction be prevented, not merely no current, but no stock of electricity at the battery poles ready to produce a current can be evolved in the slightest degree. In like manner I am fully persuaded that the northness and southness of magnetism (in whatever they may be supposed to consist) cannot exist alone;—nor without exact proportion to each other;—nor without mutual dependence upon each other;—but that they are subject to the mutual relation and dependence of all dual force.

3325. Let us consider a hard invariable magnet in space, fig. 4. If a piece of soft iron, I, be brought towards it, the N end of the magnet will cause southness in the near end of the iron and northness in the further end, and this will continue until the iron is removed, the southness and northness at the two ends or halves of the magnet having remained all the time unchanged in their equality and

Fig. 4.

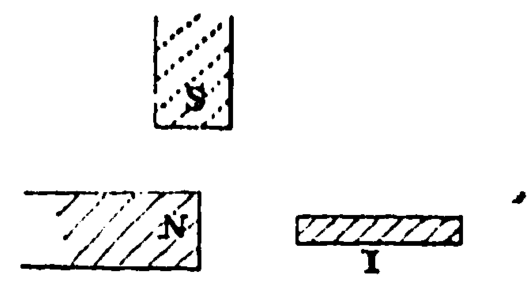
amount (3223. 3221.). Now to say that the force emanating from N could act on the iron, producing like and the contrary force, and then, by removal of the iron, cease to act there *or elsewhere*; and then again act on the iron if approached, or anything else, and then cease to act, and so on; would in my mind be to deny the *conservation of force*:—and we know that there is no equivalent action within the magnet, to explain by any alternate excitement and suppression of the dual parts, any supposed appearance and disappearance of the powers at the different times; for a helix closely applied round the middle part of the magnet during the experiment gives no current, and by that shows that there is no equivalent *internal* derangement of the power, when the outer exercise of it may be supposed to change between active and inert.

3326. Suppose the power of such a magnet to be due to magnetic N and S fluids; can it be thought that the N particles can be sometimes exerting their attraction for S particles, and sometimes not? Would not that be equivalent to the assumption of a suppression, *i. e.* a destruction of force?—which surely cannot be. Such an assumption could be surpassed only by that which supposes that the N fluid might sometimes attract S and repel N, and at other times repel S and attract N fluids (3311. 3312. 3317.).

3327. As to the soft iron under induction (3325.), its dual magnetic forces do re-enter into their former mutually dependent and mutually satisfying state: but suppose it to be replaced by steel, and that the magnetisms produced in it do not recombine or disappear on the removal of the dominant magnet, then on what is their power ultimately turned, *if not on each other*? (3257. 3324.) Where is the S power of the steel disposed of when it is separated from its relation with the N power of the magnet that evolved it? The case cannot be met except by affirming the independent existence of the two powers (3329.); or, admitting the suppression of force, and of either of these forces the one without the other (3330.); or allowing the mutual dependence of the two polarities of the magnet (3331.).

3328. When the N pole of a magnet (fig. 5) is acting in free space, its force is sensible around to a certain amount (114.); when a piece of soft iron, I, is brought near it, much of its force gathers up upon that iron, but the whole amount of

Fig. 5.



force from and about the N pole is the same; when an S pole is brought up, either of another magnet or of itself (for the effect is precisely the same), much of the force exerted upon the iron is removed from it, and falls upon the S pole, but the amount of force about the pole N remains the same; all of which can be proved experimentally by a helix on the soft iron and loops carried over the N pole (3218. 3223.). Indeed the way in which the power of one pole over either iron or bismuth is affected and diminished by the approximation on the same side of a contrary pole, is perfectly well known, and there are hundreds of cases in which the disposition in direction of the magnetic power can be varied in a great variety of ways, without the slightest change in the *sum* of its amount at the source, each of which gives evidence of the antithetical and inseparable condition of the two forms of force.

3329. As to independent existence of the two powers (3327.), how is it then that they cannot be shown separately?—not even up to the degree which is exhibited, so to say, by static electricity. There is nothing like a charge of northness or a charge of southness in any one of the innumerable phænomena presented by magnetism (3341.). The two are just as closely connected as the two electricities of a voltaic battery; whether we consider it as giving the current when properly connected, or exhibiting induction at its extremities when unconnected. The difficulty, indeed, is to find a fact which gives one the least hold for consideration of the thought that the two magnetic forces can be separated, or considered apart from each other.

3330. As to the suppression of force (3327.), I conceive that the creation, annihilation, or suppression of force, and still more emphatically of one form only of a dual force, is as impossible as the like of matter. All that is permitted under the general laws of nature is to displace, remove, and otherwise employ it; and these conditions are as true of the smallest suppression of a force, or part of a force, as of the suppression of the whole. I may further ask, whether, as it is *physically* impossible to annihilate or suppress force, it is not also *mathematically* impossible to do so, consistently with the law of the conservation of force?

3331. If we say that the forces in the cases of removal (3327.) are disposed of, sometimes in one direction and sometimes in another, but with the preservation of their full and equivalent amount, then how are we to consider them disposed of in the case of a cylinder or globular magnet, placed in air or *vacuo*, so as to be entirely self-dependent?—or in the case of a magnetic sphere placed in an inverted position in the magnetic field, so as to be entirely surrounded and enclosed by magnetic forces having a contrary direction to its own (3321. 3238.)?

3332. If we say that the dualities of such a magnet are dependent on each other (which is the third case (3327.)), then we have to consider how this can be, consistently with the distant mutual action, either of magnetic fluids or electric currents, acting in right lines only. Such action must then be through the body of the magnet (3260.). If we confine our attention to magnetic fluids, then the direction of their forces towards each other through the magnet when it is alone, must be of the like nature as their direction to approached iron, in which they are supposed to induce collections of the contrary fluids, or towards the fluids at the contrary poles of approached equal or superior magnets; *i. e.* the two poles of the magnet must be conceived of as centres of force, sometimes exerting their power towards each other in a given direction through the body of the magnet, and at other times exerting them outwardly to external poles in a direction exactly the contrary. But the currents which are evolved by the rotation of the magnet, or of discs of metal combined with it (3119. 3163.), show that the direction of the force (which is its polarity) is not thus reverse in the two halves of the case, but is the same within the magnet as in the prolongation of direction through and beyond the pole; and also, that whether the magnet be alone, and therefore supposed to have the polar forces exerted on each other through it, or be in relation to outer magnets, so as to have this exertion of force entirely removed from its interior, still it is always the same; having in both cases the same condition, direction, and amount of power within it (3116.).

3333. If the charged and polar state of the magnet be supposed to depend upon molecular electric currents, held by some internal condition in a position of parallelism, it is impossible that these can act backwards upon each other through the magnet in straight lines, so as to put the northness and southness of the pole in mutual dependence, as they are supposed to be in relation to external poles, without the currents themselves being *displaced and turned*, until the whole magnet is neutralized; falling back into the undeveloped state, just as a piece of soft iron falls back. When this return of state happens in soft iron or steel in any degree, a helix round these shows the induced currents consequent on such a change; and a loop (3133. 3217.) shows the difference when the iron or magnet is polar outwards and when its state has fallen. No such effects happen with a hard magnet, when it is alternately left to itself or put in relation to external poles of other magnets. The body of the magnet, and the forces passing through it, remain unchanged, whether examined by the loop (3223.) or by its own motion, or that of discs and wires associated with it (3116. &c.). Its force ever remains the same in quantity and general direction.

3334. The case of the steel ring magnet (3283.) is well known, and the manner in which such a magnet, showing no external relation, developes strong poles when it is broken. The phænomena assure us, I think, that when broken the northness and southness then appearing, cannot, when the pieces are by themselves, be determined upon each other backward through the magnet; there is no sufficient reason to suppose such a thing. And, again, the mutual destruction of highly-charged linear magnets, such as steel needles, when many of them are made into a thick, short bundle, shows the same thing; for if when alone the polar powers are not external, but are determined upon each other through each individual magnet, they are as free for a like disposal when the elementary magnets are associated as when they are separated:—and then there remains no sufficient reason to expect a dominant action over each other superior to that which each has over itself.

3335. It is not to be supposed that the change of force which occurs when the magnet first acting externally is then made to act internally or through itself, would be small and unnoticeable. It should be as great as the whole amount of power which the magnet can show under the most favourable circumstance; and the means are abundantly sufficient, by moving wires and discs, to make that evident in any case which might imply its passing through, or being removed out of, the magnet:—so that no difficulty can occur in that respect, and there remains, therefore, in my mind, but two suppositions; either the N polar force of a magnet when taken off from external compensating S polar force, is not exerted elsewhere as magnetic force at all; or else it is externally thrown upon and associated with the S polar force of the same magnet, and so sustained and disposed of, for the time, in its natural, equivalent, and essential state. If converted into any new form of power, what is that form? where is it disposed of? by what effects is it recognized? what are the proofs of its existence? To these inquiries there are no answers. But if it be directed externally upon the opposite S pole of the magnet, then all the consequences and foundations of my hypotheses of magnetic force and its polarity come forth; and, as I incline to believe, a consistent and satisfactory account of all magnetic phænomena, short of the idea of the nature of the magnetic force itself, is supplied.

3336. For if the dual forces of the poles of a magnet in free space are related to, and dependent upon, each other, and yet not through the magnet (3331.), then it must be through the space around. Then space must have a real magnetic relation to the force passing across it, just as it has to the ray of light passing from an illuminating to an illuminated body. Then the directions in which the two forces are exerted upon each other cannot be in right lines, which must, if they existed, pass of necessity through the magnet; but in curved lines, seeing that it is impossible that any but curved lines can hold the poles in relation to each other through the surrounding space (3297.):—and if they be curved lines, then I cannot imagine them to be anything else than physical lines of force; lines fitted to transfer the power onwards in consistency with its inevitable dual relation, and in conformity with that direction which ought, as I think, to be properly called *polarity*. And it further appears to me, that if we once admit the magnetic relation of a vacuum, then all the phænomena of paramagnetic and diamagnetic bodies; of differential polarity and individual polarity; of solutions, needles, crystals and moving conductors, are presented in a simple mutual relation, without any contradiction of fact or hypothesis, and in perfect harmony with each other.

