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On the Notion of Field in Newton, Maxwell, and Beyond

In a strict and philosophical sense, it seems to me, there is no such subject as "field theory."[1] There is not, on the one hand, an approximately well-defined physical subject matter (i.e., class of phenomena) that can define such a theory—in the way light, for instance, or electromagnetism, or heat, defines optical or electromagnetic theory or thermodynamics. (It can be argued that today, in the quantum theory of fields, such a subject matter does exist. This is a point of some interest, and I shall comment upon it later. But presumably the scope of the assigned topic was not intended to be restricted to quantum field theory.) On the other hand, I do not think there is a clearly delimited fundamental kind, or form, or structure, which distinguishes field from non-field theories. More precisely: the historically natural use of the term "field" allows its application within many theories, and these are characterized by several features of fundamental interest; but not all the theories share all the features; and I see no convincing reason to designate any special set of features as necessary and sufficient to confer upon a theory the dignity of the appellation "a field theory." I intend therefore to take as my theme, not field theory as such, but the role, in several investigations, of concepts and principles loosely associated with the notion of field. My aim is simultaneously historical and philosophical: for I believe that understanding of the issues controlling particular investigations is a necessary condition for understanding inquiry (data for the philosophy of science can come only from the history of science); and I believe that a clear appreciation of the content of the science we possess—itself, in my view, a primary object of the philosophy of science—is facilitated by, in practice even perhaps impossible without, some attention to the routes by which that science has been acquired. Toward the end of my paper, I shall suggest certain general bearings of the historical part, or of principles illustrated by it, for issues of recent methodological concern. But I must confess that, for me, the philosophical interest of particular scientific investigations does not exclusively consist in what general methodological conclusions may be drawn from them; indeed, the contrary connection is to me more compelling: that one philosophical test of methodological or epistemological principles is their ability to illuminate particular investigations.

I

Historically—and by a great margin—the first physical field to be made the subject of a coherent theory was the gravitational field (which, ironically, is today the least well understood, so far as concerns its incorporation within the set of principles that we have come to regard as fundamental in physics). It is, I think, not usual to associate Newton's name with field theory. But I should like to point out several respects in which Newton's thought has made significant use of notions which, for us, do belong to the cluster surrounding the word "field." Such uses occur at the level of heuristic detail, or "research"; at the level of major conceptual organization, or "fundamental theory"; and—in a way that has become apparent comparatively recently (thanks to the publication by the Halls of a remarkable Newton manuscript)—at a still more general level, for which I can really find no term more apt than "metaphysical."

1. At the beginning of the *Principia* Newton gives us definitions of three quantities, which he calls three different "measures" of a "centripetal force": the absolute quantity, the accelerative quantity, and the motive quantity. These definitions have certain peculiarities of detail which afford

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[1] This paper when presented was introduced by (substantially) the following remark, having particular reference to the paper delivered at a previous session by Mr. Arnold Thackray: "One word of orientation: the principal aim of this paper is clarification—or to promote understanding—of the content and methods of the science of physics. I believe that science to have existed (with a coherent subject matter), and to have had a coherent development, from (say) Galileo's day to our own. Whether work of this sort ought to be countenanced, or damned as 'irrelevant' and out of tune with the times, is a question I shall not discuss."

interesting matter for textual explication; but their essential drift is altogether plain, from the discussion that follows them. By a "centripetal force," in this context, Newton means what we should call a central force field. The motive quantity measures the action of the field upon a body, and is exactly what we call the force on the body. The absolute quantity is intended as a measure of the entire field associated with a particular center. Newton does not specify how this measure is determined,[2] and such a specification would require some more restrictive condition than Newton imposes upon the general concept of a centripetal force; but in the cases that arise, there is no real ambiguity: for force fields governed by what is now called a "Newtonian potential," the absolute quantity attached to any center is the source strength at that center; for gravitational force in particular, it is gravitational mass. As for the accelerative quantity, what Newton truly means by it is the field intensity. This is not what he says in the definition; he says that the accelerative quantity measures the centripetal force by the acceleration it produces. But in explaining his intention, he tells us that the motive quantity may be ascribed to the body acted upon (i.e., as measuring that action); the absolute quantity to the center (as measuring the strength of the entire tendency toward that center); and the accelerative quantity to the *place* at which it is measured—as expressing "a certain efficacy, spread from the center to the individual places around it, to move any bodies that are in them." The conception is unmistakably that of field intensity: a function defined on a region of space, whose value at a point measures the tendency, or "disposition," for bodies at that point to be acted upon. In general, it makes no sense to require the values of this field function to be accelerations; but for the force Newton intends to deal with, acceleration is the proper measure of intensity; and (at the cost of some illogicality) he states the definition as if such were the general case.

[2] In this respect, the definition of the absolute quantity of a centripetal force is comparable to the much-discussed definition of the quantity of matter, or mass. Newton treats "definition" as an expository rather than a logical category; in our stronger sense, the definitions he intends are always to be gathered from his supplementary explanations and from his practice in applying the term in question. In the present case, there seems to be an implicit acknowledgment by Newton of the deficiency of his definition. Each of the other "quantities" is defined as "the measure . . . proportional to" a certain well-defined magnitude; but for the absolute quantity there occurs what seems a deliberately vaguer expression: "The absolute quantity of a centripetal force is the measure thereof that is greater or less according to the efficacy of the cause propagating it from the center through the surrounding regions." (Motte's translation here is slightly inaccurate.)

This so far is mere conceptual trappings; the question for us is, what role does this array of concepts play in Newton's investigation of the solar system and of gravitation? Of course we know that the results of the investigation can be expressed in terms of field intensity, source strength, and force. What I want to suggest is that the investigation itself makes essential use of these concepts in their interrelationship—and crucial use, in particular, of the concept of field intensity.

Indeed, the basic inductive conclusion upon which Newton's solution of the problem of the solar system and of gravitation rests is the following: There is a centripetal force, of which each major body in the solar system is a center, having the properties that (1) the field intensity is the acceleration, and (2) this intensity, for each center considered singly, is inversely proportional to the square of the distance from that center. Consider, for example, the sun and the planets. For the Copernican-Keplerian system of reference we have Kepler's laws. Newton solves these for the accelerations, and deduces the inverse-square law of acceleration, (a) for each planet separately, as its distance from the sun varies by the relatively small amount determined by its eccentricity, and also (b) from one planet to another, over the vast range of distances (but the rather small sample) provided by the actual series of planets. The induction is very convincing. The fact that the acceleration is the field intensity is critical, for the evidence comes entirely from six bodies, each exploring the field in a fixed and severely restricted range; the inductive basis would therefore be rather weak if we were not, by good luck, able to relate directly to one another purely *kinematical*—and, thus, ascertainable—parameters of the several bodies' motions. This lucky fact is not the work of Newton's definitions, but of nature. Newton's merit was to know how to use what he was lucky enough to find. And my point is that his analysis of the situation makes an essential use of the notion of a field. To regard the planets as test bodies, exploring a field, may seem a vivid and suggestive way of thinking, of heuristic value in searching for a theory—and I think that is so; but it is not the claim I am now concerned to make. My claim is both simpler and more far-reaching. That the accelerations of the planets, severally and collectively, are inversely as the squares of their distances from the sun is not the conclusion of Newton's induction; that is his deductive inference from the laws established by Kepler. Newton's inductive conclusion is that the accelerations toward the sun are *everywhere*—i.e., even where there are no planets—determined by the position relative to the sun; namely, directed toward that body, and in magnitude inversely proportional to the square of the distance from it. And although the inductive argument is very straightforward—certainly not dependent upon tortuous constructs—that argument cannot be made, because its conclusion cannot even be sensibly formulated, without the notion of a field. From a mathematical point of view, the idea of an acceleration attached to each point in space is the idea of a function on space, hence a field; from the physical and methodological point of view, the idea of an *acceleration* characterizing a *point where there happens to be no body* makes no sense at all, unless one accepts the notion of a disposition, or tendency; subject to probing, but not necessarily probed.

This conclusion is reinforced by a careful analysis of the later phases of Newton's argument.[3] First, he arrives at a similar result for the acceleration fields explored by the satellite systems of Jupiter and of Saturn (considered separately). In these cases, the spatial reference frames are of course different; the central body is always taken as at rest. The evidence here is more slender: not only because there are fewer test bodies, but especially because the accuracy of the data allows Newton only a degenerate analogue of Kepler's first two laws; namely, that the orbits are roughly circular, traversed with constant speed (so that no evidence is present for the inverse-square law within the motion of a single body). Next he finds the same for the moon, in its course round the earth; but here the reverse restriction obtains, since there is only one test body: only over the small range of distances from the earth explored by the moon do we have direct evidence for the inverse-square law. The relation is, moreover, within this range, noticeably inaccurate—a fact which Newton (of course correctly) attributes to the influence of the sun's field. This leaves a very weak direct basis for the law as applied to the moon over such a range of distances as from its actual orbit down to the very surface of the earth. But just this application is required by Newton, in order, by comparing the moon's acceleration with the acceleration of falling bodies at the earth's surface, to infer the identity of the centripetal force of astronomy with the terrestrial force known as weight or gravity. It is plain that what justifies this procedure is the fact that the relationships established for the several systems, taken collectively, form a *single* body of evidence for the proposition I have formulated earlier (namely, that each major body is the center of an

[3] "Later" in the order I am sketching here; in Newton's exposition, the proposition about the satellite systems of Jupiter and Saturn is stated first. This is of no moment.

inverse-square acceleration field); so that the data from the planets and the other satellite systems support the conclusion about the moon.[4] Indeed, terrestrial evidence enters obliquely to provide more support; for while no direct astronomical evidence supports the proposition that the region about the earth carries an "acceleration field," Galileo's law of falling bodies tells us precisely that.

I shall dwell no further upon details of this investigation, but only remark that it involved the assumption that the reaction to the centripetal motive force of gravitation is exercised upon the central body—an assumption which, as Huygens pointed out and as Newton seems to have implicitly acknowledged, enters the argument as a pure hypothesis; and that it also involved the fitting together of the several fields, with their several spatial reference frames, into a single coherent spatial system—which is then demonstrably unique up to a uniform translational velocity. I have discussed these aspects of the matter in another place.[5] What I shall turn to next is what I have referred to as the level of "fundamental theory." (Of course, the theory of gravitation itself, in the definitive formulation that Newton found, has its place at this level.)

2. The general conceptual framework within which Newton placed both his own work and his program for natural philosophy is expounded by him in several places with great clarity. He takes for granted the notions of space and time, with all of Euclidean geometry and the classical kinematics of absolute motion. He assumes the notion of bodies, located in space but not necessarily—and indeed not actually—filling it; and he pictures these bodies as constituted of minute (but not punctiform) indivisible particles. (So far as Newton's positive work is concerned, the hy-

[4] I do not mean here to take a position on whether induction may involve general propositions in an essential way or is always from particulars to particulars. Even if a satisfactory technical analysis of the logic of induction—which, despite much very good work, we certainly do not yet have—should rest upon the latter alternative, the points I have made above would stand: (1) The particular conclusion about the moon cannot be formulated without a dispositional term or a contrary-to-fact conditional. (2) If the inference can be made at all, it must support the general law of the field in the sense of Carnap's "instance-confirmation": there is clearly no basis for a particular statement about a body transported between the earth and the moon that does not equally support an analogous statement about analogous transport within the field. (3) The conclusion must rest upon the diverse sources of data I have described; therefore the logical analysis, however its details are organized and whatever the special language in which it is couched, must allow essentially that assimilation of "kinds" which is expressed here by saying that (a) all the satellites of one body explore "the same field," and (b) the fields of all the centers involved are of the same sort.

[5] "Newtonian Space-Time," *Texas Quarterly*, 10 (Autumn 1967), 174–200.

pothesis of indivisible particles really plays no very important role.) Finally, he assumes a general notion for which he uses the word *force*, or *vis*. In Newton's most basic usage, a force is a principle of motion: to know a particular force is to know an associated law of motion. One such force is both universal and intrinsic to all bodies: the *passive* principle of motion, or force of inactivity, *vis inertiae*, whose expression in the form of law is the three famous laws of motion.[6] Another universal principle is *impenetrability* (although I do not know of a place where Newton calls this a "force"): a principle by virtue of which no two bodies can be simultaneously present in the same point of space. But the object of greatest attention in Newton's natural philosophy is the class, largely unknown (or known very imperfectly), of *active principles*. These are the forces that can produce, and more generally can change, motion; and Newton assumes—presumably as involved in the third law (namely as asserting that the force of inertia qua "resisting" force must always have an object to exercise resistance upon)—that they are principles of *interaction* of bodies. When such a force acts, or is "impressed," upon a body, Newton speaks of "an impressed force"; the latter "consists in the action only; and does not remain in the body after the action." Impressed force is, therefore, the particular and fluctuating action of "force" in its fundamental sense; the two categories are distinct, and when Newton says that impressed forces are of various origins, one of which is centripetal force, he is not committing a blunder in saying that a force comes from a force.

