JED Z. BUCHWALD

REFLECTIONS ON HERTZ AND THE HERTZIAN DIPOLE

It's one of those rare times in physics when you discover a really new effect. It makes you feel kind of strange – you're seeing something that nobody else has ever seen before. (Physics graduate student Marc-Olivier Mewes's reaction on realizing that his group had succeeded in creating the first atom laser.)[1]

Heinrich Hertz has for some time attracted the attention of philosophers of science who are interested in the impact of his highly abstract *Principles of Mechanics.* Yet he has not until recently been much investigated by historians of physics, who, in considering electrodynamics, have for the most part concentrated on figures such as Kelvin, Maxwell, or Lorentz. There is a nice symmetry between the philosophers' interest and the historians' lack of it, because both interests exhibit a long-standing concern with figures who were deeply engaged in the production of new theories or who developed influential abstractions. Hertz himself never did produce a theoretical system comparable to Maxwell's or to Lorentz's, but he did generate an elaborate scheme for the foundations of mechanics that had a substantial impact on foundational thinking in late 19th and early 20th century philosophy.

One might ask who at the time would have taken the trouble to read the *Principles* if they had been written by an obscure German physicist with little previous work to his credit? It is of course dangerous to speculate about what might have been, but in the light of contemporary reaction to the *Principles* it seems probable that they were so widely discussed precisely because Heinrich Hertz, the discoverer of electric waves and heir-apparent to the doyen of German physics, Hermann von Helmholtz, was their author. "Anything written by Hertz" the Irish physicist George Francis FitzGerald remarked in the very first sentence of his 1896 review of Hertz's *Miscellaneous Papers*, "is of interest" (FitzGerald 1896, 6). It is hardly likely that FitzGerald's opinion on this point was unique. Yet why was this so? Why did Hertz's contemporaries consider his physics to be so interesting, if in fact he, unlike, e.g., his teacher Hermann von Helmholtz, had not produced major theoretical innovations?

One might after all argue that Heinrich Hertz was at best engaged in confirming the existence of something, namely electric waves, that had long been thought to exist, and that his theoretical work amounted to the presentation of Maxwell's field theory to a new audience with a few changes introduced primarily to avoid questions that Hertz did not deem significant (such as the qualities of the ether or the nature of charge). What is so wrong with this picture that the last half decade has seen the publication of several books on Hertz, including my own, as well as numerous articles? Is it merely that the higher ground that had been occupied for so long

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by people like Maxwell and Lorentz has been cleared, so that less influential figures, such as Hertz, are being turned to for lack of more interesting subjects? Or is it perhaps that contemporary historiographic fashion, which emphasizes the significance of less well-known figures for reconstituting the practice of an era, has at last brought Hertz to the center of historians' attention?

One cannot easily quarrel with the claim that Hertz did not produce a major system of his own, that many of his contemporaries outside of Germany (and even within it) did think that he had confirmed something which others had predicted, and that his own excursions into electrodynamic theory seem in retrospect to have been consolidations rather than innovations. Nevertheless, our interest in Hertz is not at all misplaced, nor are the assessments of his contemporaries surprising, once we recognize that the electromagnetic world of the early 1900s was produced by people who worked within an instrumental universe that Hertz himself had created in the laboratory and on paper in the years from 1887 through 1890.

To put Hertz into proper perspective, it is essential first to recognize that before his creation in 1887 of the dipole oscillator and resonator no one, including most British Maxwellians, had any clear idea of how artificially to produce freely-propagating electric waves. In Britain, optical radiation constituted the only known instance of these sorts of waves, and, therefore, they were generally associated with optical instrumentalities. Furthermore, until the mid-1880s at least some British Maxwellians, in particular FitzGerald, did not even think it possible to generate such waves at all by means of electromagnetic devices. FitzGerald eventually changed his mind about this, but other views militated against any Maxwellian conceiving of a suitable way to generate sufficient power for electric waves that detach themselves from the radiating object to be detectable.