3337. I wish to avoid prolonging this paper by a repetition of the considerations and reasons already advanced on former occasions, and therefore will very briefly call to mind the idea I have put forth, that there are such lines of force in the space around a magnet; that the mutual dependence of the dualities, which is essential in the isolated magnet, is thus sustained; and that bodies in this space produce paramagnetic or diamagnetic phænomena, according as they favour or oppose the degree of sustaining power which mere space possesses. That these bodies, or *media* as they may be called, have a magnetic relation like that of space, is easily shown by numerous experimental results; but as they have a further relation amongst themselves, dependent upon their relative electro-conducting power, I think a little time may be usefully employed in considering how far the consequent results illustrate the probable condition of space where they are not present. Consider a magnetic pole N, fig. 6, placed in relation to an equal magnetic pole S, so that their powers are mutually related and sustained, and the space between them, *a*, *a*, *a*, occupied by a vacuum, nitrogen, or some other gas at magnetic zero (2770. &c.):—the force exerted by N on S, or reciprocally, is easily taken cognizance of by spirals, &c. as regards any change in direction or degree. Then consider the medium *a*, *a*, *a* to be all copper or all mercury, still the forces are undisturbed: or consider it part mercury or copper, and part vacuum or glass, divided either by a line running from S to N, or along *a a a*, or any other way, still the forces are undisturbed; any of these media act exactly like space, or so like it, we can scarcely trace a difference. Then consider the metal moving, either as a finely divided stream at *a*, *a*, *a*, or as a solid globe (of copper) C, fig. 7, revolving rapidly round the line from N to S; still it is exactly like the vacuum or indifferent gas or glass, and there is no effect as yet by which we might distinguish the material medium from the mere space. But let the stream of metallic particles be converted into a continuous plate, and then we know it becomes filled with abundant currents of electricity; or if we apply the wires of a galvanometer to the revolving copper globe C, at the axial and equatorial parts, we can then cause it to

Fig. 6.

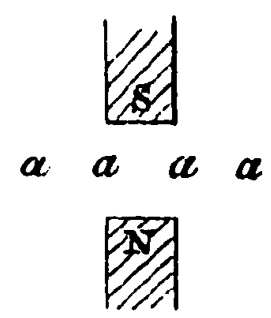
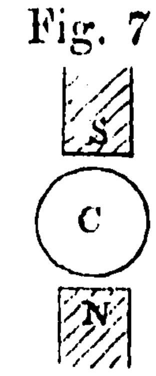


Fig. 7.

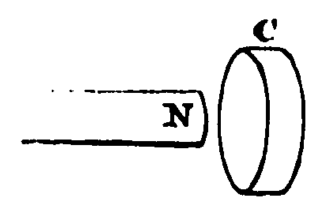


develope (by permission of currents) a new effect, and the currents are sent out most abundantly by the conductors applied. If the copper globe C be rapidly revolved upon an axis perpendicular to the line S N, so strong and influential a medium is it, magnetically considered, that the two poles, N and S, if free to move, do move in the same direction as the near parts of the globe; and are absolutely carried away from each other, in opposition to their mutually attractive force, which tends strongly all the while to draw them together. Now, how is it possible to conceive that the copper or mercury could have this power in the moving state, if it had no relation at all to the magnetic force in the fixed state? or, that it should have like power in the compact state, and yet have no relation to the magnetic force in the divided and moving state? The mere addition of motion could do nothing, unless there were a prior static dependence of the magnet and the metal upon each other. We know very well that the actions in the moving cases involve the evolution, or a tendency to the evolution, of electric currents; but that knowledge is further proof that the metals *are* in prior relation to the magnetic forces; and as bodies, even down to aqueous solution, have these electric currents set up in them under like circumstances, we have full reason to believe that all bodies when in the magnetic field are in like static relation as the copper when not moving:—and that when motion is superadded, they would all evolve electric currents, were it not for their bad electro-conducting powers.

3338. These effects of motion are known to be identical with those of the moving wire (36. 55.), or those of voltaic induction (6. &c.); and their intensity and power is very well shown in the force of Elkington's magneto-electric apparatus and Ruhmkorff's induction coil. *Time* is concerned in their production, and Professor Henry has shown us, in some degree, that when the currents are moving in helices, the magnetic action across them is for a time cut off or deflected (1730.). These actions are, in every case, simple; *i. e.* a line of force in a given polar direction produces, or tends to produce, in a body moving across it, whether paramagnetic, neutral, or diamagnetic (3146. 3162.), a current in the *like* direction; which current must, as I conceive, be dependent upon a previous like *static* state. Nothing in the slightest degree analogous to the supposed oppositely polar states of paramagnetic and diamagnetic bodies has ever been discovered amongst them;—and it has never been said, or supposed, as far as I know, that the two actions, *i. e.* the magnetic and the magneto-electric, are separate in their essential nature, or that they are not the consistent and accordant, and I must add reciprocal, actions of one force.

3339. That the copper, &c. are effectual as magnetic media when in the field, may be stated also thus:—

Fig. 8.



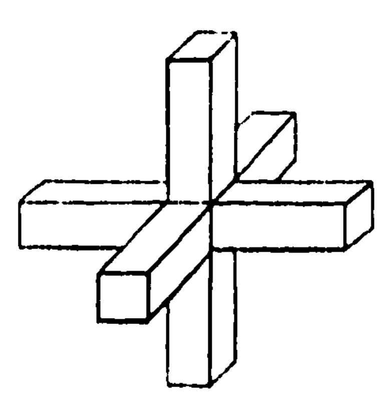
Let N, fig. 8, be a magnetic pole, and C a thick disc or short cylinder of copper. If the copper revolve ever so rapidly on its axis, there will be no production of currents in it; and the magnetic action of N on other magnets will be the same, as if the metal were quiescent or even away. If N recede from C there are then currents in C, though it be not moving; and though the effect of N upon other magnets, as far as we know them, is unchanged; yet there is then a slight attraction between C and the N pole. If N be made to approach C, the reverse currents and actions occur. As N approaches or recedes more quickly or slowly, the currents produced, and consequent temporary magnetic state, are higher. A cylinder electro-magnet will show these effects very well. The copper has all the time been still, no motion has been purposely given to it; it has been affected by the approximation and recession of the pole, has passed from one state to another, which states remain stationary as long as the poles are quiescent, and it shows every character of a medium affected by the magnetic force. By expedients the currents in the copper may be allowed or prevented; but whether they be allowed or not, the state the copper medium arrives at is the same. If disallowed as the magnet approaches, but allowed as it recedes, then the current due to the last change occurs, an effect easily shown with a magnet and helix; and this seems to prove very distinctly that the copper within the constant influence of the magnet has a permanent, static, magnetic condition; and is therefore a magnetic medium, having lines of force passing through it. If C be of bismuth instead of copper, the same currents in the same direction occur, though in a far smaller degree; and, as it is believed, only because of deficiency in its conducting power.

3340. There can be no doubt that very much is involved in these phænomena, of the nature of which we have little or no knowledge; and the results obtained by Matteucci will probably lead to developments and discoveries of great importance. He states[1] that copper, when finely divided, presents very persisting phænomena, proving its right to be considered as a diamagnetic body; but that when aggregated, all, or nearly all, its diamagnetic character disappears. Nothing is known as yet of the manner in which the mere difference of cohesion or division can so affect the diamagnetic character. He finds, too, that in other respects, as in Arago's rotation, particles of matter act in a manner not to be anticipated from what is at present known of them as masses; and it is to be hoped and expected that when these results are enlarged and developed, we shall be able to form a better judgement of the true physical action of magnetism than at present.

Places of no magnetic action.

3341. The essential relation and dependence of the two magnetic dualities is manifested, I think, in a very striking manner, by the results which occur when we attempt to isolate northness or southness, by concentrating either of them on one space or piece of matter, and looking for their presence by effects, either of tension or any other kind, whether connected with polarity or not. A soft iron bar, an inch square, 3 or 4 inches long and rounded at the edges, had thirty-two convolutions of covered copper wire 0·05 of an inch in diameter put round it, so that covering the middle part of the bar, chiefly, it could be shifted if needful a little nearer to one end than the other; such a bar could be rendered magnetic by an electric current passed through the wire, and a degree of adjustment, in the strength of the N and S extremities, could be effected by this motion of the iron in its helix. Having six of these, it was easy to arrange them with their like poles together, so as to include a cubical space or chamber, fig. 9; and in this space I worked by every means at my disposal. Access to it was easily obtained by a previous removal of a portion of the solid angles

Fig. 9.



[1] *Cours spécial sur l'induction, &c.*, 1854, pp. 165, 269.

of the ends which were to be brought together, or by withdrawing the electro-magnets a little the one from the others, and then a ray of light could be passed into or across it; magnetic needles or crystals of bismuth could be suspended in it;—a ring helix could be introduced and rotated there; and the motions of anything within could be observed by the eye outside.

3342. A small magnetic needle hung in the middle of this space, gave no indication of any magnetic power; near the open edges and angles vibrations occurred, but they were as nothing compared to the powerful indications given outside the chamber; even when the needle was many inches away. A crystal of bismuth was entirely indifferent. A piece of soft iron hung on a jointed copper wire within the chamber showed no trace of magnetic power, whether examined by the little needle or in any other manner. Iron filings on a card across the chamber were not affected in the middle part, but only near the partly open angles. A ring helix of many convolutions, having its terminations passing out at opposite corners, was connected with a very sensitive galvanometer and rotated; it showed no trace of inductive action. Numerous other experiments were made, but with results altogether negative. Attempts (though desperate) were made to ascertain if any electro-chemical conditions were induced there, but in vain. Every kind of trial that I could think of, not merely by tests of a polar character, but of all sorts, were instituted, but with the same negative result.

3343. It was of course not to be expected that any polar, *i. e.* any dually related polar, action could be exerted in this place; but if the polarities can exist without mutual relation, we might surely expect some condition, some tonic or static state, in a chamber thus prepared and surrounded with a high intensity of magnetic power, acting in great concentration on one particular spot or substance. But it is not so; and the chamber offers a space destitute of magnetic action, and free, under the circumstances, from magnetic influence. It is the complete analogue of the space presented within a deep metallic vessel or globe[1], when charged with electricity (1174.). There is then no electricity within, because that necessary connexion and dependence of the electric duals, which is essential to their nature, cannot be. In like manner, there is no appearance of magnetic force

[1] Phil. Mag., Oct. 1846, vol. xxix. p. 257, note.

in the cubical chamber, because the duals are not both there at once, and one cannot be present without the other.

