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FIELD THEORY IN DE LA RIVE'S *TREATISE ON ELECTRICITY*

Auguste De la Rive, 1801–1873, is nowadays scarcely remembered even amongst historians specializing in 19th century electricity. The major work in this field still is the revised and enlarged 1951 edition of Sir Edmund Whittaker's *A History of the Theories of Aether and Electricity, The Classical Theories.* In it Whittaker refers to De la Rive a few times as to a supporter of the chemical theory of the voltaic pile. He does not refer to this *Treatise* at all, perhaps because he centers only on original results, and the *Treatise* claims no priority of any kind.

A glance at Jean-Baptiste Dumas' *Eloge Historique D'Arthur-Auguste De la Rive* (Institut de France, Académie des Sciences, Paris, 1874) offers a different picture. Let us note, only, the following few points. Dumas considers De la Rive's contribution to electrochemistry sufficiently important, though secondary to those of Faraday's (p. 19). He mentions other researches in his obituary (p. 20) and in his notes (pp. 47–48); he refers to his other works, including literary essays as of some significance; yet he declares his *Treatise* to be his major work (*l'oevre capitale de sa vie,* p. 48), where both his own work is summed up and at the same time work of researchers of the whole world were analyzed.

No doubt, De la Rive's *Treatise* is fairly comprehensive. It describes innumerable experiments, offers some background, sketches and contrasts scientific opinions; it includes little by the way of mathematics, and even this is consigned to appendices. The work is declared to be aimed at the knowledgeable rather than the dilettante, but at least nowadays it does not look too hard to read. Let me explain my interest in it.

First, and minor, is a historiographic point: there is little doubt in my mind that the little history offered by De la Rive has become extremely influential. I shall mention only two points of similarity between De la Rive and Whittaker. First, they both mention the experiment of Désormes and Hachette of 1805 as a prelude to Oersted. Neither explain this. Second, the fusion of Faraday's work into the pattern of the continental theory of action at a distance, where works of Ampère, Weber, and Neumann, are treated as the evolutionary stages in the development of one idea. Doubtlessly, Weber did not consider his own work the mere elaboration and corraboration of Ampère's work; yet De la Rive and Whittaker (and between them Duhem) did.

My second, and still not very great, interest is in Faraday's own attitude to De la Rive. Faraday was a decade senior to Arthur-Auguste De la Rive, and two decades junior to his father Charles Gaspard. Old De la Rive, a Swiss aristocrat, had been a refugee in Britain. Ha studies medicine in Edinburgh and practiced it in London, where he befriended another refugee, Dr. Marcet, whose wife, Jane, wrote the *Conversations on Chemistry* which helped Faraday as a lad to teach himself chemistry. (Auguste De la Rive wrote essays on both Faraday and Jane Marcet, among other eminent scientists.) When Davy came to Europe Faraday accompanied him as a servant and was handicapped by his ignorance of any foreign language. Old De la Rive befriended him. Later, when Davy visited Geneva, Faraday was treated as an equal. The friendship grew. They corresponded; old De la Rive published a letter of Faraday on metallurgy. As Dumas notes in his eulogy, when Gaspard De la Rive died many of his functions were naturally passed on to his son Auguste. The friendship and correspondence with Faraday was one of them. The house in Geneva was perphaps the only private place where Faraday would relax and feel at home. He mentions his visits in a few of his letters. But I have in mind his remarks to De la Rive on his *Treatise*. It is mentioned in his letters of March 11, 1854, May, 29, 1854, and March 21, 1856 (H. Bence Jones, *The Life and Letters of Faraday, 1870,* Volume 2, pp. 328, 344 and 375 of extended edition). Let me quote only from the last one: "I rejoice" is his general response, "for now, when asked for a good book on electricity, I know what to say." Is this a friendly note of encouragement or a sincere appraisal?

Either of these hypotheses is hard to uphold. Faraday followed a very strict code of conduct: he spoke his mind diffidently and politely but very candidly; or else he frankly and firmly declined comments. Yet it is hard to see how he could be satisfied with a book which so maltreated him, as we shall see, particularly as he was very sensitive about his being maltreated. No doubt, all this is partly resolved by Faraday's praise of the book as a well of information, especially about German sources. But this is hardly the whole story. My own hypothesis is comparative: a misrepresentation as De la Rive's work surely was, it was far better a presentation than the average.

This, indeed, is the chief interest I find in De la Rive's work. There is a literature about the penetration of Maxwell into the Continent; as long as Faraday's revolutionary ideas were not sufficiently appreciated —

and prior to recent studies, particularly L. Pearce Williams *Faraday* of 1965, he was considered an etherist — there was little reason to study the penetration of his ideas into the Continent. Now, however, it seems obvious that even in converting the Continental scientists — for in the 19th century science was still much a matter of creed, and for many it still is even today — Faraday was the trail-blazer.