We might with justification assert that before the mid-1880s no one, whether Maxwellian or otherwise, had any clear notion that electric waves in air could be manufactured by means of the sorts of devices that might be found or made in the typical laboratory of the day. In Berlin, where Hertz learned the technical practice of electromagnetics as Helmholtz's apprentice, the situation was in one major respect even more obscure than it was in Britain, since the depths of Maxwell's field theory remained unplumbed by nearly all German physicists, who otherwise differed greatly from one another. Helmholtz had himself produced a scheme that could yield waves in structures that were capable of electric polarization, but neither he nor anyone else in Germany considered whether or, better, how this might be done artificially. Instead, Helmholtz, like his British contemporaries, evidently considered optical radiation to be the paradigm for, and perhaps the only proper instance of, electric waves, except for processes that are confined to or on conducting media. Moreover, the hypotheses that (on Helmholtz's system) yielded electric radiation in non-conducting media raised questions that did not have altogether straightforward answers during Hertz's Berlin years. Indeed, Helmholtz tried to convince his young apprentice to devote himself to their experimental elucidation.

Hertz's route to the production of artificial electric waves was, not surprisingly, hardly straightforward. Indeed, even after he became convinced that he had

observed propagation he did not at first imagine that he had also produced waves in the fullest sense of the term. That is, he did not initially think that what he had produced constituted a particular instance of a well-known natural kind (namely optical waves), with all of the latter's inherent properties, albeit ones that could be accessed only through devices that were foreign to optical practice (such as wire grids, or huge, opaque prisms of pitch). He did soon come to this conclusion, but he then focused his technical discussion on the new form of radiation *per se* and not at all on the entities and processes that produced the radiation in the first place. It is here that Hertz's laboratory experience merged synergistically with his considerable skills in analysis to produce a novel system that did have a substantial influence at the time and not merely among physicists. For Hertz's analysis of dipole radiation presented the new, and intriguing, case of an elaborate mathematical theory for an effect which is produced by a laboratory object that itself eludes theory's grasp. On the one hand the dipole was a real entity, a construction of metal, that Hertz worked with in the laboratory in order to produce an appropriate effect. On the other hand, it was an abstract, paper object that did not appear at all in the equations that Hertz had built to analyze the effect.

1. THE ABSENT DIPOLE

Physical schemes often live in and through schematic images. Think for example of Newton's diagrams in the *Principia*, whose lines are drawn to exemplify the concepts of force and motion with which he worked to generate a new paper world. Or consider diagrams of Augustin Fresnel's wave surfaces for crystals, which to several generations in the 19th century embodied the essential properties of optical radiation. In both of these instances there is something missing from the image, something that must be absent in order for the image to convey an appropriate physics. In Newton's case, centers of force are not present as physical entities; they are in effect simply points. Fresnel's optical surfaces likewise have no physical origin. They emerge, like Newton's forces, from diagrammatic points. In both cases the diagram warns the viewer away from the unimportant, just as much as it attracts the viewer's attention to the important. Do not wonder about force centers, Newton's diagrams implicitly warn; they are simply the loci from which distances are measured in calculating forces. Do not ponder the origin of light, Fresnel's diagrams warn; think only about how light behaves after it is born in a mathematical point. What is enjoined can be made apparent by its absence from a canonical drawing, which may accordingly serve as an exemplification of a system. In both of these cases the analytical system gains considerable power from what it forbids or ignores.

In 1888 Hertz drew an influential series of diagrams to accompany his 1889 paper on dipole radiation, which was translated as "The Forces of Electric Oscillations, Treated According to Maxwell's Theory" (EW 137–159). Hertz took a great deal of care with these drawings. They have been reproduced innumerable times since their first appearance, often directly from Hertz's originals, though also from redone computations as well. The original drawings, which are reproduced

