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History of Science and Criteria of Choice

Although falsifiability, confirmation, and predictive power—all involving appeals to observational data, however conceptualized—are important criteria for the choice and acceptability of hypotheses, they are not sufficient, even if one does not go so far as to hold them not to be necessary either. The belief that more is required has a very long ancestry. Writers and scientists of many ages—explicitly and implicitly—have searched for "explanations of the phenomena" at a "deeper" level, more solidly entrenched, even if this sometimes means no more than integrating their hypotheses within a well-established explanation scheme, or making reference to general conceptual classifications or to some purely logical aspects. There is a good example of this at the start of the "modern" period of science in certain comments on the system of Copernicus made by Kepler in his *Mysterium Cosmographicum*. In a somewhat critical vein Kepler there remarks that Copernicus had really achieved no more than to "derive his system" from the phenomena of nature, from "the effects." Now, Kepler goes on to say, "if this system was correct," this only showed that Copernicus had to a certain extent been "lucky." By contrast, "what is necessary" —and this Kepler here claims as his own achievement—was to show that one's system has been "articulated in accordance with grounds of reason which precede the natural phenomena and which can be understood from its causes, from the pure concept involved in its creation."[1]

Now these "causes" to which Kepler here alludes are not, as one might perhaps have expected, a set of "higher-level" physical explanations. Instead, Kepler introduces considerations of certain privileged concepts. Much in the vein in which Descartes later was to argue, he notes that in choosing hypotheses we must start with some limitations of or constraints on our concepts; begin with the most basic and simple ideas, such as the

[1] J. Kepler, *Die Zusammenklänge der Welten* (Jena, 1918), *Mysterium Cosmographicum*, sections 1–2, pp. 141–149.

straight, the circular, or (among the solids) the "platonic" bodies (regular polyhedra)—all of which moreover Kepler (somewhat fancifully) relates in turn to certain esthetic, mathematical, and even theological concepts, the relation being established under the guidance of the free play of analogy. For the sake of what is to follow, let us note especially that—as in this case—the response to the search for a "reason" or "cause" of some phenomenon (basic or derivative) is not always conceived in terms of a physical mechanism, but rather relates to some general notions and principles of a more "philosophical" kind. These are introduced because they are believed to yield a more substantial foundation, with the further heuristic function of serving as signposts and guidelines, as "anticipations" of what is not only hoped to be a correct theory but also felt to provide a more penetrating or "deeper" explanation. Such approaches are usually most prominent during periods of gestation and crisis, transitions from one conceptual scheme to another, when the novel ideas are still being questioned, frequently because of clashing with received modes of thought.

Thus, to take some examples from the case study that forms the subject of this paper, Newton will worry endlessly about the "cause or reason" of gravitational attraction. But one of the many and varied solutions of this problem which he entertains, as we shall see, turns out to provide a "reason" by alleging that gravitational phenomena exhibit teleology, and that the "cause of gravity" is its having the character of a "final cause." Again, Boscovich will be found to introduce his theory of force points as an application of the principle of continuity. Maupertuis in his attempt to secure acceptance for action at a distance—that "metaphysical monster" of the philosophers (as he calls it)—argues in its favor by noting that it satisfies the requirements of uniformity and analogy, for good measure adding the observation that the "effects" of attraction seem subject to the law of least action, a principle which in his eye not only provides the most all-embracing explanation of many of the laws of the physical sciences but possesses also a special rationale since it appears to him to imply a divine teleology.[2] Or, to mention one more example, Fechner's *Atomenlehre*, after giving an account of classical Newtonian atomic theory, regarded as a physical explanation, proceeds to a "philosophical" defense of Newtonian conceptions, by presenting the idea of simple material point-atoms with intervening forces as one which lies at the intersection, so to speak,

[2] Maupertuis, *Oeuvres* (Lyon, 1756), I, 46, 169; II, 257.

of a number of associated conceptions, for instance, simplicity, discontinuity, infinity, indivisibility, denumerability.[3]

