XLII. *On Magneto-electric Induction; in a Letter to M. Gay-Lussac.* *By* MICHAEL FARADAY, *D.C.L., F.R.S.**

MY DEAR SIR,

I BEG to address to you the following pages upon the subject of electro-magnetism, and request the favour of their insertion in the *Annales de Chimie et de Physique*. They may, I fear, provoke a controversy that I would willingly avoid; but under the existing circumstances I feel compelled to adopt the present course of proceeding, for silence, should I maintain it, would be regarded as an admission of error, not only in a philosophical, but also in a moral point of view, from which I believe myself wholly exempt.

You will undoubtedly understand that I allude to the Memoir by Messrs. Nobili and Antinori. I address myself to you, because your judgement was sufficiently favourable to my former memoir for it to obtain a place in your excellent and truly philosophical Journal; and because Messrs. Nobili and Antinori's memoir being also inserted, the *Annales* contain all that has been written upon the subject. I therefore venture to hope that you will not refuse to admit the present article.

On the 24th of November, 1831, my first memoir, which you did me the honour to insert in the *Annales* for the month of May, 1832 (p. 5—69.), was read before the Royal Society; and it was the first announcement that I made of my researches in electricity.

On the 18th of December, 1831, I addressed a letter to my friend M. Hachette, which he was pleased to communicate to the Academy of Sciences on the 26th of the same month †.

[* Translated from the *Annales de Chimie et de Physique*, vol. li. p. 404. Mr. Faraday, in the preface to his collected "Experimental Researches in Electricity," published last year, and reviewed in L. and E. Phil. Mag., vol. xiv. p. 468, refers to several papers of his own, long since published, in the following terms. "Before concluding these lines I would beg leave to make "a reference or two; first, to my own papers on Electro-magnetic Rotations in the Quarterly Journal of Science, 1822, xii. 74. 186. 283. 416., "and also to my Letter on Magneto-electric Induction in the *Annales de* "*Chimie*, li. p. 404. These might, as to the matter, very properly have "appeared in this volume, but they would have interfered with it as a "simple reprint of the 'Experimental Researches' of the Philosophical "Transactions." As the papers here alluded to are now scarce, and as one of them has appeared in the French language only, we propose to transfer them in succession to our pages, in order that the purchasers of Mr. Faraday's volume may be enabled to possess, in a small compass, the entire series of his researches in electricity and magnetism. We begin with the letter on Magneto-electric induction, addressed to M. Gay-Lussac.—EDIT.]

† Vide the *Lycée*, No. 35.

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It was also inserted in the *Annales* for December, 1831 (p. 402.). My second series of Researches, bearing date of 21st of December, 1831, was read before the Royal Society on the 12th of January, 1832, and may be found in the *Annales* for June, 1832 (p. 113—162.). These are my only publications upon this subject, up to the present time, excepting a few notes appended to memoirs by other authors; and the whole were written, and publicly read, anterior to any publication on the same subject, by any individual whatever.

During this time, my letter to M. Hachette, which was inserted in the *Annales*, attracted the attention of Messrs. Nobili and Antinori, and those laborious philosophers published a memoir, of the date of 31st of January, 1832, and which was thus posterior to all my publications. This memoir is contained in the *Annales* for December, 1831 (p. 412—430.). A second memoir, entitled "*New Electro-magnetic Experiments*," by those gentlemen, dated 24th of March, 1832, appeared in the *Annales* for July (p. 280—304.).

My letter to M. Hachette, which in his kindness to me he read before the Academy of Sciences, has, I fear, become a source of error and misunderstanding, and has been productive of injury, rather than benefit, to the cause of philosophic truth. At the same time I know not how to explain my meaning, and place the facts in their proper light, without having the air of complaining in a manner of Messrs. Nobili and Antinori, which to me is particularly disagreeable. I respect those gentlemen, on account of all they have done, not in relation to electricity alone, but for the cause of science in general; and were it not that the contents of their memoirs oblige me to speak, and leave me only the alternative of admitting or denying the exactitude of their assertions, I should have passed unnoticed the scientific errors discoverable in them, leaving to others the task of animadversion. These gentlemen had, unfortunately, no further knowledge of my researches than they gathered from my short letter to M. Hachette, and without taking the trouble to refer to my memoirs, which in these circumstances I cannot but think they ought to have done, they at once misinterpret the sense of an expression relating to M. Arago's beautiful observations, assume that I had not previously done that for which they take credit to themselves; and finally, they advance what to me appear to be fallacious ideas upon the magneto-electric currents, and present these ideas as *corrections* of mine, though with mine they were as yet unacquainted.

First, allow me to rectify what I regard as the most important mistake of all, the false interpretation given of my

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words, for the correction of the errors committed in the experiments might have been left to time.

Messrs. Nobili and Antinori write (*Annales*, vol. xlviii. p. 428.), "Mr. Faraday considers M. Arago's magnetism of rotation to be entirely connected with the phænomenon which he discovered ten years ago. *He ascertained* THEN, *according to the statement in the notice, that by the rotation of a metallic disc under the influence of a magnet, electric currents may be produced in the direction of the radii of the disc in sufficiently considerable quantities to render the disc a new electrical machine.* We are entirely ignorant how Mr. Faraday discovered this fact, and we know not how a result of this nature *could remain* so LONG *generally unknown, and, so to speak,* FORGOTTEN in the hands of the discoverer," &c.

