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ON THE HISTORY OF THE DISCOVERY OF THE MAXWELL EQUATIONS

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1. INTRODUCTION

THE Maxwell equations represent an example of a fundamental physical law which clearly has been guessed, and not "deduced," in the rigorous meaning of this word, from experimental data. Electromagnetic waves were predicted by Maxwell and only 25 years later were discovered in the experiments of Hertz. It is noteworthy in this connection that the inclusion in the equations of the famous additional term—the displacement current—was not brought about by any kind of necessity: neither by facts known at the time, nor by the dominant physical ideas, nor by the requirements of mathematical consistency of the apparatus of the theory. These circumstances combined, of course, with the exceptional place occupied by the Maxwell equations in modern physics give rise to particular interest in the history of their discovery. This event until now has remained somewhat of a riddle, although to it and, in particular, to the problem of the displacement current, quite a few pages have been devoted in the historical literature (cf., for example, the article by Borc[1] and the references given there).

It is always difficult to elucidate the truth concerning the development of physical ideas for two reasons. Firstly, it is hardly possible to adopt completely the scientific point of view of the past which can even be not too distant chronologically, but which is separated from us by a fundamental discovery which in a radical manner alters the points of view concerning fundamental phenomena and concepts. Secondly, at all time scientific articles have been written not in the manner in which the work itself was carried out, and it is far from simple (if at all possible) to reconstruct reliably from the text the manner of thinking of the author. Sometimes materials are of assistance which were not published during the lifetime of the authors—draft notes, letters and diaries. Several letters of Maxwell to W. Thomson and P. Tait are known which contain discussions of questions in electrodynamics[1]. However, they add very little to the published articles—Maxwell is silent regarding his basic ideas and reports only his results. Apparently the latter is not accidental. The point is that both correspondents (and especially Thomson) had a negative attitude to Maxwell's field theory and, in particular, to the displacement current. This would hardly have encouraged Maxwell, or anyone else in his place, to describe his searches in detail. He primarily tried to convince his correspondents that his concept is an admissible one and leads to nontrivial physical consequences. It should be noted in general that Maxwell was singularly alone in his views concerning electrodynamics. From among his contemporaries one can pick out as being like-minded K. Gauss (1777—1855) and B. Riemann (1826—1866), but Maxwell found out about this only after both had died. M. Faraday(1791—1867) was sympathetic to Maxwell's ideas, but the language of formulas was always foreign to him, and, moreover, in the sixties the ailing and feeble Faraday was already approaching the end of his life. In letters to him Maxwell restricted himself to a general description of the theory and to an explanation of its connection with the concepts which had been earlier developed by Faraday himself. For all these reasons the epistolary heritage of Maxwell so far has not helped us to elucidate the manner in which is equations were born. It remains only to extract everything possible from Maxwell's papers themselves, and from an analysis of the views of the scientific community of his time (which doubtlessly must have been taken into account in the preparation of any publication).

We begin with a brief review of the situation in electrodynamics prior to the appearance of Maxwell's papers. Since we are interested in the spirit of the time, and not in a detailed chronical of events, we shall dwell only on several principal facts which influenced the formation of the views of the physicists of the pre-Maxwellian era concerning electricity and magnetism.

2. ELECTRODYNAMICS PRIOR TO MAXWELL (1800—1855)

Intensive experimental investigations in the field of electrodynamics began after the invention by A. Volta (1745—1827) of galvanic batteries (1799). Twenty years later H. Oersted (1777—1851) discovered the magnetic effect of an electric current (1820)[2]. In the very same

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[1] W. Thomson (Lord Kelvin, 1824-1907), being only seven years older than Maxwell, occupied an incomparably higher position in the scientific world of England. In the sixties he was already "physicist number 1,"a recognized authority not only in scientific, but also in industrial and government circles. Thomson had extensive scientific contacts on the continent (particularly in France) where he visited almost annually.

Peter Tait (1831-1909) occupied a chair in Edinburgh University, and worked in the fields of mathematics and thermodynamics. Tait essentially did not have any significant original results, as became evident later, but he was regarded as a prominent physicist and was close to W. Thomson together with whom he wrote a textbook well known in its time. Contemporaries noted the doctrinaire nature of his mind, and also his active and energetic character. Tait was quite an influential figure, and his opinions were listened to. Maxwell was associated with Thomson and Tait by a common birthplace (all three were Scotsmen) and, moreover, was connected with Tait by their being classmates in Edinburgh and Cambridge.

[2] Oersted's experiment is so simple (even for 1820), that the question arises as to why it had not been performed earlier. This is all the more strange since the "transformation of electricity into magnetism" was sought already starting with 1801. Twenty years were spent in dis-

year A. M. Ampere (1775–1836) proposed the idea of interaction of currents and experimentally proved its correctness[3]. The mathematical formulation of the law of interaction of currents was completed by Ampere in 1826. Theoretical electrodynamics begins in fact from that moment. Two points in the works of Ampere have exerted an essential influence on its further development. The first of them is the idea concerning the nature of all magnetism, the second is the differential law of the interaction between currents.

Ampere's assertion concerning the current nature of all magnetic phenomena, which was at first met quite unfavorably by a number of prominent scientists (among them Laplace, Davy, Faraday) and which remained unproven in the XIX century, nevertheless played a prominent role in the reconstruction of physical ideology. The magnetic substance ceased to be obligatory in the eyes of the scientific community: one could do without it—this is what become clear after Ampere's work.