An attitude which may well be very characteristic of the time in Europe is that exhibited in works of Johanes Mueller of the University of Frieburg, author of a textbook on electricity and of reports translated and published by the Smithsonian Institution. In the Annual Report of 1856 there is a Report on the recent progress in physics — galvanism, by Mueller — pp. 311–423. And in 1857 a report on static electricity — pp. 357–456. There is another report in the 1858 volume on electricity and galvanism — pp. 333–431, and the next year — pp. 372–415, too. Let me note a few general points on this report, expecially its attitude to Faraday.

Mueller's report is much more analytic than that of De la Rive, but otherwise fairly similar. It refers to Faraday's data as true almost invariably. (The exception is in the one in the Report for 1857 of 1858 where, on p. 373, a seemingly continuous spark breaks down into a rapid succession of sparks by moving the eye rapidly, a technique all too obvious in the days of the flicks and fluorescent light; Mueller reports that it "has not succeeded perfectly in my trials." He does not even mention that he only contests Faraday's technique — since the fact was also established by Wheatstone's revolving mirrors.) But, not only Mueller dissents from all of Faraday's views; he does so condescendingly and inaccurately.

Even when Mueller has no special reason to be condescending, he is. For example, when he reports on the debate on the cause of electrochemistry, he sides with the chemical theory (which identifies the pile's action with chemical action, by identifying chemical forces as a kind of electric force) as against the contact theory (which asserts, with Volta, that the contact points, between the electrodes and the solutions, are poles which act at a distance). To be broadminded, I suppose, he makes a concession to the contact theorist. He puts it thus (p. 314): "Even Faraday", he says, and I draw attention to the word "even", "who is prominent in maintaining the chemical [theory] ... concedes that decomposition is preceded by a state of tension..." He quotes Faraday and repeats: "Thus Faraday himself concedes".

This is incredibly crude. The idea is that since Faraday admits the existence of tension, he concedes that there are centers of force causing the tension; since there is polarity, there are poles. And, the contact theory is a theory of poles. Yet Faraday was at pain to stress his dissent on this point. He renamed the poles "electrodes" just for this reason.

In a letter to William Whewell, the person whom he had consulted and who had suggested electrode, anode, cathode and ion, anion, cation, Faraday relates the enormous opposition to his renaming which he crushed on Whewell's authority. Of course, his audience were as aware as he that names are not theories, but they were clear about the purpose behind his renaming. Indeed, Faraday's very approach to the pile was an attempt to look for an electric phenomenon where the medium plays an undeniable role, and he tried to abolish the electric poles as causes of polarity, similarly to his prior investigation into magnetoelectricity where he showed that cutting the lines of force is the cause of the phenomenon, and that the lines of force, i e., of polarity, do not depend on the magnetic poles. No doubt, Faraday began his researches with the pile because be thought that the medium of electrolysis was least susceptible to be ignored by his opponents; for his own part, he saw empty space as the medium just as much.

The point came sharply with Faraday's study of electrostatic induction. To explain this phenomenon most physicists assumed the existence of latent electricity — the existence of positive and negative electrities in equal amounts, to use the two-fluid language, but the same holds within the one-fluid system — and normally the existence of electricity is assumed to be undetectable until some electric transfer takes place. Faraday rejected this theory because it assumes that polarization is caused by poles; rather, he identified electricity not with the electrified body or its content but with the polarization itself. He argued, first, that the medium cannot be ignored when it is filled with a dielectric material, especially inhomogeneous. But he then argued that even a single body in the vacuum, when electrified, so-called, is merely a center of induction, homogeneous or not, as the case may be.

Mueller speaks of Faraday's researches on latent electricity. Though one can understand it, one cannot avoid the impression that it is an insult. In our own century, by distinction, even those who considered Schroedinger's equation as good for diagonalizing matrices were not so rude as to speak of his method of diagonalizing matrices, and almost every writer does him the courtesy of giving his own reading of the meaning of the wave function, heretical though it is.

Mueller does not have any criticism of Faraday's electrostatic doctrine. He puts this fact nastily thus (p. 393): "Faraday's experiments are perfectly correct, but it appears to me that he has erroneously interpreted these experiments and drawn conclusions from them which he is not justified" and he goes to say what Faraday should have proven empirically if he were to convince him (Mueller). He goes on to dismiss Faraday by declaring (p. 397) Faraday's view of insulators as poor conductors "a truth which no one, to my knowledge, has disputed" — whereas everyone before Faraday followed Stephen Gray in denying this truth —

and by scolding Faraday for not noticing that electrostatic induction in the vacuum must be an action at a distance.

One must be indulgent toward Mueller here. Faraday's empty space as a medium was very hard to comprehend, and at the same time Tyndall wrote an open letter to Faraday (*On the Existence of a Magnetic Medium in Space,* Phil. Mag., Vol. 1, 1855, pp. 205–209), saying so. Faraday himself could only clarify the difference between the action-at-a-distance theory and the medium theory when applied to empty space only a little later, in his lecture on the conservation of force of 1857 (*Exp. Res. Chem. Phys.*) where he said, all action takes time. Hence, if you abolish the center of force, the action-at-a-distance theory will tell you that the action will there and then disappear, whereas the medium theory will tell you that the medium will be able to act for a while without it. But this Mueller did not know as yet.

