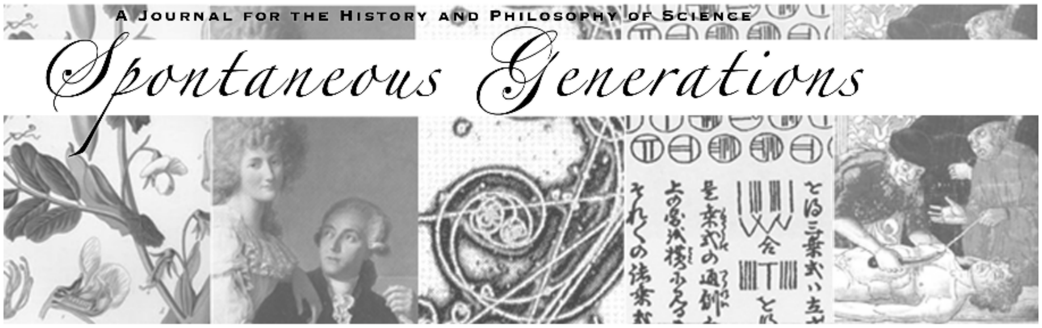

The Genesis and Development of Maxwellian Electrodynamics: Its Intratheoretic Context

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The Genesis and Development of Maxwellian Electrodynamics: Its Intratheoretic Context*

Rinat M. Nugayev

Why did Maxwell's programme supersede the Ampére-Weber one? To give a sober answer one has to delve into the intratheoretic context of Maxwellian electrodynamics genesis and development. What I want to stress is that Maxwellian electrodynamics was created in the course of the old pre-Maxwellian programmes' reconciliation: the electrodynamics of Ampére-Weber, the wave theory of Young-Fresnel and Faraday's programme. The programmes' encounter led to construction of the hybrid theory at first with an irregular set of theoretical schemes. However, step by step, on revealing and gradual eliminating the contradictions between the programmes involved, the hybrid set is "put into order" (Maxwell's term). A hierarchy of theoretical schemes starting from ingenious crossbreeds (the displacement current) and up to usual hybrids is set up. After the displacement current construction the interpenetration of the pre-Maxwellian programmes begins that marks the commencement of theoretical schemes of optics, electricity and magnetism real unification. Maxwell's programme surpassed that of Ampére-Weber because it did absorb the ideas of the Ampére-Weber programme, as well as the presuppositions of the programmes of Young-Fresnel and Faraday properly co-ordinating them with each other. But the opposite statement is not true. The Ampére-Weber programme did not assimilate the propositions of the Maxwellian programme. Maxwell's victory over his rivals became possible because the gist of Maxwell's unification strategy was formed by Kantian epistemology looked in the light of William Whewell and such representative of Scottish Enlightenment as Sir William Hamilton. Maxwell did put forward as basic synthetic principles the ideas that radically differed from that of Ampére-Weber approach by their open, flexible and contra-ontological, strictly epistemological, Kantian character. For Maxwell, ether was not the ultimate building block of physical reality, from which all the charges and fields should be constructed. "Action at a distance," "incompressible fluid," "molecular vortices," etc. were contrived analogies for Maxwell, capable only to direct the researcher at the "right" mathematical relations. Namely putting to use Kantian epistemology enabled Hermann von Helmholtz and his pupil Heinrich Hertz to arrive at such a version of Maxwell's theory that served a heuristic basis for the radio waves discovery.

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I. Introduction

The aim of the present paper is to answer the question "*Why did Maxwell's programme supersede the Ampére-Weber one?*" It appears that to give a convincing answer one has to take a further step in revealing the intratheoretic context of Maxwellian electrodynamics genesis, development and acceptance and to bolster rational reconstruction of the process. The latter should provide a "theoretically progressive problemshift" relative to other "internal" reconstructions and disclose that Maxwellian revolution is a more complex phenomenon than appears from the standpoints of some well-known scientific revolution conceptions (Kuhn 1977; Lakatos 1978).

Previous nineteenth-century physics studies have oscillated between *two extremes*. On the one hand, in the more traditional vein, differences between research traditions were considered to be insignificant and communication unproblematic. On the other hand, in the more recent, post-Kuhnian, studies, differences between traditions are often taken to be so radical that communication is impossible among them.

This account originates from an intermediate picture. According to it, profound differences between the "field" and "action at a distance" research traditions had existed at various levels, ranging from ontological commitments and up to epistemological beliefs. Yet these antagonistic traditions were able to communicate in the creative acts of such men of science as Thomson, Maxwell, Helmholtz and Hertz. They communicated in the ways that permitted comparisons, adaptations and cross-fertilizations of different traditions as well.

The intermediate picture stems from the critique of Kuhnian and Lakatosian conceptions' drawbacks: they lack the mechanisms of the paradigms' (or scientific research programmes') *interactions* (Nugayev 1985 a; b). To meet the critical comments, a "mature theory-change" epistemic model was proposed (Nugayev 1999a). According to the model, the sources of scientific revolutions refer not to clashes of fundamental theories with "facts," but to the clashes of "old" mature research traditions with each other, leading to contradictions that can only be eliminated in more sophisticated ways. The key role in theory change is played by the encounter of the old paradigms and their *dialogue* that leads to interpenetrations and cross-fertilizations of the participants' views.

The very realization of reductionist and synthetic research possibilities is brought about by the clash of mature theories which they are designed to eliminate. Having compared the heuristic potentials of the reductionist and the synthetic programmes, I favour the latter group since it has the following *objective* advantages (Nugayev 1999b). Firstly, synthetic programmes should provide a greater empirically-progressive shift of problems solved than the reductionist ones. Secondly, only these programmes can rationally explain the use of the so-called *crossbred* theoretical objects which spring from the coincident theories. For instance, if one scrutinizes the structures of two modern theories—quantum theory and general relativity—she finds that their global theoretical schemes were constructed in the course of the unification of the crossbred theoretical ones.

Each case of different research programmes' meeting leads to a situation when a domain of *hybrid* models occurs formed by simple conjunctions of the models from different research programmes. However, the hybrid models appear to be self-contradictory; and when this is realized, the crossbreeds from the basic objects of all the cross-theories are constructed. A new mature theory is formed in the course of crossbred domain growth.

All the aforesaid is not intended for diminishing the role of experiments in science. On the contrary, the epistemic model proposed seems to display the point of view stated in the current literature that both theorists and experimentalists have *breaks* in their respective traditions, but they are not usually simultaneous (Pickering 1985; Galison 1987). Theory development must have, to some extent, a life of its own. When two main cultures flourish within science it does not mean that the two do not speak to each other.

The epistemic model was illustrated with reference to physics in the early twentieth century, the three "old" paradigms in this case being Maxwellian electrodynamics, statistical mechanics and thermodynamics (Nugayev 1985b). The world of "old," pre-Einsteinian physics was conceptually and socially fragmented. It had been split on at least three mature research traditions. Traditions organized around different groups of phenomena generated little support for one another. The practitioners of each theoretical tradition acknowledged the existence of the other but went their own separate ways. With the advent of relativity and quantum theory, the conceptual unification of world views commencement was accompanied by a social unification of practice.

Thus, it is one of my basic aims to expose that the above mentioned remarks are especially appropriate for Maxwellian electrodynamics genesis, development, and acceptance. I'll try to demonstrate that the Maxwellian programme had superseded that of Ampére-Weber because it had constantly communicated with it. The Maxwellian programme did absorb some of the propositions of the Ampére-Weber "hard core," as well as some propositions of the Faraday and Young-Fresnel programmes. But the opposite statement is not true. The Ampére-Weber programme did not assimilate the propositions of the Maxwellian programme.

Maxwell's research programme did supersede that of Ampére-Weber because it was a "synthetic" one (in the sense that was in more detail disclosed in Nugayev (1999b). It appeared, according to one of Maxwell's philosophical teachers (who was himself a Kantian), one of "successive steps by which we gradually ascend in our speculative views to a higher and higher point of generality" (Whewell 1847, vol. 2, 74). Contrary to Maxwell's, the Ampére-Weber programme was a reductionist one (see Nugayev 1999b for details) for it tried to reduce all the theoretical ontologies to one and the same ontology of "action at a distance."

In this connection the following passage from Ludwig Boltzmann's 1904 paper is especially convincing: "It is certainly useful to set up Weber's theory as a warning example for all times that we should always preserve the necessary *mental flexibility*" (Buchwald 1994, 261). Boltzmann constantly emphasized the need for a "*plurality of approaches*," including both mathematical formalism and picture-based physical theories.

In particular, Maxwell's programme was not only successful to assimilate the propositions of the Ampére-Weber hard core, combining them with Faraday's "field" notions, as well as with those of Fresnel-Young optics; it was open for fusions with other research traditions as well. As Heinrich Hertz put it,

> From the outset Maxwell's theory excelled all others in elegance and in *the abundance of the relations* between the various phenomena which it included. The probability of this theory and therefore the number of its adherents increased from year to year. (Hertz 1893, 19)

This "abundance of the relations" was owing to that Maxwell did put forward as a synthetic principle the idea, that differed *radically* from that of Ampére-Weber by its flexible and contra-ontological, strictly epistemological, Kantian character.

