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Physics in Perspective

The Origins of the Field Concept in Physics

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The term, "field," made its first appearance in physics as a technical term in the mid-nineteenth century. But the notion of what later came to be called a field had been a long time in gestation. Early discussions of magnetism and of the cause of the ocean tides had long ago suggested the idea of a "zone of influence" surrounding certain bodies. Johannes Kepler's mathematical rendering of the orbital motion of Mars encouraged him to formulate what he called "a true theory of gravity" involving the notion of attraction. Isaac Newton went on to construct an eminently effective dynamics, with attraction as its primary example of force. Was his a field theory? Historians of science disagree. Much depends on whether a theory consistent with the notion of action at a distance ought qualify as a "field" theory. Roger Boscovich and Immanuel Kant later took the Newtonian concept of attraction in new directions. It was left to Michael Faraday to propose the "physical existence" of lines of force and to James Clerk Maxwell to add as criterion the presence of energy as the ontological basis for a full-blown "field theory" of electromagnetic phenomena.

Key words: Johannes Kepler; Isaac Newton; Roger Boscovich; Immanuel Kant; Michael Faraday; magnetism; gravity; field theory.

Introduction

How does one go about tracing the origin of a *concept*?** It is not simply a matter of establishing the first use of a particular term or of an already familiar term in a new way. That can be relatively straightforward. But the concept may have been in the process of crystallization long before an agreed-upon term became attached to it. Nor is it usually enough to lay out the context within which the concept received its accepted formulation. There very likely would have been groping steps leading in that direction first, developments that prompted imaginative extensions of older concepts, perhaps, in response to new questions. In this paper, I retrace these first steps, the exploration of these extensions, in the case of the field concept.

It is relatively easy to say when "field" first appeared as a technical term in physics. In ordinary usage, the term has for long had as one of its many meanings: "an area of influence." This was the meaning that physicists took over and made

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more precise in the mid-nineteenth century when electricity and magnetism were blending into one, and arduous reconceptualization was the order of the day. In 1851, William Thomson (later Lord Kelvin) proposed as a definition of the newly-technical term: ‘‘Any space at every point of which there is a finite magnetic force is called ‘a field of magnetic force’ or (*magnetic* being understood) simply ‘a field of force,’ or sometimes ‘a magnetic field’.’’[1] By 1864, James Clerk Maxwell could take the term for granted in the title of his groundbreaking paper: ‘‘A dynamical theory of the electromagnetic field.’’[2] The Oxford English Dictionary (OED) cites an 1884 textbook definition: ‘‘In physics, a body which is within the range of action of another body is said to be in the field of that body.’’[3] And from this the OED takes its own definition of a field: ‘‘The area or space under the influence of, or within the range of, some agent.’’ As we shall see later, the main architects of the ‘‘field’’ tradition very likely would have found these last definitions too broad. But the general idea is clear: when a ‘‘field’’ is present, a body is taken to have an ‘‘area of influence’’ around it and at a distance from it. What are the historical antecedents of this sort of suggestion? That will be my topic.

Beginnings

If there was one principle in early natural philosophy on which there was nearly general agreement, it was the principle that excluded the possibility of unmediated action at a distance: a body simply could not act where it was not present.[4] Only through actual contact can action be transmitted between bodies. Virtually every major natural philosopher from Aristotle to René Descartes agreed on this. No room for indefinite ‘‘areas of influence’’ then, it might seem. But matters were not really quite that simple. There were several sorts of phenomena that seemed to challenge the supposedly self-evident principle. First, of course, was the obvious case of the faculty of vision. Did not an object seen at a distance influence the distant viewer? Or was it perhaps the other way around, as Aristotle tended to believe? But, of course, light is a necessary condition for vision and light suggests a medium of some sort. Could there not be some sort of transmission across the intervening space that would restore a (mediated) contact? Aristotle suggested the migration of immaterial forms (species) and this, in one version or another, became a more or less standard account.

Magnetic phenomena posed a more troublesome challenge. A magnet apparently could cause a distant piece of iron to move without any evident intermediary between. This puzzling phenomenon already aroused comment in the literature of the ancient Greek world. The anomaly it posed came up for discussion once again in the Middle Ages. Pierre de Maricourt (Petrus Peregrinus) in a short work, *De magnete* (*ca*. 1269), emphasized the importance of careful experiment to determine such properties of the magnet as polarity. And he went on to show how this property could be utilized to construct an effective compass.[5] The lodestone has a ‘‘natural virtue’’ that enables it to draw iron objects at a distance from it; he believed that it obtains this virtue from the poles of the heavens. The notion of a ‘‘virtue’’ that enables something very like unmediated action at a distance was adopted by other writers (Girolamo Cardano and Marsilio Ficino, for example) though their explanations of its operation were quite diverse.

William Gilbert's *De magnete* of 1600 was, many would say, the first major work in the broad experimental tradition that defined and transformed natural philosophy in the seventeenth century. Gilbert began by drawing a sharp distinction between two apparently similar but, in his mind, basically different sorts of phenomena. One was the effect noticed when amber is rubbed: it attracts all sorts of small objects. Amber (electrum) gave its name to this sort of effect; Gilbert called the class of objects that could, when rubbed, produce similar effects "electrics." And he postulated effluvia emitted from the rubbed body as the cause of motion in the distant object. Magnetic materials on the other hand share the "primary form" of earth, so that no effluvia are needed. Rather, the magnet is surrounded by an "orb of virtue" (recall the "area of influence" defining a field, according to the OED). This "orb" extends in all directions around the magnet, and Gilbert was careful to note that not only does the attracted object move, so too does the magnet, if allowed. Gilbert's best-remembered contribution was, of course, the idea that the earth itself is a giant magnet, as the behavior of the magnetic compass strongly suggests. An even more important contribution to our theme here is the "orb of virtue" that seemed to soften the prohibition against unmediated action at a distance.

One further phenomenon that seemed to involve causal action across spatial gaps was the ebb and flow of the tides. The correlation between the position of the moon in the sky and the tides directly beneath it was well known in antiquity; commentators like Posidonius invoked the influence of the moon as somehow responsible for the swelling upwards of the waters. An Arab natural philosopher of the ninth century composed what would become for many centuries the standard account of the tidal motions. Abu Ma'shar (Albumasar) published his *Kitab al-madkhal al-kabir* (*The Great Introduction to the Science of Astrology*) around 850 A.D. The importance of the topic of the tides in this sprawling work was that its author was eager to find validation for the "science" of astrology, and he saw in the evident correlation between the position of the moon in the sky and the state of the tide beneath it the strongest possible testimony to the sort of influence a celestial body can have on terrestrial affairs, the sort of influence that astrologers rely upon. He called on extensive records of joint tidal and lunar observations to show the significant correlations between the two. He noted the significance also of the lunar phases (and hence of the solar position as a further factor). That there are normally *two* high tides in the day, and not just one, posed a problem (as it would do for many centuries after), but he was confident that the moon had to be assigned the power to attract the waters of earth; his explanation, prompted by a standard astrological belief, was that the moon shared with the seas a watery nature. His book was translated into Latin, not just once but twice, as early as the first half of the twelfth century; its advocacy of lunar influence on terrestrial affairs appears to have had a wide influence on later medieval thought.

One important writer who entirely rejected the astrological reading of the tidal phenomena was Robert Grosseteste who, in his *Questio de fluxu et refluxu maris* (*On the Ebb and Flow of the Sea*), written around 1227, made use of these phenomena to testify to his own particular interest, the neo-Platonic metaphysics of light.[6] He accepted the by-then standard view of the influence of the lunar position on the tidal motions, but saw the cause as the *light* of the moon, alternately condensing and rarefying the waters beneath. Light serves, then, as intermediary; the immaterial species it transmits are the immediate causes of the rising and falling of the tidal waters. The moon, he asserts, does not itself act directly on these waters.[7]

By 1600, therefore, the idea of an influence that in certain special cases extends outwards from a body into the space around it, taking the form most often of an attraction, sometimes mediated, sometimes not, had become fairly widely disseminated in natural philosophy. In some quarters it was contested because of its astrological associations; in others, it was challenged because of its apparent violation of the principle of contact action.