Now *I never said* what is here imputed to me. In my letter to M. Hachette, quoted at the head of the notice, I gave a short account of what I had recently discovered, and read before the Royal Society on the 24th of the preceding month. This notice may be found at page 402 of the same number of the *Annales*, and is as follows: "The fourth part of the memoir treats of M. Arago's equally curious and extraordinary experiment, which, as is known, consists in making a metallic disc revolve under the influence of a magnet. Mr. Faraday considers the phænomenon which is manifested in this experiment as *intimately connected* with that of magnetic rotation which he was so fortunate as to observe ten years ago. *He has ascertained that by the rotation* of the metallic disc under the influence of a magnet, electric currents *may be* formed in the direction of the radii of the disc, in sufficient number to render the disc a new electrical machine."

I never either said, or intended to say, that I had obtained these electric currents by the rotation of a metallic disc, at an epoch previous to the date of the memoir that I was then engaged in writing; but I said that the extraordinary effect discovered by M. Arago was connected in its nature with the electro-magnetic rotation I had discovered several years before, both being due to a tangential action; and that by the rotation of a disc near a magnet I could (at the time I was writing) cause currents of electricity to escape, or have a tendency to escape in the direction of the radii, thus rendering the disc a new electrical machine; and this I think is fully proved in the part of my memoir of which I gave a sketch: it may be seen in the *Annales*, vol. l. p. 65—118.

I am extremely desirous of explaining this error, because I

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have always admired M. Arago's prudence and philosophic reserve, in resisting the temptation to give a theory of the effect which he had discovered until he could offer one perfectly applicable, and in refusing his assent to the imperfect theories of others. Admiring him I adopted his reserve in this respect, and for that reason, perhaps, had my eyes open to recognise the truth as soon as it was presented.

We now arrive at that part of the subject which relates to the philosophy of my memoirs. The fourth part of my memoir of the 24th of November, 1831, contains my opinion on the cause of the phænomenon discovered by M. Arago, an opinion that even now I see no reason to alter. Messrs. Nobili and Antinori, in their papers of the 31st of January and 24th of March, 1832, animadvert upon certain errors which they attribute to me, and enter upon extended developments of magneto-electric phænomena. I cannot, however, discern that they have added a single fact to those contained in my memoirs, unless it be the obtaining of a spark with a common magnet, a result that I had myself previously obtained, but only with the electro-magnet. On the other hand, these gentlemen's memoirs appear to me to contain erroneous ideas upon the nature of magneto-electric currents; they exhibit also mistaken views as to the action and direction of those currents in Arago's revolving disc. They say, "*We have recently verified, extended, and perhaps corrected in some particulars the results of the English philosopher*," &c. (*Annales*, vol. l. p. 281.) And again at page 298, commenting on what they *suppose* to be my ideas (for though my papers had been read, and were published, they had not thought proper to consult them), they say, "We have already given our opinion upon this idea, but if at the commencement of our researches it were difficult to reconcile it with the nature of the currents discovered by M. Faraday, what can we say after all the new observations that we have made during the progress of our investigations? We say that we have a competent judge in the galvanometer, and that by its means the question must be decided."

With the most sincere desire to be set right when I am in error, I yet find it impossible to discover any corrections in the memoirs of these gentlemen by which I can profit; but I fully admit the competency of the galvanometer, and shall proceed as briefly as possible to submit our different ideas to its decision, in all that relates to Arago's phænomenon: and I am at the present time so satisfied with the facts and results stated in my published memoirs (though were I to rewrite

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them I should make alterations in some parts), that it will not be necessary to refer to any experiments that are not therein contained.

It is not my intention further to animadvert upon Messrs. Nobili and Antinori's first memoir. An English translation of it appeared in the Philosophical Magazine*, to which I added some corrections in the form of notes, copies of which I had the honour of sending to you, and to the authors. My present object is to compare the second part of their publications with the fourth part of my first memoir, and with parts of the other memoirs, as a means of throwing light upon the general principles. The intention of the two articles is to explain Arago's phænomenon, and as fortunately they are both contained in the fiftieth volume of the *Annales*, they may be referred to with facility. The reference to my own papers will be thus, (F. 114.), and to those of Messrs. Nobili and Antinori by a simple indication of the page of the *Annales*.

At page 281, after a few general remarks, we read, "We have recently verified, extended, and perhaps corrected in some particulars the results of the English philosopher; *we then said* that magnetism of rotation found a real support in the new facts developed by Mr. Faraday, and that, consequently, the theory of such magnetism then *appeared* to be so far advanced as fully to merit an effort to develope the physical principles upon which it depends. *It is to this object that the present article is devoted*," &c. Upon this extract I shall only remark, that exactly four months previously I had said the same thing, in the memoir that I read before the Royal Society, and had given, what I hope will prove, a true and exact explanation of the philosophy of the effect under consideration (F. 4—80.).

At page 282 we read, "We have already noticed these currents in our first researches, that is, in the first paper inserted in the Number for December" (p. 412.). But I had "already noticed these currents" four months earlier (F. 90.).