> By referring everything to the purely *geometrical* idea of the motion of an imaginary fluid, I hope to attain *generality* and precision, and to avoid the dangers arising from a premature theory professing to explain *the cause* of the phenomena. (Maxwell [1856/1890] 1952, 159)

For Maxwell, ether was not the ultimate building block of physical reality, from which all the fields and charges should be carefully constructed. "Action at a distance," "incompressible fluid," and "molecular vortices" were "*contrived analogies*" (Hon and Goldstein 2012) for Maxwell, capable only to direct the researcher at the "right" mathematical relations: "my aim has been to present the *mathematical* ideas to the mind in an embodied form" (Maxwell, [1856/1890] 1952, p. 187). Maxwellian analogies are contrived ones and are not intended to illustrate anything in nature. Maxwell gave a new meaning to analogy that comes close to modelling in current usage:

> the sciences do not try to explain, they hardly even try to interpret, they mainly make models. By a model is meant a mathematical construct which, with the addition of certain verbal interpretations, describes observed phenomena. The justification of such a mathematical construct is solely and precisely that it is expected to work... (John von Neumann 1955; quoted from Hon and Goldstein 2012, 238)

Usually the defining feature of all analogies is supposed to be a *bidirectional* relation between the two domains for which an analogy is established. Neither domain is privileged over the other. Relation should hold both ways: one can move from one domain to its analogue and vice versa. But this feature does not hold in Maxwell's genuine methodology of mathematical analogy—it is *unidirectional*, from a fictional system to a physical system, where the purpose of introducing the fictional system is to gain insight into the physical system and ultimately to recast it into the mathematical formalism (Hon and Goldstein 2012, 239).

The principle of usual ("physical") analogy between theories in two different domains that are identical in nature came from William Thomson. But for Maxwell the methodology of analogy was only a tool. Contrary to Thomson, both mathematically identical systems need not exist in nature. In a pair of such systems one of them could be imaginary ("imaginary fluid"), and the other could be real ("physical").

From the "representational" point of view all these obsolete hydrodynamic models were doomed to failure efforts to describe what cannot be described in principle—things in themselves, the "nature" of electrical and magnetic phenomena. On the contrary, Maxwell directed his programme at finding empirically meaningful mathematical relations between the electrodynamics basic objects, i.e. the creation of self-consistent electromagnetic field equations system.

Hence even Ludwig Boltzmann agreed with Hertz that Maxwell's concepts of charge and current were "irremediably obscure." In his electrodynamics lectures he adopted Hertz's view that electricity was a "thing of thought, serving to picture the integrals of certain equations" (quoted from Buchwald 1994, 258).

According to a widespread opinion, Maxwellian electrodynamics was a stage of the Faraday programme development based on the field concept (see, for instance, Chalmers 2007 and references cited therein). The programme had provided prediction and verification of the radio waves phenomenon and have superseded at last the Ampére-Weber research programme based on the action at a distance concept.

Yet a more thorough account of the nineteenth-century physics that became possible first of all due to the studies of Daniel Siegel (1991), Margaret Morrison (2000) and Olivier Darrigol (2001) enables to challenge this point of view as an obvious oversimplification and to provide a modified version of it—with the help of the following arguments.

(A) At first, James C. Maxwell himself many times, beginning from his first "electric" paper and up to the last ones, had punctuated that the key ideas of the Ampére-Weber electrodynamics were as useful for electrodynamics development as those of the field theories. Futhermore, in accordance with his rather ambiguous intentions, Maxwell eventually had created a hybrid electromagnetic theory combining the elements of Ampére-Weber theory with that of Faraday.

Maxwell, for the first time, quoted Weber in a letter to William Thomson, dated May 15, 1855. On Thomson's advice, Maxwell had read Weber's *Electrodynamische Maasbestimmungen* and his comment was

> I have been examining his mode of connecting electrostatics with electrodynamics, induction etc., I confess I like it not at first…but I suppose the rest of his view are founded on experiments which are trustworthy as well as elaborate. (quoted from: D'Agostino 1984, 150)

Hence it is not surprising that even at the beginning of his electrodynamics studies, in May 1855, a post-graduate student at Cambridge writes a letter to his father, stressing the importance of scrutinizing the theories of "heavy German writers":

> I am working away at electricity again, and have been working *my* way into the views of heavy German writers. It takes a long time to reduce to order *all* the notions one gets from *these* men, but I hope to see my way through the subject, and arrive at something intelligible in the way of a theory. (quoted from Campbell and Garnett 1882, 105)

As Maxwell developed his thoughts in preparation for the draft of "On Faraday's lines of force," he communicated to Thomson:

> I am trying to construct *two* theories, mathematically identical, in one of which the elementary conceptions shall be about fluid particles *attracting at a distance* while in the other nothing (mathematical) is considered but various states of polarization tension and existing at various parts of space. (quoted from Hon and Goldstein 2012, 241)

According to modern historians of science, Maxwell was much impressed and "indeed *a bit intimidated*"—by the elegant unification of electromagnetic phenomena that the electrodynamics of Ampére-Weber offered (Siegel 1991, 10).

Moreover, as Maxwell later admitted referring to M. Lorenz's paper, "on Weber's theory, periodic electric disturbances would be propagated with a velocity equal to that of light." (Maxwell [1868/1890] 1952, 137).

All that Maxwell could initially oppose to the Ampére-Weber advances was that

> it is a good thing to have *two* ways of looking at a subject, and to admit that there are two ways of looking at it. Besides, I do not think that we have any right at present to understand the action of electricity, and I hold that the chief merit of a temporary theory is, that it shall guide experiment, without impeding the progress of the true theory when it appears. (Maxwell [1856 /1890] 1952, 208)

However, not long afterwards, Maxwell offers more profound arguments in favor of new electromagnetic theory creation necessity. He points out that Ampére-Weber electrodynamics is too mathematized and ignores the important connections between the phenomena; in particular it oversimplifies the relations between static and dynamic electricities:

> ...the theory of the conduction of galvanism and that of the mutual attraction of conductors have been reduced to mathematical formulae, but have not fallen into relation with the other parts of the science. (Maxwell [1856 /1890] 1952, 155)

Further, completing his theory creation on the basis of Lagrangian formalism, in his introduction to "A Dynamical Theory of Electromagnetic Field" (read December 8, 1864) Maxwell gives a sketch of action at a distance theory accompanied by a phrase:

> This theory, as developed by MM. W. Weber and C. Neumann, is *exceedingly ingenious*, and *wonderfully comprehensive* in its application to the phenomena of statical electricity, electromagnetic attractions, induction of currents and diamagnetic phenomena; and it comes to us with the more authority, as it has served to guide the speculations of one who has made so great an advance in the practical part of electric science, both by introducing a consistent system of units in electrical measurement, and by actually determining electrical quantities with an accuracy hitherto unknown.

The mechanical difficulties, however, which are involved in the assumption of particles acting at a distance with forces which depend on their velocities are such as to prevent me from considering this theory as an *ultimate* one, though it may have been, and *may yet be useful in leading to the coordination of phenomena*" (Maxwell [1864/1890] 1952, 527).

And, at last, in his opus magnus *A Treatise on Electricity and Magnetism* Maxwell reveals the creation of his system of equations in the following way:

> I was aware that there was supposed to be a difference between Faraday's way of conceiving phenomena and that of the mathematicians, so that *neither he nor they* were satisfied with each other's language. I had also the conviction that this discrepancy did not arise *from either party being wrong.* (Maxwell [1873a/1891] 1954, 499)

The following story is appropriate here. In his last scientific work—in a review of George Fitzgerald's paper (1879)—Maxwell characterized his own treatment of the Faraday effect in 1861-62 paper as a "hybrid" one combining his electromagnetic theory of light with elements of an elastic solid theory. He had treated light waves as actual motions of the ether and had traced how these would disturb the spinning of the magnetic vortices in such a way as to cause the plane of polarization of the light to rotate. Maxwell had found this detour into a "hybrid theory," in which electrical and mechanical actions were combined, the least satisfactory part of his own explanation of the Faraday effect (Hunt 2005, 18).

(B) No-one at Cavendish laboratory—conducted and well-equipped by Maxwell—had made a serious and sustained attempt to confirm Maxwell's theory (Mahon 2003).Though Maxwell conjectured that light is a transverse electromagnetic wave, his hypothesis did not imply that he actually believed that light could be generated electromagnetically. He was silent about electromagnetic waves, and their generation and detection. Moreover, there is even some reason to think that Maxwell regarded the electrical production of the electromagnetic waves as an impossibility (Chalmers 2001; Hunt 2005), and his skepticism was supported by "the Maxwellians" (George Francis Fitzgerald, Sir Oliver Lodge and Oliver Heaviside). It took almost a quarter of a century before Heinrich Hertz, a star student of Hermann Helmholtz, discovered electromagnetic waves. And up to 1888 Hertz did not consider himself a follower of Maxwell (Darrigol 2001).

Just to imagine how unpopular Maxwell's theory was at the time—especially in Germany—one has to take into account that in all his experimental works Hertz tried to avoid quoting Maxwell. Moreover, in his trailblazing 1887 paper "*On Very Rapid Electric Oscillations*" devoted to the inductive influence of displacement currents, Maxwell was not quoted at all.