Kepler's "Celestial Physics"

In his *Astronomia Nova* (1609), Johannes Kepler (figure 1) offered a new and, as it turned out, highly significant addition to the list of phenomena for which attraction, in one form or another, seemed the only plausible explanation. Planetary

Fig. 1. Johannes Kepler (1571–1630). Courtesy of American Institute of Physics Emilio Segrè Visual Archives, Landé Collection.

motions, from Aristotle's day onwards, had been explained in physical terms by postulating carrier spheres, whether concentric and interlocking, as were Aristotle's, or much more convoluted to accommodate the epicycles of Ptolemy. It was tempting, indeed, to leave the "physical" issue of causal explanation entirely aside and focus simply on the more limited "mathematical" goal of saving the phenomena. But the demand for explanation persisted and assured to the celestial spheres a more or less permanent place in the medieval cosmological imagination. Even Copernicus, despite the radical change in cosmological ordering that his system entailed, still titled his great work, *De revolutionibus orbium caelestium* (*On the Revolutions of the Heavenly Spheres*). ("Orbis" for him meant a sphere, not a planet or merely a "body," as the more usual translation of the title would suggest.) Despite his frequent references to "spheres," he prudently stayed away from the long-disputed issue of their nature.[8] It was not at all clear how, if the spheres were taken to be solid to perform their carrier function, they could be compatible with the minor epicycles that the Copernican system still required.

Tycho Brahe's careful reconstruction of the orbit of the comet of 1577 put an end to the uncertainty: there could be no *solid* spheres, at least, since the comet would have had to traverse them. That convinced Kepler that some form of action other than ordinary contact would be needed to explain planetary motions in the heliocentric system. In his view, this system already had demonstrated its superiority over its geocentric rivals. In his *Defence of Tycho against Ursus* (1600), he had to combat the skeptical position in regard to mathematical astronomy urged by Ursus, who maintained that this kind of astronomy could do no more than merely save the phenomena. If it could *explain* them, however, Kepler argued, in particular if it could provide a *causal* explanation for their occurrence, this would serve as adequate testimonal to the *truth* of the planetary system involved. To convert the Copernican model from a merely ingenious new way to save the phenomena into something like a proof of the earth's actual motion around the sun thus would require, effectively, a novel physical explanation of the planetary motion, a new "celestial physics."

And that is what he promised in the descriptive title of his new work: *A New Astronomy Based on Causes: or A Celestial Physics Drawn from Commentaries on the Motions of the Planet Mars*. The physics of the skies would be inferred from an examination of the planetary motions themselves, and would, in turn, help to establish the exact form of those motions. He could show that Mars's orbit is not a combination of circles; it is elliptical in shape, with the sun at one of the two foci. The planet is accelerated or retarded according as it is nearer to, or farther from, the sun. From this it was not hard to infer that the sun must somehow be the source of the planet's motion.[9] But how? Here he drew on analogies from all three of the special classes of phenomena discussed above, phenomena, that is, where something like action at a distance seems to be required.[10]

The sun was already a source of light; why should it not also emit immaterial species, just as a light source does, but these now as a source of *motion* in the bodies impinged on? What had first and foremost to be explained – he did not yet have the principle of inertia that would guide Newton – was the *onward* motion of the planets. But how could species emanating from the sun act at right angles to their path to achieve this?

Here he offered a daring speculative idea: the sun must be in rotation, swinging the emitted species in a sort of whirlpool sufficiently intense to urge the planets onward. (He rejoiced when a few years later Galileo confirmed the hypothesis of the sun's motion.*) The species themselves travel instantaneously, without affecting the spaces they traverse, though they are able to move material bodies. The magnitude of the effect they produce depends on three factors, just as in the case of magnetic action: the intensity of the solar source (itself in turn dependent on the size and density of the sun); the distance between sun and planet (the motive force falls off as the distance, he predicts);** and the bulk (*moles*) of the planet moved (itself proportional to both the volume and the density of that body). These measures were promisingly quantitative; they potentially would offer a way to estimate the intensity of the motive force at any point in the surrounding region. But the vortex hypothesis compromised the ability of the model as a whole to be expressed mathematically, always Kepler's goal.

There was one further matter requiring explanation: the planet moves at regularly varying distances from the sun. The vortices could hardly account for this. Here again Kepler hit on an ingenious possibility. Referring to Gilbert's *De magnete*, he suggested that the orientation of the earth's magnetic axis (he presumes a magnetic axis in the other planets also) remains parallel to itself as the earth revolves around the sun. One pole is attracted by ("seeks") the sun, the other is repelled by ("is hostile to") it. The effect of this is to vary the magnetic force on the planet systematically as its magnetic axis alters its orientation to the sun, just as its polar axis does, causing the seasons.

How about gravity? Kepler also sets out to formulate what he calls "a true theory of gravity" in the *Astronomia Nova*. It is:

> a mutual corporeal disposition among kindred bodies to unite or join together; thus the earth attracts a stone much more than the stone seeks the earth. The magnetic faculty is another example of this sort … . If two stones were set near one another in some place in the world outside the sphere of influence of a third kindred body, these stones, like two magnetic bodies, would come together in an intermediate place, each approaching the other by an interval proportional to the bulk of the other.[11]

Kepler finds confirmation for this account of terrestrial gravity by pointing to the accepted view of lunar attraction as responsible for the tides of the earth's oceans. The moon and earth are "kindred bodies." "The sphere of influence of the attractive power in the moon is extended all the way to earth … calling the waters

* He very soon found out, however, that observations tracking the movement of the sunspots refuted his own estimates of the sun's period of rotation and of the orientation of the sun's axis of rotation (which he put as perpendicular to the ecliptic). See J. L. E. Dryer, *A History of Astronomy*, second edition (New York: Dover, 1953), p. 394.

** Kepler assumed that the motive force acts along a plane sheet instead of on a surface perpendicular to the direction of incidence. This makes it come out as inversely proportional to distance instead of to the square of the distance.

forth particularly when it comes to be overhead.''[12] It is likely, then, that ''the earth's power of attraction extends to the moon and far beyond and accordingly that nothing that consists to any extent whatever of terrestrial material, carried up on high, ever escapes the grasp of this mighty power of attraction.''[13]

No matter how far this ''mighty power'' extends, however, it will not affect the sun or the other planets. These are not, in the *New Astronomy*, ''kindred'' to the earth. One sees here the influence on Kepler of the notion of ''sympathy'' so central to the alchemical and the astrological traditions. Nor is this ''mighty power'' responsible for the moon's orbital motion. It could raise tides on the moon, were there to be waters there. But the moon's onward motion is due to the swirl of immaterial species given off by the rotating earth. Were that rotation to stop, Kepler speculates, the moon would fall inwards or recede outwards to the stars. There are evidently two different sorts of action in question here, one involving a vortex of emitted species and the other a ''seeking'' or ''repugnance'' that does not appear to require mediation. Neither, however, is equivalent to an attraction holding the planet (or moon) in orbit.

