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THE BACKGROUND TO HEINRICH HERTZ'S EXPERIMENTS IN ELECTRODYNAMICS

Hertz's theory is not as good as his practice and his theory has been developing recently in consequence of his practice . . .
George FitzGerald to Oliver Heaviside in February, 1889[1]

1. PREFACE

One hundred years ago an ambitious young German physicist demonstrated that electromagnetic radiation exists and that it behaves like light. Heinrich Hertz's experiments had, without doubt, the widest impact outside the scientific community of any in physics up to that time. Within physics they surprised the British, who did not expect to find this kind of radiation quite so simply. His results were surprising to the Germans as well, but they were also perplexing and difficult to grasp, an effect that Hertz sought to overcome through an elaborate series of theoretical articles. Then, just as his influence within German physics seemed destined to reach heights hitherto achieved only by his mentor Hermann von Helmholtz, Hertz succumbed to an extremely painful jaw malady that he had suffered from even in the midst of his most intricate experiments and complex theorizing.

Despite the fact that Hertz's articles on field theory greatly aided its dissemination beyond the Anglophone world and influenced physicists' understanding of it, he had himself always been most content in the laboratory. In 1878, when he was twenty-one years old, he had written his parents that when he was "only studying books" he was "never free of the feeling that [he was] a perfectly useless member of society". When he wrote this Hertz had only recently arrived in Berlin to work and to study in the laboratory of the renowned Helmholtz, who had at once set the young man a difficult experimental task in electricity. At this time Hertz did not know very much about the subject, but he plunged zealously into laboratory work, seeking to build and modify apparatus in just the right manner to minimize inaccuracies and so to

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reach a satisfactory conclusion. Hertz's absorption in laboratory practice insulated him to a considerable degree from the deeper reaches of the day's theoretical and philosophical issues, issues that, in his early publications, remain distant from his immediate goals, which were always tightly bound to experiment. He did of course study Helmholtz's work, or aspects of it at least, as well as other material, but in almost every case his purpose was to obtain something that could aid him in whatever experiment be was engaged in at the time. In these years he was not interested in probing the recesses of theory (in "studying books", as he put it); he only became interested in doing so when he could bring them to life, as it were, in the laboratory. Over the next eight years Hertz, who was ever moody and (though driven by ambition) not entirely confident in his abilities, experimented rather widely, but he always returned to electromagnetism. When he left Berlin after obtaining his doctorate, Hertz took up a position at Kiel where he had little access to experimental apparatus. He was unhappy there, for this reason and because he did not see where his career was going, but, ironically, his forced separation from the laboratory impelled him to ponder deeply the basic principles of electromagnetism as he had learned them from Helmholtz. His thoughts led him into extraordinary directions that challenged deep-seated contemporary beliefs. When Hertz left Kiel for a much more congenial position at Karlsruhe, where he could return to the laboratory, his thoughts merged with his experimental practice to turn issues and questions that had been troublesome to Helmholtz into new, and immensely fruitful, directions.

In the comparatively short compass of a few dozen pages I cannot hope to convey the intricate mesh of Hertz's awareness that German electrodynamics was flawed with his discovery that he could manipulate electric processes in ways that increasingly clustered about this hidden, and difficult, imperfection. However I can at least impart a sense of what Hertz perceived in the electrodynamics that he learned from Helmholtz — what it was that, in 1884, so greatly distressed him in the arid atmosphere of Kiel, only to bear fruit in his Karlsruhe laboratory two years later. I shall begin with a précis of the two major alternative electrodynamics to that of his mentor for in that way I can emphasize what it was about Helmholtz's account that perplexed and, ultimately, stimulated Hertz to achieve a discovery that had escaped his British contemporaries and that had scarcely been envisioned by his compatriots.

2. INTERACTIONS IN THE LABORATORY

Physics has developed in many ways since Hertz's day. One of the most striking changes is the startling degree of international unanimity concerning very wide ranges of principle and phenomena. Consider, for example, quantum mechanics. Nearly sixty years ago Paul Dirac wrote a text entitled *The Principles of Quantum Mechanics* that codified and synthesized its then-new principles. Dirac's text can be, and indeed is, used in graduate schools even today as a solid introduction to the subject's formalism and basic structure. These fundamental principles have changed scarcely at all in over half a century, and physicists in the Soviet Union are, in this area at least, in substantial agreement with their American colleagues.

When Hertz came to Helmholtz's laboratory in Berlin a sixty year-old text in electricity and magnetism was interesting only to antiquarians, and German physicists disagreed with one another as well as with their British contemporaries over fundamental points in the subject. The professional atmosphere of the discipline consequently differed greatly from what it has today become. Certainly a young, would-be physicist needed then, as today, to seek a subject in which to develop special expertise or else he stood no hope of employment. But in the modern era an up and coming graduate student can take for granted an extremely broad range of agreement on many fundamental issues, so that the area of expertise that must be developed can be extremely narrowly defined. A hundred years ago, particularly in Germany, no such broad range of agreement existed in almost any area that was well-suited to establishing a solid reputation. Even apparently innocuous experimental studies often resonated with important theoretical implications. Hertz's work cannot properly be understood in isolation from these currents of theory and experiment.

Helmholtz's work in electrodynamics, from which Hertz began, was very much shaped by his contant awareness of strikingly different alternatives to his own emerging views. In 1870, when he first published his own theory, there were two major approaches to electrodynamics, although only one of them was at that time well developed. The older, and better developed, scheme considered charge to consist of two kinds of massy particles and current to consist of the equal and opposite flow of these two in a conductor. The less highly developed scheme considered that electromagnetic processes occur preeminently in the space surrounding electrified and conducting bodies rather than in them. The first scheme was created by the German physicist Wilhelm Weber in the 1840s on the basis of the hypothesis concerning the nature of currents that had first been promulgated by his colleague Gustav Fechner. The second derives from the discoveries and concepts of the Englishman Michael Faraday and had been extensively developed by 1870 at the hands of the Scot James Clerk Maxwell. Helmholtz's theory (hereafter *HT*) differed remarkably from both of these two alternatives in rather complex ways that highlight by contrast its basic precepts. These differences hinge directly upon the physicist's image of laboratory processes.

Imagine oneself in a laboratory *circa* 1870 about to measure charge or current. Such things must in the first place be produced by a device that will generate enough of an effect for some other device to detect it. Charge would be produced by means of an electrostatic machine, which operates by friction; current would be produced by means of a battery or an electromagnetic generator. A device that detects charge — an electrometer — works by measuring in some manner the deflection produced in one charged object by another one against the action of gravity, spring tension or some such force. Similarly, current-detectors — galvanometers — work by measuring the deflection of a magnet or of another current-bearing body by a given one. In both cases the proximate end of measurement is a force that acts upon the material object in the requisite state of 'chargedness' or 'current-bearingness'.

Although this no doubt sounds like an elementary introduction to a positivist view of the connection between theory and measurement, nevertheless its implications must be thoroughly assimilated in order to grasp the sense of Helmholtz's theory in the context of the times. Let us begin with the Fechner-Weber (hereafter *FW*) electrodynamics. This theory postulates a direct transference between the laboratory measures and the physical interactions that produce them. The charged or current-bearing object disappears in itself from *FW*, to be replaced by a region in which electric particles move (or do not move) about within a matrix of material particles. The electric particles (which are mere points) exert specific electrodynamic forces on one another, and each responds to a given force with a fixed acceleration in the force's direction. Through some unspecified mechanism the forces between these particles are transferred directly to the particles that form the bodies in which they occur, so that the laboratory indicator actually measures the *net force* between the electric particles themselves. The bodies in which they exist are merely containers for them that are, as it were, carried along by an interaction between the electric particles and the other particles that comprise the laboratory objects. In *FW* the electric particles therefore act directly as *sources* for the actions whose overall effects are detected. The interactions consequently do not occur between the laboratory objects themselves, but they do occur between entities that subsist in these objects and that act upon their constituents.

The Faraday-Maxwell (hereafter simply *MF*) theory considers laboratory measurements in a very different way from this, for it does not postulate the direct transference that *FW* requires. Instead of envisioning entities that are different from, but that subsist in and act on, the material objects, *MF* dispenses altogether with the objects as electrodynamic entities in their own right and instead introduces something different from them. This entity, or field, subsists in the space occupied by the objects as well as in the space between them. When an object is in a 'charged' or 'current-bearing' condition then it does not, properly speaking, interact with the field; rather, its condition is a short-hand way of referring to the *local state* of the field, a state that depends upon the local presence of matter. The object's electromagnetic condition reflects, and is reflected by, the energetic structure of the ambient field, which determines the tendency of the object to move.[2] Since the local change in the field's state affects states throughout the field objects may rather loosely be said to 'interact' with one another. Here the laboratory measure does not emerge from a force-like interaction between entities that comprise the objects, but from a connection of an utterly different character between the states of the objects and the state of an entity that comprises nothing but itself.