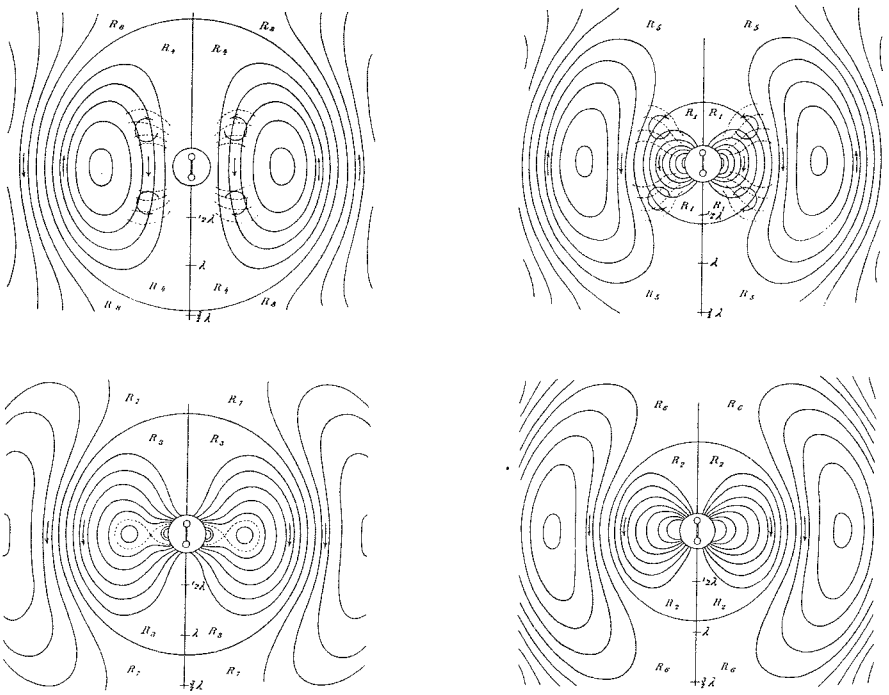

Figure 1. Hertz's 1989 diagrams of dipole radiation

here, contain three kinds of curved lines. There is the innermost dumbbell. It is immediately surrounded by a circle of variable size, on whose surface sometimes lie the termini of a sequence of nested curves. These are in turn surrounded by another, much larger circle, and past it exist nested sets of closed, distorted ovals. The terminated lines, as well as the ovals, represent Hertz's physical reality, the field. The large circle sets a boundary: it demarcates the outer, radiation-containing regions of space from the inner, non-radiation containing regions. These lines – ovals and larger circle – accordingly refer to processes that either have immediate physical reality for Hertz, or that delineate one type of process-containing region from another. They embody or differentiate the positive essence of the Hertzian radiation field. The innermost, small circle is different. Like its larger sibling, this circle also demarcates a region. But the smaller region does not contain a known physical entity. It contains instead a small drawing that represents what cannot profitably be investigated, namely the very device that produces the propagating field in the first place, Hertz's oscillating dipole.

Although Hertz's field lines are constructions that he infers from his theoretical system, whereas the oscillating dipole is a material object, nevertheless in Hertz's diagram the material object remains unknown, whereas the inferred field is known. This diagrammatic inversion encapsulates the originality and power of Hertz's physics. 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 about it – he was able to make progress where his British contemporaries had not been able to do so. They had concentrated closely on the shapes of radiating bodies, for to the British the canonical instance of electric radiation was what was later termed wave-guidance, in which radiation does not depart from the conducting boundary but, as it were, slips over the surface. For the British the geometry of the surface was critical in building a theory, and situations that eluded analysis of this sort (such as isolated conductors that yield up their energy to far-distant surroundings) were not thoroughly probed (at least in connection with radiative processes). Furthermore, British analysts already thought that an object like Hertz's dipole would reach electric equilibrium so rapidly that the radiation it emitted would simply flash away in an essentially undetectable burst. It is therefore not at all surprising that Maxwellian reaction to Hertz's experiments centered principally on his detecting resonator, and not on his (mathematically intractable) oscillator. Even today the oscillator remains an alien presence. One well-known text, Charles Papas' *Theory of Electromagnetic Wave Propagation* for example, notes that "the determination of the antenna current is a boundary-value problem of considerable complexity", and proceeds to develop the circumstances under which the problem can be bypassed.[2]

Hertz, who knew nothing about such things, did not think at all about the surface behavior of his oscillating dipole. Nor did he consider the effects that it produces to be beyond the reach of analysis or experiment. For him the paper analog of the material dipole was in itself a nuisance, and he immediately reduced it to a pictogram. The very object that enabled Hertz to investigate electric waves does not exist at all in the mathematical account that he himself developed for its field. The effects of this removal of the experimental object were far-reaching and can be followed through the literature of physics and electrical engineering during the next half-century at least. Hertz's missing dipole evolved (as objects) into the antennae of an emerging technological regime; and they evolved (as symbols) into the unknown entities that were responsible for natural radiation, in particular Max Planck's resonators.