In his later work, Kepler continued to speculate about the ''celestial physics'' underlying the orbital motions of the planets. In the *Epitome Astronomiae Copernicanae* (1618–1621), he leaves aside the requirement of a kindred nature and postulates an ''active and energetic faculty'' in the sun itself, capable of attracting or repelling or holding a planet, after the fashion of a magnet.[14] And in his last work, *Somnium* (1634), he remarks: ''The causes of the ocean tides seem to be the bodies of the sun and moon attracting the ocean waters by a certain force similar to magnetism.''[15] Here the sun and moon are classed together as capable of attracting the waters of earth. But Kepler still does not avail of this attraction to help explain the basic motion of revolution around the sun on the part of the planets; it is introduced only to explain departures of the orbits from the basic circular shape.*

In the *Somnium*, Kepler does finally hint at the idea that once a body is in motion, it will continue in that motion ''of its own accord,'' if not prevented. The idea was not a new one; it had been a staple of later nominalist natural philosophy, though these writers postulated an *impetus* as intrinsic cause of that continuance in motion. Kepler made use of the term ''inertia'' to refer to the resistance of a body to change of motion, a very different understanding of why unimpeded motion should continue. But he never got around to rethinking his ''celestial physics'' in the light of this suggestion. It could have allowed him to dispense with the cumbersome

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* In this review of Kepler's dynamics, I have left aside his discussion in the *Astronomia Nova* of an alternative explanation of the regular variation in distance from the sun of each planet in its elliptical orbit. Ought he ''summon a mind'' that would somehow steer the planet? A mind might perhaps accomplish this feat by estimating the changes in the sun's apparent diameter as seen from earth, due to departure from a simple circular orbit (p. 560). He has much to say about how such a mind might proceed but all along clearly favors a ''natural'' explanation in terms of analogies with magnetism. In the *Epitome*, he finally sets aside the hypothesis of a guiding mind and for an interesting reason: ''The elliptical shape of the planetary orbits, and the laws of motion by which such a figure is traced, reveal more of the nature of balance and material necessity than of the determination by a mind.'' See Kepler, *Werke* (ref. 14), p. 297.

apparatus of vortices of immaterial species; it could also have eliminated the need for a special moving cause for the rotation on its axis of the sun and each of the planets; for this function, Kepler (like Aristotle before him when explaining the rotation of each of his spheres) still felt forced to postulate a soul, even though this accorded poorly with his goal of a celestial physics that would rely on law-governed "natural" faculties only.

How far have we progressed in our search for early hints of the field concept? Gilbert and Kepler each describe a space "under the influence of an agent," enough to qualify as a field under the broader definitions cited earlier. They would have said that a certain sort of object placed at a point in that space would be caused to move, that the amount of that motion would depend on where in the space the point lies, on the bulk of the object moved, on certain features of the source-object. Not yet a fully quantitative specification, but moving in that direction. And their field-action takes two different forms, clearly illustrated in the *Astronomia Nova*: one, an unmediated attraction or repulsion, a "seeking after" or "aversion from" a distant object; the other, a mediated form of action involving a stream of immaterial species emitted from the source in a continuous motion that instantaneously traverse the space separating source from object.

In the wider context of the natural philosophy of the day, however, this broadened conception of action met a hostile reception. Descartes and Galileo denounced Kepler's notion of attraction as "childish" and "absurd"; in their eyes, it harked back to the suspect occult qualities of scholastic philosophy. Worst of all, it violated a cardinal principle of the "mechanical philosophy" that was now gaining ground, the principle of contact action on which Descartes, in particular, set such store. Attraction and immaterial species were to be banished; action would be communicated across the plenum by collision or pressure. The mechanical philosophy quite clearly had no place for anything like what would later be called fields.

Newton on the "Mathematical" and the "Physical"

The climate was thus unfavorable to a revival of the notion of attraction when Isaac Newton's *Principia* appeared in 1687. What made all the difference, however, was that Newton (figure 2) had worked out a coherent set of concepts linking mass, force, and acceleration, finally defining inertia in a way that removed the need to explain why uniform unimpeded motion, whether rectilinear or rotatory, continues indefinitely. Only *departures* from such motion would call for explanation and this would be given in terms of *forces* or, in the planetary context, *attractions*, the quantity of which could for the first time be precisely specified.

Do we now have fields proper, then, even though the use of "field" as a technical term still lies far in the future? Howard Stein argues that we do, that Newton's theory of gravitation can be properly described as a *field* theory, that the modern notion of field is implicit in Newton's thinking: "In Newton's investigation of gravitation, the notion of a field plays a logically ineliminable role in the inductive evaluation of the evidence."[16] Newton's law of gravitation specifies a function that

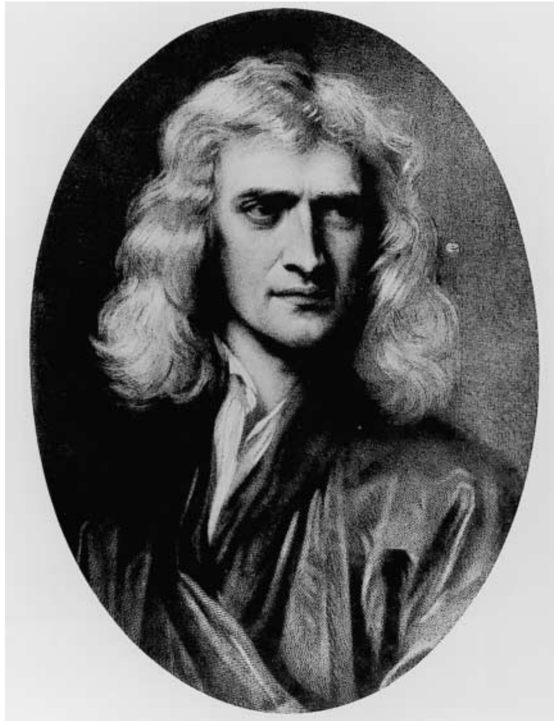

Fig. 2. Isaac Newton (1642–1727). Courtesy of American Institute of Physics Emilio Segrè Visual Archives, Physics Today Collection.

takes on a definite value at every point in the space surrounding a gravitating body, prescribing how a second body would move at that point. Thus it associates a definite *disposition* with each point in the space; this, in Stein's view, is necessary and sufficient to constitute the theory a field theory.[17] Furthermore, Stein maintains that for Newton a body is nothing more than "a region of space endowed with certain properties," notably impenetrability. Since this latter property is either present at any given point or is not, it is "quite natural," he thinks, to think of it as "a two-valued function on space," and hence, once again, as a field.[18] His conclusion: "It is not a metaphor but a literal truth that Newton's metaphysics of body reduces the notion of matter to the notion of field."*

Abner Shimony agrees that the account of gravity in the *Principia* "clearly refers to what, in modern locution, is called the gravitational field."[19] Since Shimony is notable among philosophers of physics for the emphasis he places on the ontolog-

* I am dubious about Stein's attempts to construe as a "field" the impenetrability that Newton assigns to bodies. He assigns more weight to Newton's early anti-Cartesian treatise, *De gravitatione*, in this argument than I would be inclined to do. But even apart from that, impenetrability is invariably treated by Newton as a property that *defines* body. It does not admit of degrees, nor does it have an effect anywhere except within the body. Stein's phrase "impenetrability field" thus seems to me a misnomer. It places Newton's account of body too close to that of Boscovich.

ical grounding of the theoretical constructs the physicist employs, one might have thought that Stein's dispositional definition of field would seem to him to be ontologically rather skimpy. But no; the ontological status of a field candidate, he says, can be secured by the simple assignment to each point in a given space of a quantitatively specified causal disposition, a "potential action," that would be exerted on any body occupying that point.