At the same time in Hertz's well-known paper "On the Electromagnetic Waves in Air and their Reflection" (1888) Maxwell's theory is quoted only at the end after the following eloquent reservation:

> I have described the present set of experiments, as also the first set on the propagation of induction, without paying special regard to any particular theory; and indeed, *the demonstrative power of the experiments is independent of any particular theory*. (Hertz [1888b] 1893, 136).

And in Hertz's introduction to the first collected volume on *Electrical Waves* (1893) it is stated that

> Many a man has thrown himself with zeal into the study of Maxwell's work, and, even he has not stumbled upon unwanted mathematical difficulties, has nevertheless been compelled to abandon the hope of forming for himself an altogether consistent conception of Maxwell's ideas. *I have fared no better myself.* (Hertz 1893, 20)

The last phrase can be elucidated by the fact that Hertz had planned his experiments in 1886-1887 for testing his teacher's theory—that of Hermann Helmholtz—and not of James Maxwell (see, for instance, Buchwald 1998; Darrigol 2001). Helmholtz's theory was very much like Maxwell's in that it was a hybrid one combining field notions with that of action at a distance. On the one hand, Helmholtz enhanced Maxwell's idea that electromagnetic radiation is an ether wave. On the other hand, its propagation was explained by action at a distance concepts (Patton 2009).

(C) The same hallmark—the "hybrid" character of Maxwellian electrodynamics—was punctuated by Henri Poincare, Ludwig Boltzmann, and Heinrich Hertz. The latter indicated:

> But it cannot be denied that other statements made by Maxwell appear at first sight to contradict the conceptions of this standpoint the field concept ... The statement that electricity moves like an incompressible fluid is a favorite statement of Maxwell's. But these statements do not fit in with the conceptions of the fourth standpoint [the field concept]; they lead one to suspect that *Maxwell rather viewed things from the third [hybrid] point of view* [...] And so, unfortunately, the word "electricity," in Maxwell's work, obviously has a *double meaning*. In the first place, he uses it (as we also do) to denote a quantity which can be either positive or negative, and which forms the starting

> point of distance-forces (or what appears to be such). In the second place, it denotes that hypothetical fluid from which no distance-forces (not even apparent ones) can proceed [...] M. Poincare, in his treatise *Electricite et Optique* (vol. i, Les Theories de Maxwell), expresses a similar opinion. Herr. L. Boltzmann, in his *Vorlesungen uber Maxwell's Theorie*, appears like myself to aim a consistent development of Maxwell's system rather than an exact rendering of Maxwell's thoughts. (Hertz 1893, 26)

(D) In the course of the Helmholtz programme advancement, that tried to merge the field notions with that of action at a distance, Helmholtz's pupil Heinrich Hertz had rederived Maxwell's equations from the modified version of action at a distance theory.

In 1884 Hertz had published in "*Wiedemann's Annalen*," 23, pp. 84-103 his paper "On the Relations between Maxwell's Fundamental Electromagnetic Equations and the Fundamental Equations of the Opposing Electromagnetics". In the paper Hertz did obtain Maxwell's equations in an alternative to Maxwell's way. His own method wriggled out of mechanical models and remarks on the "displacement current" (see D'Agostino 1975 for details). Hertz stated that

> Now the system of forces given by the equations (12) and (13) is just that given by Maxwell. Maxwell found it by considering the ether to be a dielectric in which a changing polarization produces the same effect as an electric current. We have reached it by means of other premises, generally accepted even by opponents of the Faraday-Maxwell view. (Hertz [1884] 1896, 288)

And at the end of the paper Hertz's methodological standpoint is articulated as follows:

> In what precedes I have attempted to demonstrate the truth of Maxwell's equations by starting from premises which are generally admitted in the *opposing* system of electromagnetics, and by using propositions which are familiar in it. Consequently I have made use of the conceptions of the latter system; but, excepting in this connection, the deduction given is in no sense to be regarded as a rigid proof that Maxwell's system is the only possible one. It does not seem possible to deduce such a proof from our premises. The exact may be deduced from the inexact as the most fitting from a given point of view, but never as the necessary. (Hertz [1884] 1896, 289)

(E) Hertz's 1887-1888 experiments on radio waves' optical properties cannot be taken as "crucial" experiments providing the definite choice between Ampére-Weber and Maxwell's programmes.

Indeed, it was already indicated that Hertz's experiments were carried out within Helmholtz's research programme. According to Hertz,

> Notwithstanding the greatest admiration for Maxwell's mathematical conceptions, I have not always felt quite certain of having grasped the *physical* significance of his statements. Hence it was not possible for me to be guided in my experiments directly by Maxwell's book. I have rather been guided by Helmholtz's work, *as indeed may plainly be seen from the manner in which the experiments are set forth.* (Hertz 1893, 20)

Maxwell's boon companion Hermann Helmholtz had sought from the middle of 1860-s to reach a reasonable compromise between two major directions in electromagnetic research of the second half of the nineteenth century, namely, Newton's instantaneous action-at-a-distance concept as used by Weber, and Faraday's contact action concept. By the time of Helmholtz's first attempt of reconciliation (1870), the research programmes of Weber and Faraday had successfully incorporated all well-established empirical facts. Hence when trying to arrive at results similar to Maxwell's *without losing the elements of action at a distance*, Helmholtz assumed that the electrostatic forces are constantly present as a field in space and that the change in the polarization or the displacement of the charges signalled the change in the electrostatic field (Helmholtz [1870] 1882). Under these assumptions, Helmholtz in 1870 paper successfully derived generalized equations very similar to those of Maxwell and found that in a limited case they yield equations completely identical to Maxwell's. Yet in addition to the ordinary transverse electromagnetic waves, Helmholtz discovered the existence of longitudinal electric waves which turned to be instantaneous at the Maxwell's limit $k=0$. To check the consequences from his theory in 1879 Helmholtz proposed a prize competition "to establish experimentally a relation between electromagnetic action and the polarization of dielectrics" and persuaded one of his pupils who's name was Heinrich Hertz to take part.

And already in 1886-1888, at Karlsruhe, Hertz attempted to establish the compatibility of the theories of Helmholtz and Maxwell in a new series of experiments. He designed his measurement procedures, taking into account Helmholtz's ingenious separation of the total electric force into the electrostatic and electrodynamic parts to which different velocities of propagation were ascribed. In his own words

> The total force may be split up into the electrostatic part and the electrodynamic part; there is no doubt that at short distances the former, at great distances the latter preponderates and settles the direction of the total force. (Hertz [1888b] 1893, 110)

According to Coulomb's law, the electrostatic component was thought to be proportional to the inverse square of the distance, whereas the electrodynamic part was only proportional to the inverse of the distance. In the usual theory of the Lienard-Wiechert retarded potential it would correspond to decreasing rates of the bound-field, or longitudinal, component and the radiation field, or transverse component, respectively.

Hertz had planned a series of experiments and his efforts appeared to be fruitful. Yet it should be noted that the title of his 1888a paper "On the Finite Velocity of Propagation of Electromagnetic Action" is perhaps misleading nowadays, because usual Maxwellian electrodynamics does not employ the Helmholtzian "action" terminology, nor does it split the total electric force into electrodynamic and electrostatic parts. But for Hertz's contemporaries who supported the Helmholtz theory, the underlying meaning of the presented results was clear enough: Hertz's experiments could qualitatively conclude about the finite propagation of the electromagnetic part, but could say nothing definite about the electrostatic component. Hence at the end of the paper one finds Hertz declaiming:

> From this it follows that the absolute value of the first of these is of the same order as the velocity of light. Nothing can as yet be decided as to the propagation of electrostatic action.

According to one of modern action at a distance devotees (Smirnov-Rueda 2001), some of Hertz's measurements tended towards the instantaneous nature of the electrostatic modes. Yet he was still not convinced of this instantaneity and preferred to be cautious:

> Since the inferences undoubtedly change sign after 2.8 meters in the neighbourhood of the primary oscillation, we might conclude that the electrostatic force which here predominates is propagated with infinite velocity. (Hertz [1888b], 1983, 110).

(F) Faraday's influence on Maxwell was strongly exaggerated.

The explanation of the field concept acceptance due to the devotion to intermediate action is not confirmed by Maxwell's papers' more thorough analysis (Shapiro 1973). It reveals that Maxwell began to take the field notion as a basic means of unifying optics and electromagnetism sufficiently late: only *after* he had derived the electromagnetic waves existence from his equations, i.e. *after* the derivation of the "displacement current." Up to that point he did apply the field notion only as an illustrative means for building up the pictorial images of complicated vector differential equations.

Faraday's apparatus of "lines of force" and "strains in the field" seemed both vague and cumbersome to most of his contemporaries, especially when compared with the precise and elegant action-at-a-distance theories (Hunt 2005).