In 1890 Hertz published two papers on the fundamental equations of electromagnetics (EW 195–240 and 241–268). They were widely read in Germany and elsewhere during the few years that remained to him. Many contemporary references indicate that these articles had a deep impact on German physicists, which is hardly surprising since Hertz here introduced many of his German contemporaries to the broad range of electromagnetic processes from the viewpoint of field theory. However, he had already presented the field equations in conjunction with their solutions for the dipole in 1889 (EW 137–159). Whereas the 1890 articles contained no diagrams of any kind, the 1889 piece contained several, including the sequence of field maps. In the immediate aftermath of Hertz's discovery, this article was frequently used as a basis for understanding Hertz's work and indeed for developing a pragmatic understanding of a new scientific object, the radiation field.

In this influential, eventually canonical, presentation Hertz abstracted completely from the dipole itself. Instead of considering it to be a physical object, he removed

it from his analysis and represented it by the product of a "quantity of electricity", E, and a "length", l. This product multiplies a fraction that contains a sinusoidal wave in the numerator ($\sin(mr\text{-}nt)$), and, in the denominator, the distance r. Hertz then shows that this function works as a solution to a special form of his "Maxwell equations".

For more than a half-century before Hertz the field of a permanent (static) dipole, magnetic or electric, had been calculated directly from an object consisting of two equal but oppositely-charged electric (or magnetic) point masses located a given distance apart. The resulting expressions contain a vector that represents the electric (or magnetic) moment of the object, defined as the product of the magnitude of the charge by the distance between the charges. Hertz's function does contain a similar product (El), and, in fact, that product reduces to the static dipole if, in his solution, the ratio n/m vanishes (which corresponds to a zero velocity of propagation for the waveform). One might therefore think that Hertz had merely replaced a static dipole (El) with a non-static one ($El\sin(mr\text{-}nt)$), otherwise retaining the form of the static solution. This would not in itself constitute a deduction of an appropriate solution from the physical characteristics of the oscillating dipole, but it would at least be a reasonable analogical move to make.

However, the product $El\sin(mr\text{-}nt)$ cannot represent a non-static dipole, because it represents a propagation. In fact, Hertz's product El has little physical significance for him because the "Maxwell equations" that he used in 1889 do not contain source terms at all: they apply only to free space. In order to give the dipole a clear analytical presence, Hertz would have had to introduce it as an oscillating current source into his equations, presumably in some form such as $El\cos(t)$. Had he done so, he would have been faced with a thorny mathematical situation that defies easy solution, and that, in later years, was dealt with in several, quite difficult ways (often through the explicit introduction of retarded solutions to the fundamental equations).

Hertz's own approach remained quite common through the mid-1890s, appearing for example in Paul Drude's *Physik des Aethers* (Drude 1894a). The second volume of Henri Poincaré's *Électricité et Optique* takes a slightly different tack, in that Poincaré does explicitly introduce an oscillating dipole as a source (Poincaré 1891a). Yet here, too, Poincaré proceeds rather by demonstrating the adequacy of an assumed solution to satisfy a set of equations than by the explicit construction of a solution (and Poincaré's presumed solutions, which have certain peculiarities that make their relation to the dipole less than transparent, did not pass subsequently into the literature, whereas Hertz's did).