In a commentary on Stein's essay, Mary Hesse expresses her disagreement. The Newtonian account of gravitation, in her view, does not qualify as a field theory in the fullest sense. Stein's point-dispositional construal of the notion of field is in her view too broad. Specifically, it is compatible with unmediated action at a distance, the kind of action that those who later introduced the term "field" were intent (as we shall see) to distinguish from continuous forms of magnetic and electrical action across space.[20] This usage is too broad, Hesse maintains; it conflates under a single term, "field," two very different ways of understanding how bodies can act upon one another at a distance, ways that ought to be kept distinct. Stein retorts that the notion of field from the beginning has been quite loose. There is, in his view, "no convincing reason to designate any special set of features as necessary and sufficient to confer the dignity of the appellation 'a field theory'."[21]

Anticipations of this tension between two very different modes of understanding in physics can readily be detected in the text of the *Principia* itself as well as in the reactions to it of its many critics, notably Gottfried Leibniz and George Berkeley. These critics pointed to the ambiguity attending Newton's use of the terms "force" and "attraction," around which the entire structure of the new mechanics was built. They *sound* explanatory, Berkeley remarks; they sound as though Newton is proposing a causal explanation of why planets move as they do. Planets are "attracted" by the sun, "drawn" by a force determined by the sun's mass and the distance between. But this gives no hint as to how exactly this attraction works. Unmediated action at a distance of one body on another is impossible, the critics were agreed. But what else does the *Principia* offer?

Newton had anticipated this reaction to his introduction of terms like "attraction" into mechanics, despite the embargo set upon them by the mechanical philosophers of the day. Indeed he was willing to concede to his critics that unmediated action at a distance was inconceivable. So what was left him? The defensive strategy that he adopted was to recall a distinction between the "mathematical" and the "physical" that had had wide currency in the earlier history of astronomy, particularly in debates between those who sought simply to save the phenomena and those who set their sights on causal explanation through spheres and the like. His choice of title for his groundbreaking work was intended to emphasize that he was proposing the *mathematical* principles of natural philosophy only. He was not claiming to give a *causal* account of gravitational action; "the cause of gravity I know not," he was quite prepared to admit.

Already in Definition VIII, he inserts an emphatic disclaimer: "I here design only to give a mathematical notion of these forces, without considering their physical causes and seats."[22] And in more detail:

> I use the words "attraction," "impulse" or "propensity" of any sort towards a centre, promiscuously and indifferently one for another, considering those forces not physically but mathematically, wherefore the reader is not to imagine that by those words I anywhere take upon me to define the kind, or the manner of any action, the causes or the physical reason thereof, or that I attribute forces in a true and physical sense, to certain centres (which are only mathematical points); when at any time I happen to speak of centres as attracting or as endued with attractive powers.

How, then, should a "mathematical" account of motion be understood? In a dispositional sense, one may assume, though Newton himself never explicitly puts it in quite that way. In the "mathematical" interpretation of the formalism of the *Principia*, to say that "a force is acting on a body" or that "a body is attracted by another body" is equivalent to saying only that the body will move in a particular way, if not prevented, or, more specifically, that the body has a *disposition* to move in that way in a given configuration of bodies. Such a disposition qualifies as real. Gravity in this dispositional sense is not an "occult quality," as Roger Cotes would later reply to Newton's critics; it is a testable manner of behaving on the part of all material bodies, one that indeed could be "deduced from the phenomena and made general by induction," exactly as the maxims of his experimental philosophy enjoined.[23] But he almost surely would have been loth to attach such real dispositions to points in space. A dispositional reading of gravitation prescinds entirely from the causal issue of how precisely the action is taking place in space, *i.e.* from the "physical." And this is what Newton was still hoping to discover.

Newton's interim restriction to the "mathematical" still left him open to several lines of objection. The first, as we have already seen, was cogently put by Berkeley in his *De Motu* (1721): the appeal of the *Principia* to the reader depended on the ambiguity of such terms as "attract" and "draw." All that the new mechanics could claim (and Berkeley was prepared to admit that this was already a lot) was that it accurately *described* the behavior of a material body in the presence of other bodies. This was, of course, precisely the "mathematical" account to which Newton had promised to limit himself, but Berkeley's point was that this commitment was undermined by a deceptive use of language.

Berkeley had his own suggestion to make, one with a resonance for later times. If Newton really wanted to claim that his *Principia* gave an *explanatory* account of gravitational motion, he could do so by distinguishing between two senses of "explain," one, the traditional one that would commit him to specifying the nature of the agency involved, the other a weaker one that would take "cause" to imply no more than lawlike succession between phenomena. In the latter sense, but in that sense only, could the law of gravitation be regarded as "explanatory." Unlike Leibniz and the Cartesian critics, then, Berkeley was allowing that the law of gravitation *is* explanatory, in his view in the only sense open to physics, indeed.

This would not have satisfied these other critics at all. The only sense of "explain" that they were prepared to countenance was the strong "agency" sense (one that Berkeley reserved to the action of mind alone). Their objection to his language was that it concealed the unpalatable fact that the only conceivable physical interpretation of his account of gravity would commit him, in their view, to unmediated action at a distance. In response to a worried query from his disciple, Richard Bentley, Newton labored to make his own position on this issue clear:

> That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance through a vacuum, without the mediation of anything else by which their action and force may be conveyed from one to another, is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it.[24]

Did Newton really believe this, or was he merely retreating to a defensive position in the light of the criticism that the *Principia* was by this time (1692) encountering? I am inclined to think, for reasons that will become clearer in a moment, that he did believe it. The significance of this point for my theme is that it means that the "mathematical" or "dispositional" interpretation of the mechanics of the *Principia* was, in Newton's own eyes, incomplete; in the long run it would have to be supplemented by a properly "physical" account of just how the active disposition of one body could affect the state of motion of another causally at a distance from it. The only reading of the mathematical-dispositional account of gravity that would transform it directly into a physical account, *i.e.* that would allow it to pose as complete, would necessarily limit it to a simple postulation of unmediated action at a distance. And this Newton did not want. For this reason Stein's proposal to treat Newton's account of gravitation as a field theory proper seems to me at odds with Newton's own clear intention.

What sort of "physical" rendering of gravity was left open for Newton? The most obvious possibility: a material aether that would mediate the attraction between one body and another, had been shown in the *Principia* itself to be inadmissible; it would cause the planetary motions to be unstable. This had led Newton to abandon the "aethereal medium," as he called it, that had played an important part in his thinking in the decade preceding his composition of the *Principia*. The aether entered continually at this earlier time into his discussions of optical, chemical, and even physiological phenomena. Though his conception of this medium had been shaped in part by his alchemical readings, his "aether" was still quite clearly material in nature, and thus subject to the inertial constraints subsequently laid down in the *Principia*.[25]

In the years that followed, Newton struggled to find a coherent solution to this challenge. He obviously did not think that his mathematical-dispositional account was sufficient of itself.[26] Several possibilities evidently occurred to him. Might not the causal agent responsible for the lawlike behavior of planet and pendulum be the Creator, moving these bodies directly without any intermediary secondary cause? This was appealing from the voluntarist perspective Newton shared with many of the philosophers of his day.* But it set the explanation of motion outside the

* Stein comments that readers like himself are liable to be "not only puzzled but annoyed" by the

competence of natural philosophy, as Leibniz for one would have been quick to point out. Newton did not dwell on this option for very long.

A more conventional alternative had been blocked already by the argument of the *Principia* itself. An aether with inertial properties was ruled out. But so urgent was Newton's effort to find a solution for his dilemma over gravity that he even explored the possibility that this ban somehow might be circumvented. In a letter to Robert Boyle in 1679, he already had speculated that gravitational motion might be explained by differences in "subtlety" of an "aethereal substance" pervading all bodies.[27] Late in his career, in an addendum to the third edition of the *Opticks* (1717), he proposed a material aether consisting of particles that strongly repel one another, hence conferring very high elasticity.[28] If density of the aether were to increase with distance from ordinary matter, it would cause material bodies to approach one another. For it to be the "cause of gravity" that he sought, the ratio of its "elastic force" to its density would have to be enormous (he guessed that it would need to be at least 49×10^{10} times greater than that of air!). The vibrations in this aether might also cause the alternate "fits of easy transmission and easy reflection" that his investigation of the phenomena of colors in thick plates had already led him to postulate. The aether of this, his last published work in natural philosophy, was an extraordinary construct, quasi-empirical, quasi-mathematical. Its relevance to my inquiry lies in the testimony it affords of Newton's persistent attempts to supplement the successful "mathematical" principles of the *Principia* with postulated "physical" mechanisms that, if successful, would convert the whole into explanation in the full agent-causal sense.