At page 283 are described "galvanometrical explorers or probes," which are nothing more than what I had previously described under the name of *collectors* or *conductors* (F. 86, &c.).

At the commencement of the investigation of the state of Arago's revolving disc adjacent to a magnet, two relative positions of the plate and the magnet are chosen; one called (p. 284.) the "*central arrangement*," in which the magnetic pole is placed vertically to the centre of the disc; and the other (p. 285.) the "*excentric arrangement*," in which the magnet acts beyond that point.

[* Phil. Mag. and Annals, N.S. vol. xi. p. 401.]

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With regard to the *central arrangement*, we read (p. 284.), "In this case the magnet acting upon the centre of the disc, the probes do not transmit any indication of a current to the galvanometer, *let them be placed where they may*; and if by chance small deviations should be remarked, they arise from imperfect centralization, so that if this *defect be corrected* all indications, &c. of an equivocal source immediately disappear. Indeed what is the result if we employ an electro-dynamic spiral which turns quite round on its own centre, always opposite to the same magnetic pole? *Absolutely nothing*. Its revolution is an *unimportant circumstance*; for the formation of the currents is *wholly due to another condition, they being manifested* only at the moment when the spirals are *brought near* to the magnets, or *removed* from them. So long as the spirals *are present*, whether they *move or not, there is no current*: so also *there is none* in the case of central rotation in which the points of the disc remain constantly at the same distance from the magnetic pole, by renewing thus the combination of continued presence, to which Mr. Faraday's *new laws* in relation to currents DO NOT ASSIGN ANY EFFECT."

This assertion is so erroneous in every part, that I have been obliged to quote the passage at full length. In the first place, *there is a tendency* to the formation of currents of electricity in the revolving disc, in the case of "central arrangement," as well as in every other case (F. 149—156.); but their direction is from the centre to the circumference, or *vice versâ*, and it is to these parts that the collectors should be applied. It is precisely this which renders the revolving disc a new electrical machine (F. 154.), and it is upon this point that Messrs Nobili and Antinori are so entirely mistaken in their two memoirs. This error is repeated throughout the whole of the memoir that I am now comparing with my first paper, which, if I mistake not, contains the theory of Arago's phænomenon in all its parts.

At page 284 we find, that when a helix turns upon its axis concentrically with a magnetic pole, the result is *absolutely nothing*, and that the condition of rotation is unimportant. Now, though I have not made any experiments on the subject, I venture to assert that there will be an effort in the electric current to pass in a transverse direction to the helix, and that the circumstance of its rotation, instead of being unimportant, is in these cases the only condition essentially requisite for the production of currents. The helix, in fact, may be considered as analogous to a cylinder which might occupy its place, but to which it is very inferior, as it consists of a long coil of wire. It may also be regarded as a simple

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wire placed in any situation occupied by a cylinder, and I have shown that they produce currents in their state of rotation, if their opposite extremities are connected with the galvanometer.

It is said at page 284, that the formation of currents is "*wholly due to another condition*, they being manifested only at the moment when the spirals are brought near to the magnets, or removed from them. So long as the spirals are present, whether they move or not, *there is no current*. So also there is none in the case of *central rotation*," &c. Now in my first paper I showed that the essential condition was not the approximation or removal of the metal in movement, but simply that it should intersect the magnetic curves (F. 101. 116. 118. &c.); and that consequently, *cæteris paribus*, the movement without change of distance is the most effective and powerful means of obtaining the current, instead of being the condition in which the result is absolutely nothing. In my second paper I *proved* that a movement through the magnetic curves was the only condition necessary (F. 217.); and that so far from the approximation or removal of the metal being necessary, currents may be produced in the magnet itself, merely by moving it in the proper direction (F. 220.).

Lastly, when treating of this "central arrangement," and the supposed absence of effect when "the points of the disc remain constantly at the same distance from the magnetic pole," Messrs. Nobili and Antinori say (p. 285.), "by thus renewing the combination of continued presence to which Mr. Faraday's *new laws* in relation to currents *do not assign any effect*;" and in a note we read, "These laws are reduced to three," which are specified, at first fully, and then in a more condensed form, as follows: "First Law. During gradual approximation: the current produced contrary to the current producing; repulsion between the two systems. Second Law. The distance unvarying. No effect. Third Law. During recession. The current produced in the same direction as the current producing. Attraction between the two systems." I have never myself given these as the simple laws which govern the production of the currents that I was so fortunate as to discover; nor do I understand how Messrs. Nobili and Antinori can say that they are *my* laws, though at page 282 one of them is so called. But I described these three cases together in my first memoir (F. 26. 39. 53.), as well as in the notice, that is, in my letter to M. Hachette, as effects that I had observed. It has been established, by what I have already said, that they are not the laws of the action of magnetic electricity, for the simple fact that cur-

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rents of electricity may be obtained by means of the revolution of a cylinder (F. 219.), or of a disc in connection with a magnet (F. 218.), or of the magnet itself (F. 220.), disproves each of them. One Law, which includes all the effects, is given in my memoir (F. 114. 116. &c.), and it simply expresses the direction in which the moving conductor intersects the magnetic curves. This law of direction being given, I endeavoured to recapitulate the whole in the terms that I shall here repeat (F. 118.).