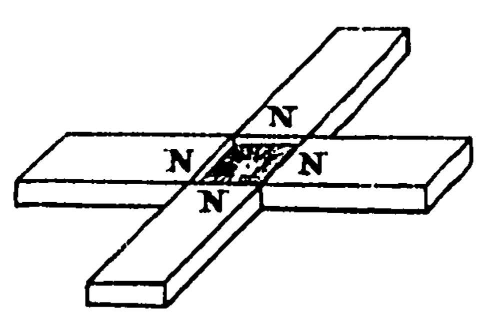
3344. There are many ways of examining in a more or less perfect manner these neutral and highly instructive magnetic places. A cavity in the end of an electro-magnetic core or a permanent magnet will present similar phænomena, and in some respects even more perfectly; for though a trace of power will perhaps appear at the bottom of the cavity, the sum or amount, as compared to the sum of power at the end of the magnet, will show how complete the analogy between this space and the interior of a metallic vessel charged with positive or negative electricity is. A cylinder of soft iron, 9 inches in length and 1·6 in diameter, had a chamber 0·9 in diameter and 1 inch in depth formed in one extremity concentric with the cylinder; and being placed in a powerful helix of thick copper wire, and associated with a Grove's battery of ten pair of plates, was ready for experiment:—a like chambered magnet can be prepared by putting a proper iron ring against the end of any electro- or ordinary magnet, and will show the phænomena I am about to describe. A piece of soft iron, not more than 0·3 of an inch in length or thickness, held at the end of a copper wire and brought near the outer edge of the excited magnet pole, will be very strongly attracted; but if it be applied to the bottom of the chamber it will present no such effect, but be quite indifferent. If applied about the sides of the chamber, it will indicate no effect until it approaches the mouth. If the magnet be placed horizontally, and a piece of card-board be cut, so that it can enter the chamber and represent a horizontal section of its cavity; and, being sprinkled over with clean iron filings, is then put into its position and the magnet excited for a moment that it may develope its power over the chamber and filings and give them their indicative position; it will be found that only those near the mouth have been driven into a new position (about the outside angles of the pole), and that four-fifths of those upon the surface of the card within the chamber have been left unaffected, unmoved. If the chamber be filled with iron filings, closed with a card, placed in a vertical position with the aperture downward, and the magnet be then excited and the card removed, the filings will fall out; as they come out they will be caught away, and form a fine fringe round the external angles of the pole, but not one will remain at the bottom of the chamber, or even anywhere within the chamber, except near to its external edge. Yet, if a piece of iron long enough to reach out of the chamber, as a nail 2, 3, or 4 inches long, touch the *bottom* of the chamber, it is strongly attracted and held there, and will support a weight of several ounces, though prevented from touching the chamber anywhere else by a card with a hole in it placed over the mouth.

3345. If a small magnetic needle, about 0·1 of an inch long, be brought towards this excited magnet, it is almost unmanageable by reason of the force exerted upon it; but, as soon as it has entered the chamber, the power rapidly diminishes, and at the bottom the needle is scarcely, if at all, affected.

3346. If, instead of the core and chamber described, an iron tube of sufficient thickness of metal (as part of a gun-barrel) be employed, then like effects occur. If the magnetic needle be introduced, it ceases to be acted upon when about 1·5 inch within the tube. If the tube be more or less filled with iron filings, and then be excited and held vertical, they will all pour out and fall away, except those which are retained at the external edges. Yet, if a long nail or iron rod be introduced, so as to be partly out of the cylinder, then it will be strongly attracted at the internal point, where it touches the iron of the tube core.

3347. The realization of like effects by grouping together the poles of ordinary magnets gives most interesting results. I have four very hard steel magnets, each 6 inches in length, 1 inch in breadth, and 0·4 nearly in thickness.

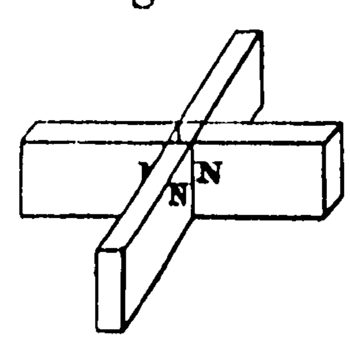
Fig. 10.



When the four like poles are put together, fig. 10, they form a flat square chamber in the same plane as that of the magnets. If a piece of stiff paper, the size of this chamber, be raised on a block 0·2 of an inch high, then sprinkled over with iron filings, and the magnets afterwards approached regularly until the square chamber is formed, a little tapping on the card will then arrange the filings in lines from the sides of the square chamber to the centre. The filings show at once the direction of the lines of force in this medium plane, and their greater abundance at the middle of each pole than at the re-entering angles; and if the filings be then removed and the indication of the course of the lines be followed out by a small magnetic needle, it will be found that the lines rise upwards from this plane above, and descend from it below, and then turn back upon their course in the free space over and beneath the arrangement towards the S poles of the different magnets. The condition will be understood in a moment, by considering the sphondyloids of power belonging to each magnet (3271.), and the manner in which they are associated when the four like poles come together.

3348. When the magnets are turned edges upwards, they form a vertical chamber 1 inch high and only 0·4 of an inch in width, and now phænomena like those just described occur, but only near the entrances to the chamber; as the little needle proceeds into the enclosed space, the power of the magnets becomes less and less, and at the middle of the chamber scarcely a trace remains; that place being, like the closed chamber, formed with six poles (3341.), or like the bottom of a chamber formed in the end of a magnetic pole, a neutral place, or place of no magnetic action.

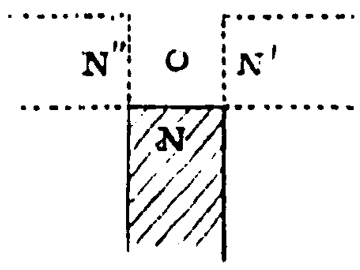
Fig. 11.



3349. The transition by degrees, from a pointed conical pole to an enclosed chamber, is, from the results described, very evident; and so also is their connexion with those belonging to the numerous neutral places produced under ordinary circumstances (3234. figs. 6, 10, 11, 15). Not the slightest difficulty or hesitation occurs, when these results are read or considered by the principle of representative lines of force; all the variations in the strength of the magnetic force and in the direction appear at once. But the great point is to observe how they all concur in showing the necessity of the complete and equivalent dual relation of the magnetic forces. When that is diminished or interfered with in any degree, in the same proportion does the power as a whole become diminished; until, at last, it absolutely disappears from a given place, though energies of the strongest kind are directing the force on to that spot, supposing that one of the dual elements could exist in any degree without, or independent of, the other.

3350. When formerly working with bismuth and magnets, I described several results (2298. 2487. 2491.) due to the principle of neutral magnetic places, more or less developed. If a sphere or cube of bismuth be delicately suspended by a vertical suspension or on a torsion balance, and an N pole be brought towards it, fig. 12, the bismuth will be repelled and the suspension deflected:—if a second N′ pole be brought up, as in the figure, the bismuth will be less repelled by N than before, will return towards it, and N′ will also seem to attract it, for on approaching the bismuth will tend to go into the angle formed by N and N′. If a third pole, N″, be brought up on the opposite side, the bismuth will then seem to be attracted by it, and by the first pole, and will, in fact, return very nearly into the position it would have if all the magnets were away. I thought at one time that magnetic structure, given by the second north pole N′ to the bismuth, might produce the approximation of it to N, and if so, that this would be neutralized by the action of a like pole N″ on the opposite side, and so the approximation of the bismuth (if due to such a cause) be prevented. On the contrary, however, such a pole *increased* it; and a moment's consideration, by showing that the three poles form a chamber of diminished or no action (3341. 3347.), shows also that such ought to be the case. All the movements of the bismuth are the result of the tendency which it has to pass from stronger to weaker places of magnetic action (2418.); and in the present case they show *that* weakened place, which in a higher degree would be a place of no magnetic force.

Fig. 12.

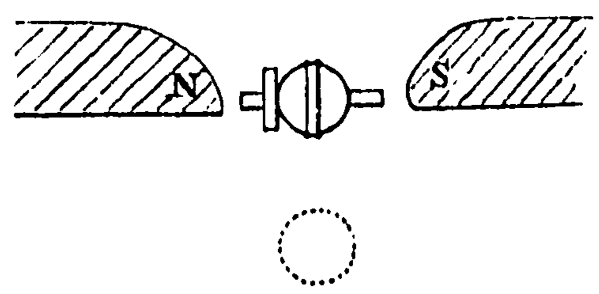


The moving conductor.

3351. I wish to make a few further remarks (3336. 3337.) upon the value of the moving conductor, as a means of investigation in magnetical science. It will be sufficient to refer to former papers for a statement of the principles, the power, and the certainty of its indications (3156. 3172. 3176. 3270.). At present, I desire to apply it in a direct form of experiment, to the supposed contrary polarities of iron and bismuth (3309.).

3352. Four metallic spheres of copper, bismuth, soft iron, and hard steel, 0·8 of an inch in diameter, have been prepared; each has a copper axis carrying a small wooden pulley, so that when in its supporting frame, rotation, more or less rapid, can be given to it by the band of a multiplying wheel; each also has a thin copper ring driven tightly on to it at the equator, which, being grooved, serves to retain a galvanometer wire pressed against that part during the revolution of the globe; the other wire meanwhile being held against the copper axis. These globes, in their frame, could be placed one by one in the magnetic field of a powerful permanent Logeman magnet, so as to be subject to the magnetic force, fig. 13; and then rotated, and the currents of electricity induced in them carried to galvanometers. Two such instruments were employed, one, a Ruhmkorff's, with fine wire (2651.), the other with a thick wire of only four revolutions (3178.). The latter was the best, but both gave good indications. The position of all things concerned was preserved undisturbed during the experiments, so that it will not be necessary to do more than to describe a standard effect, and afterwards refer other effects to it. This standard may be taken from the current indicated when the *copper* globe was in the magnetic field; and it was such, when the upper part of the globe moved westward, as to send the south ends of the galvanometer needles to the west also: eight or ten revolutions of the globe would cause the needles to pass through 80° or 90°.