There was, finally, one other quite different line of thought that attracted him from the early 1700s onwards. Francis Hauksbee had demonstrated various sorts of electrostatic phenomena before the Royal Society. The evident affinity between these phenomena and the more familiar ones associated with magnetism led Newton to postulate "a certain most subtle spirit" (as he called it in the General Scholium added to the second edition of the *Principia* in 1713).[29] He took it to pervade solid bodies and the spaces immediately surrounding them. Its effect would be to set up strong forces of repulsion and attraction between the smallest particles of bodies, thus giving rise to short-range phenomena such as the solidity of composite material bodies, chemical change, and physiological growth. The forces themselves could, in principle, be discovered from the motions they brought about, making use of the laws laid out in the *Principia*. But he evidently became discouraged by the difficulties that arose in developing this idea further (difficulties he refers to in the General Scholium itself) and it vanishes from his draft-materials.

"quibble" involved in Newton's speculation that gravitational motions might simply be due to "an immediate law of the Creator." In his reading of Newton, "bodies attracting by an immediate law of God means for him exactly the same thing as action at a distance. I think that Newton knew this equivalence clearly, and disguised it in his public utterance to avoid unwelcome embroilments … his statement is (on my reading) evasive … "; see Stein, "On the notion" (ref. 16), p. 273. This seems to me, however, to misunderstand Newton on what was for him a key issue. According to the traditional Christian account of God's creative action, an account with which Newton was in full agreement, the Creator is entirely present *per potentiam* (by his power) to all His creatures, conserving them in being and sustaining their actions. This is precisely *not* the action at a distance of one body on another that philosophers had traditionally proscribed and which Newton was at pains to avoid.

A more general notion than "electric spirit" can, however, be found in his writings from the 1670s onwards. He speaks sometimes of "active principles" associated with phenomena of a variety of kinds, including gravitation.[30] The notion itself had its roots in the alchemical and neo-Platonic traditions with which he was familiar; it was, of course, quite alien to the orthodox mechanical philosophy of his day. What particularly impelled him to search out the sources of action in such "principles" was his conviction (this time with roots in the Cartesian, as well as the strongly-contrasting alchemical, traditions) that matter is of its nature passive, a conviction that would seem strangely at odds with the rhetoric of attraction in the *Principia*. More troublesome still was that the gravitational force exerted by a body directly depended on the mass of that body, so that the body itself clearly was in some way implicated in the genesis of the force. No such direct tie seemed to be present in magnetic or electrical action.

Were these principles associated with the space *around* bodies, if the bodies themselves were assumed to be of their nature passive? It would seem so, but Newton is not clear on that point, except perhaps in the case of one such type of principle, the electric spirit. There is, so far as I can tell, no indication in his writings of active principles pervading the entire space between the bodies involved in gravitational action.

In short, despite all his efforts, he never did hit on a satisfactory "physical" account of the cause of gravitational motion that he sought so actively. Still, as we have seen, if "field" be taken in the minimalist dispositional sense of a force-function that can be defined precisely across a region, Newton's gravitational mechanics does describe a field. But it hardly can be called a field *theory*. Newton, as we have seen, was inclined to agree with his critics that the account of gravitational motion he had given in the *Principia* was not explanatory, not causal, in the traditional acceptance of those terms.

The Newtonian Legacy

Newton's achievement would dominate much of the thinking in the natural sciences for the remainder of the eighteenth century. From the perspective of our inquiry here, three whose thinking took off from that achievement may be singled out, two of whom (Boscovich and Euler) sensibly advanced the development of the field concept, and a third (Kant) whose influence on that development can be regarded as mixed. The first is Roger Boscovich (figure 3) whose *Theory of Natural Philosophy* (1758) drew on both Newton and Leibniz but formulated a mechanics in significant ways quite different from that of either of his mentors. Matter for him consists of non-extended points possessing inertia but not mass. Each point is associated with a disposition to accelerate (*vis*) towards or away from it on the part of every other point in the system, whose quantity depends only on the distance between the two points.*

* He calls these propensities or dispositions *vires* but the term *vis* means for him something quite different from Newton's *vis* (force) since the points are massless. It would be more accurate to call them dispositions to accelerate but translators customarily use the more familiar (but here potentially misleading) term, "force." See the editor's Introduction to Roger Boscovich, *A Theory of Natural Philosophy*, transl. and ed. J. M. Child (Cambridge, Mass: MIT Press, 1966), p. xv.

He is entirely non-committal about what *causes* the matter-points to behave in this way, though, like Newton, he uses terms like "attract" and "repel" that make it seem as though the points act at a distance on one another. At large distances, the mutual *vires* are entirely "attractive"; as the point-particles approach one another, the *vires* vary continuously from "attractive" to "repulsive" (which is to say that the accelerations of the particles keep reversing directions gradually) until at very close approach, the *vires* are entirely "repulsive." This allows him to dispense with impenetrability as a primitive property of material bodies. The appearance of impenetrability is explained by the "repulsion" between the point-particles when brought very close together. There is thus no actual contact between bodies, only near approaches. Though Boscovich carefully brackets the question of agency, *all* action in his system has the appearance, at least, of action at a distance.

Though Michael Faraday cites Boscovich as a predecessor, the distance between them in regard to the notion of the field is still considerable. What is relevant, though, is that Boscovich explicitly lays out a function across space, the same function for each point-particle, which represents at each point the measure of the acceleration another particle would undergo if placed at that point. The value of

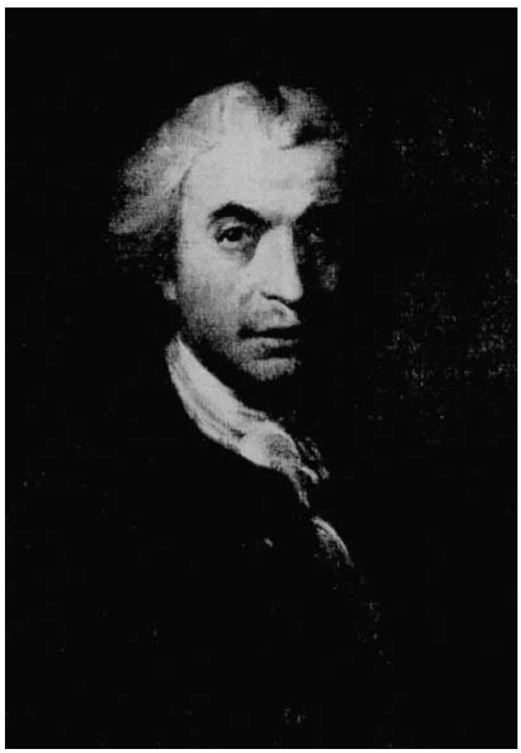

Fig. 3. Roger (Rudjer) Boscovich (1711–1787). Painting in City Archive, Dubrovnik; painter unknown. Courtesy of Maria Sesic, Director of the Nikola Tesla Museum Belgrade.

the function varies continuously from acceleration towards the first particle ("attraction") to gradually alternating directions of approach and withdrawal, to withdrawal ("repulsion") only. And so he can claim to represent in a general way, with just a single function, a range of different phenomena, gravitation, cohesion, and impact. In the weak dispositional sense of the term, this would qualify as a field.