Hertz himself pointed out only that his assumed solution "corresponds" (in the region that was later termed the near field) to an oscillating electrostatic dipole, and to an oscillating current as well. There is a powerful sense in which Hertz's critical distance from the dipole proper passed eventually into engineering practice, where attitudes among specialists in the design and analysis of antennae have usually been strikingly similar to Hertz's own views of the dipole. Consider for example the following passage by H. Bremmer of the Philips Research Laboratory:

Our first question now is, to what kind of idealized model a radio transmitter, in its most simplified form, does answer. This ideal transmitter may be represented by a line element L through which passes a

current Ie^{-it} ... the length L of the aerial being infinitely small and the amplitude of the current infinitely great, in such a way that the product IL (the 'moment') has a finite value. In practice such a transmitter already resembles a real one whose dimensions are small with respect to the wavelength. Such a source of electromagnetic waves was studied for the first time by Hertz, and it is therefore called the Hertzian dipole. An actual antenna, of finite length and carrying a current not necessarily uniform, may be regarded as a superposition of such dipoles.[3]

Bremmer in fact followed Hertz's own presentation quite closely, and he was not alone in doing so among antenna engineers, though by the 1940s physicists normally approached the problem through retarded fields.

In the 1890s, before antenna engineers had come into being, Hertz's dipole constituted a new kind of scientific object, one that was at once conspicuously absent from the analytical structure of the effect that it produces, and that was nevertheless physically present as an actual device in the laboratory. Among physicists the dipole never did become an object of intrinsic interest or significance because it did not, from their point of view, produce something altogether novel; it just generated, as it were, a kind of artificial light. Nevertheless for physicists the dipole did serve as a useful tool, as a canonical source for electromagnetic radiation, and it was often inserted without much discussion into radiation calculations during the 1890s and early 1900s. For the evolving coterie of radio engineers during these years, the dipole constituted the sole material method for manipulating the new (and entirely artificial) electromagnetic spectrum. As such it was essential as a technological object, but it remained a tool that was to be used for the effect that it produced, and not itself an object of analysis.

Only Heinrich Hertz was likely to have produced such a multivalent device, because only he among all his contemporaries had combined Helmholtz's approach to physics with superb laboratory acumen and analytical finesse, all mixed finely and potently with an intense desire for professional recognition. From Helmholtz Hertz learned to watch for novel interactions between objects in the laboratory without worrying overmuch about the hidden processes that account for the object's effect-producing power. His dipole and detecting resonator evolved out of attempts to investigate interactions of that sort. Neither device required or attracted analysis from Hertz, because he had learned from Helmholtz to probe rather the character of the interaction between the devices than their inherent, perhaps deeply hidden, structure. British Maxwellians worried intensely about what occurred at the surfaces of conductors set free to achieve electric equilibrium. Most German physicists, gripped by the ethos of exact measurement, would not have dealt so cavalierly as Hertz with the numbers his experiments produced. Other Germans, convinced that conductors were mere containers for hidden, active entities, would have been much more concerned than Hertz was to understand as rapidly as possible the processes that must take place within the dipole and resonator proper.

2. MECHANICS AND ELECTRODYNAMICS

Many of the articles in the present collection are not directly concerned with the universe of wires, induction coils, dischargers, capacitors and batteries that populated Hertz's laboratory. They discuss instead Hertz's *Mechanics*, including both its

technical structure and its extraordinarily influential conception of *Bild* – Hertz's belief that the connection between a scientific system and its natural referent has much the same character as that between signifier and signified. The universe of Hertz's *Mechanics* was an abstract world, far removed from the laboratory, and indeed one that he was investigating not in order to produce knowledge of new effects (which was ever his aim in experiment) but rather to achieve as great a consistency and clarity as possible among the signs and their connections in mechanics.

Nevertheless, an intriguing similarity links Hertz's *Mechanics* to his dipole. One might even say that Hertz's analytically-absent dipole functioned in respect to the physical reality of the electromagnetic field rather as the second-order *material particles* of his mechanics functioned in respect to his first-order *material points*, which are collections of material particles. The purpose of the material particles in the *Mechanics*, Lützen suggests, was to justify (on Euclidean grounds) the Riemannian metric that enabled Hertz to produce a satisfyingly coherent system. Similarly, the purpose of the dipole in Hertz's electrodynamics was to justify the solution that he had developed for his field equations.