The formula for the force of interaction between line elements of two currents was written by Ampere with the aim of creating a basis for the calculation of the interaction of currents of arbitrary configuration. In this connection it was, of course, assumed that all the electric currents realized in nature are closed. Ampere's law in modern notation has the following form:

$$\mathbf{F}_{12} = (I_1 I_2 / 2c^2 r^2)\left[d\mathbf{s}_1 \cdot d\mathbf{s}_2 - \frac{3}{2}(\mathbf{n}\, d\mathbf{s}_1)(\mathbf{n}\, d\mathbf{s}_2)\right]\mathbf{n}, \qquad (1)$$

where I_1 and I_2 are the intensities of the currents in electrostatic units, c is a constant equal to the ratio of electromagnetic and electrostatic units of charge which numerically coincides with the velocity of light (this was not yet known in Ampere's time), ds are the elements of length of the current, $\mathbf{n} = \mathbf{r}/r$, and $\mathbf{r}$ is a vector joining these two elements (directed from ds_1 to ds_2). It was considered that Ampere's formula was "deduced from experiment." In actual fact, as is well known, it is not possible to derive uniquely the differential law from the data on the interaction of closed currents, but Ampere imposed an additional condition which seemed to him to be completely obvious: he required that $\mathbf{F}_{12}$ should be a central force, i.e., directed along n[4]. This erroneous idea was completely shared not only by Ampere's contemporaries, but also by later investigators who derived from his formula the law of interaction between moving charges.

The next fundamental event in the development of electrodynamics was the discovery by Faraday of electromagnetic induction (1831) and the almost simultaneous with it (1832) discovery of self-induction by J. Henry (1799–1878). The induction of a current by a magnet, i.e., a process inverse to Oersted's phenomenon was sought by Faraday (and not only by him alone) in a *completely directed manner*. But the *nonstationary* nature of induction was completely unexpected[5]. The effect, the existence of which gave rise to no doubts, nevertheless seemed to be so strange that it even gave an impression of something ugly, something lacking a natural internal logic—it could not be understood "why nature should require" that the current should be induced only as a result of the motion of a magnet or of a change in the value of the current in the primary circuit.

The new phenomenon found its place in physics only after 16 years due to the publication in 1847 of a paper by Helmholtz (1821—1894). He showed that electromagnetic induction necessarily follows from Ampere's law if one takes conservation of energy into account[6].

Since the induction effects of magnets and currents turned out to be quite the same Ampere's hypothesis concerning the electrodynamic nature of all magnetism became a common conviction. Although "molecular currents" remained just as mysterious as before, there were very few who believed in a "magnetic fluid". The completion of the theoretical basis of electrodynamics was now seen to consist of finding the general law for the interaction of charges (in motion and at rest). In

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covering an easily observable phenomenon because in connection with the then dominant concepts the effect was being sought not where it might have been discovered. Oersted had his own point of view on the interrelationship between electricity and magnetism. Although it was just as incorrect as other more popular points of view, nevertheless it led to a new way of conducting the experiment (concerning Oersted cf., [2], and concerning his work in Russian translation see the monograph [3]).

[3] Ampere who had never previously conducted experiments carried *out his experiment in the course of two weeks immediately after* he had learned from D. Arago about Oersted's results. The speed of his reaction to this discovery was possibly due to the fact that he had even earlier doubted the existence of a separate magnetic fluid (see the works of Ampere in the monograph [3], and concerning Ampere himself see the book [4], which is provided with a bibliography).

[4] Ampere considered that only in this manner was it possible to guarantee that Newton's third law would hold for the interaction of currents, -in any case in setting $\mathbf{F}_{12} \parallel \mathbf{n}$ he used this particular argument.

[5] The experiment of J. Colladon (1802-1893) is characteristic of the dominant views of the time. The layout of his experiment was quite analogous to that of Faraday, but the galvonometer included in the secondary circuit was placed in another room; the experimenter switched on the current in the primary circuit and then went over to the galvonometer. Colladon, a professor of the Geneva Academy, was a well qualified and prominent physicist (he is known for his accurate measurement of the velocity of sound in water—the experiment was carried out in Lake Geneva in 1827). He, probably not without a reason, separated the *galvonometer from the rest of the experimental set-up—it* was rumoured that he saw the reaction of his apparatus to the switching on of the current in the primary circuit, but, being completely convinced of the uniqueness of the possible nature of the phenomenon he ascribed the actually observed effect as being due to the apparatus, and tried to get rid of it. It is worth while to recall that Faraday also expected to see a steady deflection of the galvonometer needle. But he, moreover, had the courage to believe his eyes more than a priori opinion of the majority.

[6] Here the development of electrodynamics comes into close contact with progress in the understanding of conservation laws. The paper by *Helmholtz* quoted above "On the Conservation of Force" contained a general formulation of the law of conservation of energy in mechanics. As one of the examples the author considered the interaction of currents. Helmholtz's paper was given a fairly cold reception by the scientific leaders in Germany, it was supported only by K. Jacobi (1804-1851), and it became established only 4-5 years after publication. Independently of Helmholtz W. Thomson in 1851-52 also derived induction from Ampere's law and the conservation of energy. It should be noted that an essential role in the forties was played by the paper of F. Neumann (1798-1895). It did not contain any new physical ideas, but in it, as contemporaries said, "Ampere's method was applied to the phenomenon of induction", i.e., the laws of induction were formulated mathematically in the spirit of action at a distance (in integral form).