Fig. 13.



3353. The *soft iron* sphere was placed in the magnetic field; it was so good in character as to retain very slight traces of magnetism when taken out again. Being revolved, it gave a current of electricity, the same in direction as that of the standard or copper ball. It is easy to understand, that if the globe be moved parallel to itself, but away from the magnet, in a line perpendicular to the magnetic axis (as into the dotted position, 3352. fig. 13), it will pass through places of weaker magnetic action. Under such changes of place, the induced current was weaker or stronger, according to the distance, but always in the same direction. Assuming that the rotating metal supplies a true indication of the polarity or direction of the magnetic force (3077.), the results show that the polarity of the force which induces these currents, and which is the magnetic force of the dominant magnet, is the same both in the copper and in the iron. Other cases of the current from revolving iron may be referred to in the Exp. Res. (3162.).

3354. The *bismuth* globe was placed in the magnetic field. If made to revolve much, with the galvanometer wire pressing against the copper equator (3352.), the latter became warm by friction, and a permanent thermo-current was produced: this has been considered on a former occasion (3168.). Its effect is easily eliminated by revolving the globe a given number of times in opposite directions, observing the two deflections, adding them together, and taking the half of the sum for the amount of induced current in either one direction or the other; for as the thermo-current is added on the one side and subtracted on the other, such a process gives the real amount of the induced current. When, however, the bismuth sphere is revolved only five or ten times, the thermo-effect is so small as to make the galvanometer deflection very little more in one direction than in the other. When due attention was given, the rotation of the bismuth sphere produced an induced current in precisely the same direction as those obtained with the copper and iron; and so far, therefore, it indicated precisely the same direction of polarity for the magnetic force then acting *upon* and *in* it.

3355. The *hard steel* sphere, having been previously examined by a small needle and found to be unmagnetized, was placed in the magnetic field. It was then revolved, and gave an induced magneto-electric current in the *same* direction as the former currents. Being removed and again examined by the magnetic needle, it was found not to have received any sensible charge of magnetism.

3356. So these four metal globes indicate like polarity of the magnetic force, acting upon and within them, when examined thus by the magneto-electric current due to movement across the lines of force. By researches described elsewhere, it is known that all metals, and all bodies which are sufficiently electro-conductors, down even to aqueous fluids, give the same direction of the magneto-electric current:—it is never reversed without reversion of the polarity, and reversion of the polarity *always* reverses the current induced.

3357. The hard steel sphere was now made *a magnet*, and though not of good shape to retain magnetism, yet because of its hardness it was able to sustain being placed in the magnetic field, in a position the reverse of the polarity there, and yet retained its own polarity; for when taken out and examined by a magnetic needle, the polarity was found to be the same as before. Such being the case, it seemed to me that this magnet might be employed to represent, according to the view of those who conceive that iron and bismuth are polarized in opposite directions in the magnetic field, both *iron* and *bismuth*; inasmuch as it could be placed in the field in that condition of polarity, which these are supposed respectively to acquire there. The *globe magnet* was therefore placed in the magnetic field in a position conformable to that of the dominant magnet, *i. e.* with its N pole towards the S pole of the magnet, &c.; and being rotated, it gave an induced magneto-electric current like that of the standard and of iron (3352. 3353.). The dominant magnet was then withdrawn to a distance (3353.) and the globe rotated by itself; it gave, as it ought to do, the same current as before; for it, by its coercitive force, retains permanently that state of polarity, which the iron could receive only temporarily whilst in the magnetic field: being now turned 180° in a horizontal direction, the globe magnet was reversed as regarded the dominant magnet (the latter being, however, still at a distance), and now the globe magnet gave a current the *reverse* of the former, or of the standard current; and yet a very consistent current in relation to its own polarity.

3358. The dominant magnet was now gradually brought up, and its effect on the reversed globe magnet observed. The current from the latter became less and less, and at last was inverted, becoming like that of the standard current; nor can that be wondered at, when it is considered that the dominant magnet was the largest supplied by Logeman to the Great Exhibition, and able to sustain a weight of 430 pounds, and the sphere magnet only 0·8 of an inch in diameter, and very imperfectly hardened in the interior. But when the dominant magnet was withdrawn a little, a place was soon found for the globe magnet, where its rotation in either direction produced no current at all. Outside of this place, the rotated sphere gave a current, the reverse of that of the standard; whilst the iron and bismuth spheres in the same place, gave currents alike in kind and the same as that of the standard. In this region, therefore (and it is like the whole of the magnetic field of many inferior yet very powerful magnets), if we represent bismuth by a magnet, reversely polar, as bismuth is supposed to be, we obtain induced magneto-electric currents, not like those of bismuth, but the contrary; and if we turn the representative magnet round, so as to give it the position in which it yields currents like those of the bismuth, then its polarity contradicts, or is the reverse of the assumed polarity of the bismuth.

3359. Now until the polarity or direction of the magnetic force which determines the course of the induced magneto-electric currents produced in every moving conductor, is distinguished and separated from the polarity or direction which causes movement amongst bodies subjected to the same force, how can these phænomena be accounted for by the supposition that the bismuth sphere is in the same polar condition as the reversed globe magnet? The reversed magnet is, in fact, the *contrary* to bismuth and to iron;—then bismuth and iron must be the same. The direct magnet is the *same* as the bismuth, in that polarity which induces a current;—then the magnet and the bismuth are the same. How easily all these effects present themselves in a consistent form, if read by the principle of representative lines of force! The reversed globe magnet at a distance from the dominant, shows, in revolving, the effect of the lines of force within it (3116.); as the magnet is approached, its external sphondyloid of power is compressed inwards (3238. fig. 15), and at last the magnet is self-contained; then showing the equalization of its own powers, and as yet the absence from within it of any of the powers of the chief magnet; so that it gives no induced currents, though in a place where bismuth and iron would give them freely. Within that distance the effect of the superior and overpowering force of the great magnet appears (3358.), which, though it can take partial possession of the little magnet, still, when removed, suffers the force of the latter to develope itself again, and present the same series of phænomena as before.

3360. Van Rees admits, I believe, that the moving wire shows truly the presence, direction, and nature of the magnetic force or forces; and it is very important to know that the setting of a magnetic needle, or crystal of bismuth, and the production of a current of electricity in a moving conductor, are like correlative and consequent effects of the magnetic force; the power of producing one or the other effect being rigidly the same. Philosophers should either agree or differ distinctly on this point; so that if they differ, they may distinguish clearly the physical separation of the phænomena; which, if established, must lead to new and important discoveries. The polarity direction which the moving conductor makes manifest, whether that conductor be one of the paramagnetic or diamagnetic bodies themselves, or whether it be a conductor moving amongst them, either by itself or with them, is *always the same.* The electric current produced never indicates a change in the direction of the polarity, from that belonging to the *first* source or seat of the power; whether it be a magnet, a solenoid, or of any other nature; the only difference being in the strength of the electric current produced, which difference is directly referable to the electro-conducting power (3143. 3152. 3163.). If such be the natural truth, how can the two modes of indication ever give opposite results? If opposite results seem to appear, and only occasionally, is it that mode of induction which gives one consistent result that we should doubt, or that which seems to be inconsistent with itself? especially when similar contrary phænomena in abundance are known to be produced by bodies having *like* polarity (3316.), and when excellent physical reasons, founded on differential action, offer themselves for their explication. There is sufficient reason to admit, that the magnetic needle cannot be always a true direct indicator of the amount or the direction of magnetic action (2868. 2870. 3156. 3293.). Should we not therefore, in respect of the above phænomena, rather conclude, for the time, that the simple and uniform results of the one mode of action, are the true indication; and that where, in the other mode, the phænomena are reversed or doubled, a part of them are *compound* in their nature? I may, in conclusion, remark, that the effects of motion and those produced in the action of magnetism on light, are never reversed in any case, whatever the medium in which they are observed; both point to one direction of polarity only, namely that of the dominant source of magnetism.

3361. I will bring these imperfect observations to an end, by a very brief statement of what I suppose to be the condition of a magnet; and by a disclaimer, as to anything like conviction on all points of that which I set forth as a supposition tending to lead to inquiry. Contemplating a bar magnet by itself, I see in it a source of dual power. I believe its dualities are essentially related to each other, and cannot exist but by that relation. I think that though related through the magnet by sustaining power, they are not so related by discharging or inducing power, a power equal in amount to the coercitive or sustaining power. The relation externally appears to me to be through the space around the magnet; in which space a sphondyloid of power is present consisting of closed curves of magnetic force. That the space is not magnetically dark (3305.) appears to me by this; that when bodies occupy that space, having like relation by known phænomena to the power as the space has, as copper, mercury, &c., they produce magneto-electric currents when moved. When bodies (media) occupy the space around the magnet, they modify its capability of transmitting and relating the dual forces of the magnet, and as they increase or diminish that capability, are paramagnetic or diamagnetic in their nature; giving rise to the phænomena which come under the term of magnetic conduction (2797.). The same magnet can hold different charges, as the medium connecting its poles varies; and so one, fully charged with a good medium, as iron between its poles, falls in power when the iron is replaced by air, or space, or bismuth. Corresponding effects occur with longer or shorter magnets (3290.), or with magnets made thick by adding many sideways together (3287.). The medium about a magnet may be mixed in its nature, and then more dual power is disposed of through the better conductor than the worse, but the whole amount of power remains unchanged. The powers and utility of the media, and of space itself, fail, if the dual force or polar action be interrupted. The magnet could not exist without a surrounding medium or space, and would be extinguished if deprived of it, and is extinguished, if the space be occupied adversely by the dual power of a dominant magnet of sufficient force. The polarity of each line of force is in the same direction throughout the whole of its closed course. Pointing in one direction or another, is a differential action due to the convergence or divergence of the lines of force upon the substance acted on, according as it is a better or a worse conductor of the magnetic force.