Yet we see that, despite these profound differences between them, in neither *FW* nor in *MF* do the laboratory objects interact directly with one another. In *FW* they interact with electric particles, which in turn interact with one another. In *MF* they are linked at any given moment only to the local state of the field, and the existence of other laboratory objects at the moment of object-field interaction is irrelevant to the process: only the field state at the object's locus activates the detectors. In 1870 Helmholtz created a theory that differs radically from both of these in refusing to abstract from the laboratory objects in the fashion of *FW*, and yet in also refusing to introduce something entirely different in nature from them in the manner of *MF*. He substituted instead a difficult *taxonomy of interactions* for the unitary forces of *FW* or for *MF*'s duality between field and object.

Seen through Helmholtz's eyes the objects in the laboratory remain entities in their own right. A 'charged' object differs from an 'uncharged' one in acquiring a condition that it did not previously have *in relation to other objects that are also charged.* Similarly, current-bearing objects have no mutual interactions before they acquire currents; afterwards they do. In Helmholtz's understanding electromagnetic interactions are instantaneous and bipartite, and the nature of the interaction depends upon the simultaneous states of the interacting objects, the objects themselves being given directly in the laboratory.

Contrast for example the three ways in which electrostatic effects could be viewed in 1870. According to *FW* a charged body is in itself no different from an uncharged one; it merely has more of one of the two kinds of electric particles than the other kind. Consequently in *FW* electrostatic interactions are in fact *always* present between any two conductors, but net forces do not always result, and in any case the interactions are between electric particles. According to *MF* also a body remains essentially unchanged by its charged state. However the *field* at its surface, or within it, has its state changed, with an accompanying alteration in the field's energy gradient at the body's surface. This translates into a force upon the object due to whatever the unknown connections are that link the body's state to the state of the field. Here, as in *FW*, the object does not in itself determine the actions. Moreover, in *MF* it is also indifferent to the simultaneous presence of other bodies, although the state of the field at any given moment and place depends upon the previous positions of other objects at other places.[3] But now consider how this appears from Helmholtz's perspective. There are no electric particles to transfer force to the laboratory objects; there is no local field for the bodies to interact with. Rather, the *states* of the bodies at any given instant directly determine their mutual interaction. A charged body acts instantly and directly upon another charged body, and *vice versa*, but it is incorrect to say that the charge of the one body acts upon the charge of the other: the bodies act, and 'charge' is just a short way of specifying the nature of the interaction.

We can now grasp what is meant by a *taxonomy of interactions.* In the broadest sense electromagnetism for Helmholz is merely one among many different classes of possible interactions between laboratory objects. As a member of a given class an object can have one or more *states*. The critical point to grasp about Helmholtz's electrodynamics — the point that makes it entirely distinct from *FW* and from *MF* — is that every distinct kind of interaction requires a unique specification of the states of the interacting objects. At the simplest level there are two states and two interactions. An object may be charged (the first state, call it *q*), and it may also carry a current (the second state, call it *c*). Charged objects can affect one another, and so can current-carrying objects. Consequently at least two kinds of interactions exist, namely *qq* and *cc*. And every type of interaction requires, we shall see, a specific energy to be determined by the pair of objects in the specific states. This has radical implications indeed, because it means for example that charged objects and current-carrying objects have no necessary interactions with one another *whatsoever* in Helmholtzian theory. If they do interact then a new entry must be made in the taxonomy, and a new form of interaction energy invented.

To see just how extreme the consequences of this understanding can be imagine a charged object (*Q*) to be placed near a current-bearing object (*C*) in which the current strength changes with time. According to *FW* the moving particles in *C* will exert forces on the stationary particles in *Q* that will cause them to move, creating a current, and they will also produce a net force that tends to move the body in which they exist — although both results must be deduced from the fundamental, particle-particle interaction. In *MF* the changing current in *C* implicates an electric field, as does the charge in *Q*. The state of the field at either object therefore depends upon the state at the other one; an interaction between them may accordingly be said to occur, albeit one that is mediated through the field. But in *HT* we can say nothing at all *a priori* about such an interaction, because it is an entirely new one: it is an interaction between an object in a state of charge and one in a state of changing current, and this is not included in either charge-charge or in current-current interactions. If it exists then it requires an addition to Helmholtz's taxonomical structure — even though both *FW* and *MF* consider such an interaction to be included in the theory's basic structure. The instrumental simplicity of Helmholtz's exclusive concentration on laboratory objects consequently exacts a heavy toll in theoretical economy. I turn now to the many peculiarities (peculiar, that is, to partisans of *FW* and *MF*) and the one major obscurity, which strongly affected Hertz, to which Helmholtz's interaction taxonomy leads.

3. THE STRUCTURE OF HELMHOLTZ'S THEORY

Nearly every physical theory embraces a certain number of canonical problems, ones that it seems designed to explicate. Field theory, in Faraday's form, seems at once to encompass the generation of *emf* (electromotive force) by motion through a magnetic field, although even here there are subtleties and obscurities.[4] Conversely, certain problems may escape the theory's easy grasp. So, for example, it is hardly obvious from the principles of *MF* that a magnetic field will exert a force on a moving, charged body, although it does follow from them after a rather lengthy analysis. Moreover the problems that are easy in one theory may be just the ones that are difficult in another theory, and *vice versa.* To continue with these two examples, *FW* requires a fairly elaborate computation of the forces between moving particles to specify what occurs when the conductors are themselves in anything but the simplest relative motion. But if the motion is simple, as when a charged conductor moves slowly relative to a current-bearing one, then the forces are easy to compute, and it is almost obvious that the charged conductor will be affected by a net force. So in *FW* the generation of *emf* by motion can be a complicated affair, whereas the deflection of a moving charged object by currents is simple to understand. In *MF* the situation is reversed.

In Helmholtz's theory canonical problems of this kind do not exist, although certain types of problems certainly require more intricate analyses than other types. This unusual, and disturbing, characteristic derives from the theory's fundamental structure, from its insistence that electromagnetic processes must be construed as *states* of bodies, and electromagnetic interactions as unmediated relationships between bodies in these states. The theory does not introduce — as both *FW* and *MF* do — a third entity that intervenes between bodies (electric particles for *FW* and the field for *MF*). Such a thing must be present in a theory in order for canonical problems to occur in it because the canon generally reflects the elementary properties of the *tertium quid.* In *FW* problems in which the interactions between the electric particles seem obvious are canonical; in *MF* problems in which the field's behavior within and at the surfaces of bodies is simple to specify are canonical. In both cases the canon resides in a third entity whose relationship to bodies can be, and often is, problematic in complicated situations. But in Helmholtz's theory there is no third entity. There are instead a potentially very large (even, we shall see, infinite) number of different electromagnetic states that bodies may possess. None of these states has in itself a privileged position, so that no special kinds of problems seem to be more closely tied to the deeper recesses of the theory than any other kinds. *HT* has no elementary problems of the usual sort.

But it does have a spectrum of interaction energies. Although Helmholtz never thoroughly laid out the theory's structure, nevertheless the many articles he wrote to explicate it, articles that Hertz carefully studied in the late '70s and early '80s, together reveal it.[5] For purposes of economy and simplicity I shall present what is certainly an overly precise specification of the theory, but one that will permit me to concentrate on the problematic elements in it that, I shall argue, eventually disturbed Hertz.

The electromagnetic world according to Helmholtz's theory consists of bodies in various states. At its most simple the theory takes account only of conductors and the states that they may have. Suppose, for purposes of argument, that n distinct states exist, some of which are mutually exclusive. Represent a conductor C_a in the i^{th} state by $\rho_i(C_a)$. According to *HT* if two conductors, C_a and C_b, interact with one another in the respective states j and k then there must exist an *interaction energy* that depends upon $\rho_j(C_a) \times \rho_k(C_b)$ (where the sign 'x' indicates that the interaction depends upon the two states). The essence of Helmholtz's method involves taking this energy and determining how it varies when the bodies are moved or the magnitudes of their states change. Again a simple example helps to clarify the situation.

