A Brief History of Electric and Magnetic Science

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Speculation

By the end of the first decade of the twentieth century electromagnetism, based on propagated actions between fundamental particles, or electrons, exhibited the three cardinal characteristics of a widely-held physical scheme during this century: it was intensely mathematical; it had deep roots in the laboratory; and it was based on a physical hypothesis that connected closely to these other two desiderata. Until well into the eighteenth century the subjects of electricity and magnetism, which were usually kept firmly apart from one another, were scarcely mathematical at all, had only the flimsiest of connections to the laboratory, and were above all loci of elaborate speculations. By the third quarter of the nineteenth century electricity and magnetism had been thoroughly pursued in the laboratory; they had also been unified beneath a powerful mathematical umbrella, and physical hypotheses had been displaced to the margins of research. A few decades later they lay at the heart of ongoing research. We shall in what follows trace this curved trajectory.

Unlike optics, neither electricity nor magnetism had roots, as subjects, in mathematics. The effects associated with them had traditionally been treated among Aristotelians as parts of physics, requiring therefore the elaborate taxonomic dissections of their place in the lexical schemes of the schoolmen that were also applied to, for example, bewitchment or pain. By virtue of their placement here, rather than in the realm of mixed mathematics, these effects were not considered to be the sorts of things that exhibited the incorruptible permanence thought to be essential for the use of geometry. Although that kind of view had radically changed by the second half of the seventeenth century, there nevertheless remained vestiges of it that continued to separate electricity and magnetism from mathematics, and that made it difficult as well to unite them fruitfully to the novel conception that reliable knowledge can be generated by forcing nature to perform uncommon tasks — the philosophy, that is to say, of experiment.

During this century the very idea of what an experiment might be, much less that knowledge could be produced by such a thing, was only slowly and with difficulty developed and propagated. On the other hand the antique speculative tradition, which sought to plumb the world's essence, remained very much a desideratum despite the rapidly-growing rejection of Scholasticism. Seventeenth-century scholars did not usually engage in penetrating dissections of the proper nature of things, or at least these kinds of discussions were not so common as they had once been. Neither did they quantify or produce controlled laboratory effects. Instead, many scholars considered electricity to be an effect of the motions or the properties of a hidden substance.

Until the late 1600s electricity as a subject reduced to what has been aptly termed the "amber effect", in which light objects move towards rubbed amber. At century's beginning the Englishman William Gilbert broadened the class of objects that could produce the effect and at the same time introduced a fundamental distinction between it and the properties of the lodestone, or magnet, that resulted in a separation of the two subjects that prevailed until nearly the middle of the nineteenth century. In many respects still a Scholastic, Gilbert considered magnetic actions to be the effects of similarities or contrarieties between a specific magnetic nature or soul that some bodies may have. Gilbert's traditionalism in this respect was however balanced by two comparatively novel characteristics of his work: first, his great interest in producing working analogs, or actual models, of the magnetic earth using spherical lodestones — a form of investigative experimentalism that permeates his work and that does not sit altogether well with his concern for essential natures; second, his firm insistence that the electric effect does not involve such things, that it derives instead from the purely mechanical action of a sticky effluvium emitted by certain kinds of bodies when rubbed. In his words, "Electrical motions become strong from matter, but magnetick from form chiefly." Magnetic bodies come together or push apart in mutual sympathy or antipathy by their very natures; rubbed electrical bodies send out tentacles to rein in their passive neighbors.

Gilbert's Scholastic understanding of magnetism contrasts markedly with what seems to be a quasi-mechanical undersanding of electricity, the latter being more congenial to the post-Scholastic way of thinking about nature. However, a modern glancing back at Gilbert's work might vice versa be confused by his sophisticated experimental manipulation of terellae as opposed to the comparative poverty of his electrical work. This reflects two things: first, that there was as yet no firm union between physical discussion and experimental manipulation, no consensual understanding of how to generate knowledge about physics from experiment, and, second, the continuing belief that the business of the natural philosopher is to provide understanding of causes, however novel the causes may be in a particular case.

This did not change radically during the century, though the causes offered for electricity did mutate quite markedly, and though quite sophisticated observations were made, particularly during the 1620s by the Italian Niccolò Cabeo, who objected to Gilbert's effluvial gripping and offered an explanation based on motions of the air stimulated by the rushing effluvia. Cabeo's conception excited some experimental work that employed the novel air pump after mid-century, but by that time the immense, encompassing scheme of the French mathematician and philosopher, René Descartes, was rapidly bringing all such things within its purview, which if anything had the effect of entrenching the traditional attempt to provide understanding through speculation, with experiment adding at most a demonstration or illustration of things thought on other, prior grounds to hold true.

Every effect in the Cartesian scheme reflects a motion of the space-filling (indeed, space-defining) continuum. Space can be divided in several ways, producing as it were particles of various shapes and sizes. Screw-shaped magnetic particles may thread their way through appropriately-shaped bores in certain bodies, driving out air between the bored objects (and so forcing them together) if they are aligned with their threads twisted in opposite ways, or else forcing bodies with parallel twists together as the screwed particles rush from one into the next. Electric bodies had their own peculiarly-shaped channels which tended to confine active particles; these could be freed by rubbing to lodge in similar bodies and then return home.

Descartes's mechanical structure, which referred everything to motions or confinements of shaped particles, obviously differed considerably from Gilbert's gluey effluvia since for Descartes gluiness had itself to result from motion, confinement and shape. Nevertheless it shared with Gilbert's scheme, and indeed with the demonstrative knowledge of the Scholastics, the deep-seated goal of a kind of systematic understanding to which experiment is at best peripheral. In many ways this did not change substantially until well into the eighteenth century, despite the increasing spread of English experimental philosophy under the influence of Newton and his followers.

By the third quarter of the seventeenth-century in England it had become a question of infecting society with an entirely new form of knowledge, one whose unit was the persuasive "matter of fact". Cartesians did not construct "matters of fact" in this sense; neither did scholastics. Instead, they accommodated sufficiently persuasive items of experience, which are altogether different kinds of things from matters of fact because they do not require the sort of specialised witnessing, transmitted through a nicely-crafted rhetoric, that Boyle for one developed for experimental philosophy. Cartesian knowledge, one might say, remained fundamentally similar to scholastic knowledge: both had their seat in the a priori, respectively concerning essential nature or primary qualities. Both forms of erudition embraced descriptions of the empirical world, but neither form constructed itself out of such things. It is therefore not at all surprising that Cartesians did not generate a vibrant experimental program.

Experiment Intrudes

Kinds of Electric Objects

Neither Newton's mathematical natural philosophy nor his signal development of experiment in optics had much immediate influence on prevailing opinions concerning electricity and magnetism. For one, canonical experimental devices like the Newtonian prism had not as yet been produced for these subjects. Because argument could not orbit about such a device and its behavior the subjects remained without centers in the laboratory. For another, nothing like the Newtonian mass-point had been developed for either electricity nor magnetism, which left the subjects without the kind of clarifying foundation that Newton had so thoroughly exploited in the first and third books of his Principia. This at first left the subjects for the most part where they had been during the previous century — in the realm of explanation and demonstration experiments designed to illustrate speculation.

Demonstration experiments were however developed, in electricity, to a high art. At the London Royal Society Francis Hauksbee, under Newton's chairmanship, produced the first of a long line of eighteenth-century electrical machines — in his case a spinning, evacuated glass globe which was excited by holding a hand to it. Limp threads hung inside the globe pointed stiffly inwards on excitation (when the globe also glowed), and this Hauksbee took, contra Descartes, to show literally the presence of taut threads of electric matter penetrating inwards, a view challenged a few years later by Stephen Gray, whose work Hauksbee used as he saw fit.

Gray spent some time trying to produce electrification in the usual ways in metals, long thought to be impossible. Two decades later (1729) he discovered that he could do so if the metals were brought into the vicinity of an already-excited glass object. Pursuing this line of investigation, he found that he could communicate the electric effect to long distances as long as the communicating wires were themselves suspended by something (silk) that did not work as a good communicator. Gray, initially an outsider to the burgeoning Newtonian community, thereby produced what some historians regard as the first central experimental development in electricity, one that could only with difficulty be fit into prevailing effluvial conceptions. Perhaps more important than this ill-fit between effluvia and travelling virtue, Gray had produced the first device (his communicating wires) that could be used to fabricate new knowledge. Together with the descendants of Hauksbee's spinning globe, Gray's wires might be said to have for the first time constituted the electric laboratory. This made it possible for the subject to be dealt with by the increasingly numerous proponents of experimental knowledge in essentially the same manner that, for example, they dealt with the air-pump or Newton's prisms: as a subject that must not be constructed on the basis of demonstrative, a priori knowledge; as something that, instead, had to be thoroughly based on the behavior of devices fabricated in the laboratory.

Gray's wires and the burgeoning production of electric machines were for the most part used either to entertain in variants of old demonstration `experiments' or else to construct processes similar to ones that had been generated for the past century. However, in the early 1730s Charles François Dufay, then Intendant of the Jardin du Roi, became aware of Gray's work. Taking off from it, Dufay produced in his laboratory two classes of objects in respect to electricity: those that can be electrified by friction and those that cannot be. In respect to electrification by contact, he discovered the eponymously-named "Rule of Dufay", according to which nearly anything could show electric effects by touching it to an already-excited body, that metals are strongest in this respect, but that the object had to rest on a third body that was an electric per se of sufficient thickness in order to be excited.