However, all above-stated does not mean that I do agree with *all* the conclusions of Margaret Morrison, Daniel Siegel and Olivier Darrigol. It seems to me that the main drawback of their studies is an *underestimation of Maxwell's own methodology* elaborated by himself for his ambitious project of mechanics, electrodynamics and optics unification. In every field of creativity (including metaphysics and epistemology) Maxwell always took his own way; and he tried to admonish his students in the same way too. It is clear from the following passage of his Marichal college speech:

> It is best that every man should be settled in *his own* mind, and not be led into other men's ways of thinking under the pretense of studying science. (quoted from Mahon 2003, 70)

As the author of *Treatise on Electricity and Magnetism* himself acknowledged in one of his letters, "I find I get fonder of metaphysics and less of calculations continually" (quoted from Campbell and Garnett 1882, 298). One can remember Gustav Kirchhoff's acrimonious comment: "He is a genius, but one has to check his calculations."

Maxwell's demeanour corresponded to William Whewell's dictum:

> Physical discoverers have differed from barren speculators, not by having no metaphysics in their heads, but by having good metaphysics while their adversaries had bad; and by binding their metaphysics to their physics, instead of keeping the two asunder. (Whewell, 1847, vol.1, X)

It seems to me that one should take Ludwig Boltzmann's comments on Maxwell's works more seriously. In his lectures on Maxwell's theory as well as in his comments on Maxwell's electromagnetic papers (that he had translated into German), the founder of statistical mechanics had pointed out that too many Maxwell's works but first and foremost his early electrical papers "were *not properly understood*". Perhaps this can be explained by the following circumstance: the works "written according to the *long-term plan*" demonstrate that their author "was as mastermind in theory of knowledge as he was in the field of theoretical physics" (Boltzmann 1895). Maxwell was a great scientist as well as a great innovator of methodology (Hesse 1973; Achinstein 2010). Maxwell's methodology that sprung out from an intention to follow his Edinburgh philosophical guru in looking for fruitful compromise between the extremes of Kantian relativism and Scottish "common sense realism" was a necessary part of his grand unification of optics and electromagnetism design.

II. Maxwell's genuine methodology of synthetic mature theory construction.

Maxwell was not the first to unify optics and electromagnetism. There were four basic domains in electricity and magnetism that would have to be treated by any general theory:

1. electrostatics—forces between electric charges;
2. magnetostatics—forces among magnetic poles and magnetic materials;
3. electromagnetism—magnetic effects of electric currents;
4. electromagnetic induction—electrical effects of changing magnetic fields.

The basic task—as perceived by the leading researchers of the time such as Oersted, Faraday, Thomson et al.—remained the treatment in one integrated theory of these four domains within electricity and magnetism.

For instance, working within integrated theory, Wilhelm Weber (1804-1890) proposed in 1846 a sophisticated theory treating all four categories of the electromagnetic phenomena.

It should be added, that already in 1847 Hermann Helmholtz had convincingly demonstrated, that electromagnetic induction phenomenon follows with necessity from the Ampére law, if one takes into account the conservation of energy. "Maxwell was much impressed—and indeed a bit intimidated—by the elegant unification of electromagnetic phenomena that it offered" (Siegel 1991, 10).

Yet he did not like the way his predecessors had unified electricity and magnetism. Why?

The following passage helps to find the answer: the theories of action at a distance were too formal and abstract to grasp the connections between the electromagnetic phenomena.

> The present state of electrical science seems peculiarly unfavourable to speculation... No electrical theory can now be put forth, unless it shows the *connexion* not only between electricity at rest and current electricity, but between the attractions and inductive effects of electricity in both states [...] The results of this simplification may take the form of a purely mathematical formula or of a physical hypothesis. In the first case we entirely lose sight of the phenomena to be explained; and though we may trace out the consequences of given laws, we can never obtain more extended views of the *connexions* of the subject. (Maxwell [1856/1890] 1952, 155)

His predecessors were Hans Christian Oersted (1777-1851), Andre-Marie Ampere (1775-1836), Wilhelm Weber (1804-1890), Michael Faraday (1791-1867) and William Thomson (1824-1907). Yet Lord Maxwell's Weltanschauung was characterized by an extraordinary high level of philosophical culture. A prodigy at Edinburgh and a brilliant post-graduate at Cambridge, he was enchanted by a profound skepticism of David Hume, George Berkley and Immanuel Kant at the lectures of Sir William Hamilton on mental philosophy at Edinburgh University.

For instance, in the March 25, 1854 letter Maxwell admits that

> I have been reading Berkeley on "The Theory of Vision," and greatly admire it, *as I do all his other non-mathematical works*; but I was disappointed to find that he had at last fallen into the snare of his own paradoxes…" (quoted from Campbell and Garnett 1882, 109). Analogously, "Comte has *good ideas about method*, but no notion of what is meant by a person" (Campbell and Garnett 1882, 108)

Hamilton's lectures, which were a prominent element in the Scottish university curriculum, "*interested him greatly*." From the Class of Metaphysics his mind "gained many lasting impressions"; Sir Hamilton's personal charisma and enormous erudition made a strong impression on him, in "stimulating the love of speculation to which his mind was prone" (Campbell & Garnett 1882) .

Sir William Hamilton (1788-1856) was one of the outstanding representatives of Scottish "common sense philosophy," an heir of Thomas Reid and James Stewart. "He alone, of our metaphysicians of this and the preceding generation, has acquired, merely as such, an European celebrity" (Mill [1889] 2008, 4). Sir William's two visits to Germany in 1817 and 1820 led to his taking up the study of contemporary German philosophy and, first of all, the philosophy of Immanuel Kant. Sir William Hamilton intended to braden the horizon of Scottish philosophy and to push it beyond the confines of Common Sense. He stressed *Kant's proposition that all knowledge is relative*; so we know nothing about things themselves except by their *relationship* to other things. He had stimulated a spirit of criticism in his students by insisting on the great importance of *psychology* as opposed to the older metaphysical method, as well as the importance of German philosophy.

Hamilton's most important work was "Philosophy of the Unconditioned," the development of the principle that for the human mind there can be no knowledge of the Infinite. Intelligence supposed principles, which as conditions of its activity , cannot be the results of its operation" (Hamilton 1853, 3). Hamilton's "philosophy of the conditioned" surely had a strong Kantian flavour. Like Kant, he held that we can have knowledge only of "the relative manifestations of an existence, which in itself is our highest wisdom to recognize as beyond the reach of philosophy" (quoted from Audi 1999, 360). But unlike Kant, however, he had argued for the position of a "natural realism" in the Reidian tradition and his philosophy can be described as a "strange compound" (Graham 2011) of Kant and Reid.

The Reverend Thomas Reid (1710-1796) directed his *An Inquiry into the Human Mind on the Principles of Common Sense* (1764) against "the heresies of Hume and Berkeley". It is here—he argued—that the "danger of the idealism" lies—in its reduction of reality to "particular perceptions," essentially unconnected with each other. Yet the unit of knowledge is not an isolated impression but a judgement; and in such a judgement is contained the reference both to a permanent subject and to a permanent world of thought, and, implied in these, such judgements, for example, as those of existence, substance, cause and effect. Such principles are not derived from sensations, but are "suggested" on occasion of sensation, in such a way as to constitute the necessary conditions of having perceptive experience at all (Reid [1764] 1997).

The doctrine of relativity of knowledge has seemed to many—including James Stewart Mill—contradictory to his realism (Mill [1889] 2008). But for Hamilton, the two are held together by a kind of intuitionism that emphasizes certain facts of consciousness that are both primitive and incomprehensible. They are though constitutive of knowledge, "less forms of cognitions than of beliefs" (quoted from Audi 1999, 360).

The relativism or phenomenalism which Hamilton adopted from Kant and sought to engraft upon Scottish philosophy is absent from the original Scottish doctrine. Thus, denying Hume's skepticism, Hamilton did his best to *find a compromise* between Kant's relativism and Reid's realism; and it was namely that that Maxwell have pointed out as a basic tenet of his metaphysical programme on moving from Edinburgh to Cambridge:

> in the meantime I have my usual superfluity of plans... Metaphysics—Kant's Kritik of Pure reason in German, read with a determination to make it agree with Sir W. Hamilton...." (Campbell and Garnett 1882, 77)

The "Copernican revolution" in epistemology initiated by Kant consisted in that the world of usual every-day experience (or Husserl's "lebenswelt") had lost its dominating position in interpreting things that can be perceived by our senses. Kant had exchanged the world of common experience by the world of Galilean experimental and mathematical physics based on the idealizations of the "lebenswelt" phenomena. Hence truth became something not spontaneously revealing and disclosing itself but something that can be comprehended only by a special ("scientific") method.

On the other hand, if truth is comprehended only in experience and we can grasp not "the things by themselves" but just the "phenomena," it is necessary to reject the opportunity of reaching the absolute truth. Our sensory representation is by no means a representation of things "in themselves," but only of the way in which they appear to us. Hence the "analogies of experience" are especially important in Kant's epistemology.