Consider two conductors each of which carries a current — or, better put, each of which is in a *current-carrying state*. Then construct (from 'experiment') an interaction energy between them. If the conductors change their distances from one another then the interaction energy may also change. By "energy conservation" such a change will translate into a mechanical force on each of them.[6] The essential point to grasp however is that there is no third entity that intervenes between the bodies, no *object* that must be thought of as having its own, independent existence. The interaction energy is not itself such a thing because its existence depends entirely on the states of the bodies in question, and indeed its locus, if one wishes to speak of such a thing, is coincident with the bodies themselves. Only the bodies exist as entities *per se*; energy is a function entirely of their states, of the characteristics which the bodies may possess.

In appearance this theory is an apotheosis of instrumentalism because it seems not to go beyond the laboratory objects and their unmediated interactions with one another. Even *force* is absent from the theory as an entity in its own right because it emerges only as a result of an energy calculation, as an epiphenomenon of the interaction energy.[7] Helmholtz did not for example consider electromagnetic interactions to involve the exertion of a force by one body on another, because forces do not inhere in bodies properly speaking: they are functions rather of bodily states than of bodily existence or even of the spatial and motional relationships between bodies.[8] And indeed Helmholtz's theory never does move far from the laboratory. But there is a heavy price to pay for instrumentalism of this kind because it requires in effect an *a posteriori* specification of energies for many situations that, in the other two theories, can be derived *a priori*.

Even what might otherwise be thought of as prototypically elementary situations can become quite intricate in Helmholtz's way of thinking. Consider for example electrostatic induction, where one charged conductor calls forth a charged state, or alters the charged state, of another conductor. In both *FW* and *MF* the phenomenon is indeed prototypical. According to *FW* the surplus or deficit of electric particles on the one body produces a net force on the particles of the other body, causing them to move until the net force on each of them again vanishes, and *vice versa* for the particles on the already-charged body. In *MF* conductors are given *a priori* as bodies that cannot sustain induction within their substance, which at once yields the usual laws of electrostatics. But *HT* requires a comparatively elaborate, and even problematic, discussion.

First of all 'charge' does not affect 'charge' because there is no such thing; there are only charged bodies. Between them an interaction energy exists that leads to their mutual force — the *electrostatic force*, properly speaking — but this cannot directly affect the state of charge *per se*. Consequently to explain induction we must bring in other interactions of some sort, and to do this we begin with the continuity equation. This equation, which in Helmholtz's theory remains a primordial, unexplained fact, links the appearance of a net charge density at a point of a conductor to inhomogeneities in the current at that point.[9] If we want to alter charge we must produce current. Consequently on must — and Helmholtz did — assume that a charged body can generate a current at a point in another conductor, that, in other words, there must be a term in Ohm's law that has the same form as the electrostatic force. But, and this point is critical, its appearance there has nothing directly to do with the electrostatic force between the conductors: it is not itself an *electrostatic* force. Rather, it represents the effect of an entirely separate interaction energy between the state of being charged and the state of carrying a current. Then, as a result of this independent interaction, a current flows through the conductor and, because this new interaction cannot produce closed or reentrant flows (since it derives from the gradient of a scalar function), the currents exhibit inhomogeneities over the conducting surface, thereby altering the state of charge. Eventually the state must be such that the electric force tangent to the conductor's surface vanishes since otherwise currents would continue to flow, yielding the usual condition in electrostatics.[10]

But what is the form of this interaction energy? Helmholtz never tells us. It remains a necessary albeit mysterious aspect of his theory that introduces an element of incompleteness into it. The probable reason for Helmholtz's silence concerns the difficulty of formulating an expression for such an energy. With charge-charge or current-current interactions the states are the same, and so they share an extremely important characteristic that makes it comparatively simple to build quantitative expressions: namely, that they either are, or are not, intrinsically changeable. The current-bearing state can change in such a fashion that the sum total of all the currents within a given connected conductor has no fixed value. The charged state behaves in a very different way: in a given connected conductor the total charge never changes at all. Consequently these two kinds of interactions occur between states whose total quantities are intrinsically changeable or intrinsically unchangeable, and this makes the variational technique on which *HT* rests comparatively simple to apply. But the mysterious interaction must occur between a variable and an invariable state, which means that the quantities that appear in it will behave asymmetrically under spatial and temporal changes, and this spoils the technical structure.[11] Yet the quantities must be linked to one another through the continuity equation. The chances for confusion here are legion. Little wonder that Helmholtz wisely chose to ignore the entire issue, preferring to sweep it quietly under the rug of Ohm's law by simply including in it an expression that is formally the same as (but that must be physically different from) the electrostatic force. The young Hertz was not quite so wise, for he appropriated these problematic elements of Helmholtz's theory and attempted to fuse them to his embryonic understanding of field theory. The result was confusion, but fertile confusion indeed.

4. THE HERTZIAN SOURCE

In 1884 Hertz wrote what is surely one of the most perplexing, and penetrating, pieces of physics in the nineteenth century. Little known today (though occasionally discussed[12]), it amounted to an attempt on his part to deduce Maxwell's equations without using the ether and without assuming that 'force' in the usual sense takes time to travel from one object to the next. What an extraordinary accomplishment this would be if it had only been coherent. But it was not. Hertz's first excursion into the deeper recesses of theory combined thoroughly incompatible approaches, not merely to electrodynamics, but to the very understanding of what it means for one object to affect another one.[13]

Hertz was certainly well aware that Helmholtz had obtained equations that looked rather like Maxwell's as early as 1870 by making the difficult and (in *HT*) problematic hypothesis that the ether is a dielectric with very high polarizability, that changing polarization must be included as a current in the continuity equation, and that polarization proper can be generated by the electromotive force due to a changing current. In the months after 1884, when Hertz immersed himself in the laboratory investigation of propagation, this aspect of Helmholtz's theory became critically important to him. But in 1884 he was engaged in something more wrenching than an attempt to trace the empirical implications of Helmholtzian polarization — hard though that was in itself. He was concentrating on theoretical primitives, on how the interactions of bodies with one another are to be conceived. His thoughts on the subject were certainly not entirely coherent at this time, in major part because he was trying to bridge two utterly different ways of conceiving bodily interactions, namely Helmholtz's with Maxwell's. But incoherence gave way in the following months to a new coherence, one that differed from both *HT* and from field theory, as Hertz strove in the laboratory to resolve the tensions that his 1884 excursion into the higher reaches of abstraction had elicited.

Twenty-seven years old, possessor of a doctorate, and the author already of eight articles in experimental electrodynamics as well as of several others, Hertz was unhappily employed at Kiel in a position without laboratory facilities. At some point during the late winter or early spring of '84 Hertz, isolated by force of circumstance from the laboratory, began, I think for the first time, to brood over theory, in particular over why there are three forms of electrodynamics that seem to be widely different from one another and yet that work equally well in the contemporary laboratory. Hertz thought himself into the laboratory even though he was physically absent from it and tried to find a common thread to unite the disparate theories together.

As he pondered "Maxwellian electromagnetics" in May Hertz came to see that a single process, one that could easily be examined in the laboratory, was fraught with implications for the connection between field theory and *HT*, as well as between these two and (he believed) *FW*. And that was the difficult and problematic — in *HT* — assertion that a changing electric current can not only generate another current, but that it can also affect a charge at a point in the same manner that another charge can. The avidity with which Hertz pressed the point in his published article, the very strength with which he insists on its being a fundamental necessity in all theories, itself indicates how long and hard he must have thought about it. He wrote:

> It has perhaps nowhere been explicitly stated that the electric forces, which have their origin in inductive [*i.e.* electromagnetic] actions, are in every way equivalent to equal and equally directed electric forces of electrostatic origin; but this principle is the necessary presupposition and conclusion of the chief notions which we have formed of electromagnetic phenomena generally. According to Faraday's idea the electric field exists in space independently of and without reference to the method of its production; whatever therefore be the cause which has produced an electric field, the actions which the field produces are always the same. On the other hand, by those physicists who favour Weber's and similar views, electrostatic and electromagnetic actions are represented as special cases of one and the same action-at-a-distance emanating from electric particles. The statement that these forces are special cases of a more general force would be without meaning if we admitted that they could differ otherwise than in direction and magnitude. But, apart from all theory, the assumption we are speaking of is implicitly made in most electric calculations; it has never been directly rejected, and may thus be regarded as one of the fundamental ideas of all existing electromagnetism. Nevertheless to my knowledge no one has yet drawn attention to certain consequences to which it leads, and which will be developed in what follows.[14]

These sentences reflect in their structure the convoluted path that

Hertz followed in that fateful spring month of 1884. The 'principle' with which he begins — that of the equality between 'electric' forces of electromagnetic and electrostatic origins — is, he asserts, a "necessary presupposition and conclusion". But which is it? Does it come first, or does it follow after, "the chief notions which we have formed of electromagnetic phenomena generally"? Or is it perhaps *equivalent* to these notions? He does not say. But he goes on to give us two particulars in theory, no doubt meant to support the claim that the assumption is a *presupposition*, and one in calculation (hence experiment), intended to support the claim that it is a *conclusion*: first Faraday then Weber are mentioned, and finally an assertion that the "assumption is made in most electric calculations" appears. But is it indeed? And note that Hertz does not feel it necessary to gloss the claim that the assumption holds for Faraday, whereas he adds an additional (and doubtful) sentence for Weber. The uneasy mix of these pregnant sentences betrays the incoherence among the conceptions that Hertz was trying to merge.