The material particles of Hertz's *Mechanics* came in two varieties: those that populated the phenomenal world (or, at least, its analog in Hertz's *Bild*), and those that, though linked by rigid connections to their siblings, were not themselves directly accessible to experiment but were known only through their effects. Hertz's dipole did not, properly speaking, come in varieties, but it did have a dual character. Like the accessible material particles of the *Mechanics*, it was an object of experience. Yet it was also inimical to analysis – just as the concealed particles of the *Mechanics* also escape analysis. Of course, the dipole is a single entity that has a two-fold character and not (like the particles) a single type with two distinct sub-kinds. Nevertheless, the multiple valences between Hertz's dipole and his particles are sufficiently striking to suggest a degree of commonality between them.

3. FIELDS WITHOUT INTERACTIONS

That commonality may itself reproduce a pattern that characterized Hertz's work as his electrodynamic researches evolved beyond Helmholtz's physical conception of lab-oratory objects. Hertz absorbed from his mentor the notion that proper and effective physical theories are built on the basis of potential functions that represent the interaction at a given moment in time between two physical objects. Such a potential can be a function solely of the distance between the objects and the states that they are in at that specific moment. In order to determine how the objects behave, the potential function must be subjected to a virtual change. If the change involves solely the spatial coordinates of the interacting objects, then the variation will yield expressions that determine their accelerations, i.e. bodily forces. If the change involves only time, then the resulting expressions determine the (temporal) rates at which the object states themselves change (e.g., electromotive forces). In either case "force" becomes a shorthand way of referring to a function that emerges from a variational calculation that is performed on the potential, which alone, and altogether, embodies the interaction between the objects.

It is almost certainly the case that Helmholtz himself did not think of these functions as inherently fundamental, although it is difficult to be certain about this given the fact that he did not explicitly discuss the point. In the case of electrodynamics, which is the only case for which he worked out an appropriate function, Helmholtz probably thought the potential to derive from some sort of kinetic process that has its seat in the ether (particularly given that the electrodynamic potential itself behaves like a kinetic and not like a potential energy). Nevertheless, in practical terms – in the posing and the solution of problems – Helmholtz treated the potential as an unreduced entity, and he derived all of the acting forces directly and exclusively from it. Anything beyond that involved speculation, and might even lead to the dangerous territory inhabited by such things as Wilhelm Weber's electric atoms. To a student like Hertz, who sought to make his own the latest work in Berlin physics, Helmholtz's way of working – which is essentially what Michael Heidelberger has termed Helmholtz's experimental interactionism, with its reserved attitude towards ultimate causes – would have appeared to be not merely an efficacious method for making progress but a philosophy for doing science.

During the 1880s Hertz did do physics as Helmholtz prescribed, and he seems to have taken that prescription to be a fundamental one. If we consider in broad view Hertz's successive forays into electric circuitry, elasticity, evaporation, and cathode rays in the early 1880s we find a common pattern. Each of the first three forays works in model Helmholtzian fashion by constructing (in the laboratory or on paper) sets of objects in specific states, and proceeds by varying the object states or distances (either by calculation or by experimentation). In his work with circuitry in 1878 and 1879, Hertz acted essentially as a neophyte Helmholtzian, perturbing coupled circuit elements to acquire the information he was looking for (Misc 1–34 and 137–145). A year or so later Hertz was pursuing a thorny question involving an attempt on his part to connect a body's elastic properties to its "hardness". Here, too, Hertz worked in a thoroughly Helmholtzian manner by concentrating on a pair of interacting (in fact colliding) objects. In addition, though, Hertz was probing for the possible existence of an entirely novel state, a body's intrinsic "hardness", which none before him had conjectured. There was no attempt on his part to produce "hardness" out of more fundamental (to say nothing of hidden) processes. On the contrary, he assimilated it entirely to what was later termed the set of a body under deformation and built his account directly on this quintessentially phenomenological effect (Misc 163–183). In all of these areas Hertz was tilling Helmholtzian fields, since he had not gone beyond questions concerning the existence and properties of the states of a pair of interacting, and controllable, objects that existed as such on his laboratory workbench.

When Hertz began working intensely with the extremely rapid oscillations in wires that eventually led him to his experiments with electric waves in air, he initially conceived of wire-wire interactions on this same Helmholtzian pattern: wires in particular electrodynamic states simply interact directly with other wires in similar states. When he turned to the dipole oscillator proper, Hertz still did not think about it in an essentially novel way, not even when his experiments indicated that the interaction between it and other electrodynamic objects might be propagated in time. For Hertz in late 1887, the dipole still constituted a Helmholtzian object.