3362. But though such is my view, I put it forth with all the reservation made on former occasions (3244. 3299.). I do not pretend to explain all points of difficulty. I have no clear idea of the physical condition constituting the charged magnetic state; *i. e.* the state of the source of magnetic power:—or of the coercitivity by which that state is either resisted in its attainment, or sustained in its permanent condition; for the hypotheses as yet put forth give no satisfaction to my mind. I profess rather to point out the difficulties in the way of the views, which are at present somewhat too easily accepted, and to shake men's minds from their habitual trust in them; for, next to developing and expounding, that appears to me the most useful and effectual way of really advancing the subject:—it is better to be aware, or even to suspect, we are wrong, than to be unconsciously or easily led to accept an error as right.

Royal Institution,
20 *Dec.* 1854.

On some Points of Magnetic Philosophy[1].

The magnetic and electric forms of power, being dual in their character, and also able to act at a distance, will probably aid greatly in the development of the nature of physical force generally; and if (as I believe) the dualities are essential to the forces, are always equal and equivalent to each other, and are so mutually dependent, that one cannot appear, or even exist without the other, the proof of the truth of such conditions would lead to many consequences of the highest importance to the philosophy of force generally. A few brief experiments with the electric power quickly place the dual cases before the contemplative mind. Thus, if a metallic vessel, as an ice-pail, be insulated and connected with a delicate gold-leaf electrometer, or other like instrument, and then an insulated metallic globe, half the diameter of the ice-pail, be charged with positive electricity and placed in the middle of the pail, the latter being for the moment uninsulated by a touch outside, and then left insulated again, the whole system will show no signs of electricity externally, nor will the electrometer be affected; but a carrier applied to the ball within the vessel will bring away from it positive electricity, showing its particular state of charge; or being applied to the lower inside surface of the vessel will bring away negative electricity, proving that it has the contrary state: or the duality may be proved by withdrawing the ball, when the vessel will show itself negative by the electrometer, and the ball will be found positive. That these dualities are equal, is further shown by replacing the ball within the vessel, observing the electrometer, bringing the ball and vessel in contact, and again observing the electrometer, which will remain unchanged; and finally withdrawing the ball, which comes away perfectly discharged, and leaves the vessel externally in its unchanged and previous state. So the electric dualities are equal, equivalent, and mutually sustained. To show that one cannot exist alone, insulate the metallic vessel, charge it strongly by contact with the machine or a Leyden jar, and

[1] Proceedings of the Royal Institution, January 19, 1855.

then dip the insulated ball into it; and after touching the bottom of the vessel with the ball, remove it, without touching the sides: it will be found absolutely free from charge, whatever its previous state may have been; for none but a single state can exist at the bottom of such a metallic vessel; and a single state, *i. e.* an unrelated duality, cannot exist alone.

The correspondent dualities, *i. e.* the northness and the southness, of the magnetic force are well known. For the purpose of insulating, if possible, one of these, and separating it in any degree from the other, numerous experiments have been made. Thus six equal electro-magnets, formed of square bars, were put together in the direction of three lines perpendicular to each other, so that their inner ends, being all alike in polarity, might enclose a cubical space and produce an experimental chamber. When excited, these magnets were very powerful in the outer direction, as was found by nails, filings, spirals, and needles; but within the chamber, walled in on every side by intense north poles, there was no power of any kind: filings were not arranged; small needles not affected, except as they by their own inducing powers caused arrangement of the force within; revolving wire helices produced no currents; the chamber was a place of no magnetic action. Ordinary magnetic poles of like nature produced corresponding results. A single pole presented its usual character, attracting iron, repelling bismuth; a like pole, at right angles to it, formed a re-entering angle, and there a weak place of magnetic action was caused; iron was attracted from it to the prominent corners; bismuth moved up into it; and a third like pole on the opposite side made the place of weak force still weaker and larger; another pole or two made it very weak; six poles brought it to the condition above described. Even four poles, put with their longer edges together, produced a lengthened chamber with two entrances; and a little needle being carried in at either entrance passed rapidly through spaces of weaker and weaker force, and found a part in the middle where magnetic action was not sensible.

Other very interesting results were obtained by making chambers in the polar extremities of electro-magnets. A cylinder magnet, whose core was 1·5 inch in diameter, had a concentric cylindrical chamber formed in the end, 0·7 in diameter, and 1·3 inch deep. When iron filings were brought near this excited pole, they clung around the outside, but none entered the cavity, except a very few near the outer edge. When they were purposely placed inside on a card they were quite indifferent to the excited pole, except that those near the mouth of the chamber moved out and were attracted to the outer edges. A piece of soft iron at the end of a copper wire was strongly attracted by the outer parts of the pole, but unaffected within. When the chamber was filled with iron filings and inverted, the magnet being excited, all those from the bottom and interior of the chamber fell out, many, however, being caught up by the outer parts of the pole. If pieces of iron, successively increasing from the size of a filing to a nail, a spike, and so on to a long bar, were brought into contact with the same point at the bottom of the inverted chamber, though the filing could not be held by attraction, nor the smaller pieces of iron, yet as soon as those were employed which reached to the level of the chamber mouth, or beyond it, attraction manifested itself; and with the larger pieces it rose so high, that a bar of some pounds weight could be held against the very spot that was not sufficient to retain an iron filing.

These and many other results prove experimentally, that the magnetic dualities cannot appear alone; and that when they are developed they are in equal proportions and essentially connected. For if not essentially connected, how could a magnet exist alone? Its power, evident when other magnets, or iron, or bismuth is near it, must, upon their removal, then take up some *other form*, or exist *without action*: the first has never been shown or even suspected; the second is an impossibility, being inconsistent with the conservation of force. But if the dualities of a single magnet are thrown upon each other, and so become mutually related, is that in right lines through the magnet, or in curved lines through the space around? That it is not in right lines through the magnet (it being a straight bar or sphere) is shown by this, that the proper means as a helix round the magnet, shows that the *internal* disposition of the force (coercitive or other) is not affected when the magnet is exerting its power on other magnets, or when left to itself (Experimental Researches, 3119. 3121. 3215. &c.); and like means show that the *external* disposition of the force is so affected: so that the force in right lines through the magnet does not change under the circumstances, whilst the force in external (and necessarily) curved lines does.

The polarity of bismuth or phosphorus in the magnetic field is one point amongst many others essentially dependent upon, and highly illustrative of the nature of, the magnetic force. The assumption that they have a polarity the reverse of that of paramagnetic bodies involves the consequence, that northness does not always repel northness or attract southness; or else leads to the assumption that there are two northnesses and two southnesses, and that these sometimes associate in pairs one way, and at other times in the contrary way. But leaving the assumptions and reverting to experiment, it was hoped that a forcible imitation of the imagined state of bismuth in the magnetic field, might illustrate its real state, and, for this purpose, recourse was had to the indications given by a moving conductor. Four spheres of copper, iron, bismuth, and hard steel have been prepared, and rotated upon an axis coincident with the magnetic axis of a powerful horseshoe magnet; each sphere has a ring of copper fixed on it as an equator, and the ends of a galvanometer wire were brought into contact with the axis and the equator of the revolving globe. Under these circumstances, the electric current produced in the moving globe was conveyed to the galvanometer, and became the indicator of the magnetic polarity of the spheres; the direction of rotation, and the poles of the magnet, being in all cases the same. When the copper sphere, as a standard, was revolved, deflection at the galvanometer occurred in a certain direction. When the iron sphere replaced the copper and was revolved, the deflection at the galvanometer was the same. When the bismuth sphere was employed, the deflection was still the same:—and it still remained the same when the steel sphere was rotated in the magnetic field. Hence, by this effect, which I believe to be a truthful and unvarying indication of polarity, the state of all the spheres was the same, and therefore the polarity of the magnetic force in the iron, copper, and bismuth, in every case alike. (Experimental Researches, 3164. &c.) The steel sphere was then magnetized in the direction of its axis, and was found to be so hard as to retain its own magnetic state when in a reverse direction between the poles of the dominant magnet, for upon its removal its magnetism remained unchanged. Experiments were then made in a selected position, where the dominant magnetic force was not too strong (a magnet able to lift 430 lbs. was used)—and it was found that when the steel magnet was placed in accordance, *i. e.* with its north pole opposite the south pole of the dominant magnet, the deflection was in the same direction as with the bismuth sphere; but when it was changed so as to be in the magnetic condition assigned by some to bismuth (*i. e.* with reversed polarities), it then differed from bismuth, producing the contrary deflection. For a further account of these considerations and investigations, a paper may be referred to, which will appear in the Philosophical Magazine[1].

It is, probably, of great importance that our thoughts should be stirred up at this time to a reconsideration of the general nature of physical force, and especially to those forms of it which are concerned in actions at a distance. These are, by the dual powers, connected very intimately with those which occur at insensible distances; and it is to be expected that the progress which physical science has made in latter times will enable us to approach this deep and difficult subject with far more advantage than any possessed by philosophers at former periods. At present we are accustomed to admit action at sensible distances, as of one magnet upon another, or of the sun upon the earth, as if such admission were itself a perfect answer to any inquiry into the nature of the physical means which cause distant bodies to affect each other; and the man who hesitates to admit the sufficiency of the answer, or of the assumption on which it rests, and asks for a more satisfactory account, runs some risk of appearing ridiculous or ignorant before the world of science. Yet Newton, who did more than any other man in demonstrating the law of action of distant bodies, including amongst such the sun and Saturn, which are 900 millions of miles apart, did not leave the subject without recording his well-considered judgement,

[1] See *ante*, p. 528; or Philosophical Magazine, 1855, vol. ix. p. 81.

that the mere attraction of distant portions of matter was not a sufficient or satisfactory thought for a philosopher. That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is, he says, to him a great absurdity. Gravity must be caused by an agent, acting constantly according to certain laws; but whether this agent be material or immaterial he leaves to the consideration of his readers. This is the onward-looking thought of one, who by his knowledge and like quality of mind, saw in the diamond an unctuous substance coagulated, when as yet it was known but as a transparent stone, and foretold the presence of a combustible substance in water a century before water was decomposed or hydrogen discovered; and I cannot help believing that the time is near at hand, when his thought regarding gravity will produce fruit:—and, with that impression, I shall venture a few considerations upon what appears to me the insufficiency of the usually accepted notions of gravity, and of those forces generally, which are supposed to act at a distance, having respect to the modern and philosophic view of the conservation and indestructibility of force.