"All these results show that the power of inducing electric currents is circumferentially excited by a *magnetic resultant* or *axis of power*, just as circumferential magnetism is dependent upon and is exhibited by an electric current."

I have quoted this passage of the Italian physicists at full length, because it contains nearly all our points of difference, both as to fact and opinion, concerning this part of the subject. Having thus shown all the errors included in it, I shall endeavour to be more concise, while I exhibit, *with the assistance of the galvanometer*, such others, derived from them, as are dispersed over the remainder of the memoir. It is indeed curious to remark, how, with galvanometrical indications generally correct, these gentlemen have suffered themselves to be led astray under the influence of preconceived opinions. For example, at page 287—288, and in fig. 2. plate iii. is shown the result of an examination by the galvanometer of the currents in a revolving disc. These currents are indicated nearly correctly by means of arrows; but the *two consequences* deduced from them agree with the theory enunciated, and are diametrically opposed to the facts.

"The immediate inspection of the arrows which mark the currents in the two regions of the disc (fig. 2.) leads to one of these consequences (p. 287.), and it is *that a system of currents is developed upon the parts that enter contrary to those produced on the other side.* The other consequence arises from comparing the currents produced upon the disc with the currents of the producing cause, and it is *that the direction of the currents upon the parts that enter is contrary to that of the producing currents, while on the other side the direction in the two systems is identical.*"

But I showed in my first memoir (F. 119.), that "when a piece of metal is passed either before a single pole, or between the two opposite poles of a magnet, or near electromagnetic poles, whether ferruginous or not, electric currents are produced across the metal transverse to the direction of motion. This fact is proved by means of wires (F. 109.), plates (F. 101.) and discs (F. 92. &c.); and in all these cases

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the electric current was in the *same direction*, whether the metal were brought near to, or caused to recede from the magnet, provided that the direction of its movements were unaltered. In Arago's revolving disc the electricity that I was able to obtain from one of these parts in a multitude of experiments always agreed with these results (F. 92. 95. 96.), and consequently (F. 119. &c.) I recapitulated them in a short description, as presented in Arago's disc, establishing more particularly (F. 123.), that the currents produced near or under the poles are discharged or return into the parts of the metal situated on each side of and more distant from the place of the pole, where the magnetic induction is necessarily "weaker."

[To be continued.]

XLIII. *On the Detection and Estimation of Colophony (common Rosin) when dissolved in the Fixed Oils.* By J. Denham Smith, *Esq.*

To Richard Phillips, Esq., F.R.S., L. & E., &c.

My dear Sir,

SOME samples of linseed oil were sent to me for analysis in July last, the bulks of which had been exported, but were found on their arrival to be unsaleable and perfectly useless, for when mixed with white-lead in the usual manner for making paint, the mixture became quite hard at the expiration of a few hours, it having set as plaster of Paris does when moistened with water.

At first sight it was obvious that all these oils had been considerably adulterated; for not only was their colour much deeper than that of ordinary linseed oil, but they were all extremely viscid, resembling castor oil, in this respect, much more than the comparatively thin and fluid commercial linseed oil. Suspecting both from smell and taste, particularly the latter, that the adulterant was common colophony (black rosin), I endeavoured to ascertain whether my conjecture was well-founded, and if so, to determine the proportion of colophony contained in the several samples; especially as the adulterated oils were likely to become the subject of legal proceedings.

I am not aware that there is any mode on record for separating, or even detecting common rosin when dissolved in the fixed oils, so that I was obliged to make numerous experiments, before a method was discovered which appeared

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however, was even under these circumstances still maintained. No difference was observed whether the oxygen was allowed to escape as from a zincode of platinum, or was absorbed by copper or zinc; the metals, of course, being dissolved in proportions equivalent to the hydrogen developed at the platinode. Solution of potassa, baryta, or strontia, similarly treated, exhibited a transfer of about one-fourth of an equivalent towards the *platinode*.

These curious results are easily explained by supposing that the solution is a mixture of two electrolytes; with sulphuric acid they are H + (S + 4 O), oxysulphion of hydrogen (H + O) water; the current so divides itself that three equivalents of water are decomposed, and one equivalent of oxysulphion of hydrogen. Analogous changes occur with the alkaline solutions, the alkaline metal passing as usual to the platinode.

LIII. *On Magneto-electric Induction; in a Letter to M.* Gay-Lussac. *By* Michael Faraday, *D.C.L., F.R.S.*

[Continued from p. 289, and concluded.]

I REPRESENTED this state of things under a general form, in the figures *ij* annexed to the memoir, which, as to the arrows, the designation of the parts, &c. &c., I have made to correspond, as well as I could, with fig. 2. plate iii. of the Italian philosophers' memoir (plate ii.). I proceed to show how it agrees with the galvanometrical results obtained by them, and how far with their *conclusions*.