All such justifications evidently involve an appeal to a supplementary set of ideas: maxims of simplicity and economy; considerations of an esthetic nature; principles of continuity or discontinuity; linkages with general metaphysical notions as for instance "the real does not change," "nothing comes from nothing," "the effect is equivalent to the cause"; or more generally, maxims like those of homogeneity, affinity (or the "analogy of nature"), teleological or alternative preferred explanation schemas, and even certain theological conceptions. Nor need we dispute in this context over the term "metaphysical"; it is perfectly possible to regard such ideas as having purely methodological or "regulative" significance, to use Kant's term for notions which (on his view) constitute the methodological equivalent of what had previously been misconstrued as possessing "metaphysical" status. Or we may even admit with Mach that such maxims have their source in an expression of "powerful instinctive yearnings" rather than a "metaphysical point of view."[4]

Let us mark the existence of such ideas as a separate vector of inductive thinking by labeling this supplementary component of choice the "architectonic component," to note that it contains supplementary rules for the construction of a scientific theory and its concepts, though we need not regard it as sharply separated from the other components that we shall distinguish.

In fact, considerations taken from the architectonic component, though usually forming part of the structure of a completed theory, frequently affect also a second component which relates more directly to the concepts employed in the construction of a scientific theory, and which involves what we may call an "explication" of their meaning. For the sake of brevity, we shall also say that while the architectonic component determines the "rationale" of a theory, or of a theoretical concept, explication is concerned with "intelligibility." Of course, the meanings of many terms entering into theories antedate their formulation; still, and especially in a developing science, such meanings are often found to undergo considerable change—whether in the light of considerations connected with the theoretical and experimental field forming part of the theory in question, or of more general considerations, will depend on the individual case. Certainly, in some important cases we find that explication of meaning frequently precedes the construction of theories. Also, we should note that quite often such explications, too, have been labeled "metaphysical," i.e., "metaphysical foundations" of the science in question, though once more we shall not worry overmuch about the terminology. Thus Galileo begins his disquisitions into mechanics by addressing himself to questions concerning the proper meaning of velocity, acceleration, etc.[5] Similarly Descartes's cosmology is preceded by a study of dynamics which starts with an investigation into what may properly be meant by space and motion.[6]

[3] G. T. Fechner, *Ueber die Physikalische und Philosophische Atomenlehre* (Leipzig, 1855), p. 152.

[4] E. Mach, *The Science of Mechanics* (LaSalle, Ill.: Open Court, 1960), p. 608.

It is a mark of the idea that this is not only a separate inquiry, but somehow again providing a special "basis," that the axioms arising out of such semantic investigations are frequently felt to be more firmly entrenched than purely empirical generalizations; witness for instance Einstein's reflections on the proper meaning of simultaneity, or Descartes's "a priori" elucidation of the laws of motion by reference simply to conceptual considerations concerning the notion of motion as a state, the principle of causation, maxims of simplicity and conservation, etc.[7] For many seventeenth- and eighteenth-century writers—including Locke, Newton, and Kant—such laws and properties are frequently described as constituting "the essence of matter"; witness Newton's reference to the "innate properties" of matter, including its "passive laws of motion" formed together with that matter "by God in the beginning."[8]

A considerable section of writings in the history of philosophy is in fact taken up by a critical evaluation of the very notion of an "a priori" or "metaphysical" or "conceptual" explication of the foundations of science. Thus for Kant the basic laws of matter and motion are a specially tightened form of the transcendental principles which themselves are held to be an inherent aspect of all empirical judgments.[9] Still later, for instance

[5] Galileo, *Dialogues concerning Two New Sciences* (New York: Dover, 1914), Third Day, pp. 153–169.

[6] Descartes, *Principles of Philosophy*, II, sections 1–40, in G. E. Anscombe and P. T. Geach, *Descartes: Philosophical Writings* (London: Nelson, 1964), pp. 199–218.

[7] G. Buchdahl, "The Relevance of Descartes' Philosophy for Modern Philosophy of Science," *British Journal for the History of Science*, 1 (1963), 229–249. For a classical statement and detailed exemplification of this view of the axiomatic and privileged character of conceptual axioms, see William Whewell's theory of "Ideas" as developed in his *Philosophy of the Inductive Sciences* (1840, vol. 1; *Works*, London: Cass, 1967, vol. 5).