However, Hertz decided by the spring of 1888 that the dipole could not be treated simply as an object whose interaction with other entities is delayed in time. On the contrary, he was by then convinced that his experimental data required a radically different interpretation, one that admitted the active role of a third entity as a mediator. The interaction between laboratory object *A* and laboratory object *B* was not delayed at all because, properly speaking, it simply did not exist. Instead, each of *A* and *B* must be thought to interact directly only with a third object, the ether, whose state is entirely specified by the electromagnetic field, and which itself is both ubiquitous and unchangeable. Unlike the laboratory objects, the ether in Hertz's conception has no manipulable properties whatsoever, for its qualities remain invariant (though, of course, its state varies).

It is important to understand that this conception differed considerably from one which was advanced as a possibility by Helmholtz himself, and according to which the ether itself behaves like a laboratory object. In such a scheme the ether would modify the apparent interaction between laboratory objects *A* and *B* by working separately on each of them, while *A* and *B* would continue to interact directly and immediately with one another. Hertz was quite familiar with this possibility from Helmholtz's work, and he clearly did not like it, since in 1884 he had produced his version of Maxwell's equations without using the ether at all. 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 (Misc 273–290).

Field theory, as developed in Britain, differed fundamentally from Helmholtz's image of nature. For Helmholtz, the world was filled with interacting objects, among which was the ether itself. Although the Helmholtzian ether was ubiquitous, it was nevertheless in principle an object like any other, with its own states and properties. Among British field theorists, the image of nature was very nearly the reverse of this one, because, strictly speaking, there were no interacting objects at all for them. Instead, the ether's properties might vary from point to point as a result of the local presence of matter, and whatever effects the material objects evinced reflected these local ether states. As a Helmholtzian, Hertz thought of the ether as an object, but he was apparently uncomfortable with its hidden character and wished to avoid introducing it as an object like all others. He wanted, that is, to remain entirely with laboratory objects proper.

In seeking to understand how field theory might be possible, Hertz in 1884 developed a novel way to multiply interactions between laboratory objects proper, thereby yielding Maxwell's equations, but not field theory itself, because the objects (sources) remained critical conceptual elements in this early analysis of his. His route to Maxwell's equations at the time accordingly required an understanding that mixed field theory's refusal to grant sources (material objects) any active role whatsoever in electrodynamics (a belief that is partially reflected in Hertz's 1884 statement that all forms of electric force have the same qualities whatever their physical sources might be) with Helmholtz's interaction potential, which required sources to be directly active entities (Misc 274). Helmholtzian objects, that is, remained critical elements in Hertz's 1884 deduction, but they had there already been deprived of their character as proper sources through Hertz's principle that electro-

magnetic forces did not bear the imprint of the object that exerted them. Although Hertz put these vexing issues aside for several years, he had in fact already encountered an evocative situation in an altogether different physical regime that bears a striking resemblance to the conceptual solution he advanced in 1888 to the conundrum posed in 1884 by Hertz's unstable mixing of Maxwellian with Helmholtzian elements. That solution will, finally, bring us back to his oscillating dipole.

In the spring of 1882 Hertz had begun experiments on evaporation. Here the standard Helmholtzian pattern is again apparent, but with an interesting difference. Hertz's previous experimental work had concerned unmediated interactions – e.g., an inductor acting directly on another inductor, or two bodies interacting through collision. In his experiments on evaporation, Hertz was again examining an interaction between two objects in particular states, this time between a pair of evaporating surfaces which are surrounded by a common enclosure. Here, however, the interaction is mediated by the evaporate that exists between the two surfaces, with each surface interacting directly only with the neighboring evaporate, and the system as a whole consisting of three entities. But, of these three, Hertz worked only on the two evaporating surfaces, and not on the evaporate itself, which in his experiment acts solely as a mediator between the thermal states of the surfaces, which alone control the system. The two surfaces themselves do not directly interact with one another at all. Moreover, the behavior of each of the surfaces is specified in relation to the mediating substance rather than in respect to each other. At the same time, the properties of the mediator (though not its state) remain in the background (Misc 186–200).

