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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

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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."

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

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

[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

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,

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.