Dufay's two classes — electrics per se and non-electrics, corresponding to bodies that could or could not, respectively, be electrified by rubbing — together with his rule produced that essential characteristic for all laboratory-based science: an instrumentally-founded classification of objects in respect to the subject under investigation. Further experimental work, as well as conceptual developments, could be molded about this framework. Arguments could be developed that relied upon these distinctions and that connected strongly to devices whose behavior could now, in some respects at least, be treated as comparatively unproblematic.

In the early 1730s Jean Antoine Nollet became Dufay's assistant. In 1746 he produced a grand electric synthesis in his Essai sur l'électricité des corps that, while strongly connected to Dufay's classifications and rules, nevertheless exhibited that same spirit for speculative systematization which had been for so long the rule and model in natural philosophy. Nollet envisioned a world filled with electric stuff. Like all things Cartesian, Nollet's electric matter defined a space and acted primarily by displacing other matter, electric or otherwise, from its path. Present in all bodies, this electric fabric can be set into motion by rubbing (of electrics) or by contact (with non-electrics). Once stimulated, this catholic material flows out from the excited object; but since there can never be any voids in nature, at the same time other electric matter must flow back into the object, thereby keeping space filled. Nollet, basing his intuition on impressive German productions of brush discharge, conceived that the outgoing, or effluent flows emerge from comparatively few points on the body's surface, each fanning out therefrom like a fast-moving jet of liquid. The incoming, or affluent, streams move in much more slowly because they penetrate over the broad reaches of the body's surface between the points of effluence.

Nollet's scheme, much more closely tied to reproducible effects than most others before it, concentrated particularly on the motions of small objects near electrically-excited bodies. These motions could be nicely mapped in his system, as could Dufay's influential discovery that small bodies move away from electrified ones, after first moving to them, on contact. However, like every other system before it whose purpose was primarily to produce understanding, Nollet's was only peripherally related to the fabrication of novel facts in the laboratory. It could explain everything then known; it could not, or at least certainly did not, impel further experimental work, though it was capable of purely qualitatively accommodating nearly anything that involved electrically-stimulated motions. The spirit of system was still an overwhelmingly powerful presence among natural philosophers; prestige and material rewards continued to accrue to the successful systematizer, and Nollet became famous.

The Leyden Jar Renovates the Electric Laboratory

Gray's novelties, Dufay's rules and classifications, and the proliferation of increasingly large and elaborate electric rubbing machines had certainly produced a regime under which electric experimenters operated in a commonly-agreed manner. Bodies electric and non-electric, action followed by contact and repulsion, communication of effect by non-electrics, these were used as the basis for further laboratory claims without usually generating controversy. The rapid and wide acceptance of Nollet's system, which strikingly embodied these rules and devices, testifies to their comparatively unproblematic status by the 1740s. But an explanatory scheme bound so closely to a specific set of rules and devices as to amount nearly to an intellectual embodiment of them not only has difficulty birthing new and unrelated processes, it has as well difficulty incorporating them. The discovery of an electric object — the Leyden jar — whose behavior had little to do with previous devices, and nothing to do with the kinds of things that captured Nollet, posed difficulties for him.

Discovered by Ewald von Kleist in 1745 (according to one historian as a result of his search for "a portable sparking machine" [Heilbron, pg. 310]), whose reports did not enable its reproduction, the device was independently fabricated by Andreas Cunaeus, who informed the Leyden professor of natural philosophy, Musschenbroek, of it. The latter generated a recipe for fabricating the device, which enabled its reproduction throughout the laboratories of Europe. Nollet, for one, generated the effect with little difficulty.

The Leyden jar itself went through many variations, but in its early form consisted of a bottle or globe of glass partly filled with water; a metal wire in contact with an excited electric dipped into the water. To excite the device required holding the globe in one hand only. Subsequently to activate it required touching the wire with one hand while continuing to hold it in the other, producing thereby an overwhelmingly powerful shock, one vastly larger than any frictional machine had by itself ever produced.

The central difficulty that the jar posed for Nollet's system derived from its method of excitation. Recall the Rule of Dufay, according to which one excited objects by placing them on electrics per se — which is precisely what one does not do with the Leyden jar. Because Nollet's system was so closely integrated with Dufay's classifications and requirements, the Leyden jar posed immediate and powerful difficulties for it. Nothing in his scheme could have led Nollet to anticipate the powerful bottle; nothing in it enabled him easily to accommodate that power. Nollet was able to deal with the jar only by making special allowance for it. This again illustrates the signal characteristic of his system, namely that it was intended to provide comprehensive understanding rather than knowledge aimed at the laboratory.

In 1747 an obscure American from Philadelphia named Benjamin Franklin produced a new approach to electricity that, unlike Nollet's — just then becoming influential — kept far from system and that was powerfully bound to the laboratory, in particular to the Leyden jar. The foundation of Franklin's scheme was its entirely novel conception of how electrified objects interact with one another. In Nollet's system, as indeed in essentially all effluvialist schemes, the electric matter is ubiquitous, lying in bodies and flowing through the space between them. When a body becomes electrified it shoots effluvia out, and takes them in, setting up a perpetual commotion. Although the effluvial matter — electricity — never vanishes from the universe, nor is it now being created, nevertheless the amount of electricity that a body possesses has no bearing on effluvialist accounts, which either require the amount to be constant or else make no direct use of quantity. These systems might therefore be said to conserve electric matter, but only in a sense that has essentially no experimental consequences.

Franklin took his stand on what might be called the laboratory conservation of charge. According to him whenever one object loses a quantity of electric matter some other object must gain an equal quantity. That single principle of conservation, creatively applied, soon produced an avalanche of novel work, primarily because it integrates closely to the kinds of experimental manipulation that characterised contemporary electric work after the invention of the Leyden jar. According to Franklin, glass as a body remains perpetually saturated with electricity. If electricity is thrown onto one side of the jar, then a precisely equal amount must flow out the other side to maintain saturation. Glass however resists this shift, which reveals itself as the tremendous shock that occurs when the inner and outer surfaces are brought mediately into contact with one another. Franklinists could create and explain a vast range of experiments that depend upon some object giving electricity to another object. An example drawn from the Franklinist presentation in the first edition of the Encyclopaedia Britannica conveys the power:

Place a man on a cake of wax, and present him the wire of the electrified phial to touch, you standing on the floor and holding it in your hand. As often as he touches it, he will be electrified plus; and any one standing on the floor may draw a spark from him. The fire, in this experiment, passes out of the wire into him; and, at the same time, out of your hand into the outside of the bottle. Give him the electrical phial to hold, and touch the wire; as often as you touch it, he will be electrified minus, and may draw a spark from any one standing on the floor. The fire in this case passes from the wire to you, and from him into the outside of the bottle.

Systematists like Nollet had nothing at all comparable to deploy in the laboratory since their explanations were almost always singular and after the fact. Franklin's plus-and-minus, grounded in laboratory charge conservation, made possible the transformation of electricity into a quantitative, experimental science.

Quantity, Intensity and Newtonian Calculations

In the early 1750s Nollet responded forcefully to Franklin's claims; Franklinists had difficulties in answering all of his critiques. On the whole, those who continued to think that the Leyden jar should be treated as an addition to the standing body of electric effects remained with Nollet; those who became convinced that electric science should revolve about the jar became Franklinist. The spread of Franklinism in the face of powerful resistance was in no small measure due to its association with a novel and compelling technology, the lightning rod, which, in conjunction with the theory's intense concentration on jar processes, seemed to endow it with the kind of manipulative capacity that Nollet's system lacked. The power of pointed rods, strongly advocated by Franklinists, became a subject of political argument during the 1770s, but by that time effluvialism had waned markedly among natural philosophers, and the laboratory focus that underpinned Franklinism had become a widespread desideratum.

Franklinism had never been without its problems, most of which stemmed from Franklin's embrace of the traditional notion that electric matter must in many respects behave very much like ordinary matter: it must, among other things, fill space. Franklin had recourse to these subsidiary aspects when faced with demands coherently to accommodate such effects as electric repulsion, which is to say the sorts of things that the effluvialists had built their own systems about. Here there was still no question of producing novel experiments but of embracing effects that the opposing system could handle. To do so Franklin conceived that electricity thrown onto a body formed about it an extended, mechanically-capable atmosphere, and that the atmospheres of two electrified bodies do not mingle but rather push one another apart. Of course, Franklin was well aware that bodies electrified negatively also repel one another, and he simply refused to provide an explanation here. His scheme was built upon the Leyden jar as the canonical laboratory device; it could not easily deal with bodily motions engendered by electrification, nor could it deal with induction phenomena since it tended to assimilate the latter to the same cause that accounted for the former, namely the mechanical behavior of electric atmospheres.

Franklin was no more of a mathematician than his adversary Nollet. However Franklinist doctrine, with its grounding in laboratory charge conservation, was much more amenable to quantification thad the explanatory effluvialism of Nollet, except for those parts of it — its deployment of mechanical properties — that remained traditional. Mathematics was brought to bear when, in the late 1750s, a comparative outsider, Franz Aepinus, removed the atmospheres and provided an avenue for further laboratory investigation and mathematization. While thinking about a puzzling experiment brought to his attention by Johan Wilcke, Aepinus realized that it could be understood if an air gap could act like the glass in a Leyden jar. Experiments undertaken with Wilcke confirmed this hypothesis, which rapidly led Aepinus to abandon electric atmospheres since the primary Franklinist locus for electric matter, namely glass, could now be replaced by something as insubstantial as air.