The notion of the gravitating force is, with those who admit Newton's law, but go with him no further, that matter attracts matter with a strength which is inversely as the square of the distance. Consider, then, a mass of matter (or a particle), for which present purpose the sun will serve, and consider a globe like one of the planets, as our earth, either created or taken from distant space and placed near the sun as our earth is;—the attraction of gravity is then exerted, and we say that the sun attracts the earth, and also that the earth attracts the sun. But if the sun attracts the earth, that force of attraction must either arise *because* of the presence of the earth near the sun; or it must have *pre-existed* in the sun when the earth was not there. If we consider the first case, I think it will be exceedingly difficult to conceive that the sudden presence of the earth, 95 millions of miles from the sun, and having no previous physical connexion with it, nor any physical connexion caused by the mere circumstance of juxtaposition, should be able to raise up in the sun a power having no previous existence. As respects gravity, the earth must be considered as inert, previously, as the sun; and can have no more inducing or affecting power over the sun than the sun over it: both are assumed to be *without* power in the beginning of the case; how then can that power arise by their mere approximation or coexistence? That a body without force should raise up force in a body at a distance from it, is too hard to imagine; but it is harder still, if that can be possible, to accept the idea when we consider that it includes the *creation of force*. Force may be opposed by force, may be diverted, directed partially or exclusively, may even be converted, as far as we understand the matter, disappearing in one form to reappear in another; but it cannot be created or annihilated, or truly suspended, *i. e.* rendered existent without action or without its equivalent action. The conservation of power is now a thought deeply impressed upon the minds of philosophic men; and I think that, as a body, they admit that the creation or annihilation of force is equally impossible with the creation or annihilation of matter. But if we conceive the sun existing alone in space, exerting no force of gravitation exterior to it; and then conceive another sphere in space having like conditions, and that the two are brought towards each other; if we assume, that by their mutual presence each causes the other to act,—this is to assume not merely a creation of power, but a *double creation*, for both are supposed to rise from a previously inert to a powerful state. On their dissociation they, by the assumption, pass into the powerless state again, and this would be equivalent to the *annihilation* of force. It will be easily understood, that the case of the sun or the earth, or of any one of two or more acting bodies, is reciprocal;—and also that the variation of attraction, with any degree of approach or separation of the bodies, involves the same result of creation or annihilation of power as the creation or annihilation (which latter is only the total removal) of either of the acting bodies would do.

Such, I think, must be the character of the conclusion, if it be supposed that the attraction of the sun upon the earth arises *because* of the presence of the earth, and the attraction of the earth upon the sun, because of the presence of the sun; there remains the case of the power, or the efficient source of the power, having pre-existed in the sun (or the earth) *before* the earth (or the sun) was in presence. In the latter view it appears to me that, consistently with the conservation of force, one of three sub-cases must occur: either the gravitating force of the sun, when directed upon the earth, must be removed in an equivalent degree from some other bodies, and when taken off from the earth (by the disappearance of the latter) be disposed of on some other bodies;—or else it must take up some *new* form of power when it ceases to be gravitation, and consume some other form of power when it is developed as gravitation;—or else it must be *always* existing around the sun through infinite space. The first sub-case is not imagined by the usual hypothesis of gravitation, and will hardly be supposed probable; for, if it were true, it is scarcely possible that the effects should not have been observed by astronomers, when considering the motions of the planets in different positions with respect to each other and the sun. Moreover, gravitation is not assumed to be a dual power, and in them only as yet have such removals been observed by experiment or conceived by the mind. The second sub-case, or that of a new or another form of power, is also one which has never been imagined by others, in association with the theory of gravity. I made some endeavours, experimentally, to connect gravity with electricity, having this very object in view (Phil. Trans. 1851, p. 1); but the results were entirely negative. The view, if held for a moment, would imply that not merely the sun, but all matter, whatever its state, would have extra powers set up in it, if removed in any degree from gravitation; that the particles of a comet at its perihelion would have changed in character, by the conversion of some portion of their molecular force into the increased amount of gravitating force which they would then exert; and that at its aphelion, this extra gravitating force would have been converted back into some other kind of molecular force, having either the former or a new character: the conversion either way being to a perfectly equivalent degree. One could not even conceive of the diffusion of a cloud of dust, or its concentration into a stone, without supposing something of the same kind to occur; and I suppose that nobody will accept the idea as possible. The third sub-case remains, namely that the power is always existing around the sun and through infinite space, whether secondary bodies be there to be acted upon by gravitation or not; and not only around the sun, but around every particle of matter which has existence. This case of a constant necessary condition to action in space, when as respects the sun the earth is *not* in place, and of a certain gravitating action as the result of that previous condition when the earth *is* in place, I can conceive, consistently, as I think, with the conservation of force: and I think the case is that which Newton looked at in gravity; is, in philosophical respects, the same as that admitted by all in regard to light, heat, and radiant phænomena; and (in a sense even more general and extensive) is that now driven upon our attention in an especially forcible and instructive manner, by the phænomena of electricity and magnetism, because of their dependence on dual forms of power.

January 22, 1855.

Further Observations on associated cases, in Electric Induction, of Current and Static Effects[1].

Melloni, whose loss science must deeply feel, was engaged in the latter part of his life in investigations relating to static electricity, especially concerning induction, conduction, &c. He desired, in reference to these and the results I had published respecting the charge of, and conduction by, subterraneous and subaqueous insulated wires[2], to know whether there was any difference in the *time* of transmission through such wires, of currents having greater or less intensity, *i. e.* of currents from batteries of different numbers of plates. I applied to Mr. Latimer Clark on the subject; and he, with the same earnestness as on the former occasion, sought and seized the opportunity of making experiments of the like kind, and gave me the results, which I sent to Melloni. The latter published them with some observations in an Italian Journal (whose title is not on the paper which he sent to me), and soon after he was suddenly removed from us by death. As Mr. Clark's results are not yet known in this country, I have thought that a brief account of them would be valuable. His process records, by the printing telegraph of Bain, the results obtained with 768 miles of copper wire covered with gutta percha, and laid in the ground in four lines between London and Manchester, so connected that the beginning and the end of the whole length was in London. The following are his words, dated May 31, 1854:—

"I have tried a few experiments on the relative velocities of currents of different intensities, and I enclose you some strips of paper showing the results. I was unable to equalize the deflections of a galvanometer by currents of intensity with small plates as compared with currents from a few large plates, for no size of plate would make up for the deficiency in intensity. I allude to the form of experiment suggested by Melloni;—but I believe they will be of interest to him.

"The experiments were made through 768 miles of gutta percha wire, viz. from London to Manchester and back again

[1] From the Philosophical Magazine for March 1855.

[2] *Ante*, page 508; also Royal Institution Proceedings, i. 345; or Phil. Mag. 1854, vii. p. 197.

twice, with our ordinary sulphate of copper batteries, plates 3 inches square, and with intensities varying from 31 cells to sixteen times 31 cells, or 500 cells.

"In the accompanying strips the upper line indicates the time during which the current was sent, being made by a local arrangement.

"The second line (of dots) indicates *time by seconds*, being made by a pendulum vibrating seconds, and striking a light spring at the centre of its arc of vibration.

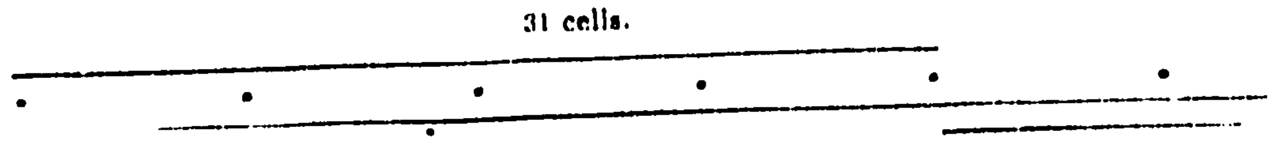
"The third line indicates the time at which the current appeared at (what we may call) the distant end of the line, 768 miles off.

"The fourth line merely shows the residual discharge from the near end of the wire, which was allowed to communicate with the earth as soon as the batteries were disconnected; this has no reference to the subject of our inquiries.

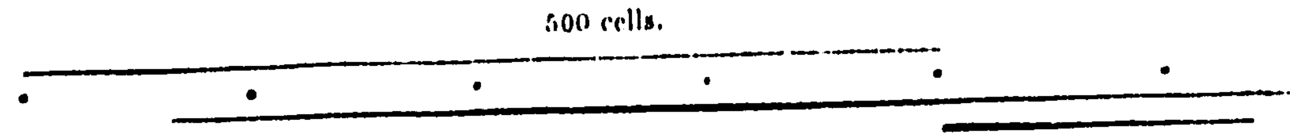
"It will be seen by the *third line*, that about two-thirds of a second elapsed in every case before the current became apparent at the distance of 768 miles, indicating a velocity of about 1000 miles a second; but the most interesting part appears to be, that this velocity is *sensibly uniform for all intensities* from 31 cells to 500."

Melloni has then given a copy of the records made when 31 pair and 500 pair of plates were employed; unfortunately the copy is inaccurate, since it makes the fourth line commence as to time at the termination of the third, whereas it ought to correspond with the termination of the first; also the third line on each does not thin off as those upon the record do. The following is a copy from other slips obtained at the same time from the Bain's printing apparatus. Experiments with 62, 125, and 250 cells, gave like results with those of 31 and 500 cells.

31 cells.



500 cells.



After certain observations, which are mainly upon the manner of the experiments, and the way in which practical difficulties were avoided, Melloni says, "It appears, then, that when the electric current possesses sufficient force to overcome the sum of the resistance offered by a given conductor, whatever its length may be, an augmentation of its intensity ten or twentyfold does not alter the velocity of its propagation. This fact is in open contradiction with the general meaning attributed to the denominations of *quantity* and *intensity*; since the first compares the mass of electricity to that of a fluid, and the second represents its elasticity or tendency to motion. The equal velocity of currents of various tension offers, on the contrary, a fine argument in favour of the opinion of those who suppose the electric current to be analogous to the vibrations of air under the action of sonorous bodies. As sounds, higher or lower in pitch, traverse in air the same space in the same time, whatever be the length or the intensity of the aërial wave formed by the vibration of the sonorous body; so the vibrations, more or less rapid or more or less vigorous, of the electric fluid excited by the action of batteries of a greater or smaller number of plates, are propagated in conductors with the same velocity. Every one will see how the hypotheses imagined by us to give a reason for natural phænomena, will serve to suggest certain experimental investigations, the results of which will test their validity or insufficiency."