this connection in accordance with the principles of Newtonian dynamics it was considered that the law being sought should uniquely express the forces of interaction between charges in terms of the distance between them and of their velocities at a given instant of time. A program of this type was particularly popular in Germany which in the later forties was already in a leading position in the experimental investigations in electrodynamics. Among the German groups the most active one at the time was the Göttingen group headed by W. Weber[7]. To him belongs the first formulation of a simple "unique law" satisfying both the experimental data and the generally accepted theoretical views. At that time it was considered that any interaction forces can depend only on the distances between the particles and on their relative velocities. It is possible to obtain such interaction forces between charges from Ampere's formula (1) only under definite assumptions concerning the structure of observed currents. In particular, if the charges e_+, e_-, constituting a current, and their velocities v_+, v_- satisfy the equation

$$e_+v_+^2 + e_-v_-^2 = 0, \text{ or } e_+ + e_- = 0, \tag{2}$$

then from (1) and the Coulomb law it is possible to derive Weber's forces

$$\mathbf{F}_{12} = \frac{e_1e_2}{r^2}\left[1 + \frac{r}{c^2}\frac{d\mathbf{un}}{dt} - \frac{1}{2c^2}(\mathbf{un})^2\right]\mathbf{n} = -\nabla V(\mathbf{r}), \tag{3}$$

where

$$V(\mathbf{r}) = \frac{e_1e_2}{r^2}\left[1 - \frac{1}{2c^2}(\mathbf{un})^2\right] \tag{4}$$

and u is the relative velocity of the interacting charges[8]. Due to the subsidiary condition (2), which can never be satisfied for moving charges of only one sign, the magnetic effect of an observed current is only indirectly related to the fundamental law of the interaction of charges. The physical reasons guaranteeing that this condition would be satisfied were not clear, and, therefore, there was no answer, for example, to such a question: do open currents exist which locally affect a magnetic needle in the same way as closed currents? According to Weber's concepts closed and open currents are in fact separated by the same abyss as in Ampere's time. Thus (we note that once again) in pre-Maxwellian electrodynamics a moving charge and an experimentally observed current are not the same thing; the latter, in principle, can be reduced to the former, but only under a specific subsidiary condition.

Weber's electrodynamics was argued against by Helmholtz who noted the fact that the negative sign of the second term in (4) can lead, generally speaking, to a physically meaningless situation—to an infinite increase in the kinetic energy of the particles in a closed system. However, this "small cloud" was not regarded by the majority as making "bad weather"—it seemed that the fundamental principles of electrodynamics were established[9]. For the completion of a quantitative formulation of the fundamental interaction law it remained to measure the constant c—the ratio of the electrostatic and the electromagnetic units of charge. The appropriate experiment was carried out by Weber and Kohlrausch[10] in 1855, and soon after that G. Kirchhof (1824—1887) noted that the value obtained by them coincides within experimental error with the velocity of light[11]. However, no special significance was ascribed to this. Weber, in particular, considered that due to the obvious difference in the nature of the phenomena of electrodynamics and of optics the equality of the two constants mentioned above is simply an accidental coincidence.

Summarizing the above discussion it must be said that pre-Maxwellian electrodynamics was essentially a completely sensible theory based on facts and on general physical principles which had been crystallized in the course of the preceding development. If one leaves out of account certain errors and confusions which could have been and, possibly, would have been eliminated later, one cannot, strictly speaking, call this theoretical picture incorrect—the retarded interaction can be represented with any prescribed accuracy as an instantaneous one but dependent on the velocities and on their derivatives of a finite order with respect to time. As experimental data were accumulated the heuristic weakness of such a theory would naturally become more and more apparent, but one could have remained for quite a long time a captive of its ideas.

The pre-Maxwellian theory agreed with experimental data, was free from obvious internal contradictions, and, therefore, no necessity was felt during that period of an alternative approach to electrodynamics. Only a few, literally three or four, scientists were persuaded by their physical intuition and, possibly, by their scientific taste, that all was not well here.

We have already mentioned Gauss, Riemann, and Faraday as being of a like mind with Maxwell. Gauss' opinions became known from his letter to Weber (1845)

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[7] W. Weber (1804-1890), a professor of physics in Göttingen University, was close ot Gauss together with whom he invented the first electromagnetic telegraph.

[8] Formula (3) was published in 1845, its second term (expression of the force in terms of a potential) and formula (4) were published in 1848. Different authors proposed several different variants of "unique laws" for the interaction of charges. In particular, in 1835 one such law was obtained by Gauss (published after his death). However, his formula gave an incorrect description of the induction of currents. Noncentral forces quadratic in the velocity were considered later by R. Clausius (1822-1888). All the laws for the interaction between charges proposed in preMaxwellian times differ from the true law (written out with an accuracy up to terms of the second order in the velocities inclusive).

[9] Weber replied to the objections of Helmholtz that the effects pointed out by Helmholtz would occur at very large velocities or at excessively small distances which are not realized in actual experiments. It should be noted that although Helmholtz was right in the main points, in his controversy with Weber, he also committed some errors. According to the testimony of the well known mathematician F. Klein (1849-1925) the attacks of Helmholtz on Weber "did not meet with decisive success" (cf., [5]). Although Klein himself belonged to the "Göttingen camp", and in the seventies collaborated with Weber, one can believe his estimate of the influence of this argument at least on German scientists—it is indirectly supported by a multitude of other data.

[10] F. Kohlrausch (1840-1910) was the author of one of the first instruction manuals for practical laboratory work in physics.