With regard to the galvanometrical results, my figure might be used instead of theirs, without occasioning any difference, and I have no reason to say that they are inexact. Relatively to "one of those consequences," which arises from "the immediate inspection of the arrows which mark the currents in the two regions of the disc," or from any other attentive and experimental examination, we see that the currents *n*, *n*, *n*, on entering, instead of being in a contrary direction to those which are in the parts *s s s*, which recede, follow exactly the same direction; that is, that as to the general movement near the pole they go from above below, or from the circumference towards the centre, transversely to the lines that the different parts describe in their course; and at a great distance (F. 92.) on each side of the pole they are in the contrary direction. In proportion to the nearness to the pole of a part of the line described by a point, it is traversed by a current, which commences, and increases in intensity

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until it reaches the shortest distance, or a little beyond, on account of time entering as an element into this effect. Afterwards, by reason of the increasing distance, the current diminishes in intensity without ever altering its direction relatively to its proper course. It is only when it arrives at the parts most distant, at which the electricity excited is discharged, that a current is manifested in an *opposite* direction, or in one more or less oblique. I apprehend that it is wholly useless to speak of the partial alterations in the direction of the currents through the parts that are the nearest to the centre, or to the circumference; two or three curves that I have rudely traced will show in what directions these alterations take place.

The second consequence arising from the memoir of the Italian philosophers is, that "the direction of the currents upon the parts that enter is contrary to that of the producing currents; (that is, of those that are considered as existing in the magnet) while on the other side the direction in the two systems is identical." This assertion is exactly contrary to the reality (F. 117.). In figures 1. and 2. I have indicated, by means of arrows, the direction of the currents in the magnetic pole, which is the same as the direction given by Messrs. Nobili and Antenori in fig. 1. pl. iii. But my figure 2, as well as the indications of the galvanometer, shows evidently that the currents in the parts that enter *n*, *n*, *n*, when they approach the magnet, pass through in the same direction as the current in this side of the pole of the magnet; and that the currents in the parts that recede *s*, *s*, *s*, follow a direction contrary to those supposed to exist in the side of the magnetic pole from which they recede.

I may be mistaken, but it appears to me that Messrs. Nobili and Antenori suppose that circular currents are excited in the part of the metal adjacent to the pole, in absolutely the same manner as those formed in the helix, when it is made to approach the magnet, and that when this part of the disc recedes, the circular currents are somehow reversed, as occurs in the helix during its recession from the magnet. A passage in their first paper, and another at the end of page 284, appear to imply that such is their opinion. This idea occurred to me above a year ago, but I soon saw from numerous experiments, some of which I have just referred to, that it was by no means satisfactory; and when I had fully verified that the action of the helix in its approach to, and recession from the pole was wholly explained (F. 42.) by the law assigned (F. 114.), I was forced to abandon my previous ideas.

The memoir afterwards proceeds (p. 288.) to explain the

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phænomena of Arago's revolving disc; but as I have shown that the theory is in general based upon two conclusions contrary to truth, it is unnecessary to make a minute examination of it. It is impossible for it to exhibit the phænomena with exactitude. Those who are anxious for full satisfaction on the subject, may decide, by means of a few experiments, whether the opinions which I put forth in the paper which first announced the discovery of these currents be true, or whether the Italian philosophers were justified in declaring that I was in error, and that they had published more just ideas on the subject.

Everybody knows that when M. Arago published his remarkable discovery, he said the action of the disc upon the magnet was resolvable into three forces: the FIRST, perpendicular to the disc, which he found to be repulsive: the SECOND, horizontal and perpendicular to the vertical plane containing the radius beneath the magnetic pole; this is a tangential force, and occasions the rotation of the pole with the metal: the THIRD, horizontal and parallel to the same radius; it becomes null at a certain point towards the circumference; but when nearer the centre, it has a tendency to impel the pole towards the centre; and when nearer the circumference, to impel it in the contrary direction.

At page 289, Messrs. Nobili and Antenori give an explanation of the first of these forces. As has been already said, these gentlemen consider that the parts adjacent to the magnet have currents contrary to those which are found near the pole to which they approach, and consequently they are repulsive; and they consider that the parts that recede have currents identical in direction with those which are near the magnet from which they recede, and consequently these parts are attractive. The sum of each of these various forces is equal one to the other, but in what relates to the needle or magnet this distribution differs; for "the repulsive forces being the nearest, invade the disc as far as the parts under the needle, and thus obtain a preponderance over the action of the contrary forces, which are exerted more obliquely, and at a greater distance. In short, it is only a part of the repulsive forces which is balanced by the attractive forces; the remainder meets with no opposition, and it is this remainder that produces the effect."

But I have shown in this letter, that the currents in the parts adjacent or distant are exactly contrary to what is supposed by Messrs. Nobili and Antenori; and that consequently where they expect attraction they would find repulsion, and attraction where they expected repulsion; so that,

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following their opinion, corrected by experiment, the result should be *attraction* instead of *repulsion*. But Arago was right in saying that it is repulsion; and consequently the theory of the effect given cannot be the true one.

My views upon the subject in question may be found in my first paper. I examined whether it were possible or probable (F. 125.) that time could be a necessary element for the development of the maximum current in the metal. In this case the resultant of all the forces would be in advance of the magnet, when the plate was rotated, or in the rear of it, if it (the magnet) were rotated; and a line joining this resultant with the pole would be oblique to the plane of motion; then the force in the direction of this line might be resolved into two others, one parallel, the other perpendicular to the plane of movement or rotation; the latter would be a repulsive force, producing an effect analogous to that remarked by M. Arago.