> This type of cognition is cognition *according to analogy*, which surely does not signify, as the word is usually taken, an imperfect similarity between two things, but rather a perfect similarity between two relations in wholly dissimilar things. Such is an analogy between the legal relation of human actions and the mechanical relation of moving forces: I can never do anything to another man without giving him a right to do the same to me under the same condition; just as a body cannot act on another body with its motive force without thereby causing the other body to react just as much on it. Right and motive force are here completely dissimilar things, but in their relation is there nonetheless complete similarity. By means of such an analogy I can therefore provide a concept of a relation to things that are absolutely unknown to me. (Kant [1783] 2002, 146-47)

In more detail, in "The Critique of Pure Reason," Kant considers a more interesting example:

> Prior to the perception, however, and therefore completely a priori, we are able to cognize its existence, provided it stands in connection with some perceptions according to the principles of the empirical conjunction of these, that is, in conformity with the analogies of perception. For, in this case, the existence of the supposed things is connected with our perception in a possible experience, and we are able, with the guidance of these analogies, to reason in the series of possible perceptions from a thing which we do really perceive to the thing we do not perceive. Thus, *we cognize the existence of a magnetic matter penetrating all bodies from the perception of the attraction of the steel-fillings by the magnet,* although the constitution of our organs renders an immediate perception of this matter impossible for us. (Kant [1787] 2010, 170)

It should be noted that *even the example of the analogy of experience was borrowed by Kant from the domain of electromagnetism* thus paving the way to Maxwell. The latter had pointed out many times that things we can measure directly, like mechanical force, are merely the outward manifestations of deeper processes, involving entities like electric field strength, which are *beyond* our power of visualization (see Mahon 2003).

A more detailed account of Maxwell's research programme that he had followed through all his life is given in his truly philosophical works—in a speech "Are There Real Analogies in Nature?" read at the "Apostles" Cambridge elitist club in 1856 (not long afterwards the publication of his most profound paper "On Faraday's Lines of Force, (1855-1856)—and in his trailblazing paper "Helmholtz" (1877).

The Cambridge speech is not a crude exposition of Kant's epistemology but a tense Maxwell's discourse with "Kant in himself." It is not accidental that the very heading of the speech appears to be a question and not an assertion: "Are There Real Analogies in Nature?"—Maxwell gives no definite and unambiguous answer—in full accordance with Kant's antinomies that occur to Human Reason as attempts to overstep the Limits of Experience. He multiplies arguments pro and contra the proposition that certainly there *are* real analogies in Nature.

For instance, he begins his speech contending that

> the whole framework of science, up to the very pinnacle of philosophy, seems *sometimes* a dissected model of nature, and *sometimes* a natural growth of the *inner surface of the mind.* (Campbell and Garnett 1882, 133)

Or in the other part of the speech he questions:

> are we to conclude that these various departments of nature in which analogous laws exist have a real inter-dependence; or that their relation is only apparent and owing to the necessary conditions of human thought? (Campbell and Garnett 1882, 134)

Further, in the matters of space and time, antinomies occur again. On the one hand, "As for space and time, any man will tell you that it is now known and ascertained that 'they are merely modifications of our own minds (Campbell and Garnett 1882, 121)." And on the other hand,

> if we conceive of the mind as absolutely indivisible and capable of only one state at a time, we must admit that these states may be arranged in chronological order, and that this is the only real order of these states. (Campbell and Garnett 1882, 121)

Besides that, turning to another pair of antinomies:

> Perhaps the 'book,' as it has been called, of nature is regularly paged; if so, no doubt the introductory parts will explain those

> that follow, and the methods taught in the first chapters will be taken for granted and used as illustrations in the more advanced parts of the course; but if it is not a 'book' at all, but a magazine, nothing is more foolish to suppose that one part can throw light on another. (Campbell and Garnett 1882, 124)

Certainly Maxwell's reflections in terms of Kantian antinomies is not accidental. Following Hamilton's traditions, Maxwell tries to find *his own way* between the Scylla of Kantian transcendentalism and the Charybdis of Scottish common sense realism.

In modern literature the Scottish view of knowledge is characterized by the following principles (Mertz 1964; Olson 1975).

1. All knowledge is relational.
2. Analogies are among the basic such relational ways of knowing.
3. Analogies are necessary for psychological reasons. For most people, understanding requires the use of analogies for simplifying and organizing knowledge.
4. Strong psychological tendencies in the Scottish Common Sense tradition encourage reconciliation with logical and analytical trends of Kant's philosophy.

Thus for Maxwell the philosophical resolution of the antinomies comes from adopting *partial points of view*, as all human knowledge is partial. No absolute truth is attainable. What remains is establishing correspondences or analogies.

> Whenever they see a relation between two things they know well, and think they see there must be a similar relation between things less known, they reason from the one to the other. This supposes that although pairs of things may differ widely from each other, the relation in the one pair may be the same as that in the other. Now, as in a scientific point of view the *relation* is the most important thing to know, a knowledge of the one thing leads us a long way toward a knowledge of the other. If all that we know is relation, and if all the relations of one pair of things correspond to those of another pair, it will be difficult to distinguish the one pair from the other, although not presenting a single point of resemblance, unless we have some difference of relation to something else whereby to distinguish them. Such mistakes can hardly occur except in mathematical and physical analogies. (Maxwell; quoted from Campbell and Garnett 1882, 124)

So, that is the first lesson taught by Kantian epistemology—(I) "*the principle of relational character of scientific truth*" stating that the *relation* is the most important thing to know. It should be noted that even the examples of the analogies are directly borrowed by Maxwell from Kant's "Prolegomena." Hence it is not surprising that the second principle—(II) "*theory ladenness of observation*" – is also extracted from Kant/Whewell:

> The dimmed outlines of phenomenal things all merge into one another unless we put on the focusing glass of theory, and screw it up sometimes to one pitch of definition and sometimes to another, so as to see down into different depths through the great millstone of the world. (Maxwell; quoted from Campbell and Garnett 1882, 125)

The importance of the principle (II) for Maxwell's methodology cannot be overestimated. In nature all the phenomena are interconnected and merge into one another; all the differences in theoretical approaches are due to the fact that their authors focus on the different facets and different levels of the phenomena investigated. Hence a theoretician's task is to provide the "*appropriate ideas*" (Whewell's term) to cover the various domains of experience. But where should he find them? In experience, springing out in the course of immediate generalizations of the experimental data? Another passage from Maxwell—a part of his 1854 letter—makes it possible to take a more intent gaze at his thought laboratory:

> It is hard work *grinding out* 'appropriate ideas.' as Whewell calls them. I think they are coming out at last, and by dint of *knocking them against* all the facts and half-digested theories afloat, I hope to bring them to shape, after which I hope to understand something more about inductive philosophy that I do at present.
>
> I have a project of *sifting* the theory of light and making everything stand upon definite experiments and *definite assumptions*, so that things may not be supposed to be assumptions when they are either definitions or experiments. (Maxwell; quoted from Campbell and Garnett 1882, 112)

Now it is clear where the "appropriate ideas" come from: they are not the slavish copies of things, but are the *a priori* forms by which a chaos of sensations is "brought to order." According to Maxwell's essay "Has everything beautiful in Art its original in Nature?" (Spring 1854),

> as the Theoretic and Imaginative faculty is far in advance of Reason, he [Man] can apprehend and artistically reproduce natural beauty of a higher order than his science can attain to. (Campbell and Garnett 1882, 133)

At first the "appropriate ideas" are vague and dim; however in the long run they are "grinded out" by knocking them with the "facts" and with the other theories. However the theoretician's task is not only to introduce and polish subtle notions "reflecting" the different facets of the phenomena under consideration, but also to unify the notions in synthesis.

The structure and the stages of such a fusion are scrutinized in Maxwell's seminal paper "Hermann Ludwig Ferdinand Helmholtz" that begins as follows:

> Hence the ordinary growth of human knowledge is by accumulation round a number of *distinct* centers. The time, however, must sooner or later arrive when two or more departments of knowledge can no longer remain independent of each other, but *must* be fused into a consistent whole. But though men of science may be profoundly convinced of the necessity of such a fusion, the operation itself is a most arduous one. For though *the phenomena of nature are all consistent with each other*, we have to deal not only with these, but with the hypotheses which have been invented to systematize them; and it by no means follows that because one set of observers have labored with all sincerity to reduce to order one group of phenomena, the hypotheses which they have formed will be consistent with those by which a second set of observers have explained a different set of phenomena. Each science may appear tolerably consistent within itself, but before they can be combined into one, each must be stripped of the daubing of untempered mortar by which its parts have been prematurely made to cohere.
>
> Hence the operation of fusing two sciences into one generally involves much criticism of established methods, and the explosion of many pieces of fancied knowledge which may have been long held in scientific reputation. (Maxwell [1877/1890] 1952, 592)

The passage is not accidental for Maxwell. In other works Maxwell himself emphasized the value of the next principle (III)—"*cross-fertilization* of the sciences" (Maxwell 1890, vol.2, 744) evoking the image of bees pollinating flowers (see Harman 2001 for further details).

The typical example of "the daubing of untempered mortar elimination" principle (IV) for Maxwell was

> the progress of science in Newton's time [which] consisted in getting rid of the celestial machinery with which generations of astronomers had encumbered the heavens, and thus 'sweeping cobwebs off the sky.' (Maxwell [1873b/1890] 1952, 315).

III. Initial Stages of Maxwellian Programme Realization

A Treatise on Electricity and Magnetism (Maxwell 1873) was mainly an encyclopedia and a textbook. The most important electromagnetic results were obtained in a sequence of three papers: “On Faraday’s Lines of Force” (Maxwell 1856), “On Physical Lines of Force” (Maxwell 1861-1862), and “A Dynamical Theory of Electromagnetic Field” (Maxwell 1864).