[8] I. Newton, *Opticks* (New York: Dover, 1952), pp. 400–401; cf. *Principia*, ed. F. Cajori (Berkeley: University of California Press, 1962), pp. 398–400.

[9] I. Kant, *The Metaphysical Foundations of Natural Science*, trans. E. B. Bax (London, 1883), Preface; chapters 2 and 3.

in Poincaré, they are regarded as "conventions," and in our own day they are often viewed as vehicles for constructing the terminology of the science in question.[10] We find that the emergent concepts, when closely considered, are based partly on primitive experience, while partly they lie at the intersection of a number of laws which themselves have both empirical and normative components. The empirical content allows for possibility of change; the normative aspect yields a more static approach and thus a degree of stability. A limiting case of concept formation occurs when terms are—in the parlance of logicians—"implicitly defined" by the hypotheses of the theory into which they enter. More normally, and to allow for the possibility of testing of competing theories, the concepts have a richer basis, as just indicated, and the meanings involved straddle a number of possible theoretical situations.

This brings us to the third component which determines the choice of hypotheses, the one paid most attention to by inductive logicians, involving not only empirical confirmation but the formulation of laws and their integration into a system (nomothetic and systemic articulation). We may call this the "constitutive" component, though it, again, can clearly not be sharply separated from the other two, the explicative and the architectonic components, since they are more or less presupposed in the systematic development of theories—we need only remember the importance of analogical considerations.

There are some important precedents for such a triadic methodological structure, though it has not usually been formulated quite so explicitly. Implicitly it is contained, as already hinted, in the movement of scientific history itself. But making it thus explicit may give us a more definite guide for the interpretation and clearer understanding of some of its episodes. But for the moment, let us note some precedents in the philosophy of science proper. Of primary importance here are some of Kant's ideas, where—as I interpret him—a profound appreciation of something like the present structure first appears.[11] Kant distinguishes two factors that determine the acceptance of hypotheses; in the language of this paper they

[10] H. Poincaré, *Science and Hypothesis*, part III (London: Walter Scott, 1905). B. Ellis, "The Origin and Nature of Newton's Laws of Motion," in R. G. Colodny, ed., *Beyond the Edge of Certainty* (Englewood Cliffs, N.J.: Prentice-Hall, 1965), pp. 29–68. D. Shapere, "The Philosophical Significance of Newton's Science," *Texas Quarterly*, 10 (1967), 201–215.

[11] Cf. G. Buchdahl, *Metaphysics and the Philosophy of Science. The Classical Sources: Descartes to Kant* (Oxford: Blackwell; Cambridge, Mass.: MIT Press, 1969), chapter 8, section 4 (iv).

are equivalent to the "explicative" and the "constitutive" components. (My use of the term 'constitutive' is of course rather wider than Kant's since I include in it not only the "empirical data" but also the nomothetic and systemic aspects of theorizing.)

According to Kant, then, before we can accept a hypothesis, two requirements have to be satisfied, first, that it should account for the data in a strongly predictive fashion, without the employment of auxiliary ad hoc constructions; this determines its "probability." Secondly, the *possibility* of the concepts involved in the hypothesis must have been antecedently established.[12]

Take the concept of action during impact. As we shall see in the course of this paper, this is just one of the concepts which—though basic for mechanics—is held by many of the physicists and philosophers of the seventeenth and eighteenth centuries to be unintelligible or "inconceivable." Kant sets out to demonstrate its "possibility." First it is shown that mutual interaction as a pure concept or principle is presupposed in the form of all empirical judgments concerning the coexistence of bodies in space. This transcendental principle is then applied to the empirical concept of matter, here defined as that which is capable of exerting moving force. When this is worked out in detail—involving a "construction" which employs the distinction between "relative" and "absolute" space—the result (according to Kant) turns out to be the law of action and reaction.[13]