The second force is that which occasions the magnet and the disc mutually to follow each other. Referring to page 290, fig. 1. or 2. (my figure 2. may also be made use of,) we read, "Forces of attraction exist in *s*, *s*, *s*, towards which it (the magnet) is attracted, and repulsive forces in *n*, *n*, *n*, which impel it in the same direction;" consequently the magnet moves either after, or with the metal; but the currents, and consequently the forces, are exactly contrary to what has been supposed, as I have just shown; the magnet and the disc should therefore move in opposite directions, if the forces act in the manner that has been supposed. But as they do not move, in fact, in opposite directions, it is evident that the theory which explains their movement by reversing the facts must be itself erroneous.

The third force is that which has a tendency to remove the magnetic pole either towards the centre or the circumference, on each side of a neutral point situated upon the radius above which the magnet is placed; this effect is described at page 281, and in fig. 4, which accompanies the memoir, and which I believe to be perfectly correct. The memoir proceeds to explain the effect by referring to the repulsive force admitted (p. 289.) to account for the first effect observed by Arago, viz. the vertical repulsion of the disc; and supposing that this repulsive force be distributed over a certain extent of the disc, beneath the magnet, it is established (p. 292. fig. 5.) that if the pole be situated very near the circumference, the portion of the body whence this force emanates will be lessened, being cut by the circumference itself; consequently the parts that are nearest to the centre are more powerful, and impel the pole in an outward direction; while if the pole be placed very near

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the centre, the extent whence the force emanates will pass it; and as this part in excess is considered, though erroneously, as inactive, the portion situated towards the circumference is more powerful, and impels the pole towards the centre.

Two or three slight objections present themselves to this opinion, but they are nothing, so to speak, in comparison with that which arises, when it is recollected, that in conformity with the author's own ideas upon the action of currents, the error with respect to the direction of those which are excited near the pole obliges us to substitute *attraction* for *repulsion*, as I have already shown when treating of the first of these forces: consequently all the movements which are connected with the third force would be in a direction contrary to those that are actually presented; and the theory which, when corrected by experiments made with the galvanometer, indicates such movements, must be abandoned.

Page 292 of the memoir refers to Mr. Faraday's "second law." As I have already said, I never stated those three assertions as laws. I really regret extremely that a letter that was never intended to convey minute details, but merely a few facts, selected in haste from a multitude described previously in the memoir read before the Royal Society,—I regret that this letter, which I never expected to see in print, should have led the Italian philosophers into error. However, after having examined anew all the facts, I do not see that I am in any degree responsible for the error they have committed, as having advanced fallacious results; nor, as far as the memoir is concerned, for not having given to the scientific world the most complete details at the earliest period possible.

I have not yet published my views as to the cause of the third force described by M. Arago; but as Messrs. Nobili and Antenori, when giving the hypotheses, which I justly regard as inexact, say (p. 293.), "In fact, what other hypothesis can reconcile the verticality that the needle preserves in the two positions n, s, n'', s'', (fig. 4) with the fact of the repulsion from below, above which raises the needle in the second position s'', n''?",—I am induced to offer another hypothesis, premising, however, that the directions and forms that I shall trace, as those of the excited magneto-electric currents, are to be considered only as general approximations.

If a piece of metal, large enough to contain without distortion all the currents which may be excited in its whole extent by a magnetic pole placed above it, be moved in a rectilinear direction beneath the pole, then an electric current will move across the direction of its motion, in the parts immediately adjacent to the pole, and will return in the opposite direction

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on each side in the parts which, being more distant from the pole, are subject to a feebler inductive force: the current will thus be completed or discharged (see fig. 3.). Let A B C D represent a piece of copper moving in the direction of the arrow E, and N the north end of the magnet placed above; currents of electricity will be produced in the piece of metal; and though they undoubtedly extend from the part below the pole to a great distance around (F. 92.), and at the same time diminish in intensity and alter in direction as they recede thence, yet the two circles may serve to represent the resultant of these currents; and it will be evident that the point of most intense action will be where they touch, and immediately under the magnetic pole, or, on account of the time required, a little in advance of it. Hence that portion of the forces which acts parallel to the plane of the metal will carry the pole forward in the direction of the arrow E, because the forces are equally powerful on the side of the pole A B, as on the side C D; and this portion, which on account of the time necessary for the production of the currents excited is perpendicular to the direction of the metal, will be, as we have said, repulsive, and have a tendency to impel the pole upwards and away.

But suppose that instead of the metal moving in a rectilinear direction, a circular disc revolving upon its axis be substituted; and then let us consider, in the first place, the case of the magnetic pole placed upon its centre (fig. 4.); there is then no production of electric currents, not because there is no tendency to their formation, for I have stated in this letter, and shown in my memoirs (F. 149. 156. 217.), that from the time the disc begins to move, currents are also ready to move; but because they have a tendency to be formed in the direction of radii from the circumference to the centre; and as all the parts are equally influenced, none of them having an excess of power over the others, and all equally distant from the centre, no discharge can take place, and consequently no current can be developed. As no current can exist, no effect dependent on the action of a current upon the pole can be produced, and consequently there is then neither *revolution* nor repulsion of the magnet. Hence the cause of the *verticality without repulsion* which occurs at this place.