One cannot, however, be as indulgent regarding the following remark of Mueller's (p. 400). "Faraday's views on electrical induction must necessarily have forced upon him the question, whether magnetic attraction and repulsion..." act throught the intervening medium as well. "The experiments which he made for the solution of this question gave invariably negative results... No sing of the influence of intermediate particles could be obtained." This is astonishing. Not only did Faraday start with the magnetic medium and then move to the electric medium. Not only did he deny that magnetic action was "attraction and repulsion." At the time when this was written diamagnetism and magnetocrystallism were the hottest topics, and due to Faraday's efforts to find a magnetic "influence on the intermediate particles."

I shall leave Johanes Mueller now, and also the 1858 Reports of the Smithsonian Institution after noting that a very learned paper on atmospheric electricity by M. F. Duprez appears there on pp. 290–371, which refers to Faraday only once, *a propos* of his theory of lightening discharge, which he dismisses offhand (p. 361). Let me also note that a similar, though less detailed, paper on the same topic occurs in the *Britannica* 1842 edition, where various theories are listed, but where Faraday is not mentioned, not even his 1841 theory of the lightening discharge, not to mention his ionization theory of its source. Even the later, 8th edition of the *Britannica* of the 1850's is unkind to him, reticent and by implication unfriendly. The ninth edition, however, has Maxwell's essay on him.

We can now revert to Auguste De la Rive and his treatment of Faraday. Against the background I have tried to illustrate he stands out as a fairly honorable opponent.

De la Rive's *Treatise* was meant to be published in a complete version of two volumes, one pure, one applied, in both French and English. The work was interrupted by private misfortunes and the first volume appeared alone, the English translation in 1853 and the French original in 1854. Volume 2 appeared in 1856 also on pure electricity and Volume 3, on applied electricity in 1858 — in both languages. The French edition of the first volume is slightly corrected, and the corrections occur as additions in the opening of volume two of the English version of 1856. I shall refer to one of these later on.

The opening of the first volume is dominated by Coulomb. The theories of action-at-a-distance of electric fluids up to Chapter 2, on the distribution of electricity on conductors' surfaces only. The principle is that electricity is distributed on surfaces only. On p. 71 Faraday appears first, or rather his "experiments, which of an elegant manner demonstate the same principle." We are soon back with Coulomb. Chapter 3 is on electrostatic induction. Induction is action at a distance. Chapter 4 explains it as the result of splitting the two fluids hidden in a matter. Chapter 5 is on dielectricity. On page 126 Poisson's authority is invoked. On p. 133 Faraday comes in again. The theories here advocated, De la Rive admits, "are now attacked by...facts, which tend to nothing less than overthrow them entirely by leading to the denial of action-at-a-distance, and replacing them by molecular action." For his own part, he thinks "they do not entirely overthrow the theories founded upon labours of Coulomb and Poisson" and he only looks for "the degree in which they must modify" these theories. On pp. 140–141 Faraday postulates the action of intervening matter; "there is no action at a distance, or at least at a distance greater then that which separates two adjacent molecules." On page 143: "According to M. Faraday [distributions cannot] be explained but by admitting that.. induction.. — is necessarily more feeble...along curved lines....than....along straight lines...". This is hardly clear even to readers of Faraday.

Faraday's general theory of static electricity is presented (pp. 144–146). De la Rive uses Faraday's last paper in the high inductive style, written just before he began to publish his speculations boldly. Quite clearly, other historians, notably Whittaker, heavily depend on De la Rive, though without being very aloquent about their debt (a point by point study might prove amusing). When De la Rive comes to his conclusions from Faraday's work (p. 147), he does so in a rather unfriedly manner: "Faraday is led to admit that the tendency of electricity to distribute itself on the surface of a conducting body is more apparent than real..."; and later (p. 148), "Faraday was not contented to follow out the consequences of his theory as far as the phenomena of static electricity alone are concerned," concluding with the judgement (p. 149) that Faraday's electrostatic theory "Although it still has need of being more precise, it deserves, however, even in its present state, to draw the serious attention of a philosopher." But he appends a promising coda to this passage, continuing it by, "It has in its favour, as we shall see, the establishing a more intimate connection between the phenomena of static and those of dynamic electricity." He continues (p. 150), "we cannot yet completely admit" Faraday's (and Mossotti's) theory, as Faraday's facts may yet be explained in a traditional way! He speaks of "a difficulty of conceiving" of electrostatic induction in a manner postulated by Faraday — which he considers an objection. "It is true that Faraday and the partisans of his theory reply.... But we do not believe, not withstanding these replies that the principle...is demonstrated." He thinks that electrostatic phenomena in the vacuum seriously conflict with Faraday's view. (This, we saw, is a common objection to Faraday's view at that time.) Moreover, continues De la Rive, Matteucci has refuted Faraday empirically. This, of course, is untrue.