The chief problem of natural philosophy, then, according to Newton, is the discovery of the active principles, or forces of nature—and then the proof, i.e., test, of the discovered principles, by their application to natural phenomena. The discovery itself is to be accomplished by the study of phenomena, and especially phenomena of motion. This proposal is, of course, a direct generalization of Newton's actual performance in discovering the principle or law of universal gravitation. Newton gives many, and convincing, reasons for believing that the principal phenomena of matter

[6] This is not speculative interpretation of Newton's doctrine, but what he states explicitly and with precision; so, in all pedantry, the "law of inertia" for Newton is not just the first law of motion, but all three together. Cf. *Principia*, Definition III and the paragraph that follows it; but especially *Opticks*, book III, Question 31, toward the end (Dover ed., p. 401): ". . . a *Vis inertiae*, accompanied with such passive Laws of Motion as naturally result from that Force . . ."; and with no possibility of misconstruction (*ibid*., p. 397): "The *Vis inertiae* is a passive Principle by which Bodies persist in their Motion or Rest, receive Motion in proportion to the Force impressing it, and resist as much as they are resisted."

depend upon fields of force, both attractive and repulsive, among the particles of bodies; and he devoted great effort to the attempt to gain information about these fields. The most striking example is the interrupted inquiry reported at the beginning of the third book of the *Opticks*. In the preceding book, Newton had made an attempt to gain from optical phenomena an estimate of the sizes of the particles of bodies,[7] and also an estimate of the power—which must be regarded as an "absolute quantity" of centripetal force—of various particles to act upon light. He had obtained some results,[8] which he clearly regarded as plausible and useful, but as insufficient, both in content and in degree of support, to serve as the foundation of a theory. In view of its place in the structure of the *Opticks*, and of the character of the Queries that Newton supplied as substitute for the investigations he was unable to carry out, it seems to me beyond reasonable doubt that the study of the phenomena of "inflection," or diffraction, was undertaken by Newton with the chief purpose of determining the basic law of interaction of particles of bodies and light—in other words, the law of the "optical field"; and in the hope that the success of this undertaking not only would represent a fundamental advance in optics, but would afford the means for progress in the study of the structure of matter. The method of investigation is clear: The test bodies for exploring the field in question are the rays of light. Within transparent bodies, these rays are in equilibrium, and move uniformly; at a reflecting or refracting surface, the behavior of the rays is accounted for by very general assump-

[7] Not of the "ultimate" particles, but of those parts of bodies, separated by interstices, "on which their Colours or Transparency depend" (*Opticks*, book II, part I, near the beginning; Dover ed., pp. 193–194). These are not ultimate particles, in Newton's opinion; he believes them to be transparent (*ibid*., part III, Proposition II; Dover ed., p. 248), hence penetrable by the particles of light, hence not the ultimate impenetrable particles. (Although in the proposition just cited Newton speaks of the "least parts" of bodies, the argument he gives applies to "very small" parts; one must take him to use the word "least" colloquially in this sense. That Newton considers these parts themselves to have an internal structure is clear from his statement—*ibid*., end of Proposition VII; p. 262—that the sense of vision, aided by microscopes, may eventually reach these parts; but, he fears, no further: "For it seems impossible to see the more secret and noble Works of Nature within the Corpuscles by reason of their transparency." Cf. also the discussion of Proposition VIII, where Newton alleges the probability that any rays of light that actually do strike any one of "the solid parts of Bodies" are absorbed, or as he says "stifled and lost in the Bodies"; and concludes that "Bodies are much more rare and porous than is commonly believed"—*ibid*., pp. 266, 267.)

[8] For instance, on the second question, he formulates the tentative conclusion that it is either exclusively or preponderantly the "sulphureous Parts" that interact with light to bend its path; hence that the power of a body to reflect and refract light is proportional to its sulphureous content—which in turn he thinks is for the most part nearly proportional to the density of the body (*ibid*., Proposition X; Dover ed., p. 275).

tions about the character of the field; but the complexity of the phenomena of diffraction suggested that a careful determination of the light paths very close to the edge of an opaque body would reveal a remarkable structure in the field, involving intensities alternately in opposite directions (i.e., forces which at certain distances are attractions, at others repulsions). As Newton was very well aware, forces of this type are essential for an account, in the terms of his program, of the behavior of solid bodies.

I have so far argued, first, that in Newton's investigation of gravitation the notion of a field plays a logically uneliminable role in the inductive evaluation of the evidence; and second, that that notion has a central place in fundamental theory for Newton—not only in the expression of particular fundamental laws (like that of gravitation), but far beyond this, in what (following Peirce) we might call Newton's "abductive" scheme for reducing all the phenomena of nature to order. Perhaps it will be objected that the second claim appears to give away the issue of action at a distance: if fields, or centripetal forces, constitute a fundamental category for scientific explanation, then we seem to have excluded the possibility of accounting for all phenomena by interactions through contact alone. This is not what I mean to imply. Discussion of the point will afford the transition to what I have referred to as the metaphysical level of Newton's thought.

3. Newton tells us emphatically and repeatedly that our only source of knowledge of corporeal nature is experience. This precludes dogmas, whether positive or negative, about the interactions of bodies. It also precludes the claim to direct insight into the nature of *any* interaction, of *any* sort: that is, for Newton (at least if he is consistent), the claim that we know how bodies can interact by contact, or more specifically how they can communicate motion in impact, in any other sense than that we have observed interactions of this type to occur, must be delusive. I believe that Newton is consistent, and that this is his view. In the light of this view, centripetal forces when gathered from experience—as gravity was, through Newton's beautiful analysis; and as at least the existence of other such forces, both attractive and repulsive, can be, through reasoning he presents at length—have as good a right as any other principles to legitimate status in philosophy: they are then, he says, "manifest Qualities." They are *verae causae*, and the evidence for their existence and manifold role in the processes of nature is just what confers upon them fundamental importance. But none of this excludes a search for the causes of these principles themselves: that is, we know they occur; but we do not know that they—or any particular ones among them—are irreducible. Nor do we know them to be reducible. The question, in each case, is therefore a proper and necessary one for natural philosophy: whether or not a particular centripetal force, such as gravitation or electricity or magnetism, whose own laws have been found out, is the effect of deeper-lying structures; and if so, what those structures and their laws are.

But what of action at a distance, and what of ultimate explanations? We know that Newton was troubled about the cause of gravity: despite his principles, as I have explained them, he was far less content to rest upon gravitation than upon impenetrability as an irreducible property of bodies; and he goes so far as to say, in his well-known letter to Bentley, that it is *inconceivable* that matter should affect other matter without mutual contact, except by the mediation of something else, or the action of some agent, material or immaterial. The last qualification, "material or immaterial," is disconcerting; and when we read in a letter by Newton's protégé Fatio de Duillier, dated thirteen months after the letter in question to Bentley, that Newton is undecided between two opinions about the cause of weight—(1) that it is caused by the impacts of streaming cosmic particles, Fatio's own hypothesis (later taken up by Le Sage); (2) that it is caused by "an immediate Law of the Creator of the Universe"—I think we are apt to be not only puzzled but annoyed, and to feel that Newton is quibbling. In point of fact, I cannot altogether acquit him of this: it is the case, as I read Newton, that bodies attracting by an immediate law of God means for him exactly the same thing as direct action at a distance. I think Newton knew this equivalence clearly, and disguised it in his public utterance to avoid unwelcome embroilments. But although his statement is (on my reading) evasive, it is characteristically precise and accurate; and if it involves a quibble, there is something interesting behind it.

What sets this whole matter in a light that seems to me to clear up all obscurities is the fragment *De gravitatione et aequipondio fluidorum*, first published (with a translation) by the Halls in 1962, in their selection from the Portsmouth Collection of Newton manuscripts.[9] The largest part of this unfinished paper is a rather extended philosophical discussion of space and motion, and of the nature of body. I have had occasion elsewhere to

[9] A. Rupert Hall and Marie Boas Hall, *Unpublished Scientific Papers of Isaac Newton* (Cambridge: Cambridge University Press, 1962).

refer to this document in connection with the former subjects;[10] what is relevant now is Newton's treatment of the nature of body (which he takes up in order to clarify and defend his divergence from Descartes).

Newton's exposition takes the form of an account of how God can create matter. He does not claim to establish the true method of creation, or the true and essential nature of bodies; but only a possible method, and (correlatively) a possible representation of that essential nature: for, he says, "I have no clear and distinct perception of this," and cannot therefore certainly know whether God in fact might have created beings in all ways like bodies with respect to phenomena, and yet different from them "in essential and metaphysical constitution"; although it seems scarcely credible that this should be so. (There can be no doubt that this is partly ironic; the parody of Descartes is obvious, and a little labored—a characteristic of the style of this paper, in marked contrast to Newton's usual lean and vigorous prose.[11]) To explain, thus hypothetically, the creation of a body, Newton postulates the potency of God's intellect and will; he does not pretend to make the power of God's will itself intelligible, but merely points out that we have a power of moving our own bodies, and that this power is no more intelligible to us than the power he will assume for God. This does not (as one might suppose) altogether trivialize his task: what he has supposed, in effect, is that God can realize any intelligible objective; what remains is to make the objective, "creation of a body," intelligible.

[10] See note 5.

[11] The Halls (*Unpublished Scientific Papers of Isaac Newton*, p. 89) indicate considerable uncertainty about the date of this work; but suggest, on the basis of several considerations, including this stylistic one, that it is quite early. This conclusion seems plausible enough; but I should like to suggest two reasons for considering the question further—if clearer evidence can be obtained. In the first place, the Halls suggest evidence of immaturity in the thought of this essay, as well as in its style. In my opinion, that view is unjustified. In the paper cited earlier (note 5 above) I have explained why I consider Newton's treatment of the problem of space and motion, in this essay, to be trenchant and deep; and this in explicit comparison with both Descartes and Leibniz on the same questions. In the present discussion, I make a similar assessment of the quality of Newton's treatment of the nature of body. Thus I think that this work, for all its prolixity and lack of stylistic balance, is in its content a profound piece of philosophy. As to the style, it has to be considered that the work is only an interrupted draft—Newton's drafts in general seem to be prolix and rambling, compared to his finished writings; and that it is on an unaccustomed subject, or at least in an unaccustomed mode, for Newton—a circumstance that might account for the awkwardness of the exposition.

In the second place, there is oblique historical testimony that points to a rather late date for the work. The analysis of God's creation of matter given here by Newton is the same as that which, according to the French translator Pierre Coste, is hinted at in an

For clarity, Newton assumes the world to exist already, and makes the problem the creation of one new body. What God must—or, rather, may—then do, he says, is this: (1) Make some region or other of space impervious to the existing bodies—i.e., simply choose not to allow the motions of those bodies to penetrate that region. (2) Having established a region of impenetrability, allow it, in the course of time, to migrate from one place to another; or more precisely, confer the *property* of impenetrability, not on a fixed spatial region, but on different regions at different times—and in such a way that the mutations of the distribution of impenetrability constitute a smooth motion in space. In doing this, moreover, ensure that the motion of the new region of impenetrability and the motions of the already existing bodies together satisfy certain laws. (3) Confer upon the mobile impenetrable region the property, or power, of affecting our minds (in sensation), and being affected by them (in volition), whenever it comes to occupy the place of some particle in (say) a brain, now possessed of that power. Newton remarks that the first two steps, the meaning of which seems to involve no obscurity whatever, already suffice to make something indistinguishable in almost every respect

obscure passage of Locke's *Essay concerning Human Understanding* (book IV, chapter x, article 18; see the edition edited by Alexander Campbell Fraser, vol. II, p. 321, n. 2, for Coste's statement). Coste received his information from Newton, who, he says, "told me, smiling, that he himself had suggested to Mr. Locke this way of explaining the creation of matter; and that the thought had struck him one day, when this question chanced to turn up in a conversation between himself, Mr. Locke, and the late Earl of Pembroke." Now, Coste may easily have misunderstood Newton: it is possible that Newton said no more than that he had suggested this thought that day, and that Coste, by immediate misconstruction or later elaboration, fancied him to mean that he had then thought of it for the first time. It is also possible that Newton exaggerated the spontaneity of his thought on that pleasant occasion with Locke and Pembroke. But the possibility that both Coste and Newton are accurate in their testimony ought not, I think, to be merely dismissed.