Mary Hesse begins her account of field theories with Leonhard Euler, "the real founder of the mechanics of continuous media."[31] The *Mechanica* of 1736 succeeded, where Newton had failed, in formulating an abstract mathematical model of fluid motions. It was in hydrodynamics, according to Hesse, that:

> the notion of a field theory first took shape. A field in mathematical physics is generally taken to be a region of space in which each point (with possibly isolated exceptions) is characterised by some quantity or quantities which are functions of the space coordinates and of time, the nature of the quantities depending on the physical theory in which they occur … . In Euler's hands, hydrodynamics became a field theory, the field of motion of a fluid being characterized by the velocity of the fluid at each point … .[32]

The metaphor here is of a flowing fluid, with specific material properties, and the equations are equations of flow, not of radiating forces. Hesse puts the matter very directly. Why *should* the gravitational potential, especially as this receives further mathematical precision at the hands of Lagrange, Laplace, and Poisson, be regarded as a field proper?

> There is a physical difference between a gravitational field and the velocity field of a fluid. In the latter case the field function is an actual property of material at every point of the field, but in the gravitational case the potential function, "V," is potential in the sense that it does not necessarily describe a physical property of the field, for it may have a value in empty space; it describes a potential property, namely, the force that *would* be exerted *if* a small mass were introduced into the field at that point.[33]

Hesse makes use of the term "field" in this passage to describe the (Newtonian) gravitational case but she makes it clear (as we have already seen in her comment on Stein's essay) that she regards this as a minimalist and ultimately unhelpful usage. A field proper must have a physical reality on its own account; dispositions in empty space are not enough. They may be, after all, no more than calculational devices describing unmediated action at a distance, action that does not lend itself to any further explanation. A velocity field in a fluid, by contrast, postulates a material medium that exhibits properties other then velocity, properties that make it independently observable. Had it been possible at that time to show that gravitation required a medium, an aether, of some sort to serve as causal intermediary, then, she allows, it *would* have qualified as a field proper. But this was precisely not the case. The moral that Hesse draws from her discussion of eighteenth and early nineteenth-century fluid theories, then, is that the most

Fig. 4. Immanuel Kant (1723–1804). Lithograph from a painting by Gottlieb Doeppler, 1781. Courtesy of the Bildarchiv Preussisches Kulturbesitz, Berlin.

appropriate (though not necessarily the only) test of the physical reality of the fluid itself and hence of the theory's qualifying as a field theory proper, would be the possession of properties other than the disposition specified by the field function itself.

From this perspective, Immanuel Kant's account would exclude gravitation from consideration as a field proper. In the opening paragraph of his magisterial *Kant and the Exact Sciences*, Michael Friedman makes an unqualified assertion: "Kant accepts Newtonian attraction as an immediate action-at-a-distance throughout his career."[34] Where Newton himself had sought, as we have seen, to find possible ways to avoid resorting to action-at-a-distance, Kant defends the propriety of this notion. The forces of repulsion and attraction are *constitutive* of matter, he argues, the first to enable matter to occupy space and thus, effectively, to exist,* the second to keep the first from dispersing matter indefinitely.

* Here Kant voices a rare disagreement with Newton, who postulated impenetrability as a separate and irreducible property of matter, unrelated to force. Kant recommends that it be "banished from natural science as an empty concept"; it has, he asserts, no place in a fully dynamical system. See his *Metaphysical Foundations of Natural Science*, transl. James W. Ellington, Part 2 of *Philosophy of Material Nature* (Indianapolis: Hackett, 1985), p. 77.

Kant (figure 4) challenges the traditional argument against unmediated action at a distance. Why should a body *not* act where it is not, he asks? The earth immediately acts on the moon; whether or not matter lies between the bodies does not affect the attraction between them. Indeed every body that acts on another body acts where it is not.[35] If earth and moon in fact were to touch, their point of contact would be where neither, strictly speaking, is. Their boundary, he asserts, is not part of the space of either. Further, to make contact a necessary condition for the transmission of action would, effectively, be to make action of the force of repulsion a necessary condition for the action of the force of attraction. Since the two sorts of force are wholly different in kind, there is not "the least foundation" for such a requirement.

Kant's arguments here are not convincing, but his Proposition 7 is unhesitating: "The attraction essential to all matter is an immediate action through empty space of one matter upon another."[36] The mention here of "empty space" is somewhat puzzling, since he makes it clear that, whether the space is empty or not, the attraction is direct from body to distant body, quite independent of what lies between. Furthermore, he is, in fact, somewhat averse to the possibility that (as the "mechanical" philosophers claimed) there can be truly *empty* space; though he admits that this cannot be proven by the transcendental method of the *Metaphysical Foundations*.[37]

Kant, therefore, would be likely to challenge the view that the dispositional version of the law of gravitation falls short of being a "field proper," that is, remains incomplete because it is consistent with unmediated action at a distance. Where Newton saw the need to supplement his "mathematical" account of gravitation with a "physical" one bridging the gap between the bodies, Kant sees the need for supplementation in a quite different quarter. A true natural science needs to be grounded in a *metaphysics*; it requires the transcendental method that he lays out with such care in the *Critique of Pure Reason* and the *Metaphysical Foundations*. And the application of this method to the problematic of dynamics establishes that the role of the force of attraction in constituting matter can be known *a priori*. Whether Kant believes that the inverse-square form of the law of gravitation can be similarly established is not clear.* In any event, however, nothing needs be said about what is (or is not) going on in the intervening spaces.

In the last years of Kant's life, the assurance conveyed in the title, *The Metaphysical Foundations of Natural Science*, began to waver. The natural science toward which the vast structure of the transcendental method had been directed was, of course, Newtonian mechanics. But a variety of new areas of research were

* See Friedman's discussion of *Prolegomena to Any Future Metaphysics* (§38) where Kant says that the inverse-square law is "prescribed to nature by the understanding"; Friedman, *Kant*, pp. 165–186. See also Observation 1.4 to Proposition 8 in the *Metaphysical Foundations*, p. 73. But Kant's assertion in the General Observation on Dynamics in the same work that "everything, even universal attraction as the cause of gravity, must, together with the laws of such attraction, be concluded from data of experience" (*ibid.*, p. 93) seems equally unambiguous. On balance, it would seem that establishing the inverse-square law as the actual law governing planetary motion does for Kant require recourse to the data of experience, but the issue is (as is not uncommonly the case with Kant!) a convoluted one. See Friedman, *Kant*, pp. 191–210.

just beginning to show results: chemistry, optics, "calorics." It had been possible in the *Metaphysical Foundations* to dismiss chemistry as "a systematic art rather than a science."[38] But by the 1790s, in the wake of the transformation worked by Antoine Lavoisier, this judgment seemed increasingly vulnerable. And so, faced with a growing gap between the foundations earlier laid and these diverse new bodies of knowledge laying claim to the title of "science," Kant began work on a "Transition" that would, he hoped, bridge that gap. The work was never finished, but over seven years (1796–1803) he wrote hundreds of pages of draft material, organized much later as his *Opus Postumum*.*

The relevance of the *Opus Postumum* to our story is real, though indirect. In these pages, Kant introduced an aether pervading all of space as his principal explanatory concept, in strong contrast with the unmediated action at a distance he still assigned to gravitation, a contrast that he himself emphasizes. This aether resembles the caloric fluid of the dominant heat theory of that day, but it differs in that heat for him consists rather in vibrations of the aether. The aether does not flow as caloric was supposed to; the vibrations pass from one part of the aether to another. The aether is also the carrier of light, understood by Kant as a periodic ("wavelike") phenomenon in a medium. Finally, it facilitates chemical interactions as the seat of the moving forces responsible. Kant's arguments in favor of this ambitious schema are not easy to follow and will have to be left aside here.[39] His aim was to achieve the desired transition from the "top-down" synthetic *a priori* validation of Newtonian concepts in the *Metaphysical Foundations* to the empirical "bottom-up" articulations then in progress in chemistry, optics, and the theory of heat.