[11] Weber and Kohlrausch measured a quantity larger by a factor of $\sqrt{2}$ than the ratio of the corresponding electrostatic and electromagnetic units. Therefore they obtained the value of the constant $\sqrt{2}c$, the relationship of which to the velocity of light was not immediately apparent.

published after his death. In it he states that for quite a long time (approximately 10 years) he was preoccupied by theoretical investigations in the domain of electrodynamics based on a retarded interaction between charges. He sought a "constructive concept" concerning the mechanism of transmission of interaction, but did not succeed in this.

In 1858 Riemann sent to the Göttingen Scientific Society a paper containing a wave equation, but only for a scalar potential. In it there was given in an explicit form an expression for a retarded potential. However, soon afterwards, Riemann withdrew his paper and it was published only after his death in 1867[12].

Neither Gauss nor Riemann state quite definitely what forced them to seek a new approach to electrodynamics. Nevertheless, there are reasons for thinking that they primarily disliked instantaneous forces dependent on the velocities.

Quite different reasons gave rise to the field concepts of Faraday. A great role was played here by his desire to have a visualizable picture of phenomena without which Faraday, who did not employ the aid of analytical apparatus, would have had difficulty in sorting out the quantitative side of the matter. Later Faraday ascribed to the field not only an illustrative meaning—the idea of the transmission of electromagnetic action from point to point through some physical medium was close and comprehensible for him (although in his statements concerning this very medium he was very indefinite). It should be noted that prior to Maxwell nobody was particularly interested in Faraday's lines of force (according to Helmholtz's testimony the theoretical aspect of Faraday's work was not accepted by his contemporaries).

A significant contribution to the development of electrodynamics was made by Helmholtz. He himself did not propose any field hypotheses and also did not openly come out to propagandize the Maxwellian point of view, but he was not satisfied with Weber's approach to electrodynamics. This was expressed not only in critical remarks concerning the theoretical work of the Göttingen school, which was mentioned above, but also in the fact that he insistently urged his pupil H. Hertz (1857—1894) to study Maxwell's work. The extent to which Maxwell's theory lacked popularity even in the eighties can be seen for instance from the fact that Hertz in his experimental work designed to test the Maxwell equations avoids references to Maxwell as much as possible. Thus, in the 1887 paper "On very rapid electric oscillations" devoted to the discovery of the inductive effect of the displacement currents there is no reference to Maxwell at all, and in the famous 1888 paper "On electrodynamic waves in air" a mention of Maxwell's theory is contained only in the concluding lines, and even then only after the disclaimer that: "Experiments described in the present paper, just as the preceding experiments on the propagation of induction, are presented without reference to any theory, since these experiments are convincing independently of any kind of theory"[6].

3. JAMES CLERK MAXWELL (Biographical Notes)

We would like to call here the reader's attention to two circumstances—firstly, to the fact that Maxwell's investigations in the domain of electrodynamics were interwoven in time with work on the solution of other problems and, secondly, to Maxwell's scientific and social position, or more accurately to the opportunities he had to exert an influence on the dissemination of his ideas apart from the publication of original papers.

The information given below is not new—it is contained in the biographies of Maxwell written by different authors (cf., for example,[7,8]).

Maxwell was born on June 13, 1831 and died (of cancer) on September 5, 1879 in the 49-th year of his life. He received his higher education in Edinburgh and Cambridge Universites. In Cambridge Maxwell spent all told approximately six years (1851—1856) and of these the last two years in postgraduate work (preparation for a professorial title). Here he carried out and published in 1855—1856 the first work on electrodynamics "On Faraday's Lines of Force" in future denoted by the numeral I). This incidentally was Maxwell's first significant physical work. He was not yet known to anyone other than his classmates and university teachers. Since, moreover, the ideas developed in this paper ran counter to the general stream, they struck no resonant chords at all. We shall return to paper I in the next section and here we note only that in it Maxwell's equations were already contained, but without the displacement current. After Cambridge Maxwell from 1856 to 1859 gave lectures in physics in Marischal College in the city of Aberdeen (a port city in Scotland). There he carried out (in a competition for the Adams Prize announced by Cambridge) an investigation on the stability of Saturn's rings (Laplace had shown that they cannot be solid, Maxwell eliminated a liquid, and proved the stability of a configuration consisting of a distributed accumulation of rocks).

In the fall of 1859 at the end of his "Aberdeen period" Maxwell published a paper on the kinetic theory of gases which contains the Maxwellian distribution of velocities. In Aberdeen, just as later in London, Maxwell also concerned himself with the problem of color vision (it is particularly in connection with these investigations that in the early sixties he became acquainted with Helmholtz who was visiting England). In 1860 Maxwell moved to London where up to 1865 he taught in King's College. During these five years he completed the formulation of his theory of the electromagnetic field, i.e., if we confine ourselves to the main point, he added the displacement current to the previously obtained equations. The displacement current first appeared in his paper "On the physical lines of force" (II) published in 1861–1862. In 1864 it was followed by the article "The dynamic theory of the electromagnetic field" (III) which in the clarity and compactness of exposition is the best of Maxwell's electrodynamic papers. In 1865 Maxwell left King's College and went back to his estate (Middleby, near Edinburgh). Thus, electrodynamics was "created" by Maxwell in the course of the first ten years of independent investigations. From the data quoted above it also follows that work on this problem suffered an interruption: between the formulation of the first differential equations (1855)

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[12] Riemann's paper was criticized by Clausius who noted, in particular, that it contradicts Weber's formula. The retarded interaction was also considered in two other papers (1868) by C. Neumann (1832-1925) and Betti. It is difficult at present to understand these papers.

and the introduction of the displacement current (1861) Maxwell concerned himself with other problems. This means that in order to add a single term—the displacement current—Maxwell had to overcome some sort of a difficult barrier, an ideological, and not a technical one. At a certain stage his "attack" seemed to be swamped—the differential equations of the field without the displacement current introduced (in the final analysis) nothing new. Doubtlessly Maxwell continued to think about electrodynamics, but "not under forced draft", being sidetracked into other fields, generally speaking less significant ones. This was partly connected with the fact that practically no one worked in this Maxwellian direction—it was most unpopular. A scientific reputation, even though not a very prominent one, was made for Maxwell not by the work on electrodynamics, but just by those results obtained by him in the period between the creation of the first variant of his equations and the introduction of the displacement current.