In any event, there can be no doubt that Locke did receive this argument from Newton, and that this is responsible for the change he made in the passage in question in the second edition of his *Essay*; therefore, Newton communicated the argument to Locke some time between 1690 and 1694; and therefore, however early its conception, he had not by then rejected it. Similarly, the agreement of the views on space, time, and motion, in this fragment, with the views stated in the *Principia* (the former being a more extended elaboration of the latter) shows that on this subject too our fragment contains the mature opinions of Newton. The interest, therefore, in the date of composition of the fragment does not have to do with assessing its relevance to Newton's mature thought; this appears to be beyond doubt; but only with the comparatively minor question of obtaining evidence for the periods at which Newton had arrived at his mature views.

I should like to add that the existence of this fragment and its connection with Locke's *Essay* were brought to my attention by a reference in Alexandre Koyré, *Newtonian Studies* (London: Chapman and Hall, 1965), pp. 91–93.

from a new body: something that would interact with all matter like any particle of matter, and that would constitute a sensible particle (since it would act like any other particle upon a sense organ). The third point—direct interaction with a mind—does indeed involve obscurity; but this resides in the deficiency of our understanding of mind; and Newton reminds us that the power, however obscure, of interaction between mind and body was the starting point of his analysis. He does insist upon the essential need for the third step, on the grounds that the operations, both active and passive, of our minds seem to involve the brain, and there seems to be a continual exchange of matter between the brain and the rest of the world; so, he says, it is manifest that the faculty of union with mind is in all bodies.

In summarizing the chief points of this analysis of the nature of matter, Newton puts first the fact that he has altogether dispensed with the unintelligible notion of substance. Or rather, since in the second place he puts the point that his beings "will not be less real than bodies, nor less able to be called substances," one should say that without *positing* an *obscure* notion of substance, he has been able to *construct* a *clear* one. Newton says that "the preconception"—namely, "of bodies having, as it were, a complete, absolute, and independent reality in themselves"—"must be laid aside, and substantial reality is rather to be ascribed to these kinds of attributes which are real and intelligible things in themselves, and do not need to inhere in a subject, than to some subject which we cannot conceive as dependent—nay, more, cannot form any Idea of." He hints that a similar analysis might be possible even of the nature of God; but adds at once that "while we are unable to form an Idea of [God's power], nor even of our own power by which we move our bodies, it would be rash to say what may be the substantial basis of mind."

I want to come back to this piece of Newtonian metaphysics later, and to say why I consider it deeper philosophy than, say, the obvious analogues in Berkeley and Hume. But at this point I think it is time to make the connection with field theory.

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. A body, Newton tells us, is a region of space endowed with certain properties. The clarity, or intelligibility, of the properties Newton specifies consists in the fact that they are conceived ideally as testable: they are dispositional characteristics of the spatial region, like the field quantities of gravity or electromagne-

tism. In particular, Newton takes as basic what it is quite natural to call the "impenetrability field"—a two-valued function on space (or rather space-time), since impenetrability either is there or is not. If, as Newton assumes for simplicity, we take for granted the existence of observable test bodies, this field is ideally testable per se. But to reduce the notion of body altogether and ultimately, Newton says, we also need the notion of the faculty of interacting with minds; and we need this notion in any case to represent our experience of nature. Newton postulates, therefore, what he thinks that experience suggests, namely that the distribution of this faculty of interaction coincides with that of impenetrability. Further, according to the Newtonian abductive scheme, there must be (1) a field of inertia, whose value at a point gives the mass density there (and whose "support"—i.e., the set of all points at which it is non-zero—coincides with the set on which impenetrability exists), and (2) such other fields—or, more generally, laws of interaction—as investigation of nature discovers. In any case, what I have called "ideal testability" is, for impenetrability as for other fields, testability only in a Pickwickian sense: I have already referred to the strenuous efforts which Newton devoted to the search for information, not even about the *ultimate* particles of matter, but about what we might call the molecules of bodies. And of course the very existence of the impenetrability field is a hypothesis. Newton tells us that we know of it only through experience, and reviews the inductive evidence for it;[12] but that evidence is less convincing than Newton thought—or it seems so, at least, with hindsight, in the light of evidence obtained later. Newton knew that induction is fallible; and I suggest that it would have surprised him, but not disconcerted him, to learn that the ultimate fields of impenetrability had been replaced by conceptions of another sort. I suggest, more specifically, that the program of deriving the properties of matter from a pure field theory in which the field variables are continuous (rather than two-valued)—a program so influential in the early decades of this century—constitutes, with respect to Newton, a deep revision of what I have called his fundamental theory, but demands no essential change in what I have called his metaphysics. The received idea that the Newtonian system involved a fundamental dualism of matter and force, or of substance and field,[13] is true of the system of physics that Newton was led to de-

[12] E.g., *Principia*, book III, discussion of the third rule of philosophizing.
[13] See, for example, Hermann Weyl, *Philosophy of Mathematics and Natural Science* (Princeton, N.J.: Princeton University Press, 1949), pp. 165–177.

velop; but when applied to Newton's own most basic conceptions, that idea is wrong.

But I have promised to relate all this to Newton's curious statements about action at a distance. The point seems to me this: In one sense—at the level of metaphysics and of theology—there is absolutely no difference in status, given Newton's analysis, between action through contact and action at a distance. Neither is intelligible from the mere conception of body as extended substance (whether filling space, as Descartes would have it, or occupying parts of space, in accordance with the atomists); both are intelligible from the conception of bodies as fields of impenetrability and inertia, accompanied by interaction fields. The arbitrariness in the specification of interaction fields—represented in Newton's account by the dependence upon God's fiat—is no greater than the arbitrariness in establishing impenetrability, inertia, and laws of interaction by contact. So when Newton tells Bentley, "It is inconceivable, that inanimate brute Matter should, without the Mediation of something else, which is not material, operate upon, and affect other Matter without mutual Contact," what he says truly expresses his views; and would still do so if he left out the words "without mutual Contact." On the other hand, at the level of fundamental physics, the situation is not quite so parallel between the two modes of interaction. For Newton did consider impenetrability to be *the first basic property of bodies*; and this means that interaction by contact (should contact ever indeed occur) is a *necessity*—a direct consequence of the fact of impenetrability; whereas interaction at a distance would represent, so to speak, a *further* arbitrary decision of God. It is in this sense, I think, that Newton's repeated denials that he holds gravity to be essential to bodies have to be understood. And this consideration certainly influenced Newton to consider seriously the possibilities of such a force as gravity being caused by a material medium. But yet again on the other hand, some of the considerations advanced in the *Opticks* tend rather persuasively to the conclusion that there is no reasonable hope of reducing all the forces of nature to effects of impacts of particles. Therefore, I think, Newton's views on this deep question in physics were in a state of considerable tension. Moreover, setting aside the issue of ultimate and total reduction, the possibility (in any given case) would remain that some force of nature already discovered, some field made manifest by the analysis of phenomena, had underlying causes that were still occult; this possibility would always call for further investigation. Newton's first re-

mark to Bentley on this subject was: "The Cause of Gravity is what I do not pretend to know, and therefore would take more Time to consider of it." When, later, he declared the inconceivability of "inanimate brute Matter" acting at a distance without immaterial mediation, he added: "And this is one Reason why I desired you would not ascribe innate Gravity to me." I have tried to suggest what his other reasons were. I think he was altogether sincere in saying that he did not know the cause of gravity, and wished to consider it further; but I think he feared that to say plainly all he thought about the question would make great trouble for him.

II

I had intended, originally, to devote as much space to the notion of field in the work of Maxwell, and as much again to developments from Maxwell to Einstein, as to Newton; but this intention has clearly been defeated. I must confine myself to a rapid series of remarks on these subjects, and a brief sketch of some general conclusions.

1. Maxwell discovered that the centripetal forces of electricity and magnetism are effects of a deeper-lying structure. To be more accurate, he discovered that all the known laws of electrical, magnetic, and electromagnetic phenomena are explicable as effects of such a structure; that some (although not all) previously obscure points in the subject are put into satisfactory condition in this new theory; that very definite new phenomena (not, however, easy to realize experimentally) are predicted; and that the same structure which has among its effects, according to the theory, the phenomena of electricity and magnetism will also account for the behavior of light.

What is the character of the underlying structure found by Maxwell? His statement is a model of lucidity: "The theory I propose may . . . be called a theory of the *Electromagnetic Field*, because it has to do with the space in the neighbourhood of the electric or magnetic bodies, and it may be called a *Dynamical* Theory, because it assumes that in that space there is matter in motion, by which the observed electromagnetic phenomena are produced."[14] It is well known that Maxwell's first account of his theory[15] was based upon a quite detailed model of the arrangement, connections, and motions of this postulated matter. Clearly, then, we have a

[14] "A Dynamical Theory of the Electromagnetic Field," *The Scientific Papers of James Clerk Maxwell* (Cambridge: Cambridge University Press), vol. I, p. 527.

[15] "On Physical Lines of Force," *ibid.*, pp. 450–513; see especially part II, pp. 467ff.

theory that fits entirely within the Newtonian abductive scheme. But this theory had two rather serious defects: The postulated details were far too detailed, in the sense that things were supposed for which there was no real basis, even of a merely suggestive kind, in the evidence (this is exactly the kind of "hypothesis" that Newton so strongly deprecated in natural philosophy: detailed models of processes that might well be altogether otherwise). And the postulated details were not really self-coherent; one can envisage clearly a small piece of Maxwell's dynamical system, and its behavior over a short time; but how these pieces could fit together, globally, over a large portion of space, and how they could be conceived to behave over an extended time, is a baffling problem. Maxwell, therefore, in a truly fine piece of philosophical self-criticism, subjected his theory to an analysis, and showed that the essential contents of that theory—all of it that had a bearing upon known phenomena—could be preserved independently of any detailed account of the medium, if one only posited certain functional dependencies (for which good evidence could be cited) of the kinetic and potential energies of the medium upon the electric and magnetic field variables themselves.

This situation presented three essentially different sorts of fundamental problem for further investigation: (a) to test the theory—for itself, and in comparison with competing theories—by experiments designed to realize the new phenomena it predicted, and to check any discrepancies among the predictions of the several theories; (b) to perfect the theory, by extending it to those electromagnetic or optical processes which Maxwell did not deal with fully—namely, to the processes in ordinary material media, both at rest and in motion; (c) to perfect the theory in respect of its foundations, by finding out more about the characteristics of the underlying medium than, according to Maxwell's analysis, is actually revealed in the electromagnetic processes that his theory treats. (Of course, (b) and (c) might well be expected to develop interdependently.) However, this account is a historical oversimplification. It contains no suggestion of the fact that for a generation following Maxwell's publications the Maxwell theory itself was very widely regarded as extremely obscure: that is to say, many competent people found it difficult to determine just what Maxwell's theory said.[16]

[16] The historical facts seem to me to merit investigation. One repeatedly finds the statement that Maxwell's theory had no real influence in Germany, and, more generally, on the continent of Europe, until Hertz's work of 1887. Yet Helmholtz, in Berlin,

Part of the difficulty stemmed from the notion of the "displacement current." This appears in Maxwell first as a consequence of a very special characteristic of his detailed model. In the refined version, the displacement current is retained, alongside—not as a consequence of—the general dynamical assumptions about the medium; and Maxwell's exposition (both in his definitive paper and in the later *Treatise on Electricity and Magnetism*) is very cryptic on the matter: one sees clearly neither what motivates nor what justifies the introduction of the displacement current; and the physical content of the hypothesis is obscure (because it is the one point of detail assumed about a medium that is otherwise left vague).[17]

2. The greatest merit for removing the obscurities of the subject belongs to Hertz. The essential general content of his contribution to this question is the following point: Maxwell's theory is independent of any solution to the problem I have stated above under (c)—the problem of a detailed account of the medium. This is the meaning of Hertz's famous aphorism, "The Maxwell theory is the system of Maxwell's equations." That remark, I think, has been treated by philosophers in a somewhat skew perspective. In Hertz, it has a good deal less to do with any quasi-positivist epistemology than with a concrete scientific judgment. The question "What is the Maxwell theory?" was an immediately serious one: scientists were having difficulty in deciding what the theory was, and in

took the theory very seriously as early as 1870, and encouraged a series of experimental tests of points related to it, in his laboratory and by his students; a series that culminated, of course, in the great discoveries of his student Hertz. And Gustav Wiedemann's encyclopedic treatise *Die Lehre von der Elektricität* (4th edition; vol. 4, part 2, Braunschweig: Friedrich Vieweg und Sohn, 1885) presents Maxwell's theory and its extensions by Helmholtz as the culmination of the theoretical development of the subject. It seems possible that local differences of perspective were important here, and that the tendency to suppress the Maxwell theory was most characteristic of Göttingen, where W. E. Weber's influence was dominant. The case of W. Thomson (Lord Kelvin) in England shows, on the other hand, that difficulty in comprehending Maxwell's theory was not restricted to the continent; and the testimony of Poincaré and of Hertz shows clearly that there was a real difficulty, not only for opponents of the theory.