In 1759 Aepinus's Tentamen was published, in which electric science became quantitative. Aepinus treated electricity as a Newtonian fluid, as, that is, a fluid whose parts are self-repulsive according to some force that acts directly between them and that depends upon the distance. In addition, he argued that there is an electric repulsion between ordinary material particles, and an attraction between them and the electric fluid. Without specifying the form of the force law, Aepinus was able through judiciously-chosen assumptions to obtain quantitative results for a considerable range of experiments. Aepinus produced in addition (indeed, this was his primary focus in the Tentamen) a novel and influential account of magnetism, in which he insisted on the separate existence of a Newtonian magnetic fluid, which differs from electricity in that it moves with great difficulty through bodies like iron, which therefore tend to hold magnetic charge in position.

Aepinus did not know the law of force between electric or magnetic particles, but even if he had he would not likely have progressed much farther. He generated calculations that could be linked to restricted sets of experiments, but they depended upon highly limiting assumptions concerning the disposition of the fluids. Moreover, he did not, indeed could not, progress very far in connecting laboratory measurements to his calculations. To do so required, at the least, knowing the force law, but even that would not in itself be enough. New sets of techniques had to be developed to fit the new electric (and magnetic) science that Aepinus had forged. In particular, the goal of experimental electric science had to be clarified; the landscape of the laboratory had to be redrawn.

For effluvialists the laboratory was a place for making things move about under electric influence; for Franklinists it was a place for revealing the transfer of electricity from one body to another. Aepinus's reduction of electricity to a Newtonian fluid made both of these goals subsidiary ones. Instead, the problem that his work — at first only implicitly — placed at the center of electric science was this: to calculate and to measure the distribution of electricity over the surfaces of conducting bodies — the old non-electrics now having become objects that simply did not impede the fluid's motion. Aepinus could not solve this problem, however, and he concentrated instead on loose computations of forces given very simple, assumed distributions. However, with the central question of the subject now shifted from the nature and behavior of electric stuff to the form of the Newtonian force that governed it, instruments were developed over the next quarter-century to probe that question.

Aepinus's views became influential in part because of a successful application of them to a device — the electrophore — that was invented by the Italian Alessandro Volta in the late 1770s. This instrument, a dielectric covered by tin foil and rubbed against a grounded plate, seemed to be able to electrify alternately without requiring re-excitation. Considered to pose a great puzzle to Franklinist science, the electrophore was explained by Volta himself after he had assimilated the hitherto-neglected work of Aepinus as an instance of induction: a process in which charged bodies influence one another through electric force without actually exchanging any electric matter.

In the late 1760s the Scottish natural philosopher John Robison produced (according to his later account) a device to measure this force; his device balanced it against gravity. In the mid-1780s the French Academician, Charles Augustin Coulomb, built an instrument based on balancing electric force against the torsion in a twisted wire. With his torsion-balance electrometer Coulomb obtained results that convinced him and his contemporaries that the force law followed the inverse square, precisely like gravity.

Coulomb was also able to argue that the electric force immediately outside a charged conducting surface must be proportional to the charge density there. This enabled him to give meaning to experiments in which he used his electrometer to measure the force over the surfaces of two charged spheres placed near one another: these numbers were now the canonical goal of all electric science, for they represented the electric distribution. But Coulomb could go no further; he did not know how to compute the distribution from the geometry of the experiment and from the force law.

Despite knowledge of the force law and the corresponding relegation of the precise nature of electricity to comparatively unimportant status, the undeveloped mathematical state of the subject revealed itself in the difficulty of untangling two different aspects of electric matter: on the one hand, the quantity of it on a given conductor, and, on the other, its power to produce electric effects, its intensity. It had been recognized since the 1740s that conductors of different surface areas had different `capacities' for electricity, in the sense that conductors electrified by the same power could acquire different amounts of electricity.

This distinction between quantity of electricity and electric tension became sharper, and was quantified, by Volta around 1780 after he had thoroughly assimilated Aepinus's views and had therefore come to think of electricity as working almost entirely through influence or force. Volta concentrated on the puzzling connection between electric power, or tension, and electric quantity. Grounding his work in the laboratory, he hypothesized that quantity and tension were proportional to one another, with the constant of proportionality representing the capacity of the conductor. Volta's relation was soon turned, particularly in England, to the production of new electric devices, ones that charged by influence.

The connection between tension and the electric force proper remained obscure for quite some time, despite the fact that a reclusive English scientist, Henry Cavendish, had gone quite far in clarifying it in the early 1770s. Long before Coulomb, Cavendish, aware to some extent of Aepinus's concepts, had produced an experiment designed to show that the electric force must obey the inverse square. Cavendish's now-famed null experiment used the property of such a force that its value inside a region surrounded by a spherically-symmetric distribution must vanish. Beyond that, Cavendish attempted for the first time actually to compute electric distributions under certain circumstances, for which he developed a way of simulating the operation of tension: conceive of an infinitely long, infinitely thin canal, filled with electric matter, and connected between two charged conductors. These last will not have the same amount of electricity, Cavendish argued, but they will be electrified to the same degree, in the sense that each of them would, when alone connected through a canal to a standard test conductor, transfer the same amount of electricity to it. In this way Cavendish was actually able to calculate relative capacities for pairs of disks or spheres, and also to develop an appropriate instrument for measuring them to high accuracy.

In one way electric science ceased to develop after the 1790s — it no longer produced what were thought to be intriguing laboratory novelties. In another way, namely in its technical structure, it changed markedly during the first quarter of the nineteenth century, at first in the hands of the French mathematician and physicist Siméon Denis Poisson. Relying on the mathematics of spherical harmonics developed by his colleague Adrien-Marie Legendre, and upon the concept of a potential function introduced by his mentor Laplace, Poisson was able to calculate the electric distribution over neighboring charged spheres that Coulomb had measured a quarter-century before. To do so Poisson relied on Coulomb's relation between force and charge density, as well as upon the condition that the potential function, whose gradient yields force, must be constant within and on a conductor. His analysis was however troubled by difficulties that derived from his continuing insistence that electricity distributes itself in a layer with finite, and varying, thickness near the surface of conductors. Only in 1828 did the English mathematician George Green completely remove physical considerations from the subject, reducing it in effect to a formal exercise in finding appropriate solutions to the Laplace equation in given circumstances. With Green's work the old electric science ceased to be an object of direct interest to physicists, at least insofar as interactions between conducting bodies were concerned. By that time, however, an entirely new sub-discipline had developed, one that derived from the work of Volta at the turn of the century and from a discovery made by the Danish natural philosopher Hans-Christian Oersted in 1820.

The Voltaic Pile

In 1780 the Bolognese anatomist Luigi Galvani discovered that the legs of dissected frogs twitched when the crural nerve was touched and at the same time a spark was drawn from a nearby electric machine. Pursuing the effect, he found that it could be produced by the mere contact of a metal passing through the nerve with a different kind of metal. He felt that he had uncovered a peculiar, vital form of electricity, which brought him into conflict with Alessandro Volta. In the early 1790s Volta argued that the bimetallic contact was alone the primary factor in the phenomenon, that it disturbed otherwise quiescent electric fluid whose motion stimulated the organs. The subject lay more or less fallow for a decade, primarily because no further effects seemed to follow from it. However in 1800 Volta fabricated a device, eponymously named the Voltaic pile, that vastly increased the power of the Galvani effect, and that provided the electric laboratory with the first new instrument of control since the production of the Leyden jar more than half a century before.

Volta's pile consisted of bimetallic pairs separated from one another by moist cardboard, the former constituting in his view the essential physical component. According to Volta, each bimetallic sandwich tends perpetually to set the electric fluid into motion. The wet cardboard permits this displaced fluid to flow through to the next sandwich, where its motion is reinforced. That is, the purpose of the cardboard is solely to realize an already-existing tendency to motion; it is not in itself electrically active. If many sandwiches, all separated by cardboard, are put together, forming a pile, then the electric motion can be greatly magnified, producing a very large effect. Moreover, Volta was also able to detect electroscopic forces from a pair of zinc-copper disks, thereby binding the pile to standard electric devices.

Although the Voltaic pile was the first entirely novel device since the Leyden jar to appear in the electric laboratory, unlike the jar it did not directly affect contemporary understanding nor was it used practically to generate interesting electric experiments. It was soon discovered by Nicholson and Carlisle in England that a strong pile can produce chemical dissolution, which was taken up by Humphry Davy at the Royal Institution in London. Davy modified Volta's views to produce an electrochemical theory of the pile, according to which chemical processes at the boundary between plate and liquid are responsible for permitting the electrostatic separation produced by the bimetallic contact to generate an actual electric motion. The new discipline of electrochemistry was molded about the pile, and that was where Volta's discovery had its major effect until 1820.

Electricity, as we have seen, was by this time a comparatively quiescent area, and for many years the pile had no substantial impact upon it. Indeed, to French physicists (in particular, Jean-Baptiste Biot) the pile seemed to be merely a powerful, intermittent Leyden jar: the bimetallic sandwiches charged up like the opposite coatings of a jar, and when the action became sufficiently large at each sandwich they discharged, as though the coatings had been connected. The sandwiches then recharge, and the process repeats. Consequently the French conceived of the pile as a particular kind of electric charging device, not as something with entirely novel properties. From this point of view there was no reason at all to suspect a connection between the pile and magnetism, precisely because electric and magnetic effects had long been stringently separated from one another.