The Marischal College in Aberdeen and King's College in London were in Maxwell's times second rate educational institutions. The quality of the students was not high, and Maxwell did not have pupils or collaborators who could aid the development and the dissemination of his new ideas[13]. In 1861 Maxwell was elected a member of the Royal Society of London. In those years this meant that he was acknowledged as a scientist carrying out independent investigations, but by itself the election did not bring any official post, particular influence or other privileges[14]. Thus, in the first half of the sixties Maxwell did not possess any additional (over and above the printed publications) possibilities of converting others to his faith. These circumstances were intensified by his departure from London. In fact, from 1865 to 1871 Maxwell was separated from university youth. Maxwell's scientific contacts abroad also were not very intense. Only a single trip by Maxwell abroad is known—for medical reasons he visited Italy in 1867. During his seclusion in Middleby Maxwell wrote the monograph "A treatise on electricity and magnetism" (IV) which first appeared in 1873 and was subsequently republished several times.

In 1871 an essential change occurred in Maxwell's life: he accepted an invitation to occupy the newly established Chair of Physics in Cambridge[15]. In connection with the chair it was proposed to create a physics laboratory—the first purely research establishment in England. The Cavendish Laboratory which subsequently became famous was built under Maxwell's direction and partially with his money (the original donation of the Duke of Devonshire—a relative of G. Cavendish—turned out to be insufficient). Maxwell occupied the posts of being in charge of the Chair of Physics and of being the director of the Cavendish Laboratory until the end of his days (1879). The staff of the laboratory was not large. But among Maxwell's pupils in Cambridge there were persons who later became professors and prominent physicists. One of them was A. Schuster (known for his investigations in physical optics). During the 1875/6 Academic year Schuster gave at Manchester the first course in England (and generally in the world) on the theory of the electromagnetic field. Three persons heard his lectures, among them J. J. Thomson.

Thus, Cambridge played a definite role in the dissemination of Maxwell's ideas. Here Maxwell for the first time acquired the possibility of communicating with sufficiently able young physicists. It is they who became the first supporters of Maxwell's electrodynamics. And although general acceptance of the theory came after Hertz's experiments which were carried out not in England but in Germany, Maxwell's pupils made their contribution—in their absence Maxwell's papers on field theory could have remained without consequences for an even longer period.

Concluding this brief biography we emphasize once again two facts. Firstly, we recall that nearly five years were needed for the introduction of the displacement current into already available equations. Secondly, it is essential that the isolation of Maxwell in problems of electrodynamics began gradually to disappear only in the last years of his life—eight-ten years after the appearance of the basic works referred to above.

4. THE INITIAL STAGE (1855–1856)

Two new fundamental ideas were introduced by Maxwell into electrodynamics—the differential equations of the field and the displacement current. Formally these innovations are to a certain extent independent, and it might appear that they were brought about by different motivations. For example, the transition to the concept of a field is most frequently associated with the attractiveness of local action, but the question of the displacement current is essentially treated separately, as a result of which it indeed turns into a riddle. The explanation of the transition to a field in terms of a sympathy to local action appears at first sight to be quite natural and plausible, but this is not supported by an analysis of Maxwell's papers. From them it follows that Maxwell began to regard the field, and the medium through which it propagates, as a physical reality quite late, only after he had deduced from his equations the existence of electromagnetic waves, i.e., after the introduction of the displacement current. Until then he utilized the field and its representation with the aid of different kinds of media with the frankly illustrative aim of constructing easily visualizable pictures of quite complex vector equations. The latter may be seen both from the fact that in a single paper in order to elucidate different analytical relations Maxwell introduces models which differ in their specific content, and also from his direct statements in this regard.

In paper I he writes, for example: "One should not regard this substance in the same way as a hypotheti-

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[13] It should be added that Maxwell apparently was not a brilliant lecturer. As far as one can judge, he had a conscientious attitude to his pedagogical duties, but his lectures were not popular among students.

[14] The Royal Society of London is sometimes called the English Academy. But in actual fact membership in the Royal Society in the last century could hardly be compared to the official general academic titles of continental Europe.

[15] Until then Cambridge University had only a chair of so-called Natural Philsophy—a peculiar historically formed conglomerate of certain aspects of mathematics, physics and chemistry. The new Chair of Physics was first offered to W. Thomson. After his refusal it was offered to Maxwell.

cal liquid in the sense which was admitted by old theories for the explanation of phenomena. It represents *exclusively a set of fictional properties constructed* with the aim of presenting some mathematical theorems in a form more easily visualizable and more easily applicable to physical problems than the form which utilizes purely algebraic symbols''[16]. The space allotted to the present article does not allow us to increase the number of similar quotations, which could be done with no difficulty.