[17] The difficulty appears to have been increased by the fact that Maxwell's *Treatise* was more widely studied than his paper. The *Treatise* is an admirable and fascinating work; but it is moderately challenging, even as a task of scholarship (with the help of good prior knowledge of Maxwell's theory), to locate the principles of that theory in this wide-ranging book. The paper, on the other hand, seems to me remarkably clear, and it is hard to believe that it could have occasioned the kind of bafflement that occurred. Is it possible that the *Philosophical Transactions* were, on the continent in contrast to England, less accessible than Maxwell's *Treatise*, and that this is partly responsible for the slower penetration of Maxwell's ideas on the continent? (Cf. note 16 above.)

particular in formulating what exactly the theory assumes about the ether. Hertz's answer does not exclude from the domain of scientific inquiry the problem of the ether; it says that this problem stands to Maxwell's theory of the electromagnetic field in the same relation as the problem of the cause of gravity to Newton's theory of gravitation, or the problem of the molecular nature of heat to the theory of thermodynamics. That Maxwell's theory is Maxwell's equations is even, in part, a biographical judgment: this is what Maxwell himself offered for us to believe, in contrast to what he left for our further investigation. As applied specifically to the displacement current, Hertz's dictum meant that to understand that hypothesis is to understand the role of the displacement-current term in Maxwell's equations; to test the hypothesis is to test the consequences of the theory that depend critically upon that term—and this, too, of course, it was Hertz's merit and glory to accomplish, thus solving the chief part of our problem (a).

3. Problem (b)—the extension of the theory to processes in material media (of which Maxwell gave only a tentative, preliminary, account)—was chiefly advanced by the electron theory of Lorentz. Passing over all details, I remark only that an ultimate radical consequence of this work was the elimination of the ether altogether as a material medium; that is, a thoroughly negative answer to what I have called problem (c).[18]

One reason why this result was radical is that it implied, for the first time in the history of physics since Newton, a failure of Newton's abductive scheme. The state of affairs is not, in this respect, parallel to the case of gravitation, where the nonexistence of a material medium propagating gravitational force has no such consequence—where, indeed, gravitation without a medium is a paradigm case for that scheme. The difference consists in the circumstance that, whereas Newtonian gravitational interaction is supposed to be instantaneous, Maxwell's fields are propagated with a finite speed; in the more abstract but closely related consideration, that a Maxwellian system with a finite number of charged mass points (indeed, even one with *no* such points) has infinitely many degrees of freedom; and finally, in the physically fundamental conception of Maxwell's theory, that the dynamical quantities energy, momentum, and angular momentum are attributes of the field. Maxwell applied the dynamics of Lagrange, conceived as a mathematical transformation of the dynamics of Newton, to the field. When the field could no longer be conceived to be a Newtonian material structure, this application proved, in retrospect, to have amounted to a redefinition of the scope and the presuppositions of dynamics.[19]

With reluctance I must, for reasons of space, forgo a more thorough discussion of this whole development, and make only some general remarks that lead me to my philosophical summing-up. It has been suggested sometimes that the notion of the "reality" and "self-subsistence" of the field in the developed Maxwell theory can after all be regarded as a convenient fiction: the theory allows one to compute the (delayed) interactions between charges, and one may regard these as what is real, eliminating the field except as a possibly convenient device of calculation.[20] Now, in fact, there are technical reasons for objecting to this account of the theory; but I cannot go into these here. The point I wish to make is that such a proposed elimination of the field—in contrast to the successive eliminations of characteristics of the ether in Maxwell's self-criticism,

[18] This elimination really had two phases in the development of the Lorentz theory. In the first place, Lorentz assumed that bodies move freely through the ether—i.e., that the ether is to no degree carried along by the motion of a body—i.e., that there is one fixed global spatial reference system with respect to which Maxwell's "equations for the free ether" hold everywhere, independently of the local state of motion. Since a material medium capable of internal motions, such as Maxwell envisaged, could hardly possess this absolute global rigidity, Lorentz's assumption seemed tantamount to rejection of such a medium. In the second place, the negative results of all experimental tests for effects of the velocity of bodies relative to the ether, and the modifications of the theory to take account of these results, which culminated in the special theory of relativity, led to the conclusion that no absolutely distinguished reference system exists. So the ether, having first been deprived of susceptibility to changes in its state of motion, having thus its unique, unchanging state of motion as its one remaining "material" property, finally lost this property as well, and therewith its last hold upon existence.

[19] This redefinition, which was not contemplated by Maxwell, was what Einstein (justly, I think) referred to as "Maxwell's contribution to the idea of physical reality."

[20] Ernest Nagel, in *The Structure of Science* (New York: Harcourt, Brace and World, 1961), p. 396, makes such a remark, referring for support to the textbook of Mason and Weaver. Emil Wiechert, who contributed significantly to the theory (indeed, who discovered the main principles of the electron theory independently of Lorentz), emphasized at the end of his excellent review monograph *Grundlagen der Elektrodynamik* that "the electrodynamic phenomena can be viewed quite universally as superposition of interactions between the single material particles, which occur for each pair as if it alone were present." But this remark was not intended by Wiechert to imply an elimination of the field. It occurs in the context of a graceful historical appreciation of Weber's work, as a demonstration that there is no impassable chasm between the conceptions of Weber and those of the new electrodynamics (it should be noted that Wiechert's work appeared as the second part of the *Festschrift zur Feier der Enthüllung des Gauss-Weber Denkmals in Göttingen*, Leipzig: B. G. Teubner, 1899); and it is followed by the comment that "today we know that the mediation of the intervening medium requires time."

Hertz's subsequent clarification, and Lorentz's and Einstein's development of the theory—constitutes what I should call a philosophically specious quasi-positivist reduction. Philosophically specious because we know that epistemological positivism can eliminate anything: there is no intellectual gain in eliminating everything indiscriminately; and it is unphilosophical to discriminate in some arbitrary way, and then, retaining A, to eliminate B on grounds which if applied to A would dispose of it as well. We have as good reason to believe in the *fields* of the electron theory as we have to believe in the *electrons*. It is for reasons related to this point that I have called Newton's metaphysics deeper philosophy than Berkeley's or Hume's. Like the latter two, Newton rejects the obscure metaphysics of "corporeal substance." But Newton does not conclude that bodies are "unreal," or that their reality consists in our perceptions of them. He concludes rather that their "substantial reality"—in the only sense that truly has significance for us—*consists* in those combinations of properties which we have (gradually) come to know, through experience and the analysis of experience; or rather, more accurately, that what we know of their reality consists in this. Therefore in the case of mind Newton concludes neither, with Berkeley, that minds are the substance in which perceptions subsist (which is obscure metaphysics); nor, with Hume, that minds are congeries of perceptions (which is specious positivism); but far more modestly, with a skepticism that in my view is genuinely philosophical, that our knowledge here is so meager that "it would be rash to say what may be the substantial basis of mind."

I have said that the development of Maxwell's theory broke the Newtonian abductive scheme for natural philosophy. I think it is clear that this breach was made by methods quite in the spirit of Newton; and it is certainly clear that the new developments still fit the wider scheme that I have identified as Newton's metaphysics. The field distributions of the dynamical variables have been liberated from their former dependence upon the field of impenetrability, and even the bond between momentum and velocity has been in a certain sense dissolved; but despite these profound revisions of the fundamental physics, the basic conceptual structure remains the same. That cannot be said, however, in the case of the quantum theory: here, I think, even the metaphysics fails, and has to be replaced by a conceptual structure of a thoroughly new order. One of the points about quantum field theory is that, in its domain, the necessary conceptual structure has in fact not yet been found; I am tempted to say

that the quantum theory of fields is the contemporary locus of metaphysical research. A second point is that the generic notion of a field, as (despite my opening remarks) a well-defined physical subject matter—but a kind of structure and process, rather than a well-defined class of phenomena—is itself one important example, or product, of what I think deserves to be called the discovery of the structure of reality. I remind you that Maxwell did already apply dynamics to the electromagnetic field (although he thought this depended upon there being a body there); and that according to the general theory of relativity, the electromagnetic field exercises gravitational attraction. I wish I had had space, in the more detailed and historical portion of this paper, to discuss the work on the Maxwell-Lorentz theory done by Poincaré. That work was of high quality and great value. Poincaré was a very great mathematician—one of the most creative in history; his interest in physical theory was intense; and he was, I think, not inferior in philosophical acumen to Hume. But his work on these subjects is nevertheless replete with failures to make the correct step, or the correct connection, in an intricate nexus of relationships: he clarifies the relationships with great brilliance, and fails to draw the right conclusion. These failures seem to me intimately connected with Poincaré's view of the relationship of theory to reality: with his not having regarded the basic principles of dynamics, the law of the conservation of momentum for example, as principles whose *truth or falsity* constitutes a deep characteristic of the world.

III

In the recent literature, there has been a certain tension between views strongly influenced by historical considerations and views strongly influenced by the systematic methodological and epistemological analyses of the logical empiricists. Since my paper has been very much concerned with historical matters, and since I have great respect for the logical empiricist tradition (and consider myself as to some extent within it), I should like before I close to say a word on this matter. It will be convenient to refer to Carnap's notion of a "linguistic framework," and to his distinction between "internal" and "external" questions—a distinction he makes specifically for "questions of existence," but which I shall construe more generally.

I think there is philosophical merit in distinguishing between precise questions and vague questions; in making as many questions precise as possible; in making a given question as precise as possible; and in not ruling out of court such questions as one is unable to deal with in a precise way. I think those notions of Carnap's are intended to facilitate these ends —and do so to an appreciable extent; I therefore think there is merit in them.

Since writing the paper in which he introduced those distinctions, Carnap has come to recognize far more clearly than before (or at least than he had stated before) that in a full account of a highly systematized theory, the principles of the theory are apt to be deeply imbedded in the linguistic structure itself. Without attempting to elaborate this in a technical way in connection with the theories we have been discussing, I shall say that to formulate a theory precisely involves (or may involve) the construction of an appropriate linguistic framework. Within such a framework, a very important philosophical task becomes possible: namely, the analysis of the empirical content of the theory—and beyond this, the analysis of the lines of connection of the theory with empirical evidence. (Of course, the possibility of such analysis depends upon the possibility, within the given framework, of distinguishing what is "empirical evidence.") I have cited elsewhere[21] Newton's analysis of the notions of space and time as affording (when slightly amended) a classic case of the analysis of the empirical content of a set of theoretical notions. I think the self-criticism of Maxwell, completed by the theoretical work of Hertz, can pass as a classic case of the analysis of the *empirically supported* content of a theory. The importance of this kind of analysis seems to me clear beyond reasonable dispute. It is also clear how the results of such analysis may lead to revision of the framework itself, by purgation of redundant elements.