The first paper (Maxwell 1856) is dedicated to elaboration of the “*analogies*” method rooted in Kantian epistemology. The method rejects the “ontological” approaches seeking the “essences” of electrical and magnetic phenomena and proclaiming that “in reality” electricity and magnetism are “fields” and not “action at a distance” entities, or vice versa. Maxwell’s main proposal is to consider Faraday’s lines of force as a kind of tubes filled with ideal incompressible fluid.

> I propose ... lastly to show how by an extension of these methods, and the introduction of another idea due to Faraday, the laws of the attractions and inductive actions of magnets and currents may be clearly conceived, without making assumptions as to the physical nature of electricity, or adding anything to that which has been already proved by experiment.
>
> By referring everything to the purely geometrical idea of the motion of an imaginary fluid, I hope to attain generality and precision, and to avoid the dangers arising from a premature theory professing to explain the cause of the phenomena. (Maxwell [1856/1890] 1952, 159)

It is crucial for a Kantian that this incompressible fluid has nothing to do with experimental reality. The constraints on the theory proposed consist in the demand that the mathematical constructs should not contradict each other. In all the other matters the physical analogies method admits an unlimited freedom of imagination. Even the conservation laws can be broken down!

> There is nothing self-contradictory in the conception of these sources where the fluid is created, and sinks where it is annihilated. The properties of the fluid are at our disposal, we have made it incompressible, and now we suppose it produced from nothing at certain points and reduced to nothing at others. (Maxwell [1856/1890] 1952, 162)

Maxwell stresses the generality of the lines of force approach, for it can account for any kind of force. For instance, it does not exclude the force of action at a distance which varies inversely as the square of the distance, as force of gravity or as observed electric and magnetic phenomena. "This is a significant remark which is probably intended to undermine possible objections that, in principle, the method excludes the dominant theory based on action at a distance" (Hon and Goldstein 2013, 243).

And in the other parts of the paper Maxwell shows the ways by which the idea of incompressible fluid motion can be applied to the sciences of statical electricity, permanent magnetism, magnetism of induction, and uniform galvanic currents. The core element of his innovations consisted in the construction of a "*neutral language game*" for description and comparison of the consequences from the rival theories. Maxwell's "neutral language" was not Carnap's and Reichenbach's "observation language" springing out from the "protokolsatze" generalizations. Maxwell is aware of the theory-ladenness of the observation data "experimental laws already established, which have generally been expressed in the language of other hypotheses" (Maxwell [1861-1862/1890] 1952, 162). He clearly understands that every observation inevitably carries the footprints of the theoretical language used to describe it. ("The daubing of untempered mortar," as he will christen them later in his "Helmholtz" paper).

In order to compare and to unite in a self-consistent theoretical scheme all the results of the different experiments carrying the footprints of different theoretical languages, it is necessary to construct an *artificial* theoretical language equally distant from the languages of theories under comparison. This language appeared to be the solid state mechanics (with hydrodynamics as its part). Maxwell's ultimate aim was to rewrite all the known empirical and theoretical laws of electricity and magnetism using the neutral language and then to compare them in order to create a system without contradictions.

The final result of the 1856 paper was a system of equations lacking the "displacement current." It was not accidental that one of the main drawbacks of the incompressible fluid theory consisted in that the latter, apart from some uncomplicated cases, was unable to explain interrelations and interactions of electrical and magnetic fields and electric currents, as well as Faraday's (1845) famous interconnection between optical and electromagnetic phenomena.

The Maxwellian programme's ultimate goal was to disclose the connection "between electricity at rest and current electricity" absent in the Ampére-Weber electrodynamics. Was it reached in 1856? Certainly not. The connection between the current density $\mathbf{j}$ and the charge density $\boldsymbol{\rho}$ was lacking in Maxwell's initial 1856 scheme. It was to appear later, after the "displacement current" introduction and finding out its consequence—the continuity equation $\operatorname{div} \mathbf{j} + \frac{\partial \boldsymbol{\rho}}{\partial t} = 0$.

So, in 1861 the publication of Maxwell's second paper (1861-1862) consisting of four parts starts. Its aim was to rederive the results of Weber and Neumann theories on the basis of a new mechanical hypothesis containing the vortices of incompressible fluid.

> My object in this paper is to clear the way for speculation in this direction, by investigating the mechanical results of certain states of tension and motion in a medium, and comparing these with the observed phenomena of magnetism and electricity. (Maxwell [1861-62/1890] 1952, 162)

Again and again he has to caution that

> the author of this method of representation does not attempt to explain the *origin* of the observed forces by the effects due to these strains in the elastic solid, but makes use of the mathematical analogies of the two problems to assist the imagination in the study of both. (Maxwell [1861-62/1890] 1952, 163)

The theory started from W. Thomson's investigations; he showed that the connection between electricity and magnetism has the same mathematical form as that between certain parts of phenomena, of which one has a *linear* and the other a *rotatory* character. It should be noted that W. Thomson introduced the vortices theory in incompressible fluid while studying Faraday's experiments on the rotation of the plane of polarized light when transmitted along the lines of magnetic force. So, they were the efforts to comprehend and theoretically reconstruct the Faraday effect that provided the real meeting of optics and theory of magnetism.

In the second Maxwellian theory the magnetic field was represented now by a set of vortices in incompressible fluid with the axes of rotation coinciding with the direction of magnetic field at a point. Yet in a second language game a role of neutral language is played not by tube hydrodynamics but by a theory of stresses in the medium where the necessary relations among the forces are described by mathematicians with the help of entities that now are called *tensors*; the most general type of a tensor describing the most general type of stress consists of a combination of three principal pressures or tensions, in direction at right angles to each other. The tensor apparatus of solid state mechanics provided the creation of new neutral language game; it enabled to calculate the force upon an element of the medium: F = F1 + F2 + F3 + F4 + F5. The first term F1 refers to the force acting on magnetic poles; the second term F2 refers to the action on bodies capable of magnetism by induction; the third F3 and fourth F4 terms refer to the force acting on electric currents; the fifth term F5 refers to the effect of simple pressure that lacks an electromagnetic analogy.

But one of the most intricate problems of the vortices theory that puzzled even Daniel Bernoulli (who invented it in eighteenth century) was: how can the rotation be transferred from one vortex to another so that "vortices in a medium exist side by side, revolving in the same direction about parallel axis" (Whittaker 1910)? The only conception that aided Maxwell in conceiving this kind of motion was that of the vortices being separated by a layer of particles called the "idle wheels." Is it possible to connect these particles with electricity?

And in the second part of his 1861 paper "The Theory of Molecular Vortices applied to Electric Currents" Maxwell comes up to the hardest problem of his research programme : what is "the physical connexion of these vortices with electric currents, while we are still in doubt as to the nature of electricity?" It is at this point where Maxwell has to admit the principal limits of pure mechanical theories and to borrow the elements of action at a distance theory! Or, using our methodological language (Nugayev 1999), we can conclude that Maxwell had to construct the "crossbred theoretical objects" from the languages of both cross-theories that combine the properties of quite different theoretical schemes.

According to Maxwell's theory, an electric current is represented by the transference of the moveable particles interposed between the neighbouring vortices. As a result,

> these particles, in our theory, *play the part of electricity.* Their motion of translation constitute an electric current, their rotation serves to transmit the motion of the vortices from one part of the field to another, and the tangential pressures thus called into play constitute electromotive force. The conception of a particle having its motion connected with that of a vortex by perfect rolling contact may appear somewhat awkward. I do not bring it forward as a mode of connexion existing in nature. (Maxwell [1861-62/1890] 1952, 345)

On introducing such abstract objects as "electrical particles" and "electric current representing the motion of such particles" Maxwell had deviated significantly from the Faraday programme's "hard core." According to Michael Faraday, the electrical charges should be considered as created by the ends of lines of force; they lack an independent substantial existence. Correspondingly, in his *genuine* research programme the electric current has to be considered not as the motion of real particles but as an "energy axis." This is the nub of the British field programme: the fields are primary, and the particles are only secondary.

But Maxwell's eclecticism was followed later on by H.A. Lorentz's dualism. Hendrik Lorentz initiated it in a seminal 1875 paper: "I shall start with instantaneous action at a distance: thus we will be able to found the theory on the most direct interpretation of observed facts" (quoted from

Darrigol 2001, 323). So it was not a temporary retreat. Even after 1861 Maxwell introduced the notions of the Ampére-Weber atomism into his theories many times (Darrigol 2001).