Another instance, even more important for what follows, is the application of the category of quality to the concept of matter, this time analyzed as that which fills space. Kant purports to prove that such a filling of space can only be conceived in terms of the joint action of repulsive as well as attractive forces, both being presupposed in order to obtain the notion of a material body with finite boundaries. Note in particular that this procedure is the opposite of the Newtonian: unlike the latter, we do not *first* posit matter and *then* "hypothetically" add action at a distance; rather this action is a necessary condition of the conception of matter as such, and defines its "possibility in general."[14] Kant distinguishes this kind of possibility ("real" possibility), the result of a "conceptual explication," quite

[12] I. Kant, *Introduction to Logic*, trans. T. K. Abbott (New York: Philosophical Library, 1963), pp. 75–76. I. Kant, *Critique of Pure Reason*, trans. N. Kemp Smith (London: Macmillan, 1933), pp. 241, 613 (A222, A770).

[13] Kant, *Metaphysical Foundations*, chapter 3, Proposition 4, pp. 223–230.

[14] *Ibid.*, chapter 2, pp. 170–190.

sharply from mere "logical" possibility, just as it is equally distinct for him from "probability," which is a function of empirical confirmation.

Finally, for Kant all this is still further distinguished from the regulative certification of a concept. Thus, to take our last example, the conception of gravity—as distinct from the demonstration of its "possibility"—is shown also to be an expression of the demand by "reason" for homogeneity and analogy, which dictates the subsumption of the various phenomena of "dynamics," described by Galileo and Kepler, under the single conception of gravity, thus exhibiting their essential affinity relative to each other.[15] As in Newton (we shall see later) so here analogy bestows a "rationale."

Of course, the three components here distinguished are not always presented in such logically tight patterns. But something like explication and regulative articulation does evidently form an intimate part of scientific activity, and at times, especially in the early stages of development, it has much the largest share of it, rather than empirical testing. This incidentally shows that the latter cannot by itself be used as a means of demarcation between science and non-science ("metaphysics"), or between good and bad science.

Some of the later historians and methodologists of science, especially those influenced by Kantian thought patterns, follow a similar classification. Whewell distinguishes explication of concepts, colligation of facts, and finally consilience of inductions together with simplification of theories. Explication of concepts is here exhibited in particular as a powerful element of scientific development, and in a number of ways, whether this concerns settling on the right sort of candidates for satisfying some general principle such as conservation, or the justifiable model for atomic constituents, or a suitable definition of species, or what have you. Whewell moreover makes it quite clear that all this is no mere dictionary activity but at all stages involves attention to observed facts, so that again one cannot too rigidly separate out these three aspects of what jointly he calls induction.[16] And this has perhaps become even clearer in the recent discussions concerning "paradigm" formation and change.[17]

To mention another precedent, Heinrich Hertz, in the introduction to

[15] Kant, *Critique of Pure Reason*, pp. 544–555 (A662).

[16] W. Whewell, *The Philosophy of the Inductive Sciences*, vol. 2, book XI, chapter 2, section ii, pp. 11–16.

[17] Cf. T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962).

his *Mechanics*, again employs a classificatory triad. His three criteria of choice are permissibility, empirical adequacy or correctness, and suitability or appropriateness of hypotheses (or conceptions or models). "Impermissible" is what contradicts "the laws of thought." (Hertz may have had in mind here not only the "laws" of formal logic, but also the "laws" of Kantian "transcendental logic," i.e., categorial principles of the understanding.) For instance, in Newtonian mechanics the idea of "force" leads to the perplexing notion of "centrifugal force" which bodies seem to possess in addition to their inertia.[18] Such conceptual explications quite frequently lead to the rejection of one notion in favor of another. Hertz rejects "force" as a primitive concept; Faraday discovers a putative "contradiction" in the Newtonian conceptions of atoms and empty space, which (he contends) seems to involve the consequence that sometimes space acts as an insulator, and sometimes as a conductor, a contradiction which (he argues) shows that the original conception "must be false."[19]