Where Carnap's notions, placed in this context and in relation to the history of science, seem to me deficient is in the treatment of the large-scale evolution of theories. For Carnap, the theory of induction itself is to be developed *within* a linguistic framework; in fact, Carnap's view of the situation appears to be that *when the empiricist program has succeeded, a framework will have been chosen more or less for good*—then only internal questions will remain. Such a prospect is hard to reconcile with the mutual dependence of frameworks and theories, and with what I believe most will agree is the unlikelihood that we are going to have—soon, at any rate—a general theory that will no longer undergo change. If, on the other hand, it is agreed that the program for a definitive "language of science," what-

[21] See the paper cited in note 5 above; the remark occurs on p. 197.

ever its prospects, has at least *not yet* achieved its aim, and that new theories may require new frameworks, then there is a danger that the internal/external distinction may lead to the neglect of important large questions that span the development of theories—on the grounds that these are questions external to the frameworks, and that only within a framework are clear criteria of meaning and truth available. Such an outcome would converge in a curious way with the tendency among historically oriented commentators to find in the succession of theories something akin to diverse artistic genres, as between which meaningful critical comparisons are dubious. I have called this a danger, because I think this tendency is wrong; and I have no doubt that Carnap, and empiricists generally, would agree with me. The general (although unsystematic) point of view that I would urge as the correct one here I have already tried to suggest—in distinguishing philosophically specious positivist criticism from analyses of constructive value like those of Newton or Hertz. No attempt to delimit, systematically and globally, the procedures and notions that are empirically legitimate—from "Hypotheses are not to be regarded in experimental Philosophy" to the verifiability theory of meaning and beyond—has really succeeded. To say this is not to depreciate the efforts that have been and are being expended upon this task—which may yet succeed, and which have contributed much of value though short of success; but it is to deprecate the appeal to programmatic notions as if the program had been realized: this leads to specious criticism. On the other hand, "hypotheses non fingo" and the verifiability theory of meaning both had a valid core; this I earnestly hope we do not forget. It has been possible for scientists, in creating, criticizing, modifying, and revolutionizing their theories, to apply what is valid in these principles, despite the lack of an adequate precise general formulation. There is no obvious reason why philosophers of science cannot do the same.[22]

COMMENT BY GERD BUCHDAHL

Comparing the papers by Professors Stuewer and Stein, and including in this comparison the contributions from Professor Schaffner as well as

[22] The author, heartily acknowledging the courtesy and consideration of the editor of this volume, feels constrained to record his objection to the circumstance that the conditions of publication have deprived him of final authority over the stylistic details of his paper. The author therefore reserves the right to publish elsewhere a version more fully to his own satisfaction.

my own, one notices a remarkable degree of unanimity concerning a certain thesis: It is insufficient to characterize scientific theories and their development, scientific thinking and its logic, by reference to certain "global" frameworks alone, be such frameworks conceived as "linguistic" (as in Stein's criticism of the Carnapian approach), or regarded as approaches toward generalized theories of confirmation and falsification. Instead of this, all the papers under review, emphasizing as they do the controversies that enter into the actual development of scientific theories, and the intellectual as well as personal or social stresses to which such a development is often exposed, draw attention to the need for an analysis of scientific theories to exhibit a much "finer structure" than is taken into account when one concentrates—as is usual—on the relative successes of theories to "account for the data."

Partly, this is due to a new recognition of the interplay between "data" and "conceptualization." As Stein affirms (and my paper implies and illustrates): an "empiricist program" and a "conceptual framework" cannot be kept rigidly apart; "the principles of a theory are apt to be deeply imbedded in the linguistic structure itself." More particularly, all these papers emphasize the presence of additional criteria of appraisal and of additional layers internal to the theories; they lay stress on how scientists do in fact argue and appraise their developing ideas at the time of their growth. And we need not for the present ask the (admittedly) important question how such additional criteria and considerations, which undoubtedly appear to such scientists rational *at the time*, can be shown to be rational when subjected to a philosophical metacritique.

In my own paper I had distinguished three groups of criteria which, respectively, relate to the conceptual articulation, constitutive (empirical) foundation, and "architectonic" determination of theories. Stuewer and Schaffner both emphasize the last of these, involving the criteria of consilience ("theoretical context adequacy") as well as relative simplicity and preferred explanation types, all requiring balance against the "empirical" component (Schaffner's "experimental adequacy"). Stein, on the other hand, pays particular attention to what he calls Newton's "metaphysical level of thought" (equivalent to my own "conceptual explication") with respect to the concepts of matter, force, and field. And we all agree that mere attention to absence of falsification, power of prediction, and similar global criteria—while necessary—is insufficient to make intelligible the na-

ture of the controversies (mentioned as illustrations in these various papers) which those scientific episodes involved.

I will first make a few comments on the examples from the architectonic component. Stuewer's example of Richardson's theory of the photoelectric effect is interesting because it presents us with an explicit case of preference for a specific explanation type: "macro-theory" is preferred to "micro-theory"; though no doubt the latter is rejected not only because of a distaste by the adherents of macro-theory (as in the days of Ostwald and Mach) for "unobservable entities," but also because the particular unobservables in question here involve "paradoxical properties."

Stuewer uses this also to illustrate the deeply conservative tendency of most, including some of the greatest, scientists. If some philosophers of science therefore characterize science as the need to strike out with "bold hypotheses," it is important to be aware of a contrary tendency, not to say "need." Nor is such conservatism at all irrational. Evidently it links with the criterion of "theoretical context adequacy." As Stuewer puts it: though the photon concept is not *entailed* by the phenomenon of the photoelectric effect, and the latter (taken by itself) is capable of alternative explanations of which Stuewer's paper mentions a great many, nevertheless, the photon concept would at present be regarded as a "valid" one because it "derives its validity from an interlocking theoretical and experimental matrix."

It seems clear that such a criterion cannot be absolute; that it is neither necessary nor sufficient, if it is regarded as bestowing "conclusive validity" on some scientific hypothesis. Nevertheless, the opposition to Einstein's hypothesis on the part of Lorentz, Thomson, and Sommerfeld, though in the event it proved unavailing, was clearly a perfectly proper expression of paying regard to theoretical context criteria. So one should not be tempted to speak of an "invalid opposition" (or some equivalent pejorative expression) merely because of this knowledge of hindsight. And it should be added that it is not easy to appraise *at any particular time* whether such controversies as those discussed in Stuewer's paper will or will not yield additional predictions and thus eventually turn out to be (to use Lakatos's terminology) examples of either "progressive" or "degenerate" research programs. The debate between the Cartesians, the Leibnizians, and the Newtonians may not in their day have yielded anything "progressive," but since the attempt to mediate between the conflicting parties yielded the *explicit* formulations of a field approach on the part of Boscovich and

Kant (as shown in my paper), it seems clumsy formalism to exclude such controversies because they seem incapable of yielding something of immediate positive value.

Consider for instance Stuewer's report of Lorentz's refusal to accept the hypothesis of light quanta because this was ("seemed"?) inconsistent with the phenomena of interference and diffraction. This shows again that the context criterion is not conclusive, if only because—in accordance with the Duhemian thesis—it is always possible that additional alteration in background theory may result in reconciling recalcitrant "old" theory (here: the interpretation of the diffraction phenomena) with a newcomer among theories. But none of this shows that the context criterion does not possess prima facie relevance. Indeed, in the tussle between the old and the new there are always a number of possibilities, a fact which is usually concealed from those who frame their methodologies "from the outside," bent on considering only the successful outcome of some theoretical or practical dispute. Thus Stuewer's paper shows that scientists usually try to reconcile a new "experimental" result (such as the photoelectric effect) with traditional existing theory (context criterion being used again) by making modifications to supplementary principles (such as the precise nature of atomic structure) which to them *at that time* were less well understood, rather than incur a clash between a well-entrenched theory and a new phenomenon. And these are "fine structure" activities, descriptive of the rationale of scientific thinking, that need somehow to be incorporated in our appraisal of the logic of science.

Stuewer's example of Richardson's theory illustrates a further important point: the very possibility of formulating a theory in terms of a preferred explanation type is often *felt* by scientists to bestow explanatory value. ("Feelings" may here again be rejected as having no bearing on the question of "rationality," but one may remark that the existence of such "feelings" of "explanatory power," attaching to some explanation type, has sometimes been used to define the notion of theoretical explanation on the part of philosophers of science, e.g., N. R. Campbell.) Now if we grasp that some particular explanation type may as such bestow explanatory power we shall be more sensitive in our reactions to Newton's "solution" of the problem of gravitational attraction at a distance. Stein, in his paper, calls Newton's opinion that gravitation occurs in virtue of an immediate action of God "disconcerting," and he intimates that no historian can be feeling anything but "puzzled," not to say "annoyed." Now as I

point out in my own paper, this reference to God's "action" should really be understood as being to "final causes," and thus to a physicotheological context. Two conclusions at once follow: First, Newton's solution is far from being so puzzling, since it is simply an actual fact of scientific development that it seeks to express its theories in terms of preferred explanation types. (Descartes opposed mechanical theories to "sympathetic relations," "formate souls," and "animate form." Fermat opposed a teleologically conceived concept of "least time" to Descartes's "incomprehensible" hypothesis of unobservable light particles moving through media with speeds proportional to their densities.) Second: "Bodies attracting by an immediate law of God" *does not mean the same thing* for Newton as "direct action at a distance" in the physical sense, contrary to what is claimed by Stein. For as I suggest in my paper, the teleological foundation which Newton postulates is meant as a "third possibility" mediating between the scylla of action by matter on matter at a distance and the charybdis of an ethereal mechanism whose modus operandi had proved inexplicable for twenty to thirty years—always assuming my interpretation of Newton's "metaphysical view" (contrary to Stein's) of the conceptual impossibility of distance action. Newton's solution may not add to our nomothetic knowledge of gravitation; it does tell us something about the forms and functions of scientific theories.

I shall return to Newton in a moment. There is, however, a problem to which I have already alluded; it is one that is brought into sharp focus in connection with the criterion of preferential explanation types. Here, if anywhere, it seems oppressively obvious that these "fine-structure" criteria have to be deeply historicist in kind. To be sure, it is trivially true that *all* criteria have a historical ingredient in that they are employed and applied at a given moment in time by putatively rational beings. But it seems blatantly obvious that what is an acceptable explanation type (e.g., teleology) to one scientist may be otiose to another. And as already mentioned, the fact that they *appear* to the actors involved to be part of a rational activity does not prove that they *express in fact* a feature of rational activity, understood in the normative sense. All this appears to introduce a degree of relativism into our account of scientific thinking.

In reply, I can urge only this much: First, that none of these criteria yield conclusive results. Second, that they can at best be no stronger than any inductive criteria, by which I understand the fact that such criteria validate an inference or "projection" on *the basis of* existing theory and evidence; they do not *entail* a result that has to stand, come what may. Third, that none of our criteria can be employed independently of the others, and that they must be given different weightings at different times, sometimes ignored, or suppressed, sometimes powerfully emphasized.

But one might go further; one might admit that there is a kind of "spectrum" of different "degrees of depth" enjoyed by these various criteria. If one for instance posits a causal hypothesis, then such a hypothesis will have to satisfy certain obvious criteria that have been elucidated by writers on inductive logic, from Bacon through Hume to Mill and our own time. Similarly, putative falsification clearly demands some modification to a hypothesis or its branching background theories, or even a reconsideration of the data given by the experiment (as in the case of Richardson and Compton, reported by Stuewer, page 259). Further along the scale would be considerations of concept formation: Leibniz and Huygens constructed gravitational theories rivaling those of Newton in response to a different conceptual evaluation of terms like matter, force, causation. Still further along there would be architectonic criteria like those of relative simplicity, theoretical context adequacy, all the way down to preferred explanation types, existence of important analogies, and so on. In general, one might say that it does not follow from the fact that none of these criteria are (a) conclusive or (b) essential that they should not play a *critical* part in the evaluation of theories, as members of a "family" of criteria.

I now turn to Professor Stein's paper, and as I have already indicated, I very much sympathize with his method of supplying a "finer structure" of Newton's physical thought, though I am not entirely clear about the denotation of his distinction between "heuristic" (or "research"), the "fundamental" and the "metaphysical" levels which he proposes. From what is said on page 278, it appears that "heuristics" corresponds to the formal mode of presentation of the *Principia* together with its explicit definitions. As I understand it, Stein further claims, however, that a proper *interpretation*—and to what "level" does such an "interpretation" belong?—of the procedures at the "research" level can be shown to involve the concept of the "field" as a "fundamental category for scientific explanation," i.e., of gravitational phenomena. The chief ground for this contention seems to be the fact that Newton's various formulations and laws involve "the notion of a disposition" and of "a contrary-to-fact conditional," as well as the notion of a single "order" of the phenomena involved, such that we can speak of "one field" surrounding a given body.