Let us not go into the poor logic of this discussion. Let me only quote the final sentence of the chapter on the theory of static electricity (p. 155): "We shall see that electrical phenomena very probably depend upon the combined action of the particles of matter and of the etherical fluid which fills the universe; and, by thus approaching to Faraday's molecular theory, we shall be nearer to the truth than with the hypothesis of two imponderable fluids, existing of themselves, and in a manner independent of bodies." Action at a distance, again.

Magnetic curves are rather prominent, but as indicators of action at a distance, still. De la Rive notes a significant paper by P. M. Rogets, published by the journal of the Royal Institution in 1831, on the mathematics of magnetic curves; need one stress, for Roget magnetism was action at a distance, and the magnetic curves were purely mathematical, with no independent physical existence (p. 185 and 542–545). Electromagnetics. Hachette and Désormes, Oersted, Ampère. We are told definitely (p. 239) that Ampère answered all objections "and established this theory upon such a solid basis that it is at the present time generally admitted." This statement is puzzling, unless we realized that it is not at all clear what De la Rive designates as Ampère's theory except that it includes, at least and perhaps at most, his molecular currents. (Current-current interactions are phenomena unless their magnitudes, etc., are specified, and Weber, for example, had severely objected to Ampère's specifications.) The lack of clarity becomes stronger when discussing Faraday's early electromagnetic work, (p. 251). Faraday's rotations of 1821 (his electric motor) looked irreconcilable with Ampère's theory, particularly since at the time Ampère "had not at that period made known his law of angular currents, by means of which he was succeeded in easily explaining..." Faraday's rotation. "Then, in order to add an experimental proof to the theoretic demonstration... that [Faraday's] facts were not contrary to his hypothesis of the nature of the magnet..." We may remember that Faraday never attacked Ampère's molecular hypothesis, yet De la Rive defends it vehemently, over a few pages. Yet, let me note, the defense has certain validity by stressing that Ampère's view holds only for currents which are closed. In 1856, soon after, Maxwell argued that Ampère's theory holds for stationary (closed) currents and leads to the same results as Maxwell's reading of Faraday for the same cases.

Arago's experiment on the magnetism of rotation of metallic discs of 1825. De la Rive notes (p. 356) that Poisson's explanation of it "was overthrown by the subsequent discoveries of M. Faraday" of 1831–1832.

The discovery of magnetoelectricity (p. 356). "In 1832" we are told (this is puzzling inaccuracy, very uncharacteristic of De la Rive), "Faraday made his discovery, of electromagnetic induction." The two experiments (magnetically induced currents and current induced currents), are presented and shown to be one — and no mention of the magnetic curves which are cut when the currents are created. For an unclear reason De la Rive introduces (p. 358) Faraday's electronic state and his withdrawal of it. Perhaps he wished to tell the reader that he may follow Faraday and then be left by him high and dry. Then, a shock (p. 358).

"The intensity of the induced current depends on many circumstances... We can give no precise rule..." And we soon move to self-induction. We move on. "Faraday, in his beautiful researches on induction" we are told (pp. 360–361), "was the first to demonstrate that induced current, as we might have expected, may be" caused by terrestrial magnetism. While he compliments Faraday's beautiful researches he tells us of a result which is expected anyway. It is understandable, but not too pleasant.

The greatest insult comes not long after (p. 365): "The learned English philosopher", this is just a buffer, one gets used to it by now, "endeavoured to establish a relation between the direction of the currents that he obtained in his experiments, and the direction of the lines of magnetic force or magnetic curves..." There is no hint at any cutting of any lines of force which Faraday viewed as the cause of the current and as the measure of the current's strength. The direction business, by the way, has precedence in Ampère's work, yet De la Rive does not like it. "All the effects" related, he says (p. 635), "appear to me explicable in a more simple manner by tracing them to the primitive law of induction discovered by Faraday himself and" by Ampère's hypothesis about magnets.

This is obscure. The facts are explicable more simply — more simply as compared to what! What does he reject? Clearly he sees no need for magnetic curves round a conducting wire and offers Faraday's own "primitive law" which says that electricity flows in a closed conductor when a current is made or broken in the vicinity. But this is intelligent guess on my part. After all, we remember, De la Rive admits (p. 358) inability to express this law precisely.

On page 391 we are told of "the important principle which Faraday had already glanced at but which [others]... have verified and established more conclusive[ly]..." namely that electricity produced by a dynamo shares all properties with friction electricity All this is trite; Whittacker has played the same game as De la Rive; Faraday's oewn point is meant to say more, but says explicitly just this trite point, since he was still using the inductive style.

On page 409 Wollaston and Faraday prove "that an electric discharge of feeble tension is able to produce chemical decomposition; but", etc. Always but, always a sense of irritation at Faraday.

On pp. 417–418 we are told that "induced discharge...is a very complex phenomenon... determination is very difficult". A theory is nonetheless given; two other thinkers are mentioned as having alternative, undescribed, theories; Faraday is not mentioned.