Melloni then says that he shall shortly have occasion to publish facts which clearly demonstrate the errors of certain conclusions admitted up to the present time respecting electro-static induction; and I am aware, from written communications with him, that he considered the results arrived at by Coulomb, Poisson, and others since their time, as not accordant with the truth of nature[1]. In the mean time he died, and whether his researches are sufficiently perfected for publication or not, I do not know.

[1] He says, "I deceive myself much, or else the fundamental theorem of electrical induction, as we find it ordinarily announced, ought to be modified so as not to confound two effects completely distinct—the electric state during induction, and after the contact and separation of the inducing body. We know perfectly what occurs in the latter case, but not in the former," &c. Again, "In my last letter I raised doubts with regard to the consequences which have up to the present been deduced from the experiments serving as a base for the fundamental theorem of electro-static induction. These doubts have passed to a state of certitude in my mind, ... and behold me at this time thoroughly convinced that the enunciation of that theorem ought to be essentially modified." (July 1854.)

The uniformity in the time and appearance of currents of different intensities at the further end of the same wire in the same inductive state, is a very beautiful result. It might at first be supposed to be in opposition to the views I set forth some years ago on induction and conduction, and the statements more recently made with regard to *time*. That, however, does not appear to me to be the case, as a few further observations on Mr. Clark's recent experiments will perhaps show. When the smaller battery is used, much less electricity passes into the wire in a given time, than when the larger one is employed. Suppose that the batteries are so different that the quantities are as 1 to 10; then, though a pulse from each would take the same time for transmission through the wire, still it is evident that the wire would be a tenfold better conductor for the weak current than for the strong one; or in other words, that a wire having only one-tenth of the mass of that used for the greater current should be employed for the smaller one, if the resistance for *equal quantities* of electricity having different intensities is to be rendered equal.

My views connect the retardation of the transmitted current with the momentary induction set up laterally by the insulated and externally coated wire. The induction will be proportionate to the intensity, and therefore its especial effect on the time of retardation proportionately diminished with the less intense current,—a result of action which will aid in rendering the time of retardation of the two currents equal.

The difference of *time* in the former experiments with air wires, and earth or water wires, very clearly depends upon the difference of lateral induction; the air wire presented a retardation scarcely sensible, the earth wire one amounting to nearly two seconds. If the insulating layer of gutta percha could be reduced from 0·1 to 0·01 of an inch in thickness, and mercury could be placed on the outside of that instead of water or earth, I do not doubt that the time would be still more increased. Yet there is every probability that in any one of these varying cases, electric currents of high and of low intensity would appear at the end of the same long wire after equal intervals of time.

Mr. Clark's results may be stated thus:—A given quantity of electricity at a high intensity, or a smaller quantity at a proportionally lower intensity, will appear at the further end of the same wire after the lapse of the same period of time. My statement assumed the discharge of the *same* quantity at *different* intensities through the *same wire*, and the quantities in the illustrative experiments were measured by a Leyden jar. In the consideration and further development of these results, it must be remembered that it is not the difference either in time, velocity, or transmission of a *continuous* current which constitutes the object in view, for that is the same both for an air wire and a subterraneous wire, but it is the difference in the *first appearance* only of the same current when wires under these different conditions are employed. After the first appearance both wires are alike in power unto the end of the current, and then a difference again appears which is complementary to the first.

There are many variations of these experiments which one would wish to make, if possible, and perhaps by degrees the possibility, or else equivalent experiments in other forms, may occur. If the wire employed were changed from a cylinder to a flat ribbon of equal weight, or to several small wires, all being equally coated with gutta percha and submerged, differences would probably arise in the time of delay with the same current; and I think that the ribbon, presenting more induction surface than the cylinder, would cause more delay; but probably any one of these, or of like varieties, would cause the *same* delay for currents of different intensities. Again, one can scarcely doubt that with different conducting substances, as iron and copper, the delay would vary, as is the case in the transmission of sound and light. That the delay for currents of high and low intensity should be the same for the same wire in any one of such cases may still be expected, but it would be very interesting to *know* what would be the fact.

The prosecution of these results and the principles concerned in them, through the various forms they may assume by such like variations of the conductors and also of the currents, offers, as Melloni has observed, most extensive and interesting inquiries: even the power of a current to induce a current in neighbouring wires and conductors is involved in the inquiry, and also the phænomena and principles of magneto-electric induction.

Royal Institution,
Feb. 7, 1855.

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N.B. A dash rule represents the *italics* immediately preceding it. The references are sometimes to the individual paragraph, and sometimes to that and such as succeed it. Those which follow *p.* or *pp.* are to pages, the others to the paragraphs of the Experimental Researches.

THE END.

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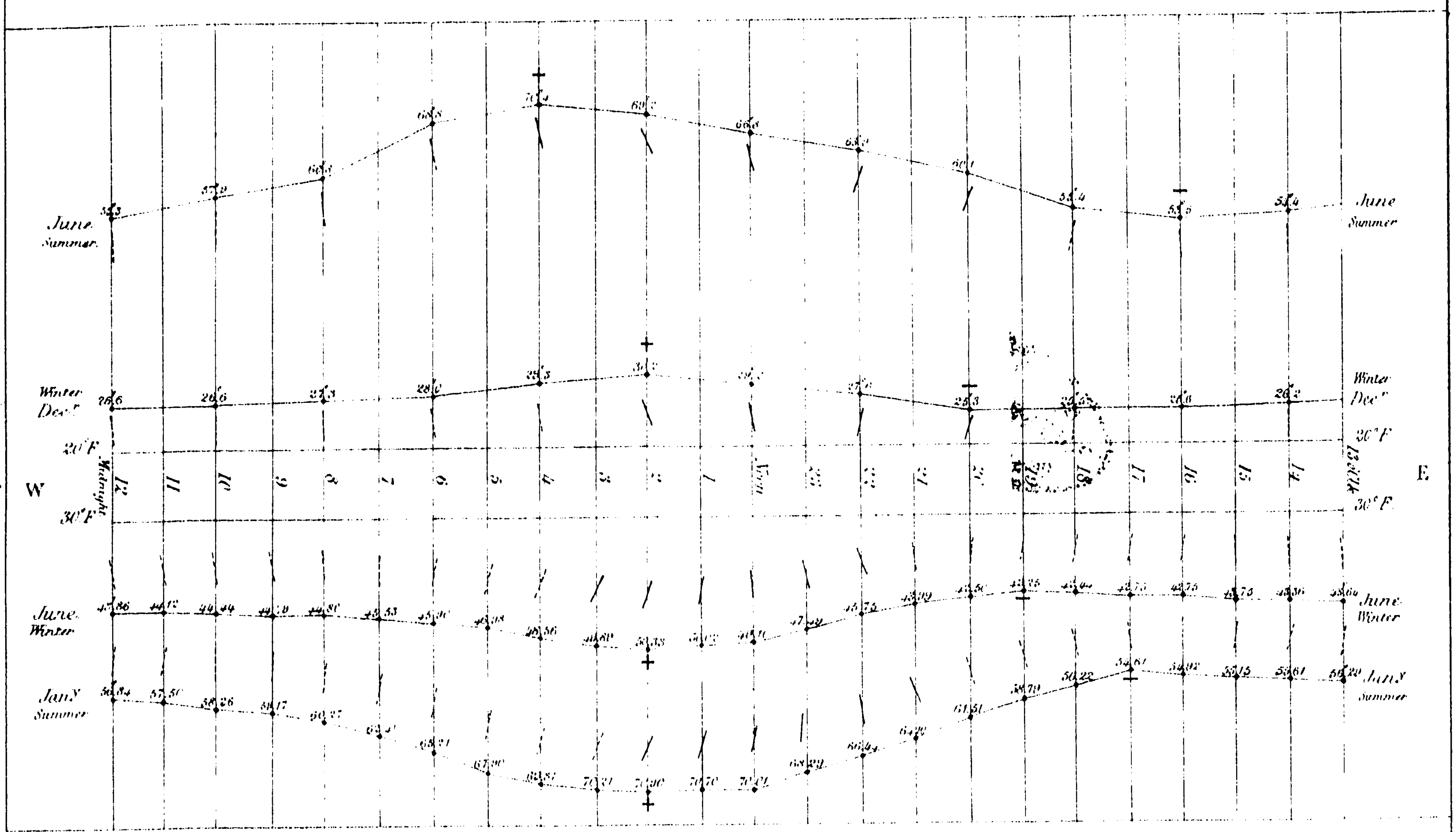
Fig. 12.