The understanding of electromagnetic radiation that Hertz developed in the spring of 1888 solves the conundrum of 1884 by insisting on the continuing role of the source, but dropping altogether its relation to other sources. Its behavior is instead specified in respect to a mediating entity, namely the ether, whose state in the immediate neighborhood of the source is determined by the source's activity. The Hertzian ether functions in this respect precisely like his mediating evaporate of 1882, with electromagnetic sources replacing the evaporating surfaces. Here there could be no question of a direct connection between sources. Nevertheless, and quite unlike Maxwellian field theory, in Hertz's scheme the source continues to exist as an entity in and of itself, since it is responsible for activating the processes that take place in the field. Where the Maxwellian source in effect merely represents a locus where ether properties change rapidly, the Hertzian source is responsible for activating specific states in an entity (the ether) whose qualities – but not whose states – remain forever the same. On the other hand, the source was not of any more direct interest to Hertz than it was to Maxwellians, except as an emitter or a receiver, because physical activities of note were thought to occur only in the ether itself. Just as Hertz had not in 1882 provided a detailed theory for the behavior of his evaporating surfaces, so in and after 1888 he altogether avoided providing a theory for the dipole oscillator.

4. THE HERTZIAN OBJECT

Like the modern physics graduate student whose remarks were quoted at the beginning of this paper, Hertz was deeply affected by the experience of finding some-

thing that no one before him had probed. "It is really at this point", he wrote his parents in March 1888, "that the pleasure of research begins, when one is, so to speak, alone with nature and no longer worries about human opinions, views, and demands" (MLD 255). His training under Helmholtz had put a tremendous emphasis on the detection and probing of novel effects; with electric waves he had succeeded in a measure beyond what Helmholtz or he himself had ever envisioned. His success cannot be separated from the characteristics of his training, background and personality, which palpably influenced as well the specific character of his electromagnetic theory. Hertz's mature physics was certainly not Maxwellian, because it retained sources and refused to play with ether qualities. Neither was it Helmholtzian, because it was not based on interactions between phenomenal objects (whether instantaneous or even delayed), but rather on the fields each such object engendered in the (invariant) ether. Hertz's novel physics emerged out of his creative engagement with Helmholtzian tools, concepts and techniques in the light of his experience in the laboratory. His physics envisioned a world of phenomenal objects that determine local states in an otherwise inaccessible entity, the field-bearing ether. The Hertzian object retained much of the character of its Helmholtzian forebears, since it was not fruitfully to be reduced to hidden structures. But it was not bound in perpetual and immediate connection with other objects.

This character of Hertz's physics distinguishes it quite markedly from H.A. Lorentz's electrodynamics, which began to emerge in detailed form in 1892 (Lorentz 1937a). Lorentz of course worked with microphysical entities, whereas Hertz did not, and this constitutes an immediate and obvious difference between them. There is however another difference, one that runs deeper. Lorentz based his electrodynamics on the proposition that the interaction of entity A with entity B is delayed in time – that a change in the state of A at a specific moment occasions an interaction with B at a later time. This kind of physics deploys the fundamental image of interacting objects, albeit microphysical ones. Hertz's physics does not deploy anything like this image, for it embodied a method for building theories that permitted, indeed that impelled, distance both from the object itself and from its connections with other objects. Hertz's premature death in 1894 ended his own development of this sort of physics, but its impact on others was enduring, both for the specific methods he introduced in dealing with electromagnetic radiation, and for the example of how to build a physical theory for certain effects without analyzing in detail the object that produces them.

Dibner Institute for the History of Science and Technology, MIT, Cambridge, Massachusetts, USA

NOTES

1 *MIT Tech Talk* (January 29, 1997).
2 Charles Papas, *Theory of Electromagnetic Wave Propagation* (New York: Dover, 1988).
3 H. Bremmer, *Terrestrial Radio Waves: Theory of Propagation* (Amsterdam: Elsevier, 1949), p. 14.