If we grasp that all three of our components determine the choice of hypotheses at any stage of a science, we shall be even more prepared to find explicative and architectonic functions emphasized at periods of "anticipation," when hypotheses have yet to be formulated, and conceptual patterns have still to be developed. Yet even A. E. Burtt, in his seminal *Metaphysical Foundations of Modern Physical Science*, otherwise so sympathetic to our view, was still apt to censure Kepler's habit of linking his physics to a background of religious speculation—thus misunderstanding its real purpose, which was that of linking it to a set of regulative notions like those of simplicity, unity, economy.[20] And older whiggish historians of science have shown of course even less sympathy, for instance, with an Aristotle who (in *De Caelo*) could define the number of possible "natural motions" by reference to quasi-grammatical considerations. All this suggests that an awareness of the existence and significance of a richer structure of methodological components supplies us with a set of historiographical tools that may yield more sympathetic insights resulting from historical investigations. Furthermore, let us note that it is a study of the classical writings of philosophy that is now found to yield some important

[18] H. Hertz, *The Principles of Mechanics* (New York: Dover, 1956), Introduction, pp. 2–6.

[19] M. Faraday, *Experimental Researches in Electricity* (London, 1844), vol. 2, pp. 286–287.

[20] E. A. Burtt, *The Metaphysical Foundations of Modern Physical Science*, rev. ed. (London: Routledge and Kegan Paul, 1932), chapter II (D), pp. 46–52.

items for our more complex historiographical toolbox. And finally, per contra, evidently aspects of scientific methodology can throw fresh light on some of the basic interests and assumptions of classical philosophy.

Still, there is a possible objection to regarding our explicative and architectonic criteria as determinants of hypotheses. Such criteria may be useful and perhaps even necessary devices for the *selection* of hypotheses, but they can hardly—so it will be said—affect the final acceptability of hypotheses, their factual strength, or more bluntly, their "truth." I will leave the question of "truth" for later. For the moment, we will reply that it is no mean or unimportant virtue of the supplementary criteria if they narrow down the choice of hypotheses. One may of course urge that before any maxims of selection can get a grip, one needs first a general principle of finitude in order to limit the potentially unlimited number of alternatives. But this is more an objection to the general possibility of a rational reconstruction of scientific reasoning, and I propose at any rate to leave its consideration for later too. Certainly, even at a more limited level, voices are divided. Thus, it is not uninteresting to note that some contemporary logicians of science, such as W. C. Salmon, have attempted to construct a model of reasoning (in terms of a "Bayesian" approach) where these kinds of supplementary criteria are held to define at least the *notion* of creating a finite antecedent probability for a hypothesis.[21] On the other hand, in a methodology like Kant's, not only is explication of "possibility" not assumed to have any significance for posterior probability, but it is not clear whether it affects explicitly his judgment of even prior probability. And it is doubtful whether Kant thought of explication as providing a straightforward old-fashioned foundation of induction; to this question, too, I shall return later. Similar considerations hold for the architectonic component: the task of regulative principles consists in guiding our search for system and explanatory depth, and he sometimes seems to hint that our criterion of truth *is* just such "success" as answers to our need, as though "by a lucky chance,"[22] to provide systematic integration of our empirical laws,[23] and "reason" tells us how we *ought* to set about doing this by means of regulative ideas.[24] Moreover, since there is no "world-in-itself" outside the forms of language created by explication and beyond systematic integration due to the regulative activity of the rational scientist, such relative "success" as we have will *define* or be "the touchstone" of the empirical truth of our rules.[25] In a similar way, many modern logicians would want to replace the "old problem of induction" by a systematic search for conditions defining models of scientific inquiry that explicate notions of reasonableness and predictive power. It has certainly become obvious that one cannot transfer any simpleminded notion of truth to the level of scientific aims without some considerable adjustments and reinterpretation.

[21] W. C. Salmon, "The Foundations of Scientific Inference," chapter 7, in R. G. Colodny, ed., *Mind and Cosmos* (Pittsburgh: Pittsburgh University Press, 1966), pp. 257–265.

[22] I. Kant, *Critique of Judgment*, trans. J. H. Bernard (New York: Hafner, 1951).

[23] *Ibid.*, p. 16.