It did not and indeed could not work. The *Metaphysical Foundations* was tied to a specific concept of matter aimed at providing a foundation for the Newtonian laws of motion and the theory of universal gravitation. But the aether-deduction came from altogether different sources in the newly-developing structural sciences that made no use of Newtonian mechanics and seemed entirely independent of it. Friedman remarks:

> To be sure, Kant conceives the [optical] aether as perpetually oscillating or vibrating through forces of attraction and repulsion, but he provides no considerations that would connect those supposed forces with the fundamental forces of attraction (underlying gravitation) and repulsion (underlying impenetrability) of the *Metaphysical Foundations*. [Nor does he when he] … imports the language of attraction and repulsion into the description of chemical affinities … .[40]

The methods of the transcendental deduction had been tailored to the action at a distance of gravitation where the operation of forces could be deduced from direct observation of the motions involved and there was no intervening explana-

* This material left behind at his death went through many vicissitudes, described in detail by Eckhart Förster in the handsome recent English edition of the *Opus Postumum*, translated by Förster and Michael Rosen (Cambridge: Cambridge University Press, 1993), pp. xvi–xxiii. The first German edition, requiring heroic editorial supplementation, appeared only in 1936–1938.

tory construct like aether or chemical element requiring justification of a less direct, and ultimately non-inductive sort. The new empirical sciences that Newton was dealing with in the *Opus Postumum* did not lend themselves to this top-down treatment. Kant's attempt to link them to the *a priori* structure of the *Metaphysical Foundations* was thus bound to fail. A quite different kind of inference, neither deductive nor inductive, would be needed for these sciences, one that postulated hypothetical underlying structures of a variety of kinds and would draw its warrant from the tested explanatory success of those structures. It would be a tentative warrant to be sure, weaker by far in appeal than the dramatic transcendental validation of Newtonian mechanics in the *Metaphysical Foundations*. But the growing successes of this retroductive form of inference would ultimately raise a question about the validity of the Kantian top-down approach itself, even in the case of the supposedly synthetic *a priori* Newtonian conceptual structure.*

The drafts that constitute the *Opus Postumum* lay unpublished and largely unread through the decades in which the field concept finally came to fruition in the work of Faraday and Maxwell. So why bother here with Kant's intensive final efforts to bring the new empirical sciences under the umbrella of the transcendental method? Because the growth of these sciences brought a growing confidence in a very different bottom-up method that would go beyond the merely dispositional level of science at which the *Principia* had perforce to halt; it would enable physicists to reach out to underlying ontological structure in an ultimately disciplined, testable, and not merely speculative way. This was, indeed, the further move to the "physical" that Newton had vainly tried to make. Had he but known it, it could not be made from his starting-point. The mechanics of the *Principia* simply did not give enough purchase to make it possible. Where the phenomena of light and color had led him to postulate with some degree of plausibility such constructs as light-rays, light-corpuscles, aether vibrations, and fits of easy transmission and reflection, the phenomena of gravitation seemed to point to action at a distance, permitting no further ontological development. A comprehensive conceptual revolution (as *we* now know) involving even the notions of space and time, would be needed first to make such development possible.

Kant's contribution to the history of the field concept I described above as "mixed." By now it should be clear why this reservation is called for. Though, like Boscovich, Kant departed from Newton's sharp dualism of matter and force, he retained the unmediated action at a distance of gravitation that would yield an epistemic ideal to which the alternative model of continuous action, with its hypothetical constructs, could not aspire. Easier far to treat such constructs as no more than mathematical devices, aids to the imagination, not to be taken in any

* This is a large issue, far beyond the scope of this paper. In "The impact of Newton's *Principia* on the philosophy of science" (to appear in *Philosophy of Science*, 2001), I discuss at some length the peculiarities of the original derivation of the theory of gravitation that led Newton to a mistaken view about the relation of hypothesis to science "proper." What I am further suggesting here is that those same pecularities, allied with the unquestionable success of the theory of gravitation itself, also misled Kant into believing that the transcendental mode of validation, apparently so successful in the case of Newtonian mechanics, in principle ought be capable of being extended to *all* empirical inquiries that could claim the title of "science."

way seriously in ontological terms. Even when Faraday, and later Maxwell, were edging towards claims of "physical existence" for the intermediaries they were beginning to believe to be essential in the transmission of the forces of electrical induction and magnetism, they inserted constant disclaimers of the form: "scarcely giving them the character of opinions" (Faraday, 1852),[41] "merely a collection of imaginary properties" (Maxwell, 1856).[42]

Part of their hesitation, of course, was due to the realization, on Maxwell's part especially, that straightforward mechanical analogies were almost certainly not to be trusted ontologically: whatever else lines of force were, they were not to be understood literally in the language of mechanics. But how else was physical reality to be grasped? Their struggle was to make plausible the claims for legitimacy of an ontology of an altogether unfamiliar sort. What made this even harder was the pervasive prevailing distrust of hypothetical explanatory structures, fostered by the easy epistemic certainties of the action-at-a-distance tradition. If it had not been for Faraday's stubborn persistence in seeking arguments for the physical existence of ontologically odd, but arguably real forms of electrical and magnetic action across space, the notion of a field as something more than a set of mathematical dispositions over space might have been appreciably longer in coming.

The Last Lap

How, in the end, it *did* come about would require a separate essay to do it justice. Still, though our concern here has been with origins, it would seem appropriate, for completeness' sake, to end with at least a brief note on the immediate antecedents of the ground-breaking work of Faraday and Maxwell and a further note about that work itself. In Britain, there had been a long-standing tradition in natural philosophy, dating back especially to the work of Joseph Priestley, notably his *Disquisitions Relating to Matter and Spirit* (1777), which challenged Newton's separation both of matter and force, and of atoms and the void.[43] Priestley proposed instead to reduce matter entirely to the forces of attraction and repulsion. In this perspective, matter is identified with a set of powers occupying space.* James Hutton and John Playfair developed these ideas further; the universal range of gravitation was held to testify to the indefinite extension of matter throughout all of space. Gravitation, therefore, should not be regarded as action at a distance; instead, it is due to the operation of powers that are indefinitely extended throughout all of space. In effect, since gravitation was believed to act everywhere, there *is* no empty space.

* Priestley goes further than does Boscovich in replacing matter by force. The latter still maintains a dualism between the point-mass and the force (changing gradually with distance from repulsion to attraction) for which it is responsible. Priestley's view is much closer to that of Kant, for whom (as we have seen) the forces of repulsion and attraction are constitutive of matter. However, Kant is hesitant to conclude that matter thus extends through all of space where gravitation acts. On transcendental grounds, at least, it would seem that the presence of repulsion would also be required. See Friedman, *Kant*, pp. 218–219.