Magnetism and the Pile

The powerful quantification of electricity that had been effected in France was intimately linked to a rigid demarcation between that subject and magnetism: electric and magnetic particles had no affects upon one another, and they had distinctly different relations with matter. Electric particles could move comparatively freely through conductors; magnetic particles somehow could not, and magnetism had not achieved the high mathematical sophistication of electricity, despite the fact that Coulomb himself, armed with long, thin magnetic bars, had demonstrated that the magnetic particles act upon one another with an inverse-square law. Magnetism had long posed the difficult experimental and conceptual problem that it seemed impossible to obtain it in only one kind — every body, if it showed magnetic action at all, showed both kinds. Coulomb solved the experimental problem of separating the actions of the two kinds, thereby distinguishing the particle-particle effect from a combined or dipole effect. But the mathematical and physical problem of understanding magnetic bodies was not solved until Poisson in 1824 developed the concept of the magnetic moment and showed how to build a theory upon it. Poisson's theory certainly reinforced the boundaries between electricity and magnetism, which he was concerned to maintain precisely because four years earlier a stunning effect had been discovered that, in France, was used to destabilize established views.

Many German and Scandinavian natural philosophers held considerably different views about nearly every aspect of physics from their French contemporaries. In particular, some among them held that the `forces' of nature, to be understood as active principles rather than (simply) as producers of spatial motions, were somehow connected and could indeed be converted the ones into the others. The Danish scientists, Hans Christian Oersted, had since 1807 been searching for a connection on this basis between electricity and magnetism, two natural forces. In 1820 he found it when he managed to deflect a magnetic needle placed near a metal wire by connecting the wire to the Voltaic pile. Oersted described the force as a circular action since the needle aligned itself along tangents to circles normal to the wire. The discovery was announced to the Paris Academy on September 11 by François Arago, and the effect was rapidly scrutinized in the laboratory by Biot and Félix Savart. They were able to produce a rule for the action which, in characteristic French style, they reduced to an action between parts: between elements of the pile-driven wire and an isolated magnetic pole.

Biot however did not believe that the action revealed something fundamentally new, and this view was shared by many of his colleagues in Paris. Rather, he conceived that the electric shocks sustained by the wire when connected to the pile must somehow dislodge otherwise-quiescent magnetic particles within it, resulting for unknown reasons in a distribution that might have the requisite effect. In the absence of a mathematics for magnetism this seemed plausible, though in fact no distribution of magnetic particles within or over the wire proper could possibly produce the appropriate circular force.

By this date French physicists, though they continued to share a distinctive quantitative approach grounded in the laboratory, were no longer of one mind on all subjects, and powerful conflicts had arisen that were in part due to career interests and that revolved at first about optics and the theory of heat. Fresnel, urged on by Arago (see the article on Optics), had developed the wave theory of light, which had brought him into conflict with Biot. Among Fresnel's supporters was André-Marie Ampère, who, on learning of Oersted's discovery, showed within a week that it was not confined to magnetism, but that a pair of wires connected to Voltaic piles exerted forces on one another. Ampère grounded his physical understanding of the effect in Fresnel's ether, conceiving that the pile sets up decompositions of electric fluids in the ether, fluids that are otherwise joined together there in a composite neutrality. However, Ampère's physical views remained for the most part unknown; his mathematics for electric currents, published in 1825, did not.

The Ampère Force Law

Ampère constructed his mathematics on four assumptions, one of which was unprecedented in previous physics. First, he assumed that current-bearing wires could be broken into circuit elements for the purpose of computing the force between them; second, that the force between a pair of elements lies along the line joining their centers; and third, that the force is a decreasing function of that distance. None of these three assumptions would have produced much difficulty at the time, except to assert that the notion of a circuit element was insufficiently fundamental, because electricity and magnetism were after all based mathematically on central forces between differential elements. But Ampère's fourth assumption raised problems, for he allowed the force to depend upon the orientations of the elements and not merely upon their magnitudes.

Put in the most general terms, Ampère required the force between a pair of circuit elements $d\mathbf{s}_1$, $d\mathbf{s}_2$ to be a bilinear function of the elements. This meant that Ampère could separately consider the nine possible additive terms that might appear in the force law, one corresponding to each product $(d\mathbf{s}_1)_i(d\mathbf{s}_1)_j$. The bulk of Ampère's analysis then consisted in bringing forward symmetry considerations and null experiments — experiments that balanced actions against one another — which eliminated some of these terms, and which also enabled him to determine the two constants in the force law. In this way he eventually obtained the following formula (which of course he gave in terms of the angles between the elements):

Ampère's Force Law

$$d^2\mathbf{F} = i_1 i_2 \mathbf{r}\left\{\left(\frac{2}{r^3}\right)d\mathbf{s}_1\bullet d\mathbf{s}_2 - \left(\frac{3}{r^5}\right)\left(d\mathbf{s}_1\bullet\mathbf{r}\right)\left(d\mathbf{s}_2\bullet\mathbf{r}\right)\right\}$$

where the i measure the respective currents strengths in the circuits, and $\mathbf{r}$ is the distance between the centers of the elements. In subsequent years this law was written in a different way, also developed by Ampère, that facilitated calculation using differential methods:

Standard Form of the Ampère Law

$$d^2\mathbf{F} = i_1 i_2 \frac{\mathbf{r}}{r^3}\left\{\frac{\partial r}{\partial s_1}\frac{\partial r}{\partial s_2} - 2r\frac{\partial^2 r}{\partial s_1 \partial s_2}\right\}$$

Ampère was also thoroughly convinced that magnetic behavior must involve decompositions in the ether, and he hypothesized that magnets contain permanent currents which, he eventually decided after a critique by his friend Fresnel, are molecular in size. This required him to demonstrate that such things might interact with one another like small magnets, which he was able to accomplish by integrating his force law about closed curves. Note that under these circumstances the second term in the law vanishes.

The extraordinary novelty of Ampère's construction, of a force law that, unlike anything before, required consideration of direction, constituted a major block to its dissemination for quite some time. In addition, and associated with this unwelcome novelty, the law did not seem to be compatible with the traditional underlying imagery of forces between particles, a tradition whose mathematics had by this time been highly developed. Ampère's law did not have the advantage that Fresnel had constructed for his equally-novel mathematics for the wave theory of light, namely of a physical model to which the analysis could be attached with some degree of conviction. However weak Fresnel's model for the ether was acknowledged by him and other proponents of the wave theory to be, it nevertheless provided a convincing basis for argument. Ampère's law lacked even that much. It stimulated little subsequent research in France, and scarcely more elsewhere in Europe, for nearly twenty years, when a German physicist provided a physical basis for it.

The Emergence of Electrodynamics

Ohm's Law

Although Ampère's force law itself may not have been immediately and widely influential, nevertheless Ampère's novel and, in contemporary context, unsettling conception of what went on in the wire did have a rapid and widespread impact, although not an entirely straightforward one. Ampère challenged and rejected the standard electrostatic conception of the pile. According to him the current involved a continuous process of dissolution and reunion, not at all a sequence of static discharges. As a result, the conceptual and instrumental apparatus that were appropriate to electricity in a state of rest could not be used at all for electricity in motion. The proponents of the static conception of the pile had however long insisted on precisely the opposite position; for them the pile was merely a special case of ordinary electric processes.

Ampère had, in effect, produced a new category in nature, the current, where nothing new had previously been seen. This did have a great deal of contemporary influence, particularly as it was (in part) taken up by Becquerel and de la Rive, though vestiges of the static conception remained for quite some time, mixing uneasily with Ampère's requirement that something entirely different was involved. The galvanometer became a device for measuring something that had nothing at all to do with electricity in the absence of a current, and a strongly-enforced demarcation was eventually drawn between the two domains. Electroscopic devices measure electric forces between objects that once endured currents but that no longer do so; galvanometers measure forces between objects carrying currents. The latter forces had nothing to do with the former.

When the German physicist Gustav Simon Ohm asserted a firm relation between static and current effects in 1827 his claim was not taken up, either in Germany or elsewhere. The lack of German interest reflects rather the continuing influence there of Naturphilosophie, which did not consider the kind of experimental-mathematical style that Ohm adopted congenial to true understanding. Elsewhere, however, Ohm's work was probably not rapidly taken up precisely because it seemed markedly to violate the boundary between static and current phenomena which Ampère had so recently established. It was moreover hardly congenial to proponents of the static conception of the pile because it compared the electric current to heat flow in the form developed by Joseph Fourier, whose refusal to engage in microphysics had long bothered his French contemporaries.

Ohm proposed that a difference in the ‘force’ measured by an electroscopic device between two points on a conductor produces a proportional current between them as measured by a galvanometer. The proportionality depended upon both the nature of the metal and the distance between the points. Ohm used Fourier's heat-diffusion equation to build his theory, proposing an analogy between electric flow and heat flow. The terms of the analogy were not entirely clear, because Ohm did not distinguish between electric substance and his electroscopic force; indeed, Ohm's analysis, which distributed electroscopic force throughout the conductor, ran directly counter to the proposition that electricity resides at a conductor's surface. Little wonder that Ohm's work, which in retrospect seems to assert an uncontroversial empirical relation, was not rapidly taken up: it connected categories that had only recently been strongly distinguished from one another (static and current electricity), and yet it also seemed to be difficult to reconcile with static conceptions. The problem of reconciliation remained for two decades; it was eventually overcome through the intervention of a physical model as well as mathematical development.