[24] *Ibid.*, p. 10; cf. *Critique of Pure Reason*, pp. 538–539, 544 (A651–A653, A660, A661).

II

I shall return to this theme toward the end; for the moment, the best defense for any methodological structure like the one here proposed is to observe it at work; to indicate the relative weights which attach to the different components within the ebb and flow of scientific development; to see what clarification this produces for our understanding of the conflicting strands of that development. There can certainly be no doubt that the satisfaction or nonsatisfaction of such criteria has often determined acceptance or rejection of hypotheses, and this is perhaps a stronger support for their being criteria than some abstract, purely logical set of definitions of what is to count as "confirmation" or "acceptability." And unless such a timeless set, together with its "justification"—not to mention justification of the notion of "justification" itself—*is* produced, any charge of "subjectivism" or "psychologism" or "historicism" against such an approach must remain somewhat vacuous.

The historical case that I want to consider in more detail is the conception of gravity, partly because of its excellent documentation, and partly because one can judge it now through the method of hindsight.[26] But my remarks are not to be taken as anything like complete or even partial history. I shall only select a few logical aspects, and consider them in the light of tensions, proffered solutions and responses, on the part of a handful of scientists, philosophers, and logicians, mostly belonging to the periods that precede twentieth-century sophistication both in the field of recent science *and* in that of its philosophy, at a time when worries about action at

[25] *Ibid.*, pp. 535, 556 (A647, A680).

[26] See M. B. Hesse, *Forces and Fields* (London: Nelson, 1961), chapters 7–8, for a critical history; also A. Koyré, *Newtonian Studies* (London: Chapman and Hall, 1965), chapter 3, appendixes A–C.

a distance were still a relatively live issue. And I shall consider the whole case of course through the light of my three components. Also, and incidentally, one of the tests for our triadic classification will be whether it can shed light on some of the apparent contradictions in Newton's own pronouncements on this subject.

The constitutive component can be dealt with very briefly. We shall regard it as formalized by Newton's account in the *Principia*, representing what he labels his "experimental philosophy," and whose achievement is summed up by him in the General Scholium of 1713 as having "explained the phenomena of the heavens and of our sea by the power of gravity."[27] Fundamentally, the enterprise is deductive and systematic, basing itself on certain empirical laws, e.g., those of Kepler and Galileo, using certain axioms such as the laws of motion, jointly yielding the formula of gravitation. By invoking in addition certain "Rules of Reasoning,"[28] including principles of causality, analogy, and induction, the scope of the gravitational formula is generalized so as to yield the law of universal gravitation, which is thereupon tested or confirmed by showing it to yield further positive predictions of its applicability. The crucial conception involved is that of gravitational attraction at a distance.

In the light of what follows, it is important for a moment to consider the language in terms of which gravitational force occurs in the body of the *Principia*. Book I, Proposition ii, speaks of bodies being "urged by a centripetal force directed towards" the center of rotation (page 42).[29] Page 56 (I, xi) speaks of the "law of centripetal force tending to the focus of the ellipse." Page 406 (III, iii) speaks of "the force by which the moon is retained in its orbit" as tending "to the earth." Page 407 (III, iv) says that the "moon gravitates towards the earth, and by the force of gravity is continually drawn off from a rectilinear motion." Finally, page 414 (III, vii) speaks of "a power of gravity pertaining to all bodies." This is pretty strong language; and its strength is attested by Newton's claim, as we have found him say, that the phenomena can be "explained . . . by the power of gravity" (page 546). Moreover, page 407 seems to suggest that quite often Newton thinks of this "power" as "drawing" bodies from their inertial path. It is, however, well known that Newton shied away from interpreting this "drawing" as a "pull" *exerted by the bodies* on other bodies

[27] *Principia*, p. 546.
[28] *Ibid*., pp. 338–340, 406–415.
[29] References are all to *Principia*.

thus "drawn" toward them, an action, moreover, which would—as far as concerns the model implied by the *Principia*—take place across empty space.