Yet the results obtained were of course insufficient; the theoretical derivation of Coulomb's law was lacking. Namely that was done in the third part of 1861-1862 paper "The Theory of Molecular Vortices applied to Statical Electricity." It should be noted that the vortices theory contained too many ad hoc assumptions; only some of them were to be eliminated with a help of "*Maxwell's miracle*." It appeared that if one transposes in the course of Fresnel optics and electromagnetism theory meeting the ether properties from optics to electromagnetism, he can eliminate at least one (dangerous) ad hoc supposition. Indeed,

> it is necessary to suppose, in order to account for the transmission of rotation from the exterior to the interior parts of each cell, that the substance in the cells possesses *elasticity of figure*, similar in kind, though different in degree, to that of observed in solid bodies. The undulatory theory of light requires us to admit this kind of elasticity in the luminiferous medium, in order to account for transverse vibrations. We need not then be surprised if the magneto-electric medium possesses the same properties. (Maxwell [1861-62/1890] 1952, 13)

This peculiarity has a vital significance for Maxwell's neutral language:

> If we can now explain the condition of a body with respect to the surrounding medium when it is said to be 'charged' with electricity, and account for the forces acting between electrified bodies, we shall have established a connexion between all the principal phenomena of electrical science. (Maxwell [1861-62/1890] 1952, 13)

Thus, the extrapolation of the molecular vortices theory on the electrostatic domain became possible due to the elasticity of the vortices that enabled the medium to maintain the elasticity waves. As a result,

> the velocity of transverse undulations in our hypothetical medium, calculated from the electromagnetic experiments of M.M. Kohlrausch and W. Weber, agrees so exactly with the velocity of light calculated from the optical experiments of M. Fizeau, that we can scarcely avoid the inference that light consists of the same medium which is the cause of electric and magnetic phenomena. (Maxwell [1861-62/1890] 1952, 22)

The displacement current introduction was due to Maxwell's efforts to link the equations relating to electrical current with that of electrostatics. It demanded the Ampere law modification for the sake of a new term introduction; the term had to describe the elasticity of the vortices medium. The displacement current introduction driving force came from Maxwell's efforts to unify all the main empirical laws belonging not only to electricity and magnetism but to optics as well.

As a result Maxwell obtained his famous system of equations along with the continuity equation describing that electrical particles that transform the rotations from one vortex to another do not appear from nothing and cannot disappear to nowhere. Nevertheless, one could not declare on any ultimate unification of optics and electromagnetism in 1861. It was possible to tell only on the commencement of their reconciliation, on the beginning of "grinding" of rather different theoretical ontologies.

And at last in 1864 Maxwell proposed a modified version of his 1861-1862 paper that avoided any special suppositions on the nature of molecular vortices. Now in his 1864 paper Maxwell starts to derive his equations from abstract dynamics of Lagrange. The Lagrangian function L is found as the difference between the kinetic and potential energies of a system. From those he was able to derive the basic wave equation of electromagnetism without any special assumptions about molecular vortices or forces between electrical particles. Although displacement retained a prominent position in "A Dynamical Theory of Electromagnetic Field," its role was rather different from the role it played in 1861-1862 paper. It was no longer associated with changes in positions of rolling particles; rather, Maxwell defined it simply as the motion of electricity, that is, in terms of a quantity of charge crossing a designated area.

However, despite Maxwell's claim to provide deductions from (three) experimental facts, his account still required the postulation of a displacement current, something that could neither be verified by nor deduced from experiment (Morrison 2000; Darrigol 2001).

As a result he sums up the main merits of 1864 paper in the letter to C. Hockin, September 7th, 1864:

> I have also cleared the electromagnetic theory of light from all unwarrantable assumptions, so that we may determine the velocity of light by measuring the attraction between bodies kept at a given difference of potential, the value of which is known in electromagnetic measure. (quoted from Campbell and Garnett 1882, 168)

And at last Maxwell's creativity ends with *A Treatise on Electricity and Magnetism*, conceived as an encyclopedia of the electrical and magnetic effects. In his *Treatise* Maxwell proceeds further in purifying his deductions from the model remnants and in strengthening the utility of Lagrangian approach. In the final chapter XX, dedicated to the electromagnetic theory of light, the basic argument in defense of electromagnetic waves is posited out:

> To fill all space with a new medium whenever any new phenomena is to be examined is by no means philosophical, but if the study of two *different* branches of science has independently suggested the idea of a medium, and if the properties which must be attributed to the medium in order to account for electromagnetic phenomena are of the same kind as those we attribute to the luminiferous medium in order to account for the phenomena of light, the evidence for the physical existence of the medium will be considerably strengthened.
>
> But the properties of bodies are capable of quantitative measurement. We therefore obtain the numerical value of some property of the medium, such as the velocity with which a disturbance is propagated through it, which can be calculated from electromagnetic experiments, and also observed directly in the case of light. If it should be found that the velocity of propagation of electromagnetic disturbances is the same as the velocity of light, and this is not only in air, but in other transparent media, we shall have strong reasons for believing that light is an electromagnetic phenomenon... (Maxwell [1873/1891] 1954, 781)

Yet it is important that in his *Treatise* Maxwell faced with the same problem as in 1864 paper: the problem of Lagrangian mathematical formalism application to the case of electromagnetic field. Maxwell himself used a fitting comparison with a belfry. He aimed to develop a Lagrangian formulation of electromagnetism in which the ether mechanism would be the analogue of the mechanism in the belfry, whilst the positions and velocities of the ropes would have their analogues in measurable charge and current distributions serving to determine the electromagnetic energy.

However on twenty pages of his *Treatise* chapter Maxwell gave a detailed Lagrangian treatment for interacting closed conduction currents only. And when, two chapters later, he came to build on his Lagrangian formulation to formulate the *general* equations of his electromagnetic theory, he simply added the displacement to the conduction current "by hands" to give the total current (see Chalmers 2001 for details). The step was justified as follows:

> We have very little experimental evidence relating to the direct electromagnetic action of currents due to the variation of

> electric displacement in dielectrics, but the extreme difficulty of reconciling the laws of electromagnetism with the existence of electric currents which are not closed is one reason among many why we must *admit* the existence of transient currents due to variations of displacement. Their importance will be seen when we come to the electromagnetic theory of light. (Maxwell [1873/1891] 1954, 252)

But this trick in fact undermined the major attraction of his Lagrangian method (see Chalmers 2001, for details). The first direct experimental evidence for the existence of displacement currents emerged only with Hertz's experiments culminating in production of radio waves in 1888. As usual, the Lagrangian formulations were retroactive attempts to accommodate the results obtained by other means.

But let us return to Maxwell's synthetic programme. Eventually Maxwell found that his elastic vortex medium would propagate waves whose velocity, calculated from electromagnetic constants, was that of light. Yet he said nothing about how electromagnetic waves might be generated, nor did he attempt to derive the laws governing reflection and refraction (Sengupta and Sarkar 2003). Hence the task of extracting a cogent theory from the *Treatise* and of casting it into a form in which it could command general assent fell to "the Maxwellians": George Francis Fitzgerald (1851-1901), Sir Oliver Lodge (1851-1940) and Oliver Heaviside (1850-1925).

Of their apparent advances one should mention the Bath 1888 meeting where the Maxwellians made clear that Maxwell's displacement current was not just a dispensable appendage to the theory, but its keystone: remove it, and the whole theoretical structure would collapse. Without displacement currents, electromagnetic waves could not exist.

In addition, Oliver Heaviside found that the ordinary radial electric field of a point charge is compressed along its line of motion by a factor of $\sqrt{1 - v^2/c^2}$ (Heaviside 1888). Heaviside's formula for the field around a moving charge exhibited (especially for Fitzgerald) that electromagnetic forces would be altered by just the factor involving to explain Michelson and Morley's 1881-1889 negative results.

But the most important move in consequent optics and electromagnetism unification, i.e. in electrodynamics principles extrapolation on optical phenomena was made by Francis Fitzgerald. He first broached the possibility of combining Maxwell's theory with Mac Cullagh's. In 1839 James Mac Cullagh had devised a Hamiltonian formulation of wave optics which yielded equations describing the main optical phenomena, including reflection, refraction and double refraction. Fitzgerald, by drawing correspondences between the terms in Mac Cullagh's theory and electromagnetic terms, was able, in 1878, to translate Mac Cullagh's theory into an electromagnetic theory of light (Fitzgerald 1878). It should be noted, however, that Mac Cullagh's theory suffered from serious mechanical difficulties, pointed out in 1862 by Gabriel Stokes. Stokes demonstrated that Mac Cullagh's theory implied attributing elastic properties to the ether which were quite unlike those of any known substances.

The merger not only resuscitated Mac Cullagh's theory but extended Maxwell's own theory in important new directions, yielding as one of its first fruits a prize that had eluded Maxwell himself: proper electromagnetic theory of the reflection and refraction of light.

Indeed, in a 1873 review of Fitzgerald's paper, Maxwell described his own treatment of the Faraday 1845 effect as a "*hybrid*" in which he had combined his electromagnetic theory of light with elements of an elastic solid theory. He had treated light waves as actual motions of the ether and had traced how these would disturb the spinning of the magnetic vortices in such a way as to cause the plane of polarization of the light to rotate.

In his review Maxwell had found this detour into a "hybrid theory" (his term), in which electrical and mechanical actions were combined, the least satisfactory part of his own explanation of the Faraday effect. And Fitzgerald's 1879 paper brought out, more clearly than before, the profound incompatibility between Maxwell's theory and an elastic ether. Fitzgerald had shown that Maxwell's theory was mathematically equivalent to Mac Cullagh's, while Stokes had shown in 1862 that Mac Cullagh's theory, considered as an elastic solid theory, was untenable.