We know from Hertz's diary that by May 11 he was thinking hard about "Maxwellian electromagnetics", and that he continued at it for eight days, when he "hit upon the solution of the electromagnetic problem this morning". Until this time he had almost certainly not looked at all deeply at field theory, though he had delved into Maxwell's formidable *Treatise* in order to retrieve useful equations for his doctoral investigation on induction in rotating spheres. We must keep this comparative novelty in mind, because what struck Hertz from the first as a signal characteristic of field theory was its insistence on what he termed the "unity of electric force". Since, in it, a body is acted upon by a separate thing from it, but one that exists at its locus, the only determinant of the action is the state of the field. To assert, as Hertz does, that electrostatic force and electromagnetic induction are one and the same means in this context that they are shorthand ways of referring to the same state of the field, but that conducting bodies can effect the state in two different ways. In which case it is almost axiomatic that changing currents can move charged bodies because they alter the state of the electric field, which is what moves charged objects. This is Hertz's point of departure, both as the first sentence in his series of examples and, most probably, in his spring thoughts. For as we have seen it is not all obvious that electromagnetic and electrostatic forces are equivalent to one another in *HT*, and it is hardly axiomatic in *FW* either. This is why, having continued with *FW*, Hertz must gloss the point for it. He must explain what it means to say that both forces are merely special cases of "one and the same action-at-a-distance". But this is hardly *à propos*. Hertz's claim specifies a relationship between the actions of charged bodies and of current-carrying ones, and not one between static and moving electric particles. Since particles comprise bodies in what may be rather complex ways it is hardly obvious that Hertz's principle holds for bodies (and indeed it does not).

Thusfar Hertz has not mentioned *HT* at all, though there can be no doubt, given his background and training, that it, and not *FW*, formed the bedrock on which he attempted to build his understanding of field theory. *HT* is the implicit subject of Hertz's remark on calculation. The only situation in which the assumption in fact appears in a stark form involves Ohm's law. Three kinds of electromotive forces appear in the law: those due to chemical, thermal or mechanical effects, those due to electromagnetic induction — and those due to static actions, the very ones that are necessary but problematic in *HT*. Hertz's opening remark, that his assumption of equivalence is a "presupposition and conclusion" consequently parallels the division between field theory and *HT*. In the former it is a presupposition, but in the latter it is a conclusion drawn from the *empirical* requirement (first insisted upon by Kirchhoff) that the gradient of the static potential must be included in Ohm's law. Here we have a fundamental asymmetry between *HT* and *MF*, one that reflects the inherent and irremediable differences between them over the ways in which bodies interact with one another. Hertz however tried to relieve this tension, one whose deepest meaning he certainly did not as yet grasp, by raising to the status of a principle in *HT* what, in it, must remain tentative and unsettled in order for the theory to retain its completeness — the actions between charges and changing currents.

Although Hertz refers frequently to the "unity of electric force" (hereafter principle *I*), his argument was actually based on two principles, only the first of which directly involves the 'unity' of force in any meaningful sense. This was fully realized by some at least of his readers (in particular by Boltzmann), and the fact that Hertz's argument seemed to be incomplete as it stood was apparently quite obvious, though perhaps not at first to him. Indeed, the difficulties Hertz had over the next few months, and that are obliquely reflected in his diary, were perhaps prompted by this incompleteness, or by its implications.

The second principle (*II*), which Hertz did not explicitly state, but which he unquestionably used, can be expressed in the following way:

Hertz's Principle II: The Indistinguishability of Sources

If a body A which has a property 1 exerts a force of type x which moves a body C that lacks property 1, and a body B which has the same property 1 as A also exerts a force of type x that moves body C, and supposing that C exerts moving forces upon A and B according to the principle of action and reaction, then A and B must, while they possess property 1, likewise exert forces of type x that move one another.

Hertz had in fact begun his article with a form of *II* rather than *I*, albeit for magnetic rather than electric actions. Ampère's old argument (1820) that currents (A and B) must exert forces on one another since they and magnets (C) do involves, Hertz wrote, a principle of the unity of magnetic force. Although Ampère may not have begun with the principle, Hertz continued, "he certainly stated it at the close of his investigations when he reduced the action of magnets directly to the action of supposed closed currents".[15] Ampère's reduction is however much better expressed as the fact of the indistinguishability of *sources* rather than as a principle of unity of magnetic *force*, because it depends upon a physical model. The forces are unitary because all sources of magnetic action are indistinguishable from one another; in fact they are physically identical. Ampère certainly sought a unitary foundation for current-current and magnet-current actions, but he himself would not have accepted *à priori* a magnetic version of Hertz's principle *II*. Yet for Hertz a general principle is at work here, one that does not rely upon particular models.

The best way to see how Hertz's principles work for electric forces, and why both of them are necessary for his purposes, is to examine his own discussion of a special (but highly significant) case, that of two "ring-magnets" or magnetized toroids T_1 and T_2. Let us assume that the magnetization of T_1 is changing. Then on all theories this will produce an electromotive force within T_2 unless their planes are mutually parallel. If the magnetization of T_2 is also changing then it will exert an *emf* within T_1. And here Hertz's principle in form *I* enters. Both T_1 and T_2 must act to move an electric charge according to principle *I* since the latter does not allow us to distinguish *emfs* that move charges from those that create currents. But go further and introduce Hertz's principle *II*, in which 1 represents changing magnetization; T_1, T_2 and the electric charge are, respectively, A, B, and C. The charge lacks the property 1 which A and B possess. Now, we assert, not only is C moved by the *emf*s exerted on it by A and B, but so also are A and B moved by one another: since each of them moves C, and C reacts, then, by *II*, they must also move each other.[16] Bodies in which magnetization (or, equivalently) currents change must therefore exert mechanical forces on one another that are proportional to the products of the rates of change.

This startling action is missing from what Hertz called the 'usual' electrodynamics,[17] among which he included Weber's.[18] And yet, he has argued, the 'usual' electrodynamics at least tacitly assumes the "unity of electric force" in the sense of principle *I*. But not only is *this* claim of Hertz's itself questionable, it is not at all the same as principle *II*. For the heart of the issue for Hertz, despite his own phrase "the unity of force", was not force *per se*, but rather how one must treat the things that generate and are acted on by force in relation to it: how, that is to say, one must conceive the nature of the source in relation to its action. Hertz's principle *II*, but not his principle *I* (which alone does not yield Hertz's conclusion), introduces into electrodynamics a *physical unity of sources in relation to their actions*, and such a unity is completely foreign to *HT* as well as to *FW*. The nub of Hertz's implicit second principle is its prohibition on distinguishing as objects to be acted on any things that produce electric (or magnetic) forces. All things that exert forces of a given kind must, in Hertz's principle, be acted on by any and all other such objects, whatever their actual physical structure may be. Insofar as these forces are concerned such things do not have direct physical relations with one another. Rather, their mutual connections are determined only in relation to something of a different kind from themselves, namely that which acts upon them (Hertz's "force"). That is the core, the elementary sense, of Hertz's tacit second principle, a sense that suggests the relationship between sources exemplified by field theory. Here, despite his claim to have used propositions that are 'familiar' in the usual electrodynamics, Hertz had actually violated its most elementary concept, the tacit but irrefragable understanding that objects act on objects, that *force* is a relation between two things of the same kind; it does not stand apart as a thing in itself.