Let us now consider the case in which the pole of the magnet, instead of being placed over the centre of the metal, is at one of its sides, as in N, figure 5. The tendency to form electric currents is due to the movement of the parts of the disc *through* the magnetic curves (F. 116. 217.), and when these curves are of equal intensity, the electric currents increase in

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force in proportion to the increase of rapidity in the motion of the parts of the disc that intersect the magnetic curves (F. 258.). Let us now trace a circle *a b* around a magnetic pole as a centre, and it will represent the projection of magnetic curves of *equal intensity* upon the disc; *a* and *b* are points situated at an equal distance from the pole, in the passing radius which is immediately under the pole; but as the part or point *a* passes by the pole with much greater velocity than the part *b*, the intensity of the electric current which is excited in this part *a* is proportionably greater. This is also true for the points in any other radius of the revolving plate cutting the circle *a b*, and true likewise for any other circle traced round N as a centre, and representing consequently magnetic curves of equal intensity; with the exception, that when the circle extends beyond the centre C of the revolving disc, as to *c d*, instead of the existence of a feebler current at the point *d* than at the point *c*, there is then a tendency to produce an opposite current.

The natural consequence of these actions of the different parts is, that as the sum of the forces tending to produce the electric current in the direction from *c* to *d* is greater on the side *c* of the magnetic pole than on the side *d*, the curvature or return of these currents by the right and left also commences on this side; and then the two circles, which as before may be considered as representing the resultants of these currents, do not come into contact exactly under the pole, but at a greater or less distance from it, towards the circumference, as in figure 6.

This circumstance of itself would not occasion any movement in a pole restrained in its motion to the direction of the radius only; but being combined with that which results from the *time* necessary for the development of the current, and to which reference has been already made, as explaining the *first* of the three forces by which M. Arago exhibits the action of the magnetic pole and disc, it will, I hope, fully elucidate all the effects that we are investigating, and will also prove the influence of time as an element. Let *c* (fig. 7.) be the centre of a revolving disc, and *r c* a part of the radius under the magnetic pole *p*; the contact of the two circles representing the currents is, as we have just seen, on the side of the pole beyond the centre *c*; but on account of the element of time and the direction of the rotation R of the plate of metal, it is also a little to the left of the radius *r c*; so that the pole is brought under the action of the two orders of currents, not symmetrically but obliquely. The necessary consequence is, that if it be free to move in the direction of the radius, and in that di-

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rection alone, it will move towards the centre *c*, for the currents produced by a marked pole (north) are exactly such as by their action on the pole to impel it in that direction.

This relation of the currents to the pole by which they are generated, may be proved by experiment as easily as by calculation. I have shown (F. 100.) that when a pole marked north is above a disc revolving in the direction of the arrows R, in the figures annexed either to Messrs. Nobili and Antenori's memoir or to mine, the currents (indicated by the circles) are as is represented in figures 3, 6, or 7. Upon arranging a metal wire which would conduct the currents in this double direction (fig. 8.), and placing over it a marked pole (north) capable of moving only in a parallel direction to *r c*, at any point in the line *r c*, I found it had not any tendency to move. There was also another line perpendicular to the first, and which crosses it at the point of contact of the circles, in which the pole had no tendency to move. If placed in any other situation than upon these two lines, it moved either in one direction or the other; and when placed in the positions marked 1, 2, 3, 4, it moved in the direction of the arrows represented at those points. Now the position of the pole, with regard to the currents in Arago's experiment, when the magnet and the disc are arranged as in figures 5 and 7, is exactly that of the point 1 in fig. 8, and hence that pole has a tendency towards the centre C.

We will now direct our attention to the result obtained if we gradually move the pole from the centre towards the circumference. Let figure 9. represent this new condition at a given time, as figure 5. represented the first state; it is evident that the velocity of the parts *a b* of the radius beneath the pole, will not differ from each other so much as they did previously, being only about 3 : 2 instead of 6 : 1; and the difference will also be less with all the curves of equal intensity comprised in this circle. This occasions the situation of the pole, and the place of contact of the circles representing the currents (fig. 7.) mutually to approach in the direction of the line *r c*, and necessarily carries the pole (fig. 8) nearer to the neutral line *l i*. If we examine the second circle *c d*, fig. 9, of magnetic curves of equal intensity, it will be seen, that as the disc does not extend to *c*, or even beyond *a*, there is nothing to add to the force of the current upon that side of the pole, while at *d* the radius, by moving through the magnetic curves, adds to the intensity of the current excited at *b*, and everywhere else on that side of the pole, and may easily, according to the position of the pole upon the metal plate (that is, according as it is nearer or further from the edge),

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render their sum equal or greater than the sum of the forces on the other side, or that towards the circumference. If the sum of the forces be equal, then the pole will be somewhere in the line *l i*, as at 5, fig. 8, and will have no tendency either towards the centre or the circumference, though its tendency to move with the disc or above it remains the same. Or if the sum of the forces be greater on the side *d*, fig. 9, than on the side *c*, then the pole will be in the position 2, fig. 8, and will be impelled outwards in the direction of the radius, in conformity with Arago's results.