Page 433. "General Considerations on Induction.... Weber and Neumann... both by means of experiment as well as by calculation...connnect the phenomena of induced currents with the laws by which electrodynamic actions in general are governed. M. Weber, in an important work... very profound... interesting approximations...we shall quote, as an example, the following experiment, which is a modification of one of Faraday's: — The English philosopher, as the result of series of experiments, had been led to observe..."

We have here Weber thinking and experimenting, the example is an experiment which is a modification of Faraday's, and then we land in Faraday's plain and simple. Faraday's experiment relates to a magnet cutting its own lines of force and thus causing a current. Faraday ascribed to it a great importance, since it showed, as he had suspected all along, the independence of the lines of magnetic force from the magnet, and that the magnet is a mere locus; i.e., it convinced Faraday personally that lines of force are more primary than ordinary matter. De la Rive mentions none of this, and only says that this experiment results from series of other experiments. I cannot say in which respect Weber's experiment differs from Faraday's. It seems to me to amount to precisely the same thing, except that it employs a more up to date arrangement for the selection of currents. "It is difficult for us to admit, with Weber and Faraday..." Never mind; the debate is directed against Weber. De la Rive nevertheless notes that much of Weber agrees with Naumann's and his own ideas.

All the same something made De la Rive withdraw all this before the French edition appeared. In the *Traité*, Volume I, page 439, we are told that Weber's experiment is but a variant of Faraday: "Weber avait également décrit...une expérience qui ne differe de celle de Faraday..." Preceding this, there is an insertion (p. 436 ff) which appears in the beginning of the second volume of the English version. In it De la Rive does two remarkable things at once: he declares — twice (pp. 13 and 16) — Lenz's theory to be utterly satisfactory, explanatory of all known facts and highly confirmed, and introduces Faraday's field theory, a field and lines of force as well. This raises the suspicion that he would not mention fields as long as he feared that the field theory is unrivalled. Let me postpone this point, however, and continue with Volume 1, so as to see how, in steps, De la Rive relaxes his own taboo on explaining Faraday's view. For the gradual relaxation may be better explained as a success to overcome some reluctance rather than a decision not to give Faraday a chance to appear as the leading thinker in the field.

Back to volume one, then. The next topic happens to be diamagnetism; p. 446. "The facts that we have been relating, would seem to prove...But these were isolated facts...and it is to Faraday that we are indebted for having established... The learned English philosopher..." Still no lines of force. De la Rive introduces Faraday's terms "equatorial" and "axial" which are more descriptive then "lines of force". Though this terminology forces him to confine his descriptions to phenomena which take place in a fairly homogeneous field, such as between two poles of a horseshoe (electro) magnet with two blocks of soft iron attached to it. Next we are told of repulsion between magnets and diamagnetic substances (p. 488), though Faraday had disproved this idea.

De la Rive manages to skate quickly over the point at which Faraday, "this clever philosopher", decides that both air and the vacuum are neither diamagnetic nor paramagnetic (p. 452) — by promising to return to the topic of the diamagnetism of gases. On page 455 we are told that according to Becquerel the "vacuum, or rather the etherial medium by the aid of which the magnetic actions are transmitted, is itself magnetic." All of a sudden magnetism is not due to action at a distance, and the magnetic ether is introduced via a qualifying clause. This is not fatal: Becquerel's view is at once rejected. It only indicates how absurd it looked to De la Rive to talk of the action of empty space even for one tentative paragraph.

Theories of diamagnetism (p. 458). Faraday, "who discovered, and who so carefully analyzed, the phenomena of diamagnetism was content with putting forth the law with which experiment had furnished him, namely, that diamagnetic substances are those which, in the field of magnetic forces, direct themselves... We must not forget that Mr. Farady distinguishes, by f i e l d o f m a g n e t i c f o r c e, the... space within which the poles of an electromagnet cause their influence to be felt...of which the curves marked out by iron filings give, to a certain degree, a very exact idea."

This is the first time fields enter De la Rive's work; the two qualifications — speaking of poles, and of those of an electromagnet — are strange but unimportant. The field comes in again, with Thomson's (Kelvin's) work, on p. 462. Faraday now comes more frequently, *a propos* of a mistake of Weber which was very hard to correct and which De la Rive first spotted (pp. 464, 466), and the diamagnetism of gases (pp. 468–471).

Magnecrystallic action. Faraday introduces "m a g n e c r y s t a l l i n e l i n e in order to distinquish it from the force which he calls m a g n e-t o - c r y s t a l l i n e" (p. 483), but again the phenomenon (discovered by Plücker) comes with no lines of force the way diamagnetism comes, again with a description of the phenomenon restricted to a homogeneous field (pp. 481-482). On p. 485 we are told, "it is easy to see that Faraday's experiments are altogether of the same order as those of Plücker" which, of course, is false as Faraday did not confine himself to homogeneous fields. Indeed, already a page earlier we were told (p. 486), "The surrounding media exercise no influence over the magnecrystalline property of bismuth, which establishes a further difference between this action and diamagnetic action. M. Faraday only..." etc.