Electromagnetic Induction and the Neumann Potential

In 1831 Michael Faraday discovered electromagnetic induction, which is to say that he discovered how to produce an electric current from magnetism or from other electric currents. The eventual key to his discovery, which had escaped previous investigators, such as Ampère (who had almost certainly observed something similar), was intermittence: that currents are produced when the cause changes, rather than when it simply exists. Faraday, who worked on an entirely different basis from either the analytically-minded French or the “Naturphilosophisch” north Europeans, began to develop the foundations of field theory within a half-decade (see the article on Fields).

For nearly fifteen years Faraday's discovery was not integrated into any electric theory, whether physically-based or purely formal. However, in 1834 (well before the enunciation of energy conservation) the German Emil Lenz formulated a general requirement to characterize induction: namely, that whatever currents are produced will exert forces that oppose the inducing action. Ten years later one of the two founders of the Göttingen mathematical-physics seminar, the first devoted to the subject, produced a mathematical structure for induction. Franz Neumann (the other seminar founder being the mathematician Carl Gustav Jacobi) took Ohm's work, as well as Lenz's, as a foundation, though he did not himself identify Ohm's original electroscopic force with static potential. Instead, he allowed the inducing action to function as though it were the same thing as Ohm's force difference. He was in this way able to find an expression for the inducing force in terms of the Ampère force when a circuit moves near an electric current or a magnet. Having done this much, he asked whether there exists a function whose gradient represents the Ampère force. Neumann easily found this potential, and by substitution in his expression for the inducing action he also recognized that the latter can be obtained from the potential's time derivative.

The Neumann Potential

$$U = \frac{1}{2} i_1 i_2 \frac{d\mathbf{s}_1 \cdot d\mathbf{s}_2}{r}$$

$$\text{Ampere force} = \nabla U$$

$$\text{electromagnetic induction} = \frac{\partial U}{\partial t}$$

The foundation of Neumann's work in a potential function acquired different, and wider, significance two years later (1847) when Hermann Helmholtz used it as an illustration of energy conservation.

German Electrodynamics

Although Neumann was the first to produce a general expression for electromagnetic induction, and indeed to unify it analytically with the Ampère force, Wilhelm Weber at Göttingen had provided a limited one six years before. Weber was assistant to the mathematician Carl Friedrich Gauss, who was then engaged in a project to produce magnetic maps and to recast magnetic theory in terms of absolute measurements. In 1846 Weber produced an immensely influential theory for electrodynamics that provided its own expression for electromagnetic induction and that unified the latter with the Ampère force on the basis of a physical model.

Neumann's potential function had done nothing to remove the disturbing character of the Ampère force, namely that it depended bilinearly on directed quantities. Indeed, the potential function itself depended on the scalar product of circuit elements. Weber instead built a scheme in which the forces do not depend explicitly on directions. They are instead functions of the scalar distance between electric particles and of the first- and second-order time derivatives of that distance; they are of course also central, which is to say that they parallel the distance. Weber founded his force-law on a physical hypothesis due to another of his colleagues at Göttingen, Gustav Fechner. Fechner argued that the electric current consists of a double stream of two kinds of particles:

positive particles move in one direction with a certain speed, while negative particles move in the opposite direction at the same speed. The Ampère force (as well as electromagnetic induction) was, Fechner supposed, an emergent effect due to the three kinds of interactions between particles in two circuits (positive-positive, negative-negative, and positive-negative). Fechner was unable to carry the model beyond qualitative assertions. Weber quantified it, showing that the Ampère law for circuit elements could be obtained by adding together the particle-particle forces between the two elements if the force has the following form:

Weber's Particle-Particle Force

$$\mathbf{F} = \frac{ee'}{r^3}\mathbf{r} + ee'a^2\left(-\frac{1}{r^2}\left(\frac{dr}{dt}\right)^2 + \frac{2}{r}\frac{d^2r}{dt^2}\right)\frac{\mathbf{r}}{r}$$

Here e, e' are the elementary electric charges, and a is a constant.

Weber's law salvages the traditional independence of forces from directions at the price of making them depend on something decidedly untraditional, namely on the first- and second-order time derivatives of the distance — not, it is essential to note, on velocities, which are directed quantities, nor even on speeds relative to some frame, but rather on the temporal changes in the relative distance between the particles. In addition, Weber's formulation requires the Fechner hypothesis to reach the Ampère force and also to encompass electromagnetic induction. This at once led to a controversy with Neumann, who initially felt quite strongly that his potential function did not in all cases produce the same results even for closed circuits as Weber's law — in particular that circuits with sliding contacts behave differently according to the two formulations, and moreover that experiment shows Neumann's account to be correct. Weber was eventually able to demonstrate that Neumann had neglected certain changes in this case, and they eventually agreed that the two formulations do always yield the same results for complete circuits, which at the time were the only kinds that had been produced. By the early 1850s Weber's electrodynamics had wide currency among German physicists, and it became the standard account, despite the fact that at least one among them believed it to conflict with more fundamental physical laws, namely with energy conservation. Helmholtz's critique had little contemporary influence, but a quarter-century later he constructed a new form of electrodynamics that completely avoided Weberean model-making and so interactions that depend upon anything beyond the distance between objects in given conditions at a given instant (see the article on Fields).

Neither Weber's nor Neumann's work had come directly to grips with the old problem posed by Ohm's law, though both made use of Ohm. They did not, that is, address directly the meaning of Ohm's electroscopic force and its connection to electrodynamics. They had instead replaced Ohm's system with the requirement that the current must be proportional to the electromotive force from all causes. This left open the questions raised by Ohm's original analysis. These were addressed by Gustav Robert Kirchhoff, who had studied under Franz Neumann, in 1849. Two years before this Kirchhoff had enunciated a series of relations for the algebraic sum of the currents at a circuit junction, and for the algebraic sum of the electromotive forces around any closed circuit path (eponymously named Kirchhoff's laws). In order to clarify the meaning of Ohm's law he identified the electrostatic potential with Ohm's electroscopic force, showing thereby that free electricity cannot exist within a current-bearing conductor in the steady-state. In 1857 Kirchhoff went further and developed an equation for propagation in wires. That equation was based on a computation for the static potential due to the charge distributed on the surface of the wire and on the assumption that the current is uniform across the wire's cross-section. Three years before William Thomson had obtained the diffusion equation for propagation (a form of which follows from Kirchhoff's analysis when self-induction is neglected) in a more general fashion without detouring through assumptions concerning the precise form of the potential. This latter "telegrapher's equation" became for many years the basis for a practical understanding of signalling; its omission of self-induction, while entirely reasonable for the slow changes involved in telegraphy, became too limiting a quarter-century later for understanding what happens at the vastly higher frequencies in telephony.

During the late 1860s and 1870s much electrodynamics in Germany revolved about questions first raised by Weber's force law and Helmholtz's understanding of energy conservation. The latter's original enunciation of the concept had excluded forces like Weber's from its embrace, because Helmholtz had insisted that the system's kinetic energy must always have the same value if its parts are in the same relative positions. Weber's law violated this requirement, and Helmholtz had accordingly rejected it. This argument became exceptionally intense during the 1870s, though it changed somewhat in character as Helmholtz gradually admitted a much wider understanding of the conservation principle than he had enunciated in the 1840s, to wit, that the conservation principle requires only that a sum of functions of the system's configuration, including if necessary first- and higher-order changes with time, is a constant. It then becomes a question of separating what can reasonably be treated as kinetic energy from what cannot be and then calling the latter `potential'. Having admitted this much, Helmholtz was forced to criticize Weber's force law on other grounds, which he did by envisioning situations in which the interaction between Weber's electric particles leads to instabilities. Webereans usually replied to this by denying that the situations could be physically realized.

Force, Field and Object Interactions

Any discussion of the field concept, particularly a broad one, must immediately grapple with the fact that for much of the nineteenth century the field was inextricably bound to the ether. Field history can slip too easily into ether history, with the result that, from the viewpoint of modern physics, it may seem as though nothing beyond some mathematics and a few experiments remain of the past, because the ether of the last century long ago vanished entirely from the texts of physics. The field, on the other hand, remains today a central part of physics, albeit in considerably altered form.

To understand the concept of the field, as well as the historical alternatives to it, in a way that distinguishes it from, but retains its links to, the ether consider first of all that, in the broadest sense, physics deals with the invariable changes that natural objects produce in one another. During the seventeenth century, in particular at the hands of Descartes, objects were reduced to the parts of a space-filling medium, and all interactions between such things were thought to occur directly and exclusively by contact. Descartes' scheme however required a difficult, second entity - force - to represent the interaction, one which he attempted with indifferent success to tame through quantification. The Cartesian scheme accordingly concentrated on the structure and behavior of the underlying medium and paid little attention to calculating the interactions between objects as they are known in nature.

By century's end Newton had produced a thoroughly different physics that permitted the calculation of interactions between natural objects. His system rested, like the Cartesian (and many other predecessors), on a bifurcation between the concepts of object and force, but it also introduced an extremely powerful novelty. Though Newton did think that all action must, in the hidden recesses of nature, occur by contact, he allowed that objects can be treated as though they possess an innate, quantifiable ability to effect the motions of other objects without contact, an ability that depends solely upon the natures of the objects and upon their mutual distance.