The 'usual' electrodynamics does not, indeed cannot, detach an action from its source because forces do not exist independently of sources. Since a "force" is always an action between a pair of objects in certain states, the concept of, say, the value of the "force" at a point in space usually lacks physical significance.[19] For it the value of the "force" at a given point — the idea that a force can be defined solely by its "value" at every point of space — can have no physical meaning: the states of the objects between which the action occurs must also be given. Consider again how the 'usual' electrodynamics, based upon electrostatic (U_s) and electrodynamic (U_d) force functions, describes interactions. For the two distinct kinds of physical objects, charges (q) and currents (c), there are two types of interactions: charge-charge, current-current. All theories agree on three points: a charged body bearing a charge q can be moved by another charged body; the body containing a current c can be moved physically by another current-bearing body; a current can be altered in magnitude by a changing current. Now it *may* also be the case that a changing current can move a charge, and that a charge can generate an *emf* at a point, but this interaction cannot be directly represented by either of the two potentials U_s, U_d alone since they are formulated solely for charge-charge and for current-current interactions respectively. Despite Hertz's principle *I*, the modes of interaction that, according to *HT*, are involved in these two cases are decidedly different from one another: one is a charge-charge, the other a charge-current interaction.

Things are rather different in a theory that, like Weber's, does not begin with a multiplicity of potentials but, instead, postulates that all electrodynamics involves a single physical entity, namely a particle. Weber's theory can, and by 1884 certainly was, presented in terms of a potential function, but here the function directly determines the interaction only of electric particles, not of charges with currents or of currents with currents. To obtain these latter, Webereans must introduce Fechner's hypothesis, which specifies the link between Weber's particles and the electrodynamic sources, between, that is, the particles and the charges and currents of laboratory experience. We may then calculate a potential function similar to U_d if we wish, and we may even compute such a thing for interactions between charges and currents. But these functions will be merely secondary representations of the fundamental interaction between particle and particle. Indeed, they may not even be precisely equivalent to the ones that prevail in the 'usual' electrodynamics since, by "usual", Hertz intends *HT*, wherein U_d is given *à priori* rather than *via* a calculation from a more fundamental interaction.

Despite these important differences between Weber's and the 'usual' systems, they both do share one overriding characteristic: namely, that the actions cannot be divorced from their physical sources. In the 'usual' system this means that energies must be given for every type of interaction. In Weber's case it means that what are usually thought of as sources, namely charges and currents, must be computed from the true sources, which are moving electric particles. Interactions do not occur between these calculated representations, but between the particles proper. So, for example, it may be true, as Hertz insists it is, that a changing current can move a charge, and that the charge cannot tell whether the force acting on it at a given point and at a given moment derives from induction or from static action. But the 'usual' system must simply postulate this as a fact; Weber can deduce it from the force between his particles. One may nevertheless say that, for these two kinds of electrodynamics, it is essential to know the physical nature of the sources. Interactions occur directly between them, and *only* between them (though, in *FW*, the sources proper are completely invariant in kind).

Field theory provides a very different way of thinking from either of these two. According to it electric and magnetic forces — fields — can, indeed *must*, be divorced from sources. The values of the electric and magnetic field at a given point are in themselves sufficient to determine how an object placed there will behave. What produced the fields, or how the fields were produced, makes no difference at all to their effects: the only important thing is the connection between the field and its object, not between object and object. Consider Hertz's "ring-magnets" from the viewpoint of field theory. Each of them produces a contribution to the E field through its changing magnetization. The field can move a charge, and the charge must, for reasons discussed immediately below, be able to move the sources that contributed to the field that moves it, namely the ring-magnets. The only field that is associated with the charge, the only means by which it can move anything, is its contribution to E. But there is no way to distinguish one contribution to the field from another one. And so the ring-magnets must move each other since each of them also contributes to the E field.

This sort of conclusion is represented analytically in field theory by energy functions of the very kind that Hertz was considering when he emphasized the conservation principle. Whether two objects interact depends upon whether or not there is a term in the field energy that depends upon both of them. If there is then they necessarily affect each other. In the example of the ring-magnets the energy varies as the square of the electric field. Since the total field consists of a linear superposition of all the partial fields, which include the two E_A, E_B from the ring-magnets, then the energy contains a term of the form $E_A \cdot E_B$, and so the ring-magnets must exert forces upon each other. The essential feature of this deduction is that one cannot distinguish between the E fields produced by the sources however different the sources may be as physical objects. In that sense this is a principle of the *indistinguishability of the sources as generators of fields.* Hertz's principle *II* is therefore a principle of field theory, but only of field theory.[20] Yet Hertz's argument also required a method that has significance only in *HT*, and which therefore depends upon a completely different way of thinking about the relationships between sources. Little wonder that his contemporaries found it to be confusing: it is confusing because it is confused, and necessarily so. And precisely because of this Hertz's argument provides unique insight into a situation in which a physicist attempts to meld two theories that are incompatible at the most fundamental level.

5. HOW TO DERIVE MAXWELL'S EQUATIONS WITHOUT THE ETHER

Hertz employed two basic assumptions in his extraordinary attempt to derive Maxwell's equations without (he hoped) any new physics. The first — his principle *II* — asserted the indistinguishability of sources in regards to the actions they produce. This is a principle of field theory. The second postulates that every distinct electromagnetic action corresponds to a distinct form of energy between the interacting objects. This is a principle of Helmholtz's theory (*HT*) but not of field theory. Although the two principles are profoundly incompatible in their understanding of how objects interact with one another, nevertheless their opposition does not leap unaided to the eye. Indeed, field theory as developed among the Maxwellians at just this time did employ a form of energy analysis that bears a superficial resemblance to Helmholtz's principle. Moreover, even though Hertz was almost certainly unaware of these developments among the Maxwellians he would not have questioned Helmholtz's principle because the entire structure of *HT*, which he imbibed as it were with his mother's milk, depends upon it. It is rather his new principle *II* that raised questions, but he did not at this time appreciate the difficulties that it produced. For him *II* derived (in the context of *HT*) rather from the exigencies of laboratory practice — in particular from Ohm's law — than from fundamental principle.

To understand Hertz's argument we must begin with Helmholtz's energy principle. An early form of the principle dates to 1847, when Helmholtz had introduced energy conservation, but he developed it most carefully only in 1874 (and even here his argument suffers from several *lacunae*). The argument can be expressed in words (albeit imperfectly) in the following way. All electromagnetic interactions, *per* the fundamental outlook of *HT*, occur between objects in particular states, and to each such interaction corresponds a special interaction energy. In many cases we can write the interaction energy in a special way as a product of two functions, one of which directly expresses the state of object *A* (call this state *C*) while the other indirectly expresses the state of object *B* (call this function *U*). Now whenever the value of the energy changes, for whatever reason, forces arise between the objects. By 'force' we mean an expression that appears in the principle as a result of the necessity to balance energy and that equates analytically to coordinate-or state-dependent effects.[21]

The core of Helmholtz's argument enables one to connect the two kinds of forces that may arise: those that tend to alter the *state* of an object, and those that tend to alter its *position*. One can easily find the two forces independently of one another from the interaction energy. If the function *U* of body *B*'s state changes with time then a force will act on body *A* that tends to alter in it the value of its state *C*. The mere existence of the two states, even if they do not vary with time, entails a force that depends upon *U* and that tends to alter the location of the body in state *C* (of course we can carry out precisely the same analysis for the action of *A* on *B*). Helmholtz (rather inadequately) demonstrates that the existence of either of these forces implies the other one *if* we assume that the forces necessarily reflect interaction energies that are determined immediately and exclusively by the states of the interacting bodies.

Hertz pondered this fundamental argument of Helmholtz's — the argument that truly underpins *HT* itself — as he attempted to grasp field theory in the spring of 1884. On the morning of May 19 he "hit upon the solution" of how to link the two theories. His argument is, as might be expected, quite difficult, in major part because it is not coherent, but it can be appreciated through the example that he gave. Suppose we again consider Hertz's "ring-magnet". When for whatever reason a current passes through a wire that coils round the ring then it becomes magnetized along its length. Now suppose we take two such things and link them together as though they formed part of a chain, and suppose further that the current in the wire that surrounds one of them changes. According to all three theories of the day this will induce a current in the wire that surrounds the other ring. However since the magnetic action is, as it were, confined to the volume of each ring the two rings should not exert mechanical forces upon other — they certainly do not according to the usual principles of *HT*, which is primarily what Hertz had in mind.