It is quite clear that in De la Rive's version Faraday holds to the action at a distance theory: "Mr. Faraday was struck" we read (p. 489), "with what is so extraordinary a force which, emanating from the poles of a magnet, directs from afar" all sorts of crystals. Needless to say, Faraday explicitly rejected the idea that magnetic forces emanate from magnetic poles; indeed, even Coulomb and Poisson, by whom De la Rive swears, had rejected this idea. And so, clearly De la Rive did not mean to be taken literally; indeed, it is quite possible that because this cannot be taken literally it can be used as a mere hint; as to what is hinted, this is another question. In my impression, the hint is that Faraday accepted the common doctrines of magnetostatics.

This impression is strengthened with the sentence immediately following the one just quoted. "He had consequently admitted that this force is neither attractive nor repulsive, but a simple directive force due to a species of radiation, which, emanating from the magnetic poles, traverses the interposed crystal, and compels it...to place itself so that its axis is parallel or perpendicular to the line according to which this radiation operates." Here we have explicitly action at a distance, emanating from poles, after all, traversing interposed bodies, and it is a kind of radiation! Needless to say, all this is the mere attempt to avoid field language, yet after fields had already been introduced! "This manner of regarding the action has been suggested to Faraday by the phenomena presented by polarized light", namely that of magnetooptics. In other words, the peculiar radiation is just the lines of magnetic forces when illuminated, to use Faraday's language. But magnecrystallic action is independent of illumination, and so the whole presentation is a mere apologetic wriggling. No sooner De la Rive presents magnetooptics, and he distorts Faraday's view on it again.

That De la Rive is uncomfortable is quite obvious. The paragraph which starts with "Faraday was struck", and continues with "as species of radiation" and all that, ends (p. 490) with "Observation... would become inexplicable without this move of regarding the phenomena." Here Faraday is reluctantly introduced as unrivalled. And in the only field in which he felt he was justly rivalled! For magnecrystallism is the only field where Faraday ever acknowledged that an action at a distance theory adequately explains all known phenomenon — the Tyndall-Knóblauch theory. Doesn't De la Rive know of this theory? Yes, indeed.

Faraday's theory, just declared necessitated by observation, immediately comes under attack (p. 490): Faraday is "Constrained... to admit that magnetic action may be exercised independently of ponderable matter" — which is a slur on Faraday since this was his point again and again, aspecially in magnetocrystallism — and he is anyway superseded in the next paragraph by Tyndall and Knoblauch (pp. 490 ff). So, it seems, Faraday is excused for having introduced a theory when there was none better, but now, thanks to others, etc.

Magnetooptics, p. 497. "We have arrived at an important discovery, by which Faraday prefaced his researches upon diamagnetism, which, however, are so independant that we have been able to explain them first, as indeed the logical connection of the facts required of it." Here is a compact wealth of puzzles. How did Faraday "preface" his diamagnetism with his magnetooptics? Two fields are either independent, or logically one comes before the other, but not both. Yet De la Rive claims that Faraday claims that magnetooptics precedes diamagnetism whereas both diamagnetism precedes magnetooptics and they are independent of each other. De la Rive manages both a historical error and a logical error in one short paragraph!

What De la Rive seems to say is this. Magnetooptics proves for Faraday the theory of fields of force, and he uses it in his diamagnetic investigations; but he is in error; diamagnetism can be presented without fields, with the geometric image of elongated objects lying between poles in a transversal or a longitudinal position, (equatorial or axial position); and then idea can be used to introduce magnetooptics, too. This reading resolves the difficulty by removing both the historical and the logical errors mentioned in the last paragraph. It leaves De la Rive with two other errors. First, his description is not as general as Faraday's as it holds only for homogeneous or fairly homogeneous fields. And it assumes that De la Rive was tongue-tied when discussing Faraday's heresies. Yet these two allegations are a running theme throughout the reading of De la Rive's first volume which is here offered.

As to De la Rive's own view, he ascribes (p. 524) Faraday's magnetooptic effect "to an action...exercized neither on the [ponderable] particles alone nor on the [particles of] ether alone, but in the manner of the existence of the particles in respect to the ether." Again, De la Rive introduces the ether in desperation and again it is not clear how; but here, at least, it is his final word. Finally he must admit the existence of the medium, even if he considers it an ether. As usual, after Faraday takes all the abuses, he wins. Doubtlessly, he was as sensitive to this as to other points, and it must have cheered him up in a small way.

The remark on the ether comes at the close of Volume 1 of the English edition. The French edition has an additional section on the general theory of magnetism which appears at the opening of the English edition of Volume 2, beginning with the additions to Volume 1.

De la Rive had intended to publish two volumes — one pure, one applied — and he published two pure volumes and one applied. The first volume contains less then 600 pages, and the second, unintended one, contains 900. This happens to all who deceive themselves about the possibility of completeness. It is significant, however, to notice that De la Rive underestimated his tasks, as it clearly indicates that his injustices to Faraday were rooted largely in the naive optimism of the age.