I am not certain of the demonstrative force of this contention. The dispositional and contrary-to-fact nature of laws is usually one of their universal characteristics, and thus seems too general a feature to permit of Stein's special conclusion. Perhaps before making this inference, one ought to explore additional criteria of different kinds of fields that have from time to time been suggested as necessary and sufficient conditions for granting the "reality" of a field. (Cf. the special criteria attached to a Faraday field in Mary Hesse's *Forces and Fields*.)

However, shelving this point, it is interesting to find Stein contending that at Newton's "metaphysical level of thought" we meet a similar obliteration of the dualism between matter and force (i.e., the *reduction* of matter to the field), and the emergence and generalization of a corresponding field concept—thus evidently supporting Stein's *interpretation* of the implications of the thought of Newton at the "research level." On the other hand, according to Stein, "fundamental theory" surprisingly does retain the dualist scheme, awarding an independent and prior place to "impenetrability" (and I would add: to inertia also), while requiring a special supplementary divine action to establish gravity, or alternatively, the intervention of the various ether mechanisms proposed by Newton from time to time. Moreover, Stein concludes, "fundamental theory" expresses the dualist viewpoint which is "the system of physics that Newton was led to develop," and which his immediate successors followed, whereas encapsuled in the bosom of the theory there lay always the unitary scheme, later to unfold itself in the approaches of Maxwell.

To find my way around these classifications I must try and compare them with my own "fine-structure" composition of Newton's thought. My "constitutive level" conflates Stein's "research" as well as "fundamental" levels and implies, as does his, a dualism of matter and force, with the merely de facto introduction of gravitational attraction. But I lack an "interpretation" of the "research level" as implying a monistic field action. All I say is that "interpretations" of formal structures are open-ended. On the other hand, my theory of Newton's "conceptual explication" (Stein's "metaphysical level") is that it supports the *dualist* and not the *monist* viewpoint. This being the case, one requires (according to my reading of Newton) a further contribution (my "architectonic component") in order to establish a rationale for gravitational attraction. The need for such a component in Stein's view is not very pressing since his "interpretation" of Newton's "research" as well as of his "metaphysics" implies a reduction of matter to the field. Therefore Stein is not inclined to take very seriously Newton's theological (or teleological) escape route as a means of relieving what he himself acknowledges nevertheless to be a "tension" in Newton's thinking.

Now I think that the "interpretation" of the "research level" that is here advocated might have seemed less plausible if Newton's "metaphysical thought" were not likewise construed in a monistic vein. Stein claims, however, to have a number of arguments which supposedly back up this version of Newton's thought. Perhaps the most global is the one noted on pages 272 and 277: Newton is there shown as holding that any knowledge of the action of matter (whether through attraction or impact) is based on experience. The sting in this is supposed to lie in the exclusion of an alternative possibility, viz., that we might gain the requisite knowledge by way of a kind of rational or intuitive insight. From this it would then follow that the affirmation of the empirical basis of physical knowledge entails the *universal* rejection of any possibility of grasping or conceiving the action of matter on matter, whether this be by impact or action at a distance. And it would then also follow that where Newton says that action without contact is inconceivable he would not be meaning to say that it does not happen, any more than he would want to argue for the physical impossibility of impact because *that* is inconceivable.

Now it is true that some writers did hold this view. In my paper for this volume I suggest that it is one of Locke's views, and that it may be found also in Maupertuis and Mill, among others. But it is not at all clear whether this was Newton's own understanding. As Stein admits, the Bentley letters ostensively contradict this: Why should Newton have *singled out* action by matter on matter "without an intervening material medium" as being "inconceivable"? (See also my comments on the Bentley passage in note 30 to my paper.) On Stein's reading of Newton it becomes only too clear why—as he notes on page 273—he cannot understand how Newton, "despite his principles, as I have explained them" (i.e., the doctrine of the *universal* unintelligibility of action, here attributed to Newton), can want to introduce ether hypotheses, let alone the "puzzling" and even "annoying" teleological escape! To overcome the obvious contradiction, Stein goes further: he suggests that we may safely interpret Newton's complaint in the Bentley letters at the inconceivability of action at a distance to be a universal one by "leaving out the words 'without mutual contact.'" I

must say that by such methods almost any conclusion could be established!

At any rate, it seems to me that Newton's true views resemble more those of the Locke quoted on page 215 of my paper (not altogether consistent, as is usual with Locke, with the views mentioned on page 224). That is to say, Newton simply *had* no clear views on the notion of action on impact, and certainly did not appreciate the bearing of the empiricist basis of our knowledge of impact on this case. However, insofar as he had any views they seem more like the ones Mr. Stein attributes to Newton's "fundamental theory," viz., that impenetrability was the "first basic property of bodies," in the sense that "interaction by contact . . . is a *necessity*," a view echoing my Locke passage just mentioned. So Newton has not as yet grasped the implication of the empiricist doctrine that "knowledge by way of experience" excludes "rational insight." It is not surprising therefore (as Stein notes) that Newton nowhere calls "impenetrability" a force, since it is beginning to look as if he *was* a dualist at the "metaphysical level," espousing a dualism of matter and force, moreover, that seemed inimical to the "addition" of attraction *except* as an experiential induction. (For which reason, we *need* of course the ether or the teleological escape; neither of these would make any sense if it were the case that Newton's "metaphysical views" were those of Stein's construction.)

Stein, in his supplementary attempts to establish his viewpoint of Newton's field metaphysics further speaks of "impenetrability fields"; and this "activist" interpretation is then supported also by an earlier argumentation that *all* forces, as capable of *producing* change of motion, are "active principles." Does he want to say that the force of inertia falls under that description? His argument seems to leave this possibility open, but once again it is important to recall that Newton emphasizes that the three laws of motion (which, as Stein rightly points out, *jointly* define "inertia") are "passive principles" (so called by Newton), as contrasted with the "active principles" of gravity, fermentation, and the like. So again it appears that impenetrability as well as inertia is placed precisely at a level different from that of gravity, in line with the "fundamental view" here ascribed to Newton.

The final argument that Stein advances is Newton's view (found in the early *De gravitatione*) that those regions of space that God has rendered "impenetrable" as well as "movable" also have the "property, or power, of affecting our minds" when copresent with our "brain." I cannot see the force of this point. First of all, as Stein rightly notes, Newton observes that this "interaction" is once again "obscure"—a technical way in those days of saying that it is nonintuitive, like gravitational interaction. Second, and like the latter, it is not sufficient to find places where Newton *speaks* of interaction, for this language could quite well belong to the levels of "research" or "fundamental theory" (my "constitutive level"). After all, philosophers like Leibniz, who explicitly deny interaction at (what they call) the "metaphysical level," nevertheless claim the right to employ this language at the level of their *phenomena bene fundata*. But language employed at the "fundamental" (or theoretical or constitutive) level cannot be used to explicate the "metaphysical" views of an author, and I therefore very much doubt whether "Newton's metaphysics of body reduces the notion of matter to the notion of field."

Still, these are small-scale disagreements compared with my fundamental agreement which the former really imply, namely that scientists' thinking proceeds at a number of different levels, has different sets of components. If the views Stein attributes to Newton's "metaphysics" are really those of the later Locke, of Maupertuis and Mill (as regards the question of "unintelligibility"), and of Leibniz, Boscovich, and Kant (as regards the interpretation of matter as force), this is relatively unimportant. What is important is to realize that beneath the surface of the "research level," with its foundation upon the "data" or "observations," with its attempted verifications or falsifications, there are other levels which tend to articulate a given theory and infuse it with a kind of historical dynamic which at one moment may incorporate traditional, at another revolutionary, features, and which at any time may come to affect the conceptual articulation of the ostensive theory at the "research level."

I want to conclude with one or two remarks on Stein's general philosophical appraisal of Newton vis-à-vis that of other British philosophers such as Berkeley and Hume, and their modern descendents, positivists and instrumentalists. I cannot see much foundation for the praise of the youthful Newton's ascription of "substantial reality" to the "attributes" of bodies, unless a philosophical articulation of what all this implies had been given also. Now this is precisely what was done in the philosophies of Locke, Berkeley, Hume, and Kant. The story is a long one (I have tried to tell a little of it in my recent volume, *Metaphysics and the Philosophy of Science: Descartes to Kant*), so I will only note one single point. The difficulty as it appears to these philosophers is just how to preserve some kind

of "reality" for entities which (and here, perhaps wrongly, they agreed with Newton) were being characterized by the logical label of "attributes." So they construe them as "existing" relative to a mind, whether they regard them as "ideas" (Locke, Berkeley), or "impressions" (Hume), or "appearances" (Kant). Now it is evident from the Queries to the *Opticks* that Newton himself was not a realist but rather something of a "subjective idealist": we know things by the "images" through which they appear to us—and it is not difficult to think of these "images" as representatives of the "qualitative" aspect of things rather than their "obscure substantial" being. Now such a viewpoint can hardly be appraised as "deeper philosophy" when compared with that of Berkeley and Hume. It is just simply no more than setting out the problems and questions and leaving later philosophers (as is their proper job) to get on with them.

Nor should we be deceived by the language of these philosophers into believing that their dispute is about the "reality" of things in the ordinary, nonmetaphysical sense of this term. (Here, too, there are many "levels" at which the discussion proceeds!) I agree with Stein that global attacks on "reality" remove too much so as to provide us with plausible analyses of the logical status of problematic scientific entities, such as the electron, or the electron field. But it is not even certain whether such radically phenomenalist thinkers as Berkeley and Hume ever contended for such conclusions. I need only refer to Hume's praise for Newton's hypothesis of an ether (mentioned on page 225 of my paper) and his frequent sympathetic allusions to "force and energy" as exemplifying a much greater largesse on the part of such philosophers than is usually accredited. And in my work referred to above I have shown that Berkeley certainly *believes* very frequently that he is not denying the existence of hidden structures, and even (sometimes) "insensible corpuscles." What he (and Hume) for the most part are concerned with is the therapeutic exercise of persuading us that the reality of a thing is not impugned just because there is no hidden *metaphysical* foundation for it to be found anywhere.

But, in conclusion, it must also be said that just because, say, a positivist analysis can be too radical in its rationing of the ontological population of the universe, and must hence be construed as really being neutral to the question of the existence of *scientific* theoretical entities, it does not follow that such entities *do* exist, any more than that they do not. I mean: the falsehood of the contentions of the positivists would by itself be insufficient to demonstrate the correctness of existentialist claims on the part of the philosophers of science. Different kinds of analyses would seem to be required in order to achieve this end, analyses of which the technical portions of Stein's paper give such a brilliant example.

COMMENT BY MARY HESSE

Professor Stein has given an excellent example of the marriage of historical and philosophical considerations in his analysis of the concept of "field." I am not competent to judge the validity of his interpretation of Newton in relation to the more "archaic" versions of Newton's metaphysics coming to light in the manuscripts (see, for example, the papers of J. E. McGuire and P. M. Rattansi referred to in my contribution to this volume). But I do wonder whether, even from the perspective of later physical categories, he has not blurred unduly the differences between Newton's conception of field and that of the nineteenth century.

Stein takes the fundamental notion of field to involve the dispositional, or counterfactual, power inhering in spatial points to exert a force on a test particle if one were present at the point (though it is not). Under this analysis Newton's and Maxwell's theories are indeed both field theories. But it is not clear that either theory *must* be interpreted in these dispositional terms. Stein asserts that without dispositions there is no induction, but this conclusion is controversial in philosophy of science, and there are cogent arguments to the effect that induction from factual instance to factual instance does not involve necessary reference to laws understood as quantified over counterfactual as well as factual instances. On Stein's criterion it seems that it would be impossible to describe any lawlike action at a distance without the intervention of a "medium of potential."

There are, however, alternative definitions of "field" which probe more deeply into their physical character, and serve to make important distinctions between Newton's action at a distance and Maxwell's (and Einstein's) fields. Stein does not mention Faraday, but it was he who gave the first and most subtle analysis of the difference between actions at a distance and actions through continuous media. Faraday had broadly speaking three tests for continuous action through space: (1) Can transmission of action be affected by changes in the intervening medium, as for example magnetic action by iron or electric action by dielectrics? (2) Are the lines of force curved? (3) Does the action take time to cross space? For gravitation Faraday thought the answer to all three questions is no; for all other

physical actions the answer to (1) and (2) is yes, and, although Faraday did not know the answer to (3) in respect to electric and magnetic induction, it turned out in the light of Maxwell's theory to be yes in all cases except Newtonian gravitation, and even this exception was removed in Einstein's theory of gravitation. Thus instantaneous transmission became the primary criterion for action at a distance, and a finite velocity the criterion for field action. Indeed finite velocity *demanded* the presence of energy in the medium in a more than dispositional sense, in order to remain consistent with the conservation of energy. As Maxwell put it, "If something is transmitted from one particle to another at a distance, what is its condition after it has left the one particle and before it has reached the other?" His answer was, it becomes the energy of the intervening medium, that is, the field.