But now Hertz brings in his new principles and Helmholtz's old one. First Hertz's principles. According to the "principle of the unity for force" the electromotive action that the ring-magnet with changing current exerts can equally well be used to move a charged body instead of to create a current in its linked partner. But then according to Hertz's principle *II* a pair of ring-magnets each of which has a changing current in its windings should exert *mechanical* forces upon each other (since they would do so upon a third object). Enter Helmholtz's principle. Here we have a mechanical force between two objects in particular states — states, that is, of *changing* current. Such a force requires a corresponding interaction energy that we have not taken account of — otherwise the force could not exist. But then we must also have an action that tends to alter the state of the one body if the state of the other changes — if, *i.e.*, the changing current of the one ring-magnet itself changes, then a tendency to alter the rate of change in the current surrounding the other ring-magnet must exist, a tendency that depends therefore upon the second-order change with time of the first magnet's current.

We do not stop here. This new action in turn entails yet another mechanical force between the ring-magnets by Hertz's principle *II*, which requires a third interaction energy. From that *via* Helmholtz's argument another action arises that depends upon temporal change, and so on *ad infinitum*. A dizzying series of decreasingly-large interaction energies that depend upon an equally dizzying series of new electromagnetic states extend to infinity. This was what Hertz must suddenly have understood that spring morning in 1884, and he saw that he could use it to obtain Maxwell's equations because it suggests the very link between magnetic and electric action that governs the electromagnetic field.

Again words can only imperfectly convey the beauty and seeming inexorability of Hertz's argument. The function *U* that I mentioned above has a particular form: it depends in the first instance upon the current in one of the two ring-magnets. Hertz's principle translates the electromotive force that arises when *U* changes with time into a mechanical force, and then Helmholtz's principle derives a new time-dependent action from this one. The mathematics of the process leads to a second-order correction to the function *U*, and in the end to an infinite series of even-order corrections in the derivatives of the function with respect to time. When Hertz then operated on the entire series that results with spatial and time-derivaties he found that the function satisfies the very same wave equation that arises out of Maxwell's equations. Indeed, these equations can be manipulated to produce *the very same* "system of forces" that Maxwell had obtained in his *Treatise* for the ether in the absence of matter. In Hertz's words:

> Now the system of forces given by [my new equations] is just that given by Maxwell. Maxwell found it by considering the ether to be a dielectric in which a changing polarisation 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 . . . From our point of view, the Faraday-Maxwell view does not furnish the basis of the system of equations* [with which Hertz's argument began], although it affords the simplest interpretation of them.[22]

Maxwell's equations, and so the propagation of electromagnetic effects, have apparently been derived without benefit of either the ether or of any assumption *à priori* that there are separate things — forces — that can be detached from the objects that determine them and can move through space over time. An astounding result indeed, and one that not even Hertz himself could entirely accept, as we shall now see.

6. OBJECTS AND INTERACTIONS

In a note near the end of his paper Hertz pinpointed the unsettling element in his analysis:

> The mode in which we have deduced conclusions from the principle of the conserva-

tion of energy clearly marks at each stage the point at which our deductions are only the most fitting, and not the necessary ones. *This mode is the most fitting from the standpoint of the usual system of electromagnetics*, for it corresponds exactly to the accepted proposition in which Helmholtz in 1847 and Sir W. Thomson in 1848 deduced induction from electromagnetic action. But perhaps it may not be the only possible method; for just as in that proposition, so we have in ours made tacit assumptions besides the principle of the conservation of energy. That proposition also is not valid if we admit the possibility that the motion of metals in the magnetic field may of itself generate heat; that the resistance of conductors may depend on that motion; and other such possibilities.[23]

What though made the energy principle problematic? Did it not for example hold equally well in field theory and in Weberean electrodynamics? Indeed it did not, though Hertz was at this time unable exactly to locate the difficulty — he knew approximately where it lay, but he did not know its precise shape.

Helmholtz's energy principle purports to deduce electromagnetic force from electromagnetic induction and *vice versa*. In fact it requires much more than this: it also requires the fundamental premise that objects in particular states determine a specific interaction energy that depends solely upon these states and the mutual disposition of the objects at any given instant. Given this (as well as a rather wide understanding of Ohm's law) then Helmholtz's argument works. Without it the argument fails. But this is the very assumption that both *FW* and *MF* in their respective ways deny. In *FW*, properly speaking, the only fundamental interaction energy subsists between electric particles; anything constructed to mimic the energy of *HT* exhibits a mere mathematical, not a physical, similarity to it. In *MF* the only interaction occurs between object and field, not between object and object: here, too, a function constructed to mimic the interaction energy of *HT* is a vacuous simulacrum of the underlying physics. Only *HT* can fulfill the demands of Helmholtz's argument because only it links interacting objects enduringly together.

Hertz did not see this much. He never mentioned the point, and indeed he did not even remark the discordance between his principles and Helmholtz's argument. The principles, as we have seen, implicitly deny that objects interact directly with one another by separating actions from sources. Helmholtz's argument forbids disjoining them. Hertz's intense contemplation of electrodynamics in the spring of 1884 had brought him to the brink of grasping the primordial difference among the electrodynamic theories of the day, but only to the brink.

The powerful hold on Hertz of Helmholtz's conception of the link between object and action has, I think, two sources. Before Hertz came to Berlin he knew, he tells us in his diary, very little electrodynamics. Advanced theory was inevitably Helmholtzian theory because Hertz learned it — to judge not only from the content of his work, but also from its citations — directly from the master's papers of the '70s. In them Hertz would have encountered at every turn the vision of objects interacting directly with one another unmediated by a *tertium quid*. Neither force nor ether as a necessary vector of force populated Helmholtz's universe. There are only objects, entities given in their nature through laboratory experience, and the energies they specify by their existence in specific states. Which raises the second probable source of Helmholtz's intellectual hold on Hertz.

The student came to the master already enamored of the laboratory. Since childhood Hertz had been happiest manipulating objects, making them do things and measuring their properties. With due homage to the veil years draw over memory we may still perhaps trust his mother's affecting account of the seventeen-year old Heinrich's infatuation with making and measuring things:

> . . . At that time [1874] he already produced various types of physical apparatus, cutting each brass screw, pouring the little weights, and making all the essential parts by himself, with sheer incredible patience.
>
> Once he came to grief. He tried to make a spectroscope. His father had promised him the prisms for it and had written about it to Herr Schroeder, who was famous for his optical glass. The reply was that Heinrich should come to see him on Sunday, he would be in his office until 12 o'clock and would fix the prisms for him. Heinrich's joy and hopes were great. He worked on his apparatus with fiery zeal, got up at 5 o'clock on Sunday, hardly permitted himself time for breakfast in order to be ready, and then set out with his father. But because they had apparently miscalculated the time required, they did not get there until five minutes past 12 o'clock and found the place closed and a sign saying that Herr Schroeder would be away for several weeks. Heins came home inconsolable, and it cut me to the quick to see him crying in his quiet way, one big tear after another rolling down his cheeks.[24]

An ardent, almost impassioned flame for making and measuring propelled the young Hertz's career, taking him at the age of twenty-one to Helmholtz in Berlin. Within a few weeks of his arrival he busied himself in the laboratory, attacking a recondite problem assigned by Helmholtz himself. Imagine the impression on the novice of the great master's intimate concern. "Since yesterday I have been busy in the laboratory", he wrote his parents on 6 November. ". . . I reported to Prof. Helmholtz yesterday that I had thought over the matter up to a point and would like to begin. He went with me to see the demonstrator and was kind enough to spend 20 minutes in discussion as to how best to begin and what instruments I should need."

There, in the Berlin lab, Hertz was molded by Helmholtz. But the Hertzian clay resisted. References to "Prof. Helmholtz" in letters home soon gave way to a more familiar "Helmholtz". Hertz complained that Helmholtz didn't read things, and he declined to undertake new experiments that Helmholtz wished him to pursue. He turned temporarily from the laboratory to a 'theoretical' issue late in '79 (albeit one that he took care to connect strongly to experiment). Hertz began to distance himself from Helmholtz at about this time, which I believe made it possible for him, when distanced also from a laboratory setting redolent with Helmholtz's influence, to play with contemporary ideas in new ways. But he could not move too far from home; he could not yet abandon, or transform, the familiar world of interacting objects for something wholly different. And he did not.