The Cartesian and the Newtonian schemes differ in the most fundamental ways from one another, but they do share a common element that permits us to distinguish both of them as force physics from field physics. In both of them objects are said to interact with one another by means of something called force, which is either vague and difficult to tame (Descartes) or else rather mysterious but thoroughly quantifiable (Newton). Objects, one may say, act directly on one another. In Descartes's case the action always requires contact; in Newton's contact action remains a future hope, and objects are considered to act directly and immediately across space.

In field physics objects do not act by means of force. In fact they do not act on one another at all. Rather, they interact with one another via a third system that differs in kind from them, and they do so in ways that have nothing at all to do with force, but that can be quantified in such a fashion that a force-like action between them may emerge as an apparent result. This third system exists throughout space and, by the 1870s in Britain, was thought to be perfectly continuous, albeit capable of possessing a given property to a different degree from point to point. Such a scheme bears a superficial resemblance to Descartes's plenum, but it differs from it in ways that are just as fundamental as its differences from the Newtonian system.

To see what is meant by this, consider that the Cartesian plenum is known à priori. All that can ever occur in nature must emerge from the intestine motions of the `parts' of Descartes' medium, each part exerting by contact a force on its contiguous neighbor. Every possible action of one object on any other object is already known, because the only `objects' are the parts of the plenum itself, and the only action is the exertion of force through contact. There is little sense in saying, e.g., that object A interacts with object B via a third system (the plenum) since A and B are themselves elements of the plenum. Interactions between natural objects are, in the Cartesian scheme, adventitious and unquantifiable. In the Newtonian scheme, by contrast, interactions between natural objects are hardly unquantifiable (at least in gravitation), and they are certainly not adventitious. But they are completely known, in the sense that the Newtonian force (the interaction) between A and B is given at once by the natures of the objects (their masses) and their distance apart. In the Newtonian scheme a new force between objects requires inventing a form of matter that hides within the objects or else it requires altering the nature of the object itself (e.g. one might add charge to mass as a fundamental property). Both alternatives might require fundamental changes throughout vast reaches of Newtonian physics.

Field physics differs from the Cartesian system in founding itself on quantifiable interactions between natural objects. It differs from the Newtonian system in presuming that natural objects can have an indefinite number of distinct kinds of interactions with one another that can be discovered independently through various kinds of laboratory manipulations. Moreover, the forces which correspond to these interactions need not depend solely on the distance between the objects. For the field physicist the existence of a new interaction between objects need not pose the profound challenge that it inevitably posed for the Newtonian (who would try to bring it within the ambit of calculation by inventing new forces and hidden matters), and that it scarcely raised for the Cartesian (who would at most explain it ex post facto). Indeed, we shall see that field physics by its very nature impelled discovering activity, and that for several decades British physicists took it for granted that they could assimilate new effects.

Field theory has often been equated to the idea of mediated action through direct contact, which does lie at the very foundation of the Cartesian scheme. From a physical point of view it has in retrospect seemed to be much closer in nature to the latter than to the discrete entities and disembodied forces of Newtonianism. And, indeed, if we reduce field physics (as well as Newtonianism) to these kinds of foundations then this point of view has a great deal of merit - because it reduces the history of field physics to the history of ether theories, which embody mediation in specific models for transmitting action through substance.

We shall take a different point of view from this one. We shall break the field away from the ether: we shall not consider the former to be merely the state of the latter. If one did know the ultimate structure of the ether - the underlying medium in which and by means of which all things take place - then it would certainly follow that a field could be nothing more than an ether state, simply because there would be nothing but ether states. And it is without doubt true that most physicists in the last part of the nineteenth century did hope that one day the great mystery of the ether's structure would be uncovered. But it had not been, and yet a great deal of field physics was nevertheless done. Consequently in what follows we shall concentrate on what it was about field theory, as it developed historically, that distinguished it at the most fundamental level as a new kind of physics.

The Inception of Field Physics

In recent years historians of physics have tended rather to seek continuities between developments than radical breaks with tradition. It has for example been argued that the creation of the electromagnetic field concept by the Englishman Michael Faraday between 1830 and the early 1850s owes much to the influence upon him of metaphysical considerations, or, if not this, then Faraday's conception had eighteenth-century precedents. Scholarship has certainly failed to demonstrate a concrete link between metaphysics and Faraday's actual work, but it has disclosed important precedents for two aspects of his concept, namely the idea of mediation and the claim that material objects do not accumulate a hidden electric matter.

Field physics requires, as a minimum, that effects must arise only from local action, that is from something that takes place in the immediate vicinity of an object. Some investigators in the late 1700s and early 1800s did think that charged objects actuate electric distributions in the circumambient air, that these distributions propagate through the air, and that electric actions on the objects are effected by them. All such accounts suffered from their being essentially unquantifiable, or at least unquantified. Nevertheless the concept of propagated effect, of mediation, certainly did exist, though it could hardly compete for influence with the highly quantified electric physics of Coulomb in the 1770s and Poisson in the 1810s. That physics localised electric material on charged objects and concentrated on analyzing the mutual action over long distance of electric distributions.

After the discovery by Oersted in 1820 of the magnetic effect of the electric current many physicists, particularly in Britain and Germany, commonly used iron filings to map electromagnetic interactions. Michael Faraday's particular concentration on these maps in the early 1830s is, consequently, not at all surprizing. Faraday's discovery of electromagnetic induction relied directly on his conviction that magnetic actions propagate, and he extensively deployed this imagery of magnetic lines of force. But neither the concept of propagation (whether magnetic or electric) nor the use of lines of force were themselves unusual at the time. During the next two decades Faraday accomplished something considerably deeper than this, for he created the underlying structure of a field physics that was entirely original with him.

Faraday introduced a fundamental distinction that was thereafter to run through all of field physics: the distinction between the intensity of a line of force and the total quantity or number of lines. These two properties, he asserted, are (in a given medium) proportional to one another. Second, Faraday assumed that quantity is always conserved when the lines of force are redistributed by connecting charged conductors together in various ways. Third, lines of electric force must begin and end on conducting surface. Finally, he reasoned that the electrometer measures a line's intensity. From these four propositions Faraday was able to calculate a value for the dielectric's `capacity' - for the factor that links quantity to intensity, and to show that it remained the same no matter how the conductors were charged.

This - and not the mere deployment of lines of force, or the use of ideas of mediation - is the true birth of field theory because it provides an entirely new way of dealing both conceptually and quantitatively with electric actions. For Faraday the electric universe (at least) now divided between objects that affect the local state of the field, on the one hand, and the field itself, on the other. The former were of two kinds: either conductors, within which the field's state is somehow completely destroyed, or dielectrics, which sustain the field but alter it. The field's state, Faraday thought, varies from point to point depending on the local presence of matter. The electric condition of an object, whether conductor or dielectric, accordingly depends only on the field state at its locus, a state that in turn depends on the distribution of other objects elsewhere. Here the idea of mediation has been joined to a specific, and quantifiable, understanding of how mediation (through the field) effects electric conditions. Faraday's introduction of the dielectric constant accordingly symbolizes the creation of a new field physics.

William Thomson and Faraday

William Thomson (much later Lord Kelvin) was a young, Cambridge-trained Scotsman when he first considered Faraday's work on electrostatics in the early 1840s. At that time he was less concerned with grasping Faraday's particular concepts than with demonstrating that Faraday’s claims for dielectric behavior could be understood in traditional terms by introducing a model for the dielectric. During the late 1840s and the early 1850s Thomson merged energy conservation with Sadi Carnot's analysis of heat engines, producing (in company with Rudolf Clausius) thermodynamics. At this time Faraday was attempting to extend his field physics from electrostatic to magnetostatics, as we now term it, which he accomplished by introducing magnetic capacity in analogy to the dielectric constant. Here, however, Faraday discovered that there are two major classes of magnetic bodies, which behave in markedly different ways - some along the direction of increasing field strength; others move in the opposite direction. This, he reasoned, requires that magnetic permeability must run from nothing to one, and from one on up, whereas dielectric capacity is always at least one (with unity being the standard, i.e. the capacity in the absence of matter).

Thomson unified Faraday's understanding of both electro- and magnetostatics with energy conservation in the following way. Taking his stand on integral transformations of the known values for the energy of an electric (or magnetic) system, Thomson showed that the product of quantity by its corresponding intensity represents an energy density for the field, a density that may be considered (the mathematics at least permits it) to subsist in the regions between bodies. Then, he continued, the tendency in all such systems will be to so arrange themselves as to minimize this total field energy. Here, then, one has for the first time a thorough, field-like replacement for the conception of an independent force that acts on objects. Forces certainly do result from Thomson's energy-based analysis, but they are its end-product, not its beginning.

Maxwell

Although the distinction between quantity and intensity - or between what we would now term flux and force - underpinned Thomson's merging of field theory with energy conservation, nevertheless he himself never went any further. In particular, he did not attempt to reconstruct all of electrodynamics from the new perspective. That was accomplished by the young James Clerk Maxwell, like Thomson a Cambridge graduate and mathematically adept. In 1856 Maxwell extended Thomson's structure to cover electrodynamics, and this for the first time brought to the fore issues concerning the electric current.