Maxwell understood perfectly well from the very beginning that a theory completely equivalent to action at a distance can nevertheless be formulated in terms of partial differential equations. Therefore, a transition to such equations in the case of electrodynamics (and this constituted the content of paper I) was not regarded by him at all as a transition to physical local action. For example, he was aware of the Poisson equation for the gravitational potential which no one intended at that time to interpret in the spirit of some kind of a field concept. Maxwell himself even thought that gravitation in general can not be interpreted within the framework of a physical field theory[17]. This is one of the few incorrect assertions contained in Maxwell's papers, and is of interest to us because of the fact that it once again shows how far Maxwell was from any preconceived a priori methodological scheme or a dogmatic system of views.

What, then, in this case pushed Maxwell towards a reconsideration of the electrodynamics, what did he find inconvenient in the Weber scheme? A certain, although very sketchy and not very distinct, motivation of this step is contained in article I. From the text of the introductory section of the article it can be concluded that Maxwell was not satisfied by the "disconnectedness," if one may use this expression, of the charge at rest and the moving charge in the electrodynamics of that time. At the very beginning of article I (see[9]) there are contained, in particular, the following lines: "The modern theory of electricity and magnetism embracing all the phenomena related to them must not only elucidate the connection between electricity at rest and electricity in motion, but also between attractions and inductive effects in both states".

If we turn to the law of interaction between charges (3), we see that although Weber did call it "unique," in reality it is not of such a nature since for the description of the interaction between moving charges in fact a new empirical constant c has been introduced, which within the framework of this approach does not have a direct physical meaning.

The fact that formula (3) is an approximate one was understood and even noted (in connection with Helmholtz's criticism) by Weber himself, but his theory did not contain any rule or approach to the calculation of the next approximations. In this sense his formula was *not a theoretical one, but rather a semiempirical one.* In essence a charge at rest and a moving charge in preMaxwellian electrodynamics were different physical objects the interaction between which was determined by different empirical constants—the charges and a "critical velocity". Maxwell apparently aimed first of all at getting rid of this internal imperfection of the theory. Having the aim of understanding the connection between "electricity at rest and electricity in motion" he went over to a new formalism and tried to find unique equations by modelling electrodynamic quantities by the motion of an ideal incompressible fluid, since even earlier (1842) W. Thomson noted the similarity between "theories of gravitation and heat" (which was also treated as a fluid). Did article I reach the goal for the attainment of which this investigation was started? No, since without the displacement current and, consequently, without electromagnetic waves, without a true physical retardation of the interaction between charges the constant c having the dimensionality of velocity remained just as uninformative an empirical quantity as in the Weber theory. In more formal language this circumstance is expressed by the fact that between the current density j (i.e., "electricity in motion") and the charge density ρ ("electricity at rest") there is no connection in the initial Maxwellian scheme. Indeed, in the equations (we write them for the case of vacuum and in modern notation)

$$\begin{array}{ll} \text{a)}\ \operatorname{div}\mathbf{E}=4\pi\rho, & \text{c)}\ \operatorname{rot}\mathbf{H}=\frac{4\pi}{c}\,\mathbf{j}, \\ \text{b)}\ \operatorname{rot}\mathbf{E}=-\frac{1}{c}\frac{\partial\mathbf{H}}{\partial t}, & \text{d)}\ \operatorname{div}\mathbf{H}=0 \end{array} \tag{5}$$

the quantities ρ and j are independent (since an equation connecting them does not follow from (5). As has been already noted previously, equations (5) encompassed all the fundamental facts known at that time (Coulomb law, Ampere law, electromagnetic induction). In particular, Eqs. (5) reflected also the fact that all the actually observed electric currents were in closed circuits. Maxwell makes special note of this by separately writing out the equation

$$\operatorname{div}\mathbf{j}=0 \tag{6}$$

which follows from (5), and notes that "...our investigations at the present time (the emphasis is mine—I. Sh.) are restricted to closed currents, since we know very little concerning the magnetic effects of any kind of open currents".

In this remark the fact is nontrivial that not only is the possibility of a magnetic effect of open currents not excluded, but even more than that the examination of such a possibility appears to be included among the topics to be investigated. We recall in connection with this that in pre-Maxwellian electrodynamics the experimentally observed current was represented by the combination of moving charges of two signs with the additional condition (2) imposed on their velocities or on the sum of the charges. Therefore, the transition from closed currents to open currents did not at all reduce to a simple stopping and accumulation of charges at some point.

Thus, before changing anything in the initial equations (5) Maxwell had to free himself from the hypnosis

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[16] Quoted according to [9].

[17] Quite a bit of space is devoted to this question in article III. Maxwell saw the impossibility of a field theory of gravitation in the fact that gravitational charges of like sign are attracted and not repelled as in electrodynamics. From this he concluded that when two charges coalesce both the kinetic energy of the particles and the field intensity must be increased, and therefore also the energy stored in the field Maxwell regarded the gravitational field in analogy with the electromagnetic field as a vector field).

of the widely accepted model constructions of a current. Although Eqs. (5) do not establish a connection between "electricity at rest and electricity in motion" and in this sense do not solve the problem posed by Maxwell as a cornerstone, nevertheless they provide good starting points for a future investigation. The reason consists of the fact that equations (5) connect in an explicit form the magnetic forces with the local properties of the current density, i.e., with its functional dependence on the space coordinates and the time. Equations (5) enable us to trace the way in which a change in the solenoidal properties of the current can influence its magnetic effects. This represents the principal significance of the results of the first electrodynamic paper of Maxwell for the further development of his theoretical search.