Instead, Hertz stripped away the unessential from Helmholtzian theory and reduced it as far as he was able to the pure form of the energy principle. Then he sought a point of contact with field theory, something that is not at once obvious in *HT* but that is In *MF*. He found it in the putative action of a changing current on a charge, and the structure of Hertz's analysis fractures over the impropriety, indeed the *incoherence*, of blending Helmholtz's energy principle in this way with field theory. Either interactions are indifferent to the nature of the source or they are not. If so then they corrupt Helmholtz's theory; if not then field theory fails. Hertz had not shown that all theories must lead to the field; he had instead shown that Helmholtz's theory transforms into field theory when the separate identity of the source dissolves. A powerful lesson indeed, but a difficult one that Hertz did not at first appreciate. He continued for some time to retain the understanding of sources that he had absorbed in Helmholtz's laboratory, but with the uneasy knowledge that something peculiar occurs when the energy principle combines with propositions that seem to be irreproachable (his own two principles).

After Hertz took up a position at Karlsruhe that returned him to the laboratory his perplexing juxtaposition intersected with material practice to forge a new understanding of electrodynamics. As Hertz discovered how to probe, to manipulate currents in extreme circumstances his understanding of how objects interact with one another also began to change. At first interactions manifested Helmholtzian properties to him. A loop connected by a wire to an intermittently closed circuit coupled to it through Helmholtz's energetics, represented by the electrodynamic and scalar potentials. The wire was removed; the coupling became less obviously Helmholtzian to Hertz. As the device mutated, as Hertz played and probed with it, a new way of thinking about coupling, about electrodynamic interactions began to form. Not the way of the field, or at least of the Maxwellian field, for Maxwell had dispensed altogether with sources. Rather, Hertz constructed an entirely novel understanding in which the sources as objects couple to the field as a separate thing, so that the structure of Helmholtz's interaction energetics was preserved formally and substantively through a coupling between object and field rather than between object and object as in the original. Hertz did not therefore embrace the object-field relation of Maxwellian theory, for he did not abandon the object.[25]

The intimate details of Hertz's work must await a more complete exposition than I can give here, but at least two significant implications emerge naturally from this way of construing his work. Hertz is famed for having (together with Oliver Heaviside) abandoned the vector and scalar potentials entirely, and (as a corollary) for having given the well-known symmetric form of Maxwell's equations. Heaviside abandoned the potentials because they did not permit a thorough foundation of field theory upon distributed energies[26]. Hertz had entirely different reasons, ones that had little if anything to do with energy *per se* but quite a bit to do with the nature of the source. In Helmholtz's theory the potentials are not merely aids for deducing the forces; they have immediate physical significance as representatives of the interaction between objects. If they are dispensed with them interactions between objects must be understood in a new way, as the indirect result of the direct interaction between the object and the field. Consequently Hertz's discarding of the potentials reflects his transformation of object-object into object-field interactions.

Hertz is also well-known for his last work, the *Principles of Mechanics,* in which he dispensed with the concept of force, referring everything to space, time and mass. It has perhaps been rather puzzling that Hertz devoted his last energies to such an abstract endeavour, one that seems to be so far removed from the laboratory. I suggest that a major (perhaps the major) reason for his intense attention to the abstractions of mechanics arises directly out of his new electrodynamics and has its seat in his understanding of laboratory experience. Hertz had only with prolonged effort transformed his Helmholtzian understanding of object-object relations into an object-field link. The core of that transformation involved separating the effect that one object has on another from the interaction that produces the effect, but Hertz did not seek to abolish the objects as actors in the fashion of field theory.

In Helmholtz's apprehension, we have frequently seen, objects do not, properly speaking, exert forces on one another because 'force' does not occur as a separate entity. 'Force' in Helmholtz's sense manifests the energy principle: objects tend to accelerate, or their states to change, whenever the motion or change of state would involve a change in their interaction energy. The concepts of 'matter' (or, more generally, 'object') and 'force' cannot here be thoroughly disentangled from one another because the latter signifies an aspect of an interaction between the former and because in the absence of the interaction ('force') the objects themselves would be inaccessible to experience. In 1893, shortly before Hertz's death, Helmholtz mulled over this difficult point in his introduction to a series of lectures on "theoretical physics":

> It can easily be shown that this abstraction, force, and natural object or body, to which we ascribe the force, cannot be divorced from one another. When we speak of moving forces we are in the habit of denoting what can be moved simply as mass or as mobile matter. A force without matter lacks meaning; this would correspond to a law that speaks of changes but where there are no objects that can be changed.[27] Such a law would contradict and repeal itself; and there is no more meaning in speaking of matter without force; for such material objects could never undergo any changes, since changes always presuppose the existence of a force. *It is already apparent from this simple consideration that material object and force are two abstractions that cannot be divorced from one another, which have determinate meaning only in their connection and association.*[28]

These concepts, as well as others concerning the interactions between objects — all of which have their origins in his formulation of the energy principle — are implicit in Helmholtz's electrodynamics, and Hertz saw them there. His *Principles of Mechanics* attempts to formalize and codify them in a way that remains closer to Helmholtz's fundamental views than Helmholtz himself remained in the early '90s when he turned to the principle of least action as a basis for physics. The master, influenced by the student's discoveries, had changed. But the student, faithful in the deepest sense to the master, chose a different path. Unlike Helmholtz, wrote Hertz in his introduction to the *Princi-*

ples, "I endeavour from the start to keep the elements of mechanics free from that which von Helmholtz only removes by subsequent restriction from the mechanics previously developed [namely force]".[29] The *Principles* are a fitting if melancholy end to Hertz's appallingly short career.

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NOTES

[1] IEE (London) Heaviside collection.

[2] If the object moves then the state of the ambient field changes, which entails an alteration as well in the local energy densities. That, in turn, translates into a stress over the region occupied by the object according to the basic principle of field theory that moving force derives from local energy gradients. See immediately below for a pertinent example.

[3] If the latter objects interact locally with the field at some moment then its changed state will propagate from point to point throughout it, ultimately reaching other objects and affecting the field's state at their positions. Consequently the interaction between field and object at a given moment depends upon other object-field interactions at previous times.

[4] As, e.g., when the moving object is a homogeneous cylinder that rotates about a central axis in a uniform magnetic field that is parallel to the axis.

[5] Helmholtz's two most informative articles are "Ueber die Bewegungsgleichungen der Elektricität für ruhende leitende Körper", *Borchardt's J. f. d. reine u. ang. Math.*, **72** (1870) 57—129: *Wiss. Abh.*, Vol. **I**, pgs. 545—628, and "Die elektrodynamischen Kräfte in bewegten Leitern", *Borchardt's J. f. d. reine u. ang. Math.*, **78** (1874) 273—324: *Wiss. Abh.*, Vol. I, pgs. 702—62. The articles are collected in his *Wissenschaftliche Abhandlungen*, 3 vols. Leipzig: J. A. Barth, 1882.

[6] The argument is rather more complicated than this simple remark suggests because it must exclude certain kinds of energy transformations, include other kinds, and the whole must presume the existence of a systemic interaction energy that acts as the sole energy source for system changes. In fact Helmholtz's several presentations of the energy principle in this form are rather incomplete, even faulty, though their results are unexceptionable. The essential point to grasp about the principle is that the elements that enter it as givens are system energy and system states, with 'force' emerging only as an artifact of the necessity to balance energy.

[7] Helmholtz's fervid development of a relational, energy-based scheme that avoids atomistics in the Weberean sense and relies instead on variable 'states' almost certainly began as an attempt to rip *force* as a category altogether out of physics rather than as a technical alternative to Weber's force-law. That goal became ever more pronounced during the 1870s, almost certainly because Helmholtz came to see Weberean physics (in the extreme form publicized by its most ardent supporter, the notorious xenophobe and anti-Semite Friedrich Zöllner) as the embodiment of a recreant Idealism that opposed everything he had long embraced — including the image of the German

university as a bastion of freedom, the goal of a science tied to the practical world of manufacture and freed from the vicious influences of Idealist philosophy. Nevertheless Helmholtz's own relational understanding of interaction shares a great deal with its Weberean antagonist since the latter had always understood *force* as a relation between point-atoms. At the most fundamental level the new understanding vehemently discards Weber's immaterial points, but it does not replace them with material atoms related by force. Instead, Helmholtz retains the essential idea of a *relation*, but uses system energy to predicate it of *states* rather than of invariant (i.e. atomic) *objects*. These points will be further developed in my forthcoming book, *The Discovery of Electromagnetic Radiation*. The Idealist thrust of Weberean physics is developed by Norton Wise in "German concepts of force, energy, and the electromagnetic ether, 1845—1880" (in Cantor, G. and Hodge, M. J. S. (eds) *Conceptions* of Ether, Cambridge: Cambridge University Press, 1981, and by A. Molella, who discusses Zöllner as well as Fechner and Weber in his illuminating but unpublished Ph.D. dissertation, *The Problem of Atomic Action at a Distance* (Cornell, 1972).