In his advertisement to the second volume he explains his delay in publishing it as due to his work on the pile. "I hope to have solved this difficult and contested questions in a manner that will be accepted by all who have turned attention to it", he says. First, let us glance at the supplements to Volume 1.

One page 2 we read, "Ampère's theory, however, failed in certain points of direct experimental demonstration. M. Weber succeeded in filling up this gap, demonstrating by certain experiments...the complete identity between the laws of electromagnets and those of natural magnets." This important result has been the means of removing all doubts, that might still have remained, as to the accuracy of Ampère's theory, and consequently has given to it a degree of probability, which approaches almost certainty.

Those who wish to snigger at this may be reminded that Max Born has said almost the same about quantum mechanics. Unfortunately De la Rive not only erred about probability and certainty; he was ambiguous as to whether Weber corrected Ampère's formula or whether he verified the same old formula by new experiments. Of course, Weber explicitly rejected Ampère's formula in a rather unfriendly way and replaced it with his own. But since both formulas are of currents acting at a distance, the later one may be viewed as a modification of its predecessor. Still, De la Rive could have said so; perhaps he would if he were not so uptight about the whole matter.

On page 13 we return to Faraday's rotating magnet. Faraday's and Weber's views on the matter are rejected. Lenz is declared to have given a general theory connecting the phenomena involved! It is a bit strange to encounter such a sweeping statement, aspecially since Lenz's theory is entirely qualitative, and thus *a priori* unsatisfactory. We may remember that when Faraday's (quantitative) theory of electromagnetic induction was introduced qualitatively only, De la Rive admitted his inability to specify the law well enough. Now, it seems, he is not troubled by such details. Rather, he is at pains to show that Lenz's law covers the case in point, the rotating magnet, well enough. So it does, but only if it is not viewed within the action-at-a-distance framework. De la Rive, of course, hints at this (p. 16), "whenever the mutual action...gives rise to...an attraction or a repulsion, or a deviation in one direction or another..."; but he does not allow himself to conclude that this refutes Ampère's and Weber's views. Rather, he pushes on bravely (p. 16).

"Still more recently, Faraday, with a view of studying the magnetic field," — incidentally, the word "field" occurs in Faraday's work only sparsely, in 1846 and later, and here in 1856, yet *Oxford English Dictionary* quotes Tyndall, 1860 — "namely, the distribution of the forces that emanate exteriorly from the poles of a magnet", which, of course, is not accurate enough, "...obtained induction effects, that are remarkable confirmation to Lenz's law. We shall return to these experiments further on, when we are speaking of Faraday's lines of magnetic force..."

Let us quickly skip a lot, including an interesting presentation and discussion of Weber's theory of diamagnetic polarity (pp. 41–44) and return to Faraday (pp. 44–47).

Mr. Faraday does not admit of diamagnetic polarity; we have already said that he regards the action exercised by magnets upon magnetic and diamagnetic bodies as the results of forces emanating from the poles of magnets, according to certain directions, and which he calls 'lines of force', and the whole of which constitute the magnetic field. The presence of a body in this magnetic field modifies the directions of the lines of force: if the body is magnetic, it concentrates the lines of force; if diamagnetic, it makes them diverge. This modification, brought about in the distribution previously uniform of these lines of force, gives rise to attractive movements for magnetic bodies, and repulsive for diamagnetic. Mr. Faraday entered into a detailed study of the magnetic field, and the direction of the lines of force, a very exact idea of which is given by the distribution of iron filings around and between the poles of magnets. We have already seen that he succeeded in employing induction to demonstrate the equality and the distribution of these lines of force in the magnetic field. It follows indeed from the experiments to which we have referred in the chapter on induction that, at whatever distance from the magnet these lines are cut, the induction current, collected by the movable wire by which they are cut, possesses the same intensity; which proves that magnetic force has a definite value, and that for the same lines of force, this value remains the same at all distances from the magnet: neither the convergence or divergence of the lines, nor yet the greater or less obliquity of the intersection, introduces any difference into the sum of their power. The study of the internal part of the magnet leads us to recognise that the lines of force have there also a definite power, and perfectly equal to that of the exterior lines, which are only the continuation of the others; and this whatever the distance may be, which may be infinite, to which they are prolonged.

We must not forget that Mr. Faraday, by the term lines of magnetic force, expresses the power of the force of magnetic polarity, and the direction according to which it is exercised. If the magnetic field is composed of equal forces equally distributed, as may easily be obtained with a horseshoe electro-magnet, we have merely to place a sphere of iron or nickel in this field, to cause an immediate disturbance in the direction of the lines of force...