5. THE DISPLACEMENT CURRENT (1861–1864)

As has been pointed out already, the displacement current was first introduced by Maxwell in article II. The first two parts of this paper are devoted to a mechanical modelling ("molecular vortices") of Eqs. (5). The displacement current appears in the third section which is entitled "The theory of molecular vortices applied to static electricity." Indeed, in this part of the paper "the molecular vortices" do not appear at all—a mention of them in the heading of the section is simply equivalent to a reference to equations (5b) and (5c). The displacement current is introduced by Maxwell both here and in the next paper III in the same manner: he notes, merely in passing, that the molecules of the medium are polarized under the action of the electric field and the displacement current arising from the motion of bound molecular charges must be added to the external current.

It is noteworthy that immediately after this Maxwell goes over to the equation of continuity with the right hand side

$$\operatorname{div} \mathbf{j} = -\frac{\partial \rho}{\partial t} \qquad (7)$$

and shows that the new equation

$$\operatorname{rot} \mathbf{H} = \frac{4\pi}{c}\mathbf{j} + \frac{1}{c}\frac{\partial \mathbf{E}}{\partial t} \qquad (8)$$

together with (7) leads to (5a). Thus, the basic equation of electrostatics is deduced from (7) and (8) (this explains the heading of the third section of reference II). Maxwell seems here to obtain the Coulomb law (a consequence of (5a)) from the magnetic properties of the current and the assumption that this current could also be an open one.

Equation (5a) had been written by Maxwell earlier in article I, while the equation of continuity (7) was at that time already well known in other domains of physics (as applied to other nonelectric currents and densities). Therefore, it appears quite plausible that Maxwell first found the famous term $(1/c)\partial \mathbf{E}/\partial t$ by starting from (7) and (5a) (if (7) is valid, then to the old equation (5b) one must add a certain vector **X** such that

$$\operatorname{div} \mathbf{X} = \frac{4\pi}{c}\frac{\partial \rho}{\partial t} = \operatorname{div} \frac{1}{c}\frac{\partial \mathbf{E}}{\partial t},$$

from where it follows that one can set $\mathbf{X} = (1/c)\partial \mathbf{E}/\partial t$).

Thus, the principal achievement of article II is Eq. (7). In the light of the preceding it is clear that the transition from (6) to (7), i.e., from closed to, generally speaking, open currents was not an easy step, since for this one essentially had to break away from the ideas concerning an observed electric current predominant in those years. On the other hand, a push in the direction of this step came from a simple and physically natural generalization of Eq. (6). The result obtained connected together the previously independent Eq. (5a) with equation (8)—a modification of the earlier Eq. (5b). In reference II Maxwell did not yet speak directly about the retarded nature of the interaction between charges in electrodynamics and, as we have noted already, did not explicitly write out the wave equation, although the fact that equations containing the displacement current yield retardation and waves was already clear to him[18]. In this connection the trend of though, or at least the presentation of the material, in article II differs considerably from the work of Riemann in which the retardation and the wave equation (for a scalar potential) are the starting point.

Article III published in 1864 in many respects is not similar to article II. In this paper Maxwell places in a prominent position the retarded nature of electromagnetic interaction and the localization of energy in the space surrounding the charge, the electromagnetic field. With the aim of reaching the reader with the basic content of the theory Maxwell emphasizes (cf.,[9]): "... Utilizing such words as electromagnetic momentum and electric elasticity with respect to the well-known phenomena of the induction of currents and the polarization of dielectrics I want only to direct the thought of the reader to mechanical phenomena which might help him to understand electric phenomena. All such expressions in the present paper must be regarded as illustrative ones and not as explanatory ones. However, speaking of the energy of the field I wish to be understood literally. Every form of energy is the same as mechanical energy, whether it exists in the form of ordinary motion or in the form of elasticity or in some other form." Further, emphasizing the problem of the localization of energy Maxwell writes: "According to our theory it resides in the electromagnetic field, in space surrounding electrified and magnetized bodies, and also in these bodies themselves, and manifests itself in two different forms which can be described without introducing hypotheses such as magnetic or electric polarization or, in accordance with a very probable hypothesis, as motion and stresses of the same medium. The conclusions at which we have arrived in this report are independent of this hypothesis since they are derived from experimental facts."

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[18] In the fourth section of article II ("Application of the theory of molecular vortices to the effect of magnetism on polarized light") Maxwell considered the wave equation, but it is not derived by him from (5) and (8) (as is done later in article III), but is derived as if anew in the form of the solution of the following problem of the theory of elasticity: "Find the equations for a wave motion in a medium containing vortices assuming that the oscillations are perpendicular to the direction of propagation." And the hypothesis concerning the electromagnetic nature of light is stated earlier in the third section of this paper, on the basis of a very exact coincidence of the constant c measured by Weber and Kohlrausch with the velocity of light.

The significant point in these excerpts is not so much the statement concerning the illustrative nature of the models utilized (this had been explained even earlier), but the fact that now Maxwell quite distinctly considers the electromagnetic field as a physically real system in which definite energy is localized. Such clear and categorical assertions were not contained in the preceding papers, even in article II where the displacement current was first introduced, in spite of a specific model realization of the quantities appearing in the theory. The displacement current is introduced in article III in exactly the same manner as in article II, i.e., as a current determined by the polarization of the molecules of the medium under the action of an electric field. Thus, if we speak of the evolution of Maxwell's views, then the principal impression remains that in article III the physical content of the retarded interaction of charges is for the first time clearly realized. Having established that the field in electromagnetic phenomena must be a physical reality Maxwell for the first time in article III concerns himself with the question of the role of retarded interaction in physics in general. With this are also connected the speculations contained in article II concerning whether the field approach is obligatory with reference to other forces known at the time—gravitational forces (as has been noted previously, a field theory of gravitational interactions appeared to Maxwell to be impossible).