[8] To grasp the distinction I have in mind consider that in *FW* the force that acts on a given electric particle does not in any way depend upon a *state* in Helmholtz's sense because the particles are unalterable in themselves. They do however move, and the forces depend directly upon the relationships between their motions and upon the distances between them. In Helmholtz's conception, on the other hand, two bodies moving in certain ways with respect to one another are not thought to have a mutual relationship with one another that depends immediately upon the motion. Rather, they may have electromagnetic states, and these states determine an interaction energy that may change *as a result* of the motion, but the bodies do not have states that are direct functions of the motion.

[9] In *FW* the continuity equation is almost obvious because current *is* moving charge, so that inhomogeneities in current are akin to inhomogeneities in flow, which (by conservation of matter) accumulates fluid. In *MF* it raises many difficult questions, though of a different kind than in *HT*. (On the latter see Buchwald, *From Maxwell to Microphysics*, Chicago: Chicago University Press, 1985; *passim*. The point to seize is that, despite its suggestive form, the equation *need not* predicate the actual transfer of substance; it can just as well specify the shift in location of a conserved condition (such, e.g., as heat).

[10] More precisely, a given region of the conductor, having a certain charged state, interacts with other regions of the same conductor to generate currents there. Over time the distribution of densities over the conductor will converge in such a fashion that the net action of every charged region on every other, together with the actions of the unconnected, external conductors, vanishes at every point within and on it. Unlike a Weberean account this one is not forced to consider that 'charges' exert forces upon one another: conducting bodies in certain states interact.

[11] In general the variation of the interaction energy contains terms that depends on time derivatives and terms that depends on coordinate derivatives. Consider a charge-charge interaction. Here the time-derivatives cannot have any affect at all because charge *per se* is invariable: if it changes then currents are involved, and we have moved outside the pure realm of charge-charge interactions. The coordinate derivatives on the other hand yield the usual Coulomb force between the charged bodies. Consider next a current-current interaction. Here the time derivatives yield the *emf* (electromotive

force) that one changing current exerts on another, and the coordinate derivatives yield the mechanical forces between the current-bearing bodies. And the reason that the time-derivatives can here have an effect, whereas they cannot in charge-charge interactions, is due to the intrinsically variable nature of currents: they may change in quantity without any other kind of thing (*viz* charge) being *necessarily* involved. In both cases interactions take place between states of the same kind so that no distinctions arise between the states in the variational procedure: it remains completely symmetric (*e.g.* a change in the state of body *I* for current-current interactions entails the generation of the same state in body *II* and *vice versa*). But a charge-current interaction must necessarily be asymmetric because what we want first of all is to obtain a motion of body *I* in state *j* (charge) as a result of the change in state *k* (current) of body *II*. Not only is this asymmetric (since a change in state *j* of body *I* cannot occur in and of itself it cannot effect the motion of body *II* in state *k*), but it attempts to mix the effects of time and coordinate derivatives, which are kept quite separate in Helmholtz's principle.

[12] Most recently and informatively by S. D'Agostino, "Hertz's Researches on Electromagnetic Waves", *Historical Studies in the Physical Sciences*, Vol. **6**, pp. 260—323. Although D'Agostino does not remark the peculiar characteristics of Helmholtzian interaction theory that inform Hertz's analysis and render it problematic, he does note that the account was limited to closed circuits. Hertz thought this to be so because he began from Helmholtz's theory, in which the vanishing of the vector potential's divergence (a necessary condition for Hertz) requires the vanishing of the current's divergence, and hence its closure *if* a certain constant in the interaction energy does not vanish. The vanishing of the constant was considered to be one of the requirements for obtaining "Maxwell's" theory from Helmholtz's, so that Hertz's analysis *could have* applied to unclosed circuits if he had adopted this 'Maxwell' limit — but he preferred not to do so in order to remain completely general.

[13] I have not the space here to discuss reactions, but much discussion followed the article's publication, discussion that for the first time in Germany mooted notions that, if understood and accepted, would have required profound changes in elementary physical conceptions. Understanding took another four years; acceptance may never have occurred.

[14] Hertz, "Über die Beziehungen zwischen den Maxwell'schen elektrodynamischen Grundgleichungen und den Grundgleichungen der generischen Elektrodynamik". *Ann Phys. Chem.*, **23**: (1884) 84—103 See his *Miscellaneous* Works. 3 volumes. Translated by D. E. Jones. London: Macmillan and Co., 1896, Vol. **I**, 274.

[15] *Ibid.*

[16] Hertz's argument is rather longer than this because he introduces a fictitious electric dipole spread over the apertures of the toroids to generate equivalent electric forces to those that are produced by the changing magnetizations. This nicely embodies the unimportance of the source since electric dipoles are not at all the same things as changing magnetization *quà* physical objects. It also represents physically the presence of an interaction term in the system energy as a product of electric dipole moments.

[17] This, he remarks in a note, is any electrodynamics that can be formulated in terms of Franz Neumann's potential U_d, according to which the electrodynamic forces that move bodies are given by spatial derivatives U_d and the electromotive forces by its time derivative. Hertz's claim is almost obvious *for HT* because the only forces that can move bodies derive from spatial derivatives.

[18] Although for Weber's theory it is hardly obvious that such a force does not exist since, unlike *HT*, it is not based directly on a potential, and, again unlike *HT*, it requires the Fechner hypothesis for the current. In fact Hertz was not clear about what *FW* does and does not assume. It does not satisfy even his principle *I*, though he thought that it did: there are situations, e.g., in which *FW* predicts that the effect of a closed circuit in which the current changes *differs* from the effect on the same body of an electric dipole layer that is spread over the region surrounded by the circuit in such a fashion as to produce the same *emf* as the changing current: the effect depends on what kinds of processes are going on in the affected body. This violates both of his principles. In view of this I think it more than likely that Hertz had never thought deeply about *FW*, that what he knew of it was mostly second-hand, gleaned from Helmholtz or else from perusing Weberean analyses (such as Jochmann's on induction in rotating spheres) that had useful results in them. Hertz probably did not perceive, at least not as yet, the deeper differences between *HT* and *FW*, which is perhaps not surprising given Helmholtz's scorn for the latter and his perplexed admiration for field theory.

[19] It can be granted an artificial meaning only if the action between objects depends solely upon their mutual distance. Then, and only then, the value of the force can be assigned an unambiguous meaning in relation to space.

[20] And, in the light of electron theory, the principle is incorrect. Electron theory requires that ring-magnets *do not* exert net ponderomotive forces on one another, for the simple reason that such a force requires the presence of a net quantity of charge, which neither ring-magnet has. Field energetics of the kind deployed by Maxwellians are not always applicable in electron theory, and here we have such an example. For other examples see my *From Maxwell to Microphysics.*

[21] For example, to deduce that the current-current energy entails a mechanical force we must include a term for the kinetic energy in the variation. Then, purely analytically, a 'force' emerges, expressed in terms of electromagnetic states, that must affect the coordinates of the object. The 'force' is not *in itself* an independent thing; rather, it reflects the exigencies of the energy principle.

[22] Johanna Hertz, *Heinrich Hertz. Memoirs. Letters. Diaries.* Translated by Lise Brinner, Mathilde Hertz, and Charles Susskind, San Francico: San Francisco Press, 1977, p. 288. Emphasis added.

[23] H. Hertz, *op. cit.*, p. 289.

[24] Johanna Hertz, *op. cit.*, p. 17.

[25] Hertz did not understand how Maxwellian theory was able to dispense altogether with sources, although he knew that it wished to do so. See Buchwald, *op. cit.*, chap. 22 for further discussion of Hertz's mature image of field theory.

[26] J. Z. Buchwald, "Oliver Heaviside, Maxwell's Apostle and Maxwellian Apostate", *Centaurus*, **28**: (1985) 288—330.

[27] Helmholtz, *Vorlesungen über Theoretische Physik* (Leipzig, 1903), p. 15. Emphasis added.

[28] H. Hertz, *Principles of Mechanics*, New York, 1956. See the Author's Preface.

[29] H. Hertz, *Principles of Mechanics*. New York, 1956. See the Author's Preface.