At that time Maxwell in effect introduced what was later termed the vector potential, or what he, following Faraday, termed the “electro-tonic intensity” to represent a function defined at the locus of the current and such that the corresponding electromotive force is given by its rate of decrease with time. This enabled Maxwell to formulate field theory in a manner that, while unfamiliar in retrospect, nevertheless captured the essential relationships between fluxes and forces that are today preserved in macroscopic electromagnetics, with the exception of the displacement current. While, as we shall see immediately below, the latter was a critical innovation on his part, his 1856 linking together of all electromagnetic variables through a system of force-flux relations formed the bedrock for his subsequent development of a full-fledged field theory.

The introduction of the displacement current into field physics was not a straightforward affair because it did not (as has often been asserted) follow directly from a perception of a mathematical inconsistency in the field equations that Maxwell had available to him in the early 1860s. Specifically, the partial differential relation that connects conduction current to magnetic force (the so-called “Ampère law”) implies that all such currents must be re-entrant, or closed, which is obviously not the case. One solution to the problem is to alter the field version of the Ampère law by adding to the conduction current a term consisting of the rate of change of electric quantity (or displacement) with time – that is, by adding the “displacement current” to the conduction current.

Maxwell however came to the displacement current rather through a physical model of the ether than through mathematics or more general field-theoretic conceptions (though the concept does undoubtedly have its roots also in Faraday's attempts to understand the relationship between electric induction and conduction). Here we do find a close binding of field theory to ether models, one that subsisted for about five years or so and that was instrumental in forging the structure of what soon evolved into Maxwellian electrodynamics.

The first form of Maxwell's model represented the electric current (which at this stage still exists only in conductors) by means of ball-bearing like objects that roll between rotating cells that store angular momentum and kinetic energy. These bearings translate against a frictional resistance in conductors but are not permitted to do so elsewhere. This posed a difficulty for Maxwell, because it meant that the model could not possibly be consistent with the absence of a current in a breached portion of a conductor: if the particles cannot move in the gap, then the mechanism, it seems, must break. It was to resolve this problem that Maxwell introduced the displacement current. He argued that the rotating cells must be considered elastic, so that they may yield to stress. This permits the bearings between them to shift slightly even within a non-conductor, which allows the mechanism to remain intact without permitting a current (a frictionally-resisted translation of bearings from cell to cell) within the gap. In other words, the original purpose of the displacement current was precisely to prohibit the occurrence of currents, properly speaking, within dielectrics. During the next decade Maxwell began to think about his theory more in terms of a highly abstract representation of field processes than in terms of the specifics of his model, and in so doing he began to formulate a new structure that reached fruition in 1873, when his influential *Treatise on Electricity and Magnetism* was printed.

Maxwellian electrodynamics

Maxwell's *Treatise* appeared six years after another extremely influential text was published in Britain, namely William Thomson's and Peter Guthrie Tait's *Treatise on Natural Philosophy*. The two *Treatises* are closely related to one another. In theirs Thomson and Tait dealt exclusively with mechanics, and they did so in a particularly significant way, for they based it directly on energy functions and either Lagrange's equations or Hamilton's principle. This set a pattern for research among mathematically-trained Cambridge 'Maxwellians' during the next quarter-century, because physicists attempted to build theories by discovering appropriate energy functions that, when fed into the Lagrangian or Hamiltonian machinery, led to empirically-testable results. Thomson's and Tait's generalized dynamics adapted particularly well to the kind of electromagnetic theory that Maxwell had been developing during the 1860s, one that was not founded ab initio on a specific structure for the ether. It is essential to grasp the wide-ranging nature of this uniquely British understanding of dynamical theory in order also to understand how it permitted the creation of a field physics that had to be abandoned after the introduction of the electron.

The essence of the dynamical method pursued extensively at Cambridge (but elsewhere as well) resided in its assumption that processes can be exhaustively described in terms of continuous energy densities. Different energy expressions, when fed through the dynamical equations, lead to different differential equations and to different boundary conditions. If a substance shows unusual behavior then the natural procedure to dynamicists is to modify the usual energy expressions and then to follow out the implications of the modification by inserting the new expressions into Hamilton's principle.

Although this may seem to be an unexceptional procedure even today, the modern physicist would raise two principal objections to it. First, one would ask where the energy expression come from? Second, modern theory allows that one can proceed in this way only when circumstances are such that the microphysical structure of the body does not extract or emit energy that cannot be taken into account in Hamilton's principle. These two objections did not occur to dynamicists for one reason, which marks the divide between their views and modern physics. Dynamicists tacitly assumed that all processes can be represented by continuous energy functions. Those few processes that did not immediately yield to the method - like dispersion – required more intricate energy expressions, perhaps ones that depended upon an inherent frequency. The primary goal of research was therefore thought to be the creation of appropriate energy formulae to whose consequences the physicist was then committed. Throughout the 1880s and the early 1890s this was a benefit and not a liability because the procedure led to important connections between a number of electromagnetic processes that seemed to be otherwise unconnected.

As seen by Maxwellians electromagnetic theory was accordingly based on the assumption that the seat of electromagnetic processes is a continuous medium, or ether, that is governed by the laws of dynamics. To solve problems in electromagnetism only requires expressions for the energy functions of the ether. We do not need to know its true structure, nor do we need to know how changes in the energy are brought about. Now the ether has certain properties that can be altered by the presence of matter. In particular, it possesses two of immediate significance: namely, specific inductive capacity and magnetic permeability. It is essential to understand that, for Maxwellians, these two properties are represented by continuous functions of position. The values of the functions may, and (in the presence of matter) do, change, but the changes are continuous. Maxwellians nevertheless did admit that changes in permeability and in capacity are due to the effects of material particles, and that the values we use in macroscopic equations are consequently averages over microscopic effects. However in their view each material molecule itself effects a continuous alteration in the ether's properties. As a result even at the microscopic level continuity is never breached.

At the core of Maxwellian field theory lay its abandonment of the conservation of charge in the previous sense of the phrase. Instead of considering charge to be a special electric substance that can accumulate in bodies, Maxwell treated it as an epiphenomenon of the field. His concept involved the transformation of energy stored in the ether into material form (as heat) through a process rather similar to that of elastic relaxation. This could occur wherever matter was present - though neither Maxwell nor Maxwellians attempted to explain why matter could have this effect, which is represented macroscopically by electric conductivity.

To understand what is involved in this imagine a region of the ether that is void of matter but in which an electric field - which stores potential energy in the ether - exists. Place a piece of matter in the region. Since all material substances, according to Maxwellians, have some conductivity, the region of the ether now occupied by matter begins to lose the energy that is stored in the electric field. This energy appears in the matter as heat. The result is the creation of a difference in the values of a certain quantity - the electric displacement, which is the product of inductive capacity by electric field intensity - at the boundary between matter and free ether. This difference represents, at any instant, the electric ‘charge’ on the boundary. Maxwellian + and – ‘charges’ are accordingly not individually conserved because there might very well be no charge at all in the universe on these principles. This contrasts with electric fluid theories, which assert only that there may be no net charge in the universe - but the individual positive and negative particles of course continue to exist. Nevertheless, Maxwellian theory satisfied the very same charge conservation equation that particle theories satisfy, despite this difference. It can do so because of its conception of electric current.

In Maxwellian theory an electric ‘current’ is the rate at which a portion of the ether is moving. If the ether is quiescent, then no ‘current’ exists. Electric ‘charge’ occurs because the ether has moved or is moving through regions in which the ration of conductivity to inductive capacity varies from point to point. To link the two processes - charge and current - we assume either that the current generates a magnetic field or else that a changing magnetic field generates an ether shift. Either assumption, well formulated, leads with other field equations to the very same equation for charge conservation that particle theories yield. The major point to understand is that, despite this agreement, the Maxwellian current properly speaking is not the rate of change of charge with time: it only may lead to such a change.

These several ideas are present in embryo in Maxwell's *Treatise* but they are there obscured by the text's novelty and comprehensive character. They and other central concepts are clearly evident in the work of Maxwell's Cambridge followers, as well as others not trained at Cambridge (though the latter differed on various points from Cambridge practitioners). Indeed, the concrete structure of Cantabridgian Maxwellianism was in major part produced as examination students worked out problems under the guidance of their tutors. For our purposes the following beliefs were in this way produced that were widely admitted by Maxwellians: ‘charge’ is nothing but a discontinuity in displacement; ‘current’ is nothing but moving ether; to create a new theory, modify the ether's energy function (which amounts to modifying the structure of the ether itself); the effect of matter upon ether is mysterious and must be put off until problems are solved through energy methods; electric conductivity is particularly mysterious and somehow involves the particulate structure of matter; boundary conditions are crucial analytical tools; mechanical models of the ether are important illustrations of energy exchanges but they are unlikely to reflect the ether's true structure.

Perhaps the best illustration of the power of Maxwellian theory, and of its difference from electromagnetism after the electron, involves the “Hall effect”. Discovered in 1879 by the American physicist Edwin Hall, this effect is today thought to demonstrate that the electric current consists of negatively-charged, moving particles. The experiment can be easily performed with modern equipment, and Hall's own technique was not fundamentally different from the modern one. Take a plate, say of copper, and send a current across its length. Attach a sensitive galvanometer across the plate's width, and place the entire device between the poles of an electromagnet, with the field normal to the plane of the plate. The sensitive galvanometer reveals a current while, but only while, the electromagnet is one. This, modern theory argues, directly reveals the deflection of the moving electrons in a magnetic field. Moreover, the direction of the deflection reveals their sign.