Now the reason for Newton's abstemiousness has something to do with his own and his contemporaries' explication of the concept of matter. It emerges at its most uncompromising in the famous letter to Bentley: Action of "brute matter . . . without mutual contact," or without something "which is not material," is there declared to be "inconceivable."[30] This view, in line with that of Newton's Leibnizian critics, is echoed also with special emphasis by Locke, when he writes that gravitational attraction "cannot be conceived to be the natural consequence of that essence [namely, essence of matter; and that it cannot be explained or made] conceivable by the bare essence . . . of matter in general, without something added to that essence which we cannot conceive."[31]

Evidently these protests have something to do with contemporary explications of the concepts of matter as well as of action, force, and—more generally—causation. Before turning to this, one or two minor points. It would of course be perfectly possible to hold that explication, and, with this, interpretation of the formalism of the *Principia*, is no more than an extra, and that the deductive development of the quantitative relationships can quite well proceed without it. Newton himself sometimes sug-

[30] Third Letter to Bentley; in I. Newton, *Papers & Letters on Natural Philosophy*, ed. I. B. Cohen (Cambridge, Mass.: Harvard University Press, 1958), p. 302. Contrary to the interpretation of some recent commentators, I take it that Newton's emphasis here is on the *impossibility* of the action of matter on matter without an intervening *material* medium, and *not* the *possibility* of such action by means of an intervening *immaterial* medium. The phrase "which is not material," in *this* context, does not mean "which is immaterial." This latter interpretation has sometimes been proposed because (as we shall also see later in this paper) Newton—even in the same letter to Bentley—and more explicitly some of Newton's disciples such as Bentley and Clarke did also hold that attraction could be given a rationale by regarding it as due to a spiritual bond whose foundation is in God. But my point is that this escape route belongs to the architectonic component, and that it never affected Newton's assessment of the explication of the concept of matter. It is *because* of that explication that recourse is taken to God and teleology, but that recourse *never canceled* the explication, nor was there any inconsistency with it, and consequently Newton did not have to abandon his views expressed in the Bentley letter. The architectonic escape—by yielding a "rationale"—is provided in order to reconcile the continued use of the concept of attraction at a distance, which certainly Newton did not give up at any stage, with the explication of the concept of matter which seemed to render it "unintelligible." The teleological approach was a halfway house between total rejection of gravity and a fullfledged physicalist interpretation.

[31] Quoted in Koyré, *Newtonian Studies*, p. 155. Some further background to the relations between Newtonians and anti-Newtonians I have also given in my "Gravity and Intelligibility: Newton to Kant," in R. E. Butts *et al*., eds., *The Methodological Heritage of Newton* (Toronto: University of Toronto Press, 1969).

gests this, as when in his comments on Definition VIII he says that he considers terms like "attraction" and "impulse," and any such "forces," "not physically, but mathematically."[32] This is the approach later summed up (for the case of electromagnetic theory) in Hertz's famous dictum that "Maxwell's theory is Maxwell's system of equations."[33]

However, it is difficult not to regard your symbols as pointing to a minimum of physical reality, and for the most part—as our quotations from *Principia* strongly suggest—Newton does employ the *term* 'force' as denoting a physical cause, as "an action exerted upon a body" (Def. IV).[34] What otherwise can he mean when he says in Query 31 of the *Opticks* that the "word" attraction is meant "here to signify only in general *any force by which* bodies tend towards one another"?[35] So the question of what interpretation ("meaning") can be given to the expression 'gravitating matter' has not been banished; the skepticism implied by "only" is quite evidently due to Newton's analysis of the concept of matter and action which still continues to play a part. The italicized words in fact look in two different directions, corresponding to two different paths.

1. Interpreting "any force by which" as "any law of force in accordance with which" gives substance and meaning to Newton's continued assertion that "gravity does really exist, and acts according to the laws which we have explained,"[36] with the possible implication that Newton wishes to restrict the concerns of his "experimental philosophy" to the lawlike kinematical phenomena involved. And this reduction of causal efficacy to rules of motion is of course the approach subsequently made familiar in the philosophy of Locke, Berkeley, Hume, and Kant, not to mention later thinkers in a similar strain.