The few words, that we have been devoting to Faraday's theoretic ideas, are sufficient to make them understood: the fundamental idea of the illustrious philosopher is in the main the negation of all action at a distance, and the explanation of the phenomena by continuous force, forming what he calls lines of force. Bodies, by their presence, modify these lines of force; and there arise directive motions, which are manifested by the disposition of these bodies to place themselves according to their nature, either axially or equatorially, namely, in the places where the force is at its maximum, or in those where it is at its minimum. A learned English philosopher, Mr. Thomson, on applying calculation and notions of mechanics to Faraday's ideas, found that they represented, in a remarkably exact manner, what takes place in this order of phenomena, providing we take into account the mutual action of the parts of which the bodies are composed that are submitted to magnetic influence...

...We cannot altogether acquiesce in Faraday's ideas, however ingenious they may be. Does the magnetic field really exist, as the learned philosopher conceives it to be, namely, independently of the bodies by which its existence is made manifest? This is the point upon which I have some doubts. I am rather disposed to admit that magnetic forces are exercised only so long as there is a body which determines their manifestation...

... Finally, we may remark further, that if the lines of force are sufficient, as Faraday admits they are, to explain all the phenomena, why have these lines need of the intervention of a body in order to act upon the polarised ray, and cannot they act directly upon this ray in vacuo? — a result which we have not been able to succeed in obtaining although employing even a very considerable magnetic power.

I have quoted De la Rive in full here because this passage is perhaps the only fairly adequate representation of Faraday's ideas made in his life-time, and indeed one of the few Faraday could even find, even if we count Snow-Harris (whom he overlooked, though he referred to his observations of discharge patterns and though they were fairly close friends); no doubt, in part the accuracy of De la Rive's description is the result of a disagreement, just as the inaccuracy of Kelvin's description — his ascribing an aether doctrine to Faraday — is the result of an agreement (in the patronizing manner of the age). I have omitted the objection which De la Rive makes, as it is question begging, and left the one which I think is very good, and which was surprisingly answered by Maxwell's theory, or rather by the guage invariance of the vector potential in it. To return to Faraday, no doubt he was pleased with this presentation, no less because it was fair but not in agreement with him.

There are only two further points to make. First, De la Rive's presentation of the theory of the pile is greatly influenced by Faraday and is very sympathetic to him (pp. 353–354, 446–450, 664 ff, and 694 ff), though he refuses to adopt Faraday's terminology (note p. 354). Similarly, many of the experimental details of conduction derive from Faraday or from those who followed his experiments. In particular De la Rive is lucid about the subtlety and importance of Faraday's corrections of experiments determining speeds of currents (pp. 196 ff). Yet, even here, De la Rive is not accurate, for example, when declaring (p. 376) that according to Faraday chemical forces act at a distance.

Second, De la Rive does not explain sufficiently why according to Faraday there is a complete symmetry between positive and negative electricity. Yet he does, correctly, record Faraday's own admissions of cases of asymmetry, in the positive dark discharge (pp. 276–277), in the difference of potential level between the negative and positive surfaces of the condenser (p. 166), and the negative spark. Yet, somehow, he manages to ruin the effect of this point. On the one hand, he does not say that Faraday himself did not find these sufficiently strong criticism. On the other hand, it is not clear that De la Rive himself thinks they are.

On the whole, and in conclusion, one can say, what is missing in the two thick volumes is a focal point, and this is clearly seen in the author's wavering attitude toward fields. In his *Notice sur Michael Faraday* of 1867 De la Rive says explicitly that he is suspicious of Faraday's immaterialism as it seems to him to be idealistic and thus anti-scientific. This, at least, is a clear position. In the *Treatise* he says that Faraday's theory is pretty coherent; but he does not explain it beyond the two or three pages which I have quoted almost in full. Clearly, De la Rive would like Ampère, Weber, and Lenz to win, but he also thinks the world of Faraday; clearly he is greatly ambivalent. Beyond this, it is hard to say.

Perhaps, then, in his very ambivalence he, *malgre lui,* presented himself as open minded, and thus won afresh Faraday's fondness and appreciation.

For the sake of completeness, may I add the following. There is little material added from volume three of over 800 pages on applied electricity regarding Faraday and nothing regarding fields. Faraday makes a small appearance when the electric fish is analyzed; he is conspicuously absent in the long (over one hundred pages) chapter on atmospheric electricity; he appears with his theory of the atmospheric causes of the variation of terrestrial magnetism and its refutation by solar influences on these variations (p. 274); his contribution to conduction in telegraphy (entirely superseded, incidentally, by the work of Kelvin on the topic) is fully acknowleged (pp. 442-443, 446, 468); and, in conclusion of the physiological part, on the last page of the text, Faraday's experiment showing that air may act as an electrode, opens the possibility of viewing a plant as a pile (p. 702). The last 100 pages or so of the last volume constitute series of appendices and notes. First electrostatics, culminating with the debate between Riess and Faraday, and the reaffirmation that Coulombian force plus dielectric polarization explain all electrostatic facts well enough. A few fleeting references to Faraday are there, including, a minor disagreement concerning the pile (p. 753). The chief significance of this volume seems to be that in it attempts to encompass technology within a scientific treatise make the enterprise burst to the seams. A few decades later such a venture would be quite encyclopedic.