Besides this cause of alteration in the motion of the pole parallel to the radius, and which is dependent on the position of the pole near the circumference, there is another cause that occurs, I apprehend, at the same time, and assists the action of the first. When the pole is placed towards the edge of the disc, the discharge of the currents excited behind is thrown against the side of the edge, from the absence of conducting matter; thus, in fig. 10, instead of having the regular form of the figures 7 and 8, the currents are deflected in their course towards the circumference, while they have all necessary latitude for their movement in the parts towards the centre; this of itself would cause the point of greatest force to fall a little nearer the centre than the projection of the axis of the magnetic pole, and assist in placing the pole in the position 2, fig. 8. I have such confidence in this opinion, that though I have not had opportunity to make the experiment myself, yet I venture to predict, that if instead of employing a revolving disc, a lamina or plate of metal, five or six inches broad, as A, B, C, D, fig. 9, were caused to move in a rectilinear direction conformably to the arrow, under a magnetic pole situated at *a*, the pole would have a tendency to move forward with the metal as well as above, but neither towards the right nor left; while if the pole were placed above the point *b*, it would be directed towards the edge A B; or if it were placed above *c*, it would have a tendency to move towards the edge C D.

Having thus replied to the question, "What other hypothesis"?, &c. proposed by the authors of the memoir at p. 293, I shall continue my examination of the memoir itself. At p. 294 the error relative to the nature of the currents, that is their supposed inversion, is repeated. The effect described is sure enough with a helix, and some particular forms of apparatus; but the simple and elementary current generated by the passage of a wire in front of a magnet is not reversed when the metal wire recedes. (F. 171. 111. 92.)

At p. 295 is the supposition that when the rotation is slow

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"the revolution of the currents is circumscribed within narrow limits, and there is *little to add* to the results that form the basis of the [our] whole theory;" but that when the motion is rapid the currents envelope the whole disc, "so as to become a species of labyrinth." For my part I believe the currents have the same general direction as has been assigned to them in the figures, whether the rotation be slow or rapid; the only difference is an increase of velocity.

A circumstance is then selected which is really simple, though it may at first appear complicated; namely, that in which the opposite poles are adjusted over a disc in one diameter, but towards the opposite edges on each side of the centre. This circumstance, with the direction of the movement and the current produced, is exhibited in fig. 7 of Messrs. Nobili and Antenori's memoir. It is unnecessary to quote pages 296 and 297, which contain the explanation of this figure, but I shall refer to fig. 12, which corresponds to it, and is in conformity with my views and experiments, so that the two may be compared together. It is very satisfactory to me to find, that in this part of the memoir, as well as in the first, there is no important result of experiment contrary to my published opinions, though I am very far from adopting the conclusions that have been drawn from them.

If figure 12 be examined, it will be instantly seen that it results in the most simple manner from the action of the two poles. Thus, as far as the upper or north pole only is concerned, the currents are as in figure 6. But as with the north pole, the current determined by it moves from the circumference towards the centre, so with the south pole, in the same or corresponding position, the currents move from the centre to the circumference (F. 100.); and consequently in fig. 12 they are continued along the diameter N, S, through the centre of the plate, to return in the direction of the arrows upon the sides E, O. The points upon which I find my views to disagree with the indications of the galvanometer obtained by Messrs Nobili and Antenori are, first, the direction of the currents at N and S, which is contrary to what I obtained; and, secondly, the existence of any oblique axis of power, as P, Q, in their figure 7.

The memoir concludes, at least as far as I am concerned, at page 298, by again mentioning the error (but not as an error) relative to the revolving disc, which becomes a *new electrical machine*. At the commencement, the authors being little conversant with the principles under the influence of which such a result is obtained, deny it; and though they say here, "What shall we say after the *new observations* that

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we have made during the continuation of our researches?"—I am not disposed to alter anything that I have published; I have even more confidence than before in my own views; for had their observations been in agreement with the results which I had obtained, I should have had great reason, after my examination of their papers, to fear that my own ideas were erroneous.

I cannot conclude this letter without again expressing my regret at having been obliged to write it; but if it be recollected that Messrs. Nobili and Antenori's memoirs were written and published *after my* original memoirs; that their last paper appeared even in the *Annales de Chimie et de Physique* after mine; and that it had consequently the appearance of advancing the science further than I had done; that both papers accuse me of error in experiment and theory, and also of dishonesty; that the last of these papers is dated in March, and though it is now December, has been followed by no correction or retractation on the part of the authors; and that I sent them several months ago (at the same time that I forwarded them to you and others,) copies of my original memoirs, and of my notes to a translation of their first memoir; and if it be considered that, after all, I have not to reproach myself with the errors of which I am accused, and that these gentlemen's memoirs are so framed as to compel me to reply to their objections;—I hope that no one will say that I have written too hastily what might have been avoided; or that I should have shown respect for the truth, and done justice to my own publications, or to this branch of science, if, being aware of such important errors, I had not called attention to them. I am, my dear Sir, yours very sincerely,

M. Faraday.

LIV. *On the Law of Storms.* *By* H. W. Dove.

To Richard Taylor, Esq.

Editor of the Philosophical Magazine and Journal.

Dear Sir,

In the year 1828, I published in Poggendorff's *Annalen*, vol. xiii. p. 596, a memoir "On Barometric Minima," in which I established the fact, that the storm which accompanies a great depression of the barometric column is a vast whirlwind, which in the northern hemisphere proceeds from S.W. to N.E. The example there more especially investigated is the storm of the 24th of December, 1821, the centre of

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