6. CONCLUSIONS

All that has been presented above testifies to the fact that the starting points of Maxwell's electrodynamic investigations were hardly the a priori conviction of the necessity of local action and the desire to reduce electromagnetic phenomena to purely mechanical ones.

As far as one can judge on the basis of Maxwell's papers and the sequence of the development of ideas in these papers, the initial stimulus to the reconsideration of the dominant ideas was the lack of satisfaction with the purely empirical nature of the law for the interaction between moving charges, and by the absence of an organic connection between electricity at rest and in motion. This connection is contained in the equation of continuity (7) and the generalization of local magnetic properties of closed currents to open currents led to the introduction of the displacement current into the differential equations written earlier. In other words, if one speaks very briefly, the appearance of the displacement current in Maxwell's equations is, in our opinion, the result of generalizing the equation of continuity (6) to the case of open currents (7). It is quite probable that this particular step was the first one.

The concepts described above are close to the point of view of R. E. Peierls[10]. However, we would like once again to recall that the very transition from (6) to (7) was connected in fact with giving up the concepts most widely spread in the Maxwellian electrodynamics concerning the nature of the current giving rise to a magnetic effect, and with the conclusions associated with them concerning the interaction of moving charges. For this reason one cannot think that in order to obtain the displacement current it was sufficient for Maxwell to introduce a medium consisting of molecules elastically polarized by the field—as should be clear from the preceding, the open current arising as the result of such a polarization was in Maxwell's time not at all regarded by the majority of physicists as being equivalent in its magnetic effect to the experimentally observed currents (satisfying Eq. (6)).

In other words, a medium with polarizable molecules could become a useful model only after Eq. (7) had been postulated, which by itself immediately led to the necessity of adding the displacement current to Eq. (5b). Thus, this model also could not, in fact, play a heuristic role—it was to a large extent also illustrative.

As regards the profusion of mechanical illustrations in Maxwell's papers this was an evident tribute to the spirit of the times and to W. Thomson (the latter, as is well known, in general considered that any phenomenon which had not been reduced to a description in terms of mechanics could not be numbered among those understood). We also note that the role played by Maxwell's mechanical models was generally speaking completely correctly understood by some followers of his theory. For example, the unambiguous statements in this regard by Hertz and Poincare (1854–1912) are well known.

Among the possible causes which induced Maxwell to introduce the displacement current a discussion has been given in the literature of the symmetry of Eqs. (5c) and (8) with respect to the vectors **E** and **H** (if $j = 0$, these equations go over into each other on the replacement $\mathbf{E} \rightarrow -\mathbf{H}$, $\mathbf{H} \rightarrow \mathbf{E}$). This question has been analyzed in detail in[1] and all that remains for us is to associate ourselves with the conclusion of the author: neither the text of Maxwell's articles I–IV nor his other publications in electrodynamics give any basis for supposing that the displacement current had been introduced from a desire to impose on the equations the symmetry indicated. It appears to be very improbable that Maxwell, if he had introduced the displacement current as a consequence of considerations of symmetry, would not at least once in the discussion of the already obtained equations draw the reader's attention to this symmetry.

In the preceding discussion we have attempted, firstly, to give a brief description of the state of electrodynamics prior to Maxwell, and secondly, to give a reconstruction of the development of the ideas of Maxwell himself in the process of his electrodynamic investigations. It is quite possible that this second problem will never be solved completely and definitely, unless some heretofore unknown documents are found. Nevertheless, the interest in this problem forces one to examine more closely the historical development of these ideas, and this appears to be instructive in many respects. The pre-Maxwellian electrodynamics was a semiempirical phenomenological theory with the aid of which a definite systemitization of experimental data was attained and a connection was established between different experiments. It was free from logical and serious physical contradictions, seemed to be the only possible natural extension of those ideas and methods which in fact created physics as a quantitative science based on a few clearly formulated assumptions. It was assumed that the specific form of the law of instantaneous interaction between charges might be altered as new experimental information was obtained, but that some such law must lie at the base of a quantitative theory of electromagnetic phenomena—the overwhelming majority of Maxwell's contemporaries had no doubts of this at all.

It appeared obvious that no matter what is the nature of electromagnetic forces the whole multitude of electromagnetic phenomena, at least in principle, can be expressed in terms of an elementary event—the interaction between two charges. This particular point appeared to be self-evident and impregnable.

The attacks by Helmholtz (and of anyone else) on the Weber (or any other) law for the interaction between charges could not shake this conviction.

Maxwell's theory (after the introduction of the displacement current) swept away this very principle, and because of this its non-acceptance was so strong and prolonged.

The history of electrodynamics presents an example of the fact how remarkably far from the truth can the "most general", "rigorous" and "phenomenological" approach sometimes turn out to be, if one is dealing with fundamental physical laws. In actual fact all these epithets frequently reflect only the fact that the opinion of the scientific community is firmly circumscribed by a certain set of ideas inherited from the preceding development. Only a few succeed in escaping from such a set of ideas, since only a few are capable of remaining internally independent in the evaluation of facts and of the theories proposed to explain them. This ability, apparently, belongs among the number of the most important components of a scientific endowment of the first order, and it was in the greatest measure characteristic of the genius of Maxwell.

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