There is no evidence that these British writers on the matter-force relationship influenced Faraday (figure 5) directly,[44] yet there can be no doubt that they, and Priestley in particular, made natural philosophers in Britain especially receptive to the view that action is propagated by means of quasi-substantial physical "powers" capable of occupying space, though not in the way a traditionally-understood mechanical medium would. Whatever of this, it took years of intellectual struggle on Faraday's part to resolve that his experimental findings on electrical induction could be understood best by supposing that induction works by means of forces that in some sense or other really *fill* the intervening spaces, leaving no gap in the transmission of action.[45] As already noted, he kept reminding his readers of the speculative character of this departure from ontological orthodoxy, that is, from the ontologically non-committal safety of the action-at-a-distance tradition. But as time went on, one can see him become more and more confident of the independent "physical existence," his favorite phrase in this context, of lines of force. Though he

Fig. 5. Michael Faraday (1791–1867). Courtesy of American Institute of Physics Emilio Segrè Visual Archives.

makes occasional use of the term, "field," to refer to the locus of continuous action in the intervening space, he never actually sets out to define it; he clearly expects the reader to know what he means by it.*

What he does labor to prove, however, by extended experiment and persistent argument is the physical existence of the lines of force (as he came to call them) in the case of both electrical induction and magnetic attraction. In his eyes, this would be to prove the reality of the continuous action (equivalently: the field, though he never, so far as I can tell, explicitly uses that term in the context of this argument). Here, however, he faced a problem. In earlier work (1844), he had approved of the view that:**

> Matter fills all space, or, at least, all space to which gravitation extends; for gravitation is a property of matter dependent on a certain force, and it is this force that constitutes the matter.[46]

He cites the "old adage" in its support that "matter cannot act where it is not," but, typically, adds a caution: "it is no part of my intention to enter into considerations such as these."

But enter into them he would have to if he were to make his case about the physical existence of lines of force in the case of magnetic and electrical action. No such case could be made in the case of gravitation, so it was essential to separate off gravitational from electrical and magnetic action. This would be, in effect, to challenge the orthodoxy then ruling on the Continent, in consequence especially of André-Marie Ampère's successful formulation of a law, analogous to Newton's gravitational law, describing the force exerted by one electrical current element on another at a distance from it, without needing to take account, any more than Newton had, of the role, if any, of the medium or of how, exactly, the force was supposed to operate.[47] The implication seemed to be that the same sort of action-at-a-distance model applied in both cases, and, indeed, the preference for this model would persist on the Continent, even after Maxwell showed that electromagnetic action travels at a finite speed, hard to reconcile with the supposition of action at a distance.

The implication of the view of matter that Faraday had earlier espoused was that matter is everywhere present where gravitation reaches. The warrant for this was a set of philosophical arguments, chief among them the "old adage" that Faraday himself had quoted. There was no *experimental* evidence for the physical existence of a material medium throughout space. Though he does not make this explicit, Faraday was thus led to shift his ground: from then on, only experimental argument would be admitted as testimony of physical existence. In that light, gravitation once again becomes indistinguishable from action-at-a-distance (though

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* He uses it for the first time in "On new magnetic actions," *Experimental Researches*, vol. 3, p. 30, mentioning "two directions of position across the magnetic field." In later years, he would refer occasionally to "magnetic fields," as, for example, when in 1850 he describes a gas or a medium as "occupying" the magnetic field, *ibid*., pp. 201–202, or in 1852, balls of iron as being "in a magnetic field," *ibid*., p. 436. But it has to be said that the term remains relatively rare in his writings.

** Attributing it, as we have seen, to Boscovich. His grounds for this claim, curiously enough, were that it "assumes as little as possible," meaning perhaps that it does not impose sharp boundaries on matter. His own view is, in fact, closer to Priestley's than to Boscovich's.

Faraday allowed that if it were found to propagate with finite velocity, he would change his mind). In a landmark paper, "On the physical character of the lines of magnetic force" (June 1852),* he set out to draw as clear a distinction as he could between actions such as gravitation, which, to all (experimental) appearances, at least, operate at a distance, and others, like light-radiation, electrical induction, magnetic force, that exhibit signs of continuous physical action across intervening space.

To that end, he formulated a number of criteria which, in his estimation, could serve to distinguish the two kinds of action. For him the most decisive: does the transmission take time? If, like light, it does, this gives convincing evidence that there is some sort of process going on across the intervening space. Second: is the transmission affected by material changes occurring somewhere in the space-interval, a bending of the lines of force or the creation or alteration of polarities, for example? Light once again provides the clearest affirmative answer, but electrical induction also seems to qualify since its lines of force can be inflected. Magnetic force is more problematic. It does not exhibit the polarization found in light. And the patterns magnets impress on iron filings conceivably could be due to action at a distance of the magnet on the individual filings or of the filings on one another. Still, Faraday "inclines to the view" that magnetic force will be found to qualify under this heading, and explores a variety of promising experimental lines of evidence in the remainder of the essay, as well as in diligent later research. Third: is the transmission, like that of light, independent of the condition of the receiving body? Or is it, as in the case of gravitation, dependent on a property of the receiving body (such as its mass)? In the latter event, the indications are at least consistent with action at a distance.**

We need not be concerned here with the effectiveness of these criteria.*** What was important was Faraday's focusing of both his experimental research and the argument of his published work in those seminal years on the distancing of electrical and magnetic forms of action from the action-at-a-distance paradigm of the later Newtonian tradition. In Newton's own terms, he strove to push beyond the safe ontological neutrality of the "mathematical" that the majority of his contemporaries, especially those in Germany and France, still favored, to an assertion of the "physical" nature of the forms of action of concern to him. At the same time, he would remind his readers that he was making no claim as to exactly what *sort* of physical reality was involved. But it evidently was not to be compre-

* Faraday, *Experimental Researches*, vol. 3, pp. 407–437. The title of the essay is significant. In an essay only six months before, "On the lines of magnetic force" (January 1852) vol. 3, pp. 402–406, he still drew back from what in June he calls the "physical character" of lines of force. He writes: "The term *line of magnetic force* is intended to express simply the direction of the force in any given place, and not any physical idea or notion of the manner in which the force may be there exerted, as by actions at a distance … or what not" (p. 402).

** A fourth criterion is not entirely clear in its application: Is the "sum of power limited" in such a way that if (like light) the power be aimed in one direction, it cannot, without diminution, be aimed at the same time in another?

*** When claiming that the field concept is as applicable to gravitation as to the other forms of action that Faraday seeks here to distinguish from it on "physical" grounds, Stein argues that these criteria on the whole are "not very good," with the possible exception of the first; see "On the notion of field" (ref. 16) p. 304.

hended in the familiar macroscopic categories of the mechanical philosophy that Newton had already begun to transgress in his search for a physical interpretation of the mathematical framework of the *Principia*.

Faraday nowhere ties the term "field" specifically to the continuous form of action he was advocating in the context of electrical and magnetic phenomena. Nor does he explicitly claim that the apparent action at a distance of gravitation does not qualify, therefore, as a field proper. As already noted, he did not look on the term as a technical one. But when he speaks of a "magnetic field," he clearly intends to refer to what is going on in the intervening space between magnet and drawn (or repelled) object. The field is the arena where the continuous action he argues for is occurring. Mathematical dispositions in an empty space will not suffice. One must not, he says, "confound space with matter"; "mere space cannot act as matter acts."[48] Were fields "material," then? Perhaps it would have been more appropriate to call them "physical," his term of choice for magnetic lines of force. The term "material" was too closely tied to the mechanical philosophy of the seventeenth century, as Newton's struggle to fashion an ontology for the *Opticks* and the *Principia* had long before brought home.

Maxwell, of course, is credited with tying up the loose ends in the powerful mathematization he called the "electromagnetic theory." It was, indeed, more than a mathematization: he had gone beyond the "mathematical" to the "physical." He had a theory proper, that is, an explanation and not just a unifying formalism. His addition of a new, and finally effective criterion for the physical existence of a field to Faraday's list: the presence in it of energy, even in the absence of matter conventionally understood, rounds out our story. An abrupt story-ending, admittedly, but the license afforded by the "origins" of our title by now has expired. With Maxwell, the field-concept assuredly had arrived, and the exotic fields of twentieth-century physics were already on the horizon.

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THE GREAT AND THE SMALL

Grook in proportions

When great things
whose greatness
is destined to fall
have turned out
too little
to matter at all,
then stoop
and discover
the great in the small.

Piet Hein