REPLY BY HOWARD STEIN

The striking affinity of theme noted by Dr. Buchdahl extends, in the case of his paper and mine, to a considerable part of the particular historical matter treated. I am grateful to him for the discussion he gives in his own paper, and for his comments on mine. That we agree on some important matters, both historical and philosophic, and disagree on others, will be apparent to all readers. It seems neither necessary nor desirable to attempt here to elucidate all points of disagreement; it may be left to our readers to weigh the considerations involved, and to ourselves to reflect further on the issues. I shall confine myself to a few points on which Buchdahl's comments seem to me to show either that my exposition has been insufficiently full and clear, or that a philosophical issue exists between us which requires sharper definition.

First let me say—not by way of argument, but statement of where I stand—that my general philosophic position is significantly closer to the empiricism of the Vienna Circle than is Buchdahl's (see part III of my paper). I should put the matter this way: Adopting Peirce's distinction of "abduction" (the process of finding hypotheses) and "induction" (the process of testing them), I think that the array of considerations that Buchdahl refers to as constituting a "fine structure" of inquiry is chiefly relevant to the former. I also think that this abductive phase of science is more important than the Vienna Circle, in its practice, seemed to hold; so I agree with Buchdahl's thesis of the importance of this fine structure. On the other hand, of the inductive phase, despite all the difficulties (the lack of a global theory of inductive inference, and the complexity of factors that have to be taken into account), I believe that the essential point does remain "the relative success of theories to 'account for the data'" (Buchdahl, page 288). "Fine-structural" grounds of dissatisfaction with a theory that accounts best (of all known theories) for (all known) data can be a significant motive for the further abductive process; but it is not a valid ground for *rejection* of the theory before a better one is found. This is what I think Newton meant by his statement that "Hypotheses are not to be regarded in experimental Philosophy," and by his Rule IV: "*In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions.* This rule we must follow, that the argument of induction may not be evaded by hypotheses." What history very remarkably shows, in relation to this issue, is that although no one has succeeded in formulating clear and acceptable general rules of inductive inference, in practice serious difficulty in deciding which of the actual theories does best account for the data has never persisted long. (A fairly recent and very instructive example is provided by Einstein's view of quantum mechanics: he greatly disliked the theory, and believed that it must eventually be superseded; but he fully acknowledged its current supremacy in accounting for the data.) One reason for insisting upon this point is my belief that, over several centuries now, the history of the inductive success of theories has occasioned very significant changes in the ends aimed at in abductive inquiry. This seems to me an important part of what has been called "the structure of scientific revolutions"; and I believe that a considerably higher degree of objective rational control has obtained in this process than has sometimes been claimed—essentially because of that feedback from induction to abduction.

Turning now to points of detail, I must amplify my evidently too compressed remarks on gravitation as "an immediate Law of the Creator of the Universe," which according to me (pages 273–274) does, and according to Buchdahl (page 291) does not, mean the same thing for Newton as direct action at a distance. I do not at all intend by my claim to impugn Newton's seriousness as a theologian; the role of God is undeniably, I think,

from Newton's point of view, a real one (although *not* one that is to be appealed to as a premise in physical reasoning—i.e., in what Newton calls "experimental Philosophy"). The claimed equivalence of "immediate Law of the Creator" with "direct action" consists, rather, in the fact that *all interactions of bodies are, for Newton, ruled by "Laws of the Creator of the Universe"*; hence "direct action" is either *an entirely vacuous notion*, or else—and this seems the more convenient idiom—ought to mean nothing but *action governed by an "immediate" Law of the Creator*. Thus, I take the actual collision of ultimate particles (which according to Newton is a rare event) to be governed by "an immediate Law of the Creator"; but impact of elastic bodies (which, Newton holds, always derive their elasticity from an internal dynamical structure)—or gravitation, if it should prove to be caused by an ether or by streaming particles—I take to be governed by a *mediate* law: i.e., one that is reducible to, or derivable from, more fundamental laws.

What evidence is there that Newton's view of these matters in general, and of the "directness" of the process of impact in particular, is what I have said it is? For one thing, we have Newton's account of the nature of body in the *De gravitatione*, which I hope I have sufficiently described. What is crucial for our present question is not the reference to interaction with mind (Buchdahl, page 295), but the fact that the establishment of mobile regions of impenetrability, and the establishment of *all* their laws of motion and interaction, are represented equally as fiats of God. (According to Leibniz, gravitational attraction at a distance, resulting in the deflection of a body from uniform rectilinear motion, would amount to "a perpetual miracle." It follows from Newton's view that the word "miracle" is no more applicable to gravitational than to inertial motion.) That *De gravitatione* is an early writing is a point I have considered in my note 12; but let us turn to the quite late statement in Question 31 of the *Opticks* (Dover edition, p. 400): "All these things being considered, it seems probable to me, that God in the Beginning form'd Matter in solid, massy, hard, impenetrable, moveable Particles, of such Sizes and Figures, and with such other Properties . . . as most conduced to the End for which he form'd them." Not, then—I think I have warrant for saying, contra Buchdahl—"the ether or the teleological escape": at the point where Newton, whether early or late, refers to teleology, it affects equally hardness, impenetrability, inertia, and all. Again: does Newton reject "any possibility of grasping or conceiving the action of matter on matter, whether this be by impact or action at a distance" (Buchdahl)—or more precisely, reject such a possibility, as I claim, "in any other sense than that we have observed interactions of [these] type[s] to have occurred"? See *Opticks* (Dover edition), page 389: "All Bodies *seem to be* composed of hard Particles: For otherwise Fluids would not congeal; [etc., etc.]. Even the Rays of Light *seem to be* hard Bodies; for otherwise they would not retain different Properties in their different Sides. And therefore Hardness may be reckon'd the Property of all uncompounded Matter. At least, *this seems to be as evident as the universal Impenetrability of Matter*. For all Bodies, so far as Experience reaches, are either hard, or may be harden'd; *and we have no other Evidence of universal Impenetrability, besides a large Experience without an experimental Exception*." In his explication of the third rule of philosophizing, in book III of the *Principia*, Newton makes an analogous statement, not only about hardness and impenetrability, but about the mobility and inertia and even the extension of bodies.

I think this, taken all together, provides a rather strong case for my conclusion that Newton's statement to Bentley, while *strictly accurate*, would remain accurate if the words "without mutual contact" were deleted. I may nevertheless be wrong; but I do not think I am irresponsible. Buchdahl's comment that "by such methods almost any conclusion could be established" seems to me a little unfair. And to his preceding remark, "On Stein's reading . . . it becomes only too clear why—as he notes on page 273—he cannot understand how Newton . . . can want to introduce ether hypotheses," I have to say that in the passage referred to I do not at all "note" such a thing, and I do not believe it to be the case; I rather claim to elucidate just this question, on the basis partly of Newton's fundamental conceptions (where impenetrability is given—as I have said, page 278—a somewhat special status), and partly of his whole program for understanding nature (which demands that *all* possibilities of "Tieferlegung der Fundamente" be explored).

Two somewhat isolated points call for brief comment: (1) I find surprising Buchdahl's characterization of Newton as "not a realist but rather something of a 'subjective idealist.'" He bases this upon Newton's account of sensation in the *Opticks*: namely, that we "know" things, in sensation, by the "images" generated, in our minds, by motions in our "sensoria"; which motions are conveyed to the sensoria by impulses propagated along the nerves from the sense organs (themselves stimulated by interaction

with external bodies). Now, the language of traditional epistemology and metaphysics is far from precise, and traditional usage far from stable; but I think it quite unusual, even within the wide variability of that usage, to call such a view "subjective idealism." (2) Buchdahl refers to my characterization of "forces capable of producing change of motion" as "active principles," and asks: "Does he want to say that the force of inertia falls under that description?" The answer is no: as I have said, "One . . . force is both universal and intrinsic to all bodies: the *passive* principle of motion, or force of inactivity, *vis inertiae*. . . . But the object of greatest attention . . . is the class . . . of *active principles*. These are the forces that can produce, and more generally can change, motion. . . ." This is perhaps a rather scholastic point; but the distinction is Newton's, and I think I have been faithful to it; see *Opticks* (Dover edition), page 397: "The V*is inertiae* is a passive Principle. . . . By this Principle alone there never could have been any Motion in the World. Some other Principle was necessary for putting Bodies into Motion . . ."; *ibid.*, page 401: "It seems to me farther, that these Particles have not only a V*is inertiae*, accompanied with such passive Laws of Motion as naturally result from that Force, but also that they are moved by certain active Principles, such as is that of Gravity, and that which causes Fermentation, and the Cohesion of Bodies."

Buchdahl expresses doubt about my distinction between what I call "heuristic detail, or 'research,'" "fundamental theory," and "metaphysics," in the conceptions of Newton. The distinction is not one which I should like to make bear any great systematic burden; it is intended only ad hoc, for local convenience. I do not understand "heuristics" to imply (as Buchdahl suggests) "the formal mode of presentation of the *Principia* together with its explicit definitions"; I mean rather—in connection with gravitation—the considerations which were instrumental for Newton in *discovering and "proving"* (i.e., testing against experience) the theory of gravitation. By "fundamental theory" I mean the comprehensive conceptual scheme in which Newton places the discoveries he has made, and in which he hopes to place essentially all subsequent discoveries. In distinguishing a third level of Newton's thought as "metaphysical," I have in mind the circumstance that, although the conceptions of this third level can be characterized in the words I have just used about fundamental theory, these conceptions do not—in Newton's work—function in a crucial way in his physics. Bodies and forces are pretty much indispensable for Newton; but whether bodies are taken (to use a modern jargon) as values of individual variables, or as higher-order logical entities (in older language: as substances, or as attributes of some sort), is a question which he can and does set aside in his physical investigations. That Newton perceived the separability of such issues from his own physics is, in my judgment, an instance of critical philosophic acumen. That he addressed himself to those issues in the way *De gravitatione* shows he did is, in my judgment, testimony to his philosophic originality and depth. And that such issues, or such notions, can *move* from the (perhaps seemingly nugatory) "metaphysical" level, and can acquire a role in physics itself, is one of the fascinating lessons of the history of science and philosophy; this is the bearing of my remarks about the "discovery of the structure of reality."

I come now to an issue raised by Dr. Hesse as well as Dr. Buchdahl. They both question the appropriateness of my use of the word "field" in analyzing Newton's theory. To me, this is not a fundamental issue, as I have tried to make clear in the opening sentences of my paper. That the notion of field in modern physics has dimensions of significance not present in the notion that I refer to by that word in the case of Newton is quite true—and I have discussed this point on pages 282–283 and page 285. I certainly do not regard the notion of field as something *merely* "dispositional": even Newton's rudimentary "field of impenetrability" (as I call it) is "ideally testable," i.e., "dispositional," only "in a Pickwickian sense"; Maxwell's electromagnetic field is sometimes to be detected experimentally by an eye, rather than by a test charge or current!