Maxwellians, including Hall, thought otherwise. For them there were only two possibilities available. The least radical, which was widely received for some time until Hall refuted it in the laboratory, referred the effect to an action of the magnetic field on the material structure of the metal (rendering the conductivity tensor asymmetric). This interpretation had the advantage of making the effect less than fundamental, thereby avoiding major alterations in Maxwell's equations proper. The second possibility was more exciting and was the most widespread. If, as Hall insisted, he had discovered a new way to produce an electric current (and not a new way to stress metals), then he had necessarily also discovered a new way to produce an electric field, since in Maxwellian theory an electric current always requires an electric field. The next question was precisely what conditions generated this new field; about this there was much room for discussion, but eventually a widely-accepted solution prevailed.

Whenever an electric current exists in the presence of a magnetic field, Maxwellians reasoned, a subsidiary electric field also exists which is at right angles to the current and to the magnetic field. Since a `current' is simply an ether flow, this meant that Hall's action should exist in non-conductors: an ether flow in the presence of a magnetic field implies a “Hall effect”. In fact, since ether flows are much simpler to understand in non-conductors than they are in conductors, theory can better deal with the former than with the latter - though Hall had found the effect only in conductors.

In Maxwellian theory field equations reflect the energy characteristics of the ether. If the ether's energy properties are changed, then the field equations are changed and vice versa. Hall had discovered an effect that required the addition of a new term to one of the field equations, though an admittedly small term. Consequently the energy properties of the field must also be altered in just the right way to yield Hall's new term. It was soon discovered that the altered energy densities, applied to dielectrics, yield equations for the Faraday effect (the rotation of the plane of polarization of light passed through a dielectric in a magnetic field). An intricate series of developments within Maxwellian physics ensued as the implications of the new energy terms were followed out during the 1880s, particularly as they applied to the difficult problem of reflection from magnetized, metallic surfaces. By the early 1890s it seemed (e.g. to J. J. Thomson) that workable results could be achieved even here.

He was wrong. The equations that he used, and that are required by field theory, cannot encompass magneto-optics because they lack a necessary constant, one that is easily provided by a micro-physical theory but that the macroscopic field equations of the Maxwellians could not embrace. The belief that Maxwellian physics was irreparably flawed did not however emerge from improved experimentation (though it might eventually have done so), but rather from theoretical penetration into the nature of conductivity.

Throughout the 1880s Maxwellians did not consider conductivity to pose a problem. This was not because they were able to incorporate it in their dynamical field equations. They were not able to do so, and in the early 1890s Oliver Heaviside even demonstrated that conductivity cannot be inserted directly into dynamical equations. Rather, Maxwellians avoided the entire question by relegating the subject to an area about which, they were willing at once to admit, they knew little - the unknown mechanism that links matter to the ether, and to do so they deployed John H. Poynting's theorem concerning energy flow through the field. That theorem could be used to trace the energy pattern around a current, and it seemed to indicate that the energy does not flow along the wire but rather radially into the wire - which nicely captured the image of the current as a byproduct of ether processes, since the wire appears rather to act as a sink for electromagnetic energy than as a carrier of it.

Poynting and others deployed this imagery in ways that permitted Maxwellians to bypass questions concerning the nature of conduction, and their techniques worked to the satisfaction of the community for about a decade. Indeed, problems with it did not emerge from within the group of active Maxwellians, but rather from an expert in hydrodynamics - Joseph Larmor - who certainly knew field theory, and who had been trained at Cambridge, but who had done little research in it until the mid 1890s. At that time he attempted to answer exactly the kinds of questions that more-experienced Maxwellians like Poynting and J. J. Thomson had long avoided, and this eventually led him into a quagmire. Larmor extricated himself from his difficulties by inventing the electron. At first he did not think of the particle as a substitute for traditional Maxwellianism so much as a way to bypass certain problems in it, but in short order he utterly revamped field physics by separating the electron, as the sole source of fields, from the fields themselves. Larmor was sufficiently persuasive that many Maxwellians eventually came to the conclusion that they could no longer play with field energetics, which was now fixed for all time. Instead, they had henceforth to invent models for material micro-structure, although that form of activity did not flourish at Cambridge in later years.

Helmholtz, Hertz and Electric Waves

In 1870 Hermann Helmholtz, newly-appointed to the chair of physics at Berlin, developed a form of electrodynamics that differed markedly both from the widely-accepted (in Germany) theory based on particles that had been developed by Wilhelm Weber, and also from British field physics. Indeed, Helmholtz attempted to produce a completely general set of equations, based on energy considerations, that could accommodate every possible variant of electrodynamics by constructing a ‘potential’ function, which had energetic significance and from which forces could be deduced by variational procedures: variation with fixed spatial coordinates yields an electromotive effect, while variation with the time fixed yields a mechanical effect. The resulting equations contain an undetermined constant *k*, and Helmholtz claimed that assigning different values to *k* would yield the corresponding theories for currents in conductors required by existing theories.

Despite Helmholtz's claims, his electrodynamic potential is not equivalent either to Weber's theory (in which the forces act instantaneously between electric particles) or to field theory (which substitutes fields for particles and in which forces propagate). One reason for this is that Helmholtz's electrodynamics has a different understanding of the relationship between objects as sources from either of the two alternatives. Unlike its competitors, Helmholtz's account never goes beyond objects themselves (to introduce, e.g., particles or fields), and it construes all actions as determined immediately and solely by the states of the interacting objects and by their mutual distance. Indeed, in his physics even force, properly speaking, does not exist as an independent entity because it emerges only via an energy calculation (as it also does, albeit with considerably different meaning, in field theory).

Yet Helmholtz's electrodynamics was the route through which many, including Helmholtz's student, Heinrich Hertz, eventually adapted aspects of field theory. Indeed, from the outset Helmholtz had kept field theory in mind, but he had early encountered difficulties in assimilating it, as one might expect. He felt that Maxwell's field (which was not completely developed in 1870) could be captured in his scheme by extracting from Maxwell the two concepts that, Helmholtz was convinced, uniquely characterize his scheme: first, the existence of an electrodynamic ether, and, second, the requirement that the disposable constant *k* must vanish.

To accommodate these two requirements Helmholtz did not adopt Maxwellian ideas. On the contrary, he attempted to adapt traditional understanding of the structure of dielectrics to the ether: to wit, that the ether must (like material dielectrics) be ‘polarizable’, and that the polarisation current must be treated (excepting Ohm's law) precisely like an electric current. In this way Helmholtz was able to show that electrically and magnetically polarizable bodies can be the sites of waves of polarization, their being (in general) both longitudinal and transverse oscillations. If, Helmholtz reasoned in 1870, the disposable constant k vanishes, then only transverse waves remain. If, in addition, the electric polarizability of the ether is effectively infinite, then one also obtains Maxwell's relationship between electric and optical properties. In later years it was realized that the requirement on k is redundant if only the polarizability is infinite, but for a decade or more both requirements were usually cited together (in part to distinguish the several ways in which Helmholtz's electrodynamics might relate to Maxwell's).

Hertz and Electric Waves

To put Heinrich Hertz, who first produced and detected electric waves, into proper perspective, it is essential to recognize that before his creation in 1887 of the dipole oscillator and resonator – the devices that, respectively, generated and detected electromagnetic radiation - no one knew how 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, at least until the mid-1880s 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 to produce free electric waves that could be detected.

In Germany neither Helmholtz nor anyone else considered how electromagnetic radiation might be artificially produced. 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 straightforward answers during Hertz's Berlin years. Indeed, Helmholtz tried to convince his young apprentice to devote himself to their experimental elucidation.

When Hertz began working intensely in 1886 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 as involving the direct action of one object on another. When he was able to produce and to detect waves in air, Hertz at first decided simply to assume that the wire-wire action was delayed in time, whatever the cause of the delay might be. However, by the spring of 1888 Hertz had decided that the dipole could not be treated in this way. 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 conductor A and conductor B, he now believed, was not delayed at all. Indeed, properly speaking, such an interaction 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 was entirely specified by the electromagnetic field, and which itself was both ubiquitous and unchangeable. Unlike laboratory objects, the ether in Hertz's conception has no manipulatable properties whatsoever, for its qualities remain invariant (though its condition or state varies).

This conception differed considerably from the one that had been advanced as a possibility by Helmholtz himself, and according to which the ether proper behaves just like a laboratory object. In such a scheme the ether would modify the apparent interaction between 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 a version of Maxwell's equations without using the ether at all. As a student of Helmholtz's, 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 entity 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 had 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. His route to Maxwell's equations at the time was based on an understanding that mixed field theory's refusal to grant sources (material objects) any active role whatsoever in electrodynamics with Helmholtz's electrodynamic potential, which required sources to be directly active entities.

The understanding of electromagnetic radiation that Hertz developed in the spring of 1888 instead insisted on the continuing role of the source, but dropped 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. 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 occurred only in the ether itself.

Although the field patterns that Hertz eventually deduced from his version of Maxwell's equations were theoretical constructions, whereas the oscillating dipole that actually produced radiation was a material object, nevertheless for Hertz the material object remained unknown, whereas the inferred field was considered to be known. This 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 involved 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 accordingly a critical factor 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 hardly surprising that Maxwellian reaction to Hertz's experiments centered principally on his detecting resonator, and not on his (mathematically intractable) oscillator.

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 physics and electrical engineering during the next half-century at least. Hertz's missing dipoles evolved into the antennae of an emerging technological regime; they also evolved into symbols for 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 that 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. Whereas the 1890 articles contained no diagrams of any kind, the 1889 piece contained several, including one that laid our a temporal 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 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 great 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.