> The conclusion was inescapable: if Maxwell's theory were to survive, it had to be cut loose from reliance on an elastic solid ether and given a fundamentally new basis. *Attempts to produce a 'hybrid' theory*, such as Maxwell had pursued in his own account of the Faraday effect, *had to be abandoned*. (Hunt 2005, 529)

Thus, in his encyclopedia of electricity and magnetism Maxwell sums his basic results up. His Copernican deeds consisted in combining arguments for electromagnetic and luminiferous ethers' identification and constructing the crossbred theory with displacement current that was capable of electromagnetism and optics unification.

By analogy, Nicolas Copernicus had pioneered in considering the Earth as an ordinary planet orbiting the Sun; hence he had created a crossbred theoretical object capable of extrapolating the mathematical principles from divine phenomena on the mundane ones. On the other hand, through the same crossbred object the physical principles were extrapolated from mundane objects on the skies (Nugayev 2013). Similarly, James Maxwell had constructed a crossbred object—the displacement current—and was able to extrapolate the electromagnetic principles on the optical phenomena, and vice versa. Introducing a kind of "*complementarity principle*" in the XXIII chapter called "Theories of Action at a Distance," Maxwell describes the difference between field and corpuscular approaches in the following way:

> Now we are unable to conceive of propagation in time, except either as the flight of a material substance through space, or as the propagation of a condition of motion or stress in a medium already existing in space. (Maxwell [1873/1891] 1954, 488)

Thus, we are ignorant of what is really moving between magnets and conductors, but if we decide to describe it we have no other "appropriate" images except the old stuff: "waves" and "particles." Maxwell's approach contains the seeds of modern quantum theory. As Richard Feynman has put it,

> Well, *it depends on our prejudices.* Many physicists used to say that direct action with nothing in between was inconceivable. (How could they find an idea inconceivable when it had already been conceived?) ... The only sensible question is what is the most *convenient* way to look at electrical effects. Some people prefer to represent them as the interaction at a distance of charges... Others love the field lines. (Feynman et al. 1964, 20)

And the lines of force serve as a "crude way of describing field" only. They have some merits since they provide a visual representation yet they have their own drawbacks too. For instance when one talks on E and B lines of force one should not exaggerate the reality of their existence. The lines may disappear when one wants to look at them in another frame of reference.

IV. Maxwellian electrodynamics in Germany: Helmholtz and Hertz

Due to Kantian background, Maxwell's programme development should be especially fruitful in Germany. And it was. Maxwell's efforts to find a reasonable compromise between the three research programmes (that of Young-Fresnel, Faraday and Ampére-Weber) were picked up by Hermann Helmholtz in his "On the equations of motion of electricity in conducting media at rest" published in 1870. In Helmholtz's paradigm charges and currents were treated as the sources of electrical and magnetic fields. It led directly to Hendrik Lorentz's dualistic picture of the field equations and the equations of motion in his 1892-1900 papers.

Furthermore it was Hermann Helmholtz who convinced Berlin Academy of Science to set up a special prize for experimental confirmation of Maxwell's theory. And it was Helmholtz's pupil Heinrich Hertz who got the prize in

1888. From two possible explanations of his experiments (see Smirnov-Rueda 2010, for details) Hertz had chosen the simplest one:

> Helmholtz distinguishes between two forms of electric force—the electromagnetic and the electrostatic—to which, until the contrary is proved by experience, two different velocities are attributed. An interpretation of the experiments from this point of view could certainly not be incorrect, but it might perhaps be *unnecessary complicated*. In a special limiting case Helmholtz's theory becomes considerably simplified, and its equations in this case become the same as those of Maxwell's theory; only one force remains, and this is propagated with the velocity of light. (Hertz 1893, 15)

It seems to me that it was the attempt to justify the rationality of choosing the simplest explanation that forced Heinrich Hertz after 1888 to give up his electromagnetic experiments fruitful both from heuristic and technological vistas and to devote the last three years of his short life to his extremely ambitious project of all the classical mechanics edifice rebuilding. As he put it clear in his *Principles of Mechanics*:

> it is premature to attempt to base the equations of motion of the ether upon the laws of mechanics until we have obtained a perfect agreement as to what is understood by this name. (Hertz 1899, XXI)

Hertz's apparent aim was to eliminate the "force" concept. But his more remote aim consisted in reconciling classical mechanics foundations with positivistic Zeitgeist:

> [...] furthermore, one would expect to find in these [electromagnetic field] equations relations between the physical magnitudes which are actually observed, and not between magnitudes which serve for calculation only. (Hertz [1890a] 1893, 196)

It should be noted that the methodological principles for classical mechanics reconstruction were to be found by Hertz in Kantian epistemology; even before he met Helmholtz, Hertz had attended in Dresden a course on Kantian philosophy.

> We form for ourselves images or *symbols* of external objects; and the form which we give them is such that the necessary consequents of the images in thought are always the images of

> the necessary consequents in nature of the things pictured. In order that this requirements may be satisfied, there must be a certain conformity between nature and our thought. Experience teaches us that the requirement can be satisfied, and hence that such a conformity does in fact exist. When from our accumulated previous experience we have once succeeded in deducing images of the desired nature, we can then in a short time develop by means of them, as by means of *models*, the consequences which in the external world only arise in a comparatively long time, or as the result of our own interposition ... *The images which we have speak of are our conceptions of things*. With the *things themselves* they are in conformity in one important respect, namely, in satisfying the above-mentioned requirement. For our purpose it is not necessary that they should be in conformity with the things in any other respect whatever. As a matter of fact, we do not know, nor have we any means of knowing, whether our conception of things are in conformity with them in any other than this one fundamental respect.
>
> The images which we may form of things are not determined without ambiguity by the requirement that the consequents of the images must be the images of the consequents. Various images of the same objects are possible, and these images may differ in various respects. We should at once denote as inadmissible all images which implicitly contradict the laws of our thought. (Hertz 1899, 1)

As a result, scrupulous analysis of the simplicity criterion arrives at the following conclusion:

> A doubt which makes an impression on our mind cannot be removed by calling it metaphysical; every thoughtful mind as such has needs which scientific men are accustomed to denote as metaphysical ... It is true that we cannot a priori demand from nature simplicity, nor can we judge what in her opinion is simple. But with regard to images of our own creation we can lay down requirements. We are justified in deciding that if our images are well adapted to the things, the actual relations of the things must be represented by simple relations between the images. (Hertz 1899, 23)

Hertz's Kantian background manifested itself not only in the epistemological scheme described. According to Jed Z. Buchwald, already in 1884 Hertz had proposed a version of Maxwell's equations that was free of the ether notion completely.

> Hertz, one might say, wished in 1884 to remove the ether, even if Maxwell's equations were to be admitted, in order to avoid working with an entity that behaved like a laboratory object but that could not itself be directly manipulated. (Buchwald 1998, 278)

And, what is more important, quite unlikely Maxwellian field theory, in Hertz's theoretical scheme the source continued to exist as an entity "in and of itself." In Hertz's pictorial diagram the material object remains unknown, whereas the inferred field is known. This diagrammatic inversion encapsulates the originality of Hertz's physics. It was because Hertz ignored the physical character of the object that produced his radiation—"because he boxed it in with a mental quarantine against asking questions against it—he was able to make progress where his British contemporaries had not been able to do so" (Buchwald 1998, 272).

Being a devoted pupil of Helmholtz, Hertz learned to watch for novel interactions between laboratory objects without worrying overmuch about the hidden processes that account for the object's effect-producing power.

Thus the nature of electromagnetic waves appeared to Hertz as a kind of "thing in itself" that admits a variety of interpretations. Researcher chooses the version that is the simplest one to work with. The most important thing is the equations depicting the relations between the objects under investigation.

> To the question, 'What is Maxwell's theory?' I know of no shorter or more definite answer than the following: Maxwell's theory is Maxwell's system of equations. Every theory which leads to the same system of equations and therefore comprises the same possible phenomena, I would consider as being a form of special case of Maxwell's theory. (Hertz 1893, 21)

To sum up, the aim of my paper is to answer the question "Why did Maxwell's programme supersede the Ampére-Weber one?" I think that the Maxwellian programme had superseded that of Ampére-Weber because it had constantly and fruitfully communicated with it. The Maxwellian programme did assimilate some of the propositions of the Ampére-Weber "hard core," as well as some propositions of the Faraday and Young-Fresnel programmes. But the opposite is not true. Ampére-Weber programme did not assimilate the propositions of the Maxwellian programme.

Hence, Maxwell's research programme did surpass that of Ampére-Weber because it was a "*synthetic*" one. Maxwell did put forward as a synthetic principle the idea, that radically differed from that of Ampére-Weber by its open, flexible and contra-ontological, strictly epistemological, Kantian character. For Maxwell, ether was not the ultimate building block of physical reality, from which all the charges and fields should be constructed. “Action at a distance,” “incompressible fluid,” “molecular vortices” were contrived analogies for Maxwell, capable only to direct the researcher at the “right” mathematical relations.

Contrary to Maxwell’s, the Ampére-Weber programme was a *reductionist* one for it tried to reduce all the theoretical ontologies to one and the same ontology of “action at a distance.”

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