Buchdahl's and Mary Hesse's references to Faraday's criteria, however, seem to me unhappy. Faraday (though I did not mention him in my paper) is one of my personal heroes; but I do not think his criteria for "continuous action through space" are very good. The second criterion in Dr. Hesse's list—"Are the lines of force curved?"—is clearly nugatory: except in the most special and trivial cases, lines of force *will* be curved. (That Faraday thought lines of gravitational force always straight is one of the rare instances in which his lack of formal mathematics betrayed him into outright error.) The first criterion—"Can . . . action be affected by changes in the intervening medium?"—played a strong positive motivating role in Faraday's work; but as a *criterion* it is deficient, because it is unclear. Newtonian gravitational action *is* affected by disposing new masses

through the "intervening medium"; and both in Faraday's time and our own, the effect of iron or of dielectrics on the magnetic or electric field is accounted for in an analogous fashion, as due to the disposition of magnets (currents) or charges in the medium. This leaves only the third criterion as having real substance. Passing from Faraday to Maxwell, this criterion—finite velocity of propagation—comes into significant play; and is related (as Dr. Hesse justly remarks) to the attribution of energy to the field (cf. page 283 of my paper). It is worth emphasizing once more that, for Maxwell, this feature was connected essentially with the notion of a *material* medium. The next-to-the-last sentence of Maxwell's *Treatise on Electricity and Magnetism* reads: ". . . whenever energy is transmitted from one body to another in time, there must be a medium or substance in which the energy exists after it leaves one body and before it reaches the other, for energy, as Torricelli remarked, 'is a quintessence of so subtile a nature that it cannot be contained in any vessel except the inmost substance of material things.' "

I do not, as I have said, consider the question how we should use the word "field" a fundamental issue; but how the word is used by the scientists we study is a point to be treated with due care. For Maxwell, attribution of energy (and other dynamical quantities) to the field is not a criterion of "a field theory"; he says (cf. my page 279) that his theory may be called a theory of the electromagnetic field *because it has to do with the space around the electric or magnetic bodies*. That *dynamical properties* are attributed to this space is rather what makes his theory "*a dynamical theory* of the electromagnetic field." This, to Maxwell, implied the existence, in the space concerned, of "matter in motion, by which the observed electromagnetic phenomena are produced." As I have briefly explained, it was only post-Maxwell that the idea of *a dynamical field theory without a material substratum* came to be seriously entertained (an idea outside the scope of what I have called Newton's "fundamental physics," but within the scope of his "metaphysics").

It may be of some (at least philological) interest to pursue the question of when—and with what precise meaning—the word "field" was first used in something like its present sense in physics. Felix Klein (*Vorlesungen über die Entwicklung der Mathematik im 19ten Jahrhundert*, volume II, page 39) says that the first such use known to him is by William Thomson (Lord Kelvin) in 1851; it occurs in Thomson's paper "On the Theory of Magnetic Induction in Crystalline and Non-Crystalline Substances," *Philosophical Magazine*, March 1851—reprinted in his *Reprint of Papers on Electrostatics and Magnetism* (London, 1884), Article XXX; see section 605 of the latter book, pages 472–473:

> *Definition.*—The *total magnetic force at any point* is the force which the north pole of a unit bar magnet would experience from all magnets which exert any sensible action on it, if it produced no inductive action on any magnet or other body. . . .
> *Definition.*—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."

The question of the role played in Newton's positive work by the notions I have emphasized—their role at the "heuristic" or investigative, and especially the *inductive*, level—may be divided into two parts. The first (easier, and less important) is historical: did Newton use these notions? I have met some skepticism from historians, especially about my construction of "the accelerative quantity of a centripetal force"; let me therefore review what seem to me decisive texts. First, Definition VI: "The absolute quantity of a centripetal force is the measure thereof that is greater or less *according to the efficacy of the cause propagating it from the center through the surrounding regions*." (Vis centripetae quantitas absoluta est mensura ejusdem major vel minor pro efficacia causae eam propagantis a centro per regiones in circuito.) This does not (for Newton) imply, necessarily, finite velocity of propagation; but it does imply affection of "the surrounding regions." Of course, it is possible that only the surrounding *bodies* are meant; however, that possibility is weakened or destroyed by the following (in the second paragraph after Definition VIII): "We may . . . refer . . . the accelerative force to the *place* of the body [acted upon], as *a certain efficacy* [Motte renders: "a certain power or energy"], *spread from the center to the several places round it, to move bodies that are in them*." (. . . licet . . . referre . . . vim acceleratricem ad locum corporis, tanquam efficaciam quandam, de centro per loca singula in circuitu diffusam, ad movenda corpora quae in ipsis sunt.)

The more important part of the question—raised with particular clarity by Dr. Hesse—is whether Newton *had to use* such notions. That he did have to use them (in some form or other) I have argued on pages 267–269 of my paper, and in note 4. I am puzzled to read in Dr. Hesse's comment that "Stein asserts that without dispositions there is no induction"; and that "this conclusion is controversial in philosophy of science." For the

first part, I do not find that I have said this; I have said that *Newton's* inductive argument—which is, it seems to me, a clear test case for any theory of induction—makes essential use of a dispositional term or a contrary-to-fact conditional. For the second part: the claim I make, I argue for. Now, when one offers arguments or evidence, either they are supererogatory, or their bearings *are* upon a controversial issue: the aim is, at best, to settle a controversy, but at least to clarify one. A *premise* may fairly be challenged as controversial; to admit this as an argument against a *conclusion* is to preclude any progress in philosophical discussion.

A really careful analysis of Newton's whole argument for universal gravitation is more than I could undertake in the paper, or can here; but a review and amplification of some salient points seems in order. It is well known that Newton's treatment of the force on the moon is a critical juncture in his argument; for this provides *the only link* connecting the planetary motions with the terrestrial force of weight. To establish the link was essential, both because (1) a major part of Newton's achievement was precisely this classification of the astronomical motions under the same head as the phenomena of weight—their exhibition as effects of the same "natural power" (this conclusion was accepted, for example, by Huygens, and regarded by him as a very great discovery: one of its implications was that any mechanical theory of weight must ipso facto be a mechanical theory of planetary motion as well; and must incorporate the law of the inverse square of the distance); and because (2) Newton's more far-reaching conclusion—that all bodies have a power of gravity proportional to their mass, affecting other bodies with centripetal accelerations inversely proportional to the squares of their distances—was arrived at only through considerations based upon that classification.

Now, the proposition that the impressed force giving rise to the moon's orbital acceleration is its weight—Proposition IV of book III of the *Principia*—is established by comparing the moon's acceleration with the acceleration of terrestrial falling bodies. The former is to the latter as 1 to 3600. The radius of the moon's orbit is to the radius of the earth as 60 to 1. How do these data allow us to infer anything of interest? We see that the ratio 1:3600 is the inverse square of the ratio 60:1; but it is certain a priori that the one ratio must be *some* power of the other: what follows from this special power? The answer, of course, is that this relation is significant because it coincides with the relations found for accelerations and radial distances among the planets, and among the satellites of Jupiter (later also among the satellites of Saturn; but only one satellite of Saturn was known at the time of the first edition of the *Principia*). Nevertheless, the question remains: what follows from this agreement of relationships? That x is to y as Y^2 is to X^2 is a propositional schema that has innumerable true instances; we cannot ordinarily draw strong inductive conclusions from them. For instance, the fact that the intensity of illumination by a point light source varies inversely with the square of the distance has no bearing upon the relation of the moon to earthly bodies.

There is more than one technical way to present the point that is crucial here. Indeed, having given one argument in the first edition of the *Principia*, Newton himself later added, in a scholium, an alternative version. One can put it that *if the moon* (or a small piece of the moon) *were brought down to the region of the earth's surface, the force upon it would increase continually in the same proportion as that by which the square of its distance from the earth's center diminishes*; and hence, by the data already cited, would—when the body reaches the earth—be just the force that we should then call its "weight." The proposition in italics is the conclusion that Newton arrives at, by induction, from the astronomical evidence: partly from that about the moon in its orbit; but, as I have explained, chiefly from that about the other bodies in the solar system. I have formulated the proposition here using a contrafactual conditional. Newton, in his first version, says rather: "Since *that force*, in approaching the earth, *increases* in the inverse duplicate ratio of the distance . . . a body in our regions *falling with that force* must describe . . . in the space of one second [the same distance that freely falling bodies do in fact describe in one second]." What, in this formulation, is meant by "that force"? Either—I claim—"the force that would be experienced by the moon if brought down," i.e., what I have said above, or "the field (whose intensity is an 'accelerative force') which the astronomical evidence shows to exist about all central bodies of the solar system." Clearly, there is no substantive difference between the two constructions of the phrase. That the second is perhaps closer to Newton's own way of conceiving his argument is suggested not only by his account of "accelerative force," which I have cited, and by his use here of the indicative mood ("that force . . . increases"), but still more by the scholium he added as a more ample explication of the argument. What he does in that scholium is to imagine *many new test bodies* introduced to (as I put it) "explore the field." He says, "Suppose several moons to revolve about the earth, as in the system

of Jupiter or Saturn; the periodic times of these moons (by the argument of induction) would observe the same law which Kepler found to obtain among the planets; [etc.]" Again we have, as in my first formulation, a contrary-to-fact conditional, whereas in Newton's first version, on my reading, a dispositional property is implicit. Point (1) of my note 4 is that one or the other of these (equivalent) devices—or, perhaps I should have said, something else, also equivalent to them!—*is needed in order to formulate the proposition in question.*

This conclusion may seem to lack demonstrative force; but, for me, it is based upon my conception of what the proposition at issue *is*: I see it as intrinsically involving such a component. If anyone is of another opinion, however, he ought to defend that opinion by showing how else to express the proposition.

What appears to me a more serious possibility is that the proposition we have been discussing might be bypassed: that is, that Newton's main conclusions might somehow be justified, on the basis of the data, through a train of inductive arguments of a more subtle structure, making no appeal to notions like field or disposition or "nomological connection." I do not see how to do this; but I don't pretend to settle deep questions about the logic of induction by that criterion—I have tried to make this clear in note 4. What I do maintain as of some significance is that, however the difficulties about inductive inference are resolved, the resolution must take account of, or be compatible with, the three points set forth in note 4. The first of these I have now defended at some length. The second, I think, is noncontroversial, and also presents no real difficulty. For the theory of inductive inference, it is point (3) that I consider most important. But this point has not been challenged; and I believe that what I have said bearing upon it in the paper and the note (and in some of the considerations advanced in the preceding portions of these remarks) is sufficient.

One further remark should still be made, however, concerning the relationship of point (1) to point (3). I have said that the possibility may exist of bypassing Newton's proposition that *if (a piece of) the moon were brought to earth, its orbital acceleration would become the acceleration due to gravity*, as a *premise* of his further inductive argument. I have given, in outline, the inductive argument which leads to that proposition as a *conclusion*; and have referred to the proposition as justifying, in Newton's logical account, the further conclusion that (to put it so for once) *gravity is the cause* of the moon's orbital acceleration. What I now want to point out, as illustrating what the theory of induction has to show how to deal with, is that this proposition of Newton's serves for him in an extremely straightforward way as a *premise to a further inductive conclusion* (of undoubted empirical value). In his argument for Proposition IV, Newton can assert that a piece of the moon, if brought to the earth, would experience a force increasing as the inverse square of the distance, for his astronomical evidence supports this. He *cannot,* at this stage, assert the symmetrical proposition: that a *terrestrial body, raised to the height of the moon, would diminish in weight* in that same ratio; for he has no evidence at all to support this. Once Proposition IV is established, however, and the acceleration field of Galileo has been identified with the acceleration field of Kepler, this new proposition *can* be asserted. Of course it has now become possible—three centuries later—to put both propositions to experimental test (in the opposite order): we have lifted terrestrial bodies to the moon, and have brought lunar bodies down to earth; and in doing so have confirmed both propositions directly.

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Outlines of a Logic of Comparative Theory Evaluation with Special Attention to Pre- and Post-Relativistic Electrodynamics

I. Introduction

It would be false to say that case studies drawn from the history of science have had no influence on the philosophy of science in the past ten years. The contributions of the late N. R. Hanson (1958), and S. Toulmin (1961), P. K. Feyerabend (1962, 1965), and T. S. Kuhn (1962) utilize examples drawn from the science of Aristotle, Buridan and Oresme, Galileo, Newton, Lavoisier, Dalton, Maxwell, and Einstein. On the basis of such examples these current authors develop their views of the nature of scientific thought, and though they by no means agree in all particulars with one another, their general "historical" approach has raised some perplexing questions for philosophers of science who have based their views on the more "logical" analyses of, say, Carnap, Hempel, and Nagel.[1]

By focusing attention on the richness and adaptability of historically discarded scientific theories, and on the many instances of theory competition that exist in the historical record, these "historical" philosophers of science have called into serious question many of the central doctrines of earlier philosophers of science.[2] In varying ways they have suggested that:

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[1] The distinction between the "logical" and the "historical" approaches to the philosophy of science is made in Shapere (1965). For representative selections of the former approach see Carnap (1956), Hempel (1965), and Nagel (1961).

[2] These central doctrines had not of course gone uncriticized before the last decade. In fact some of the work of the earlier critics of the logical empiricist approach, such