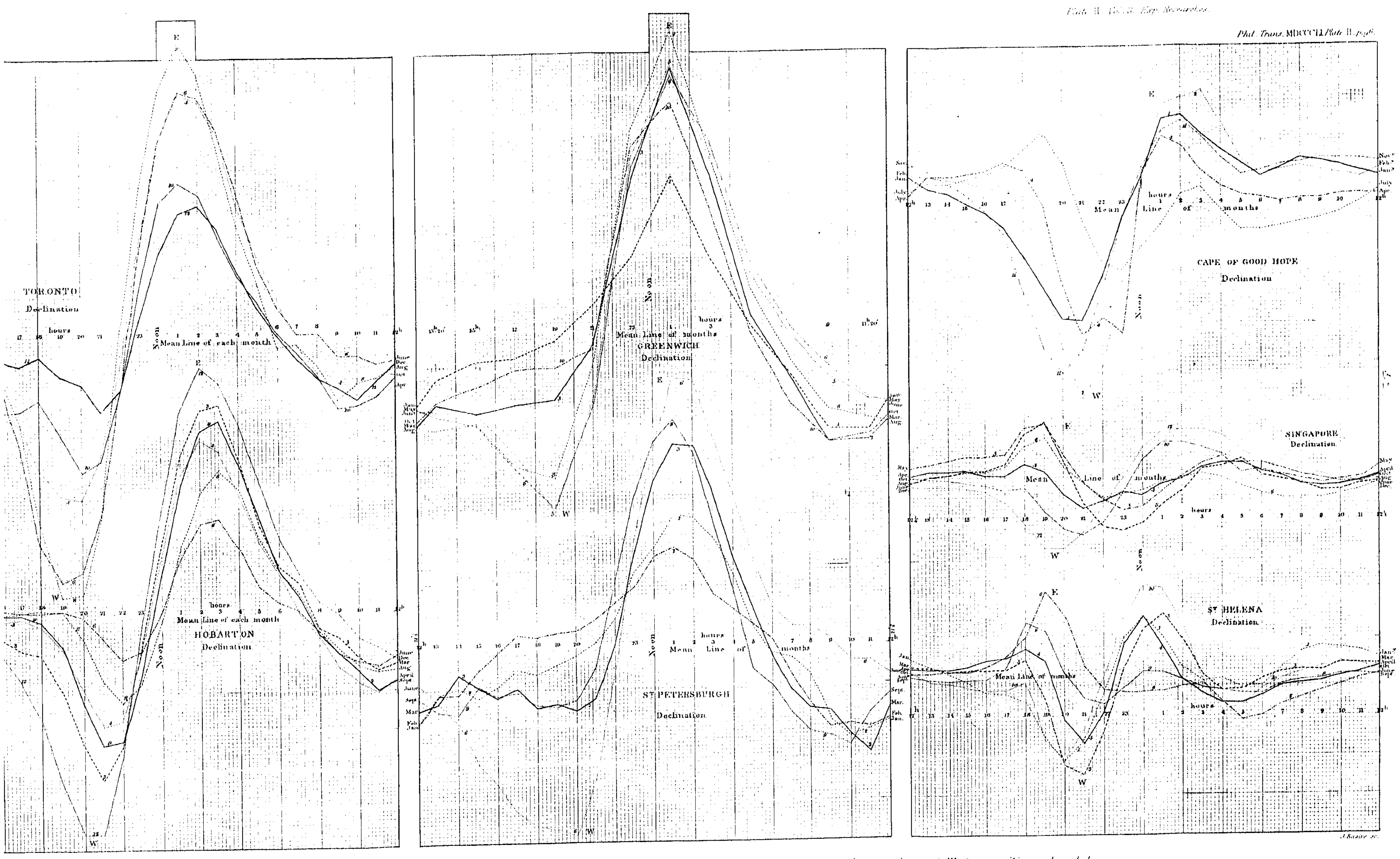
Lat. 43° 39′ 35″ N. Toronto Long^e. 79° 21′ 30″ W.



Lat. 42° 52′ 5 S. Hobarton Long^e. 147° 25′ 5 East.

Fig. 10.

J. Basire, Lith.



Eastern positions of the upper end of a free Needle are indicated when the curves are above the Mean Line of the months; and Western positions when below.

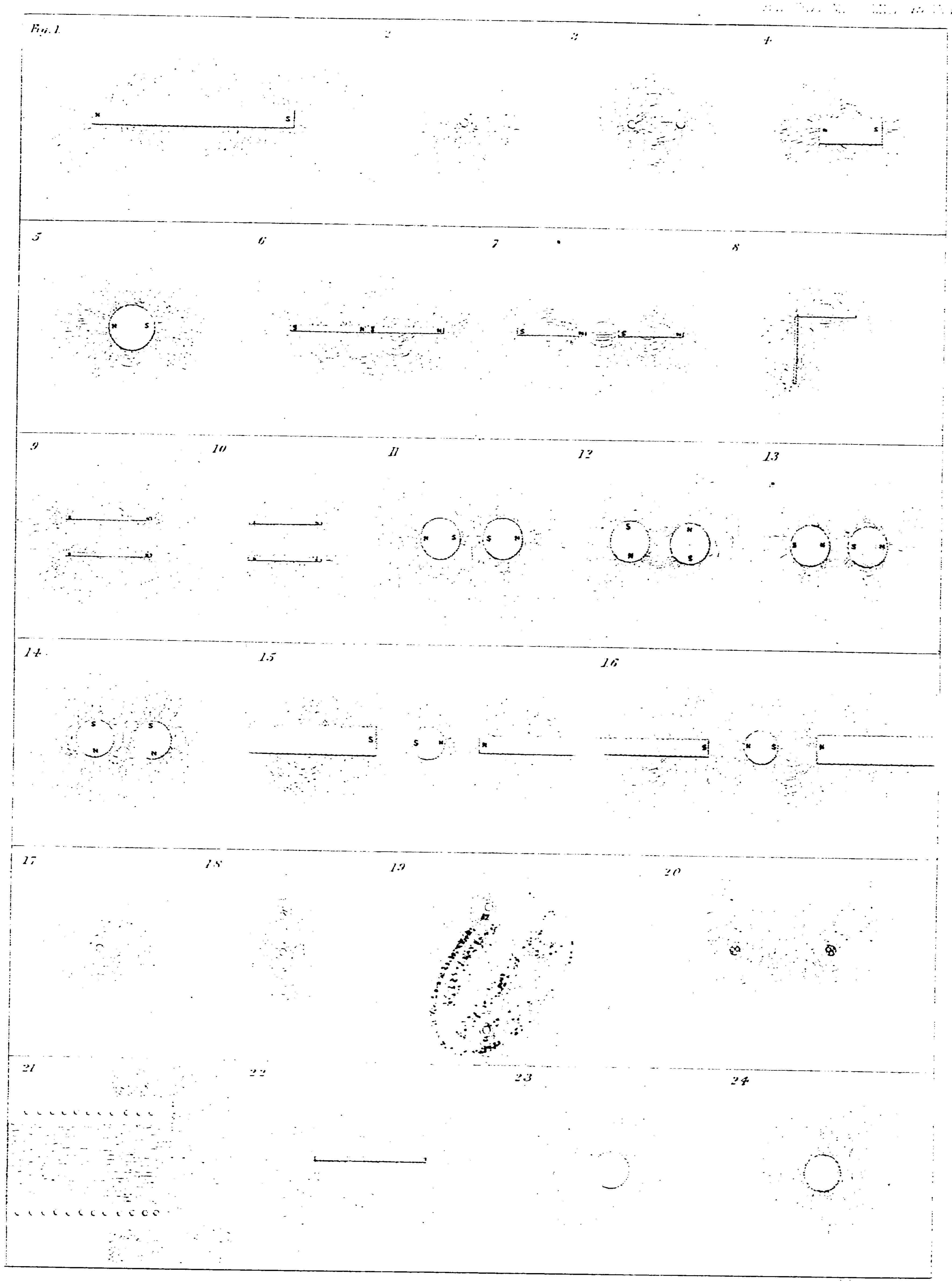

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Fig. 1

Fig. 2

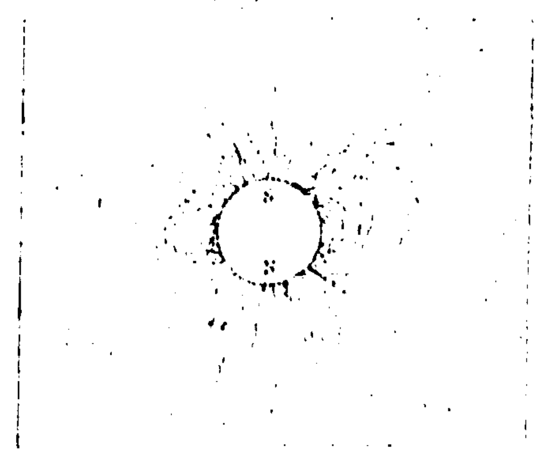
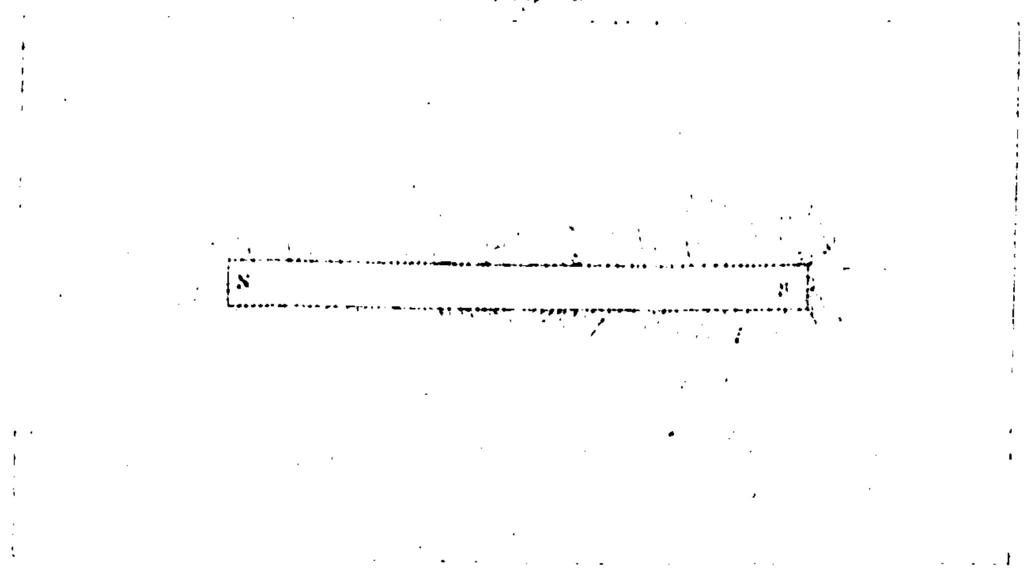


Fig. 3



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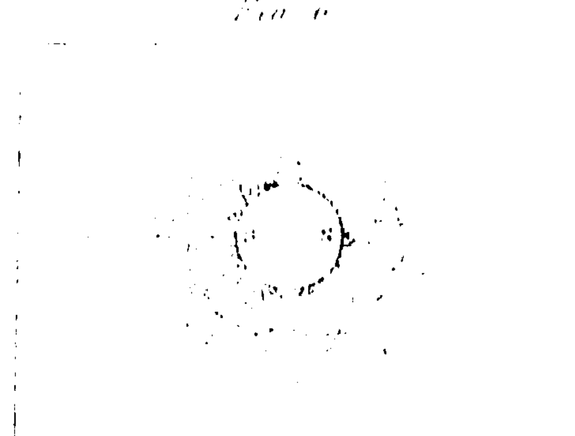

Fig. 7

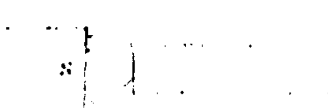

Fig. 8