2. The second path is Newton's ever-recurring remark that his reference to gravitational attraction still awaits an account of "how these attractions may be performed."[37] But here again the conceptual analysis casts its

[32] *Principia*, p. 5.

[33] H. Hertz, *Electric Waves*, trans. D. E. Jones (London, 1893), p. 21. Hertz was primarily concerned to assert the independence of Maxwell's equations, whether they are themselves given an interpretation or not, from any hypotheses concerning the particular structure of the ether transmitting electromagnetic radiation. But one may also understand his slogan in formalist fashion, so as to imply neutrality concerning the question of the physical nature of the "waves," and hence, of the "meaning" of the symbolism of the equations.

[34] *Principia*, p. 2.

[35] *Opticks*, p. 376.

[36] *Principia*, p. 547.

[37] *Opticks*, p. 376.

shadow (see footnote 30), for Newton usually makes it plain that the explanation must avoid an account that would "attribute forces, in a true and physical sense, to certain centres."[38] How is he going to avoid this?

Our methodological trichotomy suggests that the tension to which Newton is subject may be resolved in more than one way.

a. The first type of solution belongs to the "constitutive" component, involving physical or mechanical hypotheses. To be sure, Newton seeks jealously to preserve the formal purity of his axiomatic account in the *Principia*. Unlike the work of his continental competitors, for instance Leibniz and Huygens, he does not permit "contrary hypotheses" (putatively forced upon us in virtue of the conceptual impossibility of action at a distance) to impugn the "truth" of "propositions inferred by general induction from phenomena," as he formulates this in the fourth rule of reasoning.[39] On the other hand, physical hypotheses concerning "the manner of action" are permissible, provided that for the time being they do not get mixed up with the official systemic account. Such a hypothesis was for instance introduced by Newton into the 1717 edition of *Opticks*, expressly stated to "show that" he did "not take gravity for an essential property of bodies."[40] This was the hypothesis of an ether, itself possessing a particulate structure. But its introduction involves fresh difficulties. Although the hypothesis avoids attractive forces, it does operate with "forces" "by which those particles . . . endeavour to recede from one another"[41]—a curious makeshift. Our perplexity (unless we charge Newton with sheer muddleheadedness and contradictory attitudes) is perhaps somewhat lessened if we take him to offer a mechanism which reduces attraction between large bodies to repulsive forces acting between small particles at short distances, possibly in order to provide a unitary (and hence, *simpler*!) mechanism, incidentally thereby also perhaps seeming to answer his Leibnizian critics.[42]

b. Still, it suggests itself that there must be some further considerations for resolving the puzzle of how Newton could—in the light of the Bentley letters—continue to operate with the conception of distance forces as such, if we ignore his occasional formalist escape route. Such an alternative is

[38] *Principia*, p. 6.

[39] *Ibid.*, p. 400.

[40] *Opticks*, Advertisement II.

[41] *Ibid.*, p. 352.

[42] As suggested, for instance, in Joan L. Hawes, "Newton's Revival of the Aether Hypothesis and the Explanation of Gravitational Attraction," *Notes and Records of the Royal Society*, 23 (1968), 210.

provided by criteria belonging to the architectonic component, analogy, harmony, teleology being here the relevant ideas. One of the arguments which belongs to this category is stated almost explicitly in Query 31, which harks back to Newton's prolonged studies in the phenomena of electric attraction. He asks whether it is not true that "small particles of bodies" have "forces, by which they act at a distance," and goes on to say: "For it is well known, that bodies act one upon another by attractions of gravity, magnetism, and electricity; and these instances shew *the tenor and course of nature*, and make it *not improbable* but that there may be more attractive powers than these. *For nature is very consonant and conformable to herself*."[43] Evidently, then, the existence of *analogous* cases adds its own strength.

c. This reference to nature being conformable to herself occurs in many places, e.g., in the comments on Rule III, with its reference to "the analogy of nature, which is wont to be simple, and always consonant to itself."[44] The force of this architectonic component seems then to supply us with an answer to the riddle how Newton could escape between the horns of the dilemma of his talk about forces between small particles—or, for that matter, large bodies—and the impossibility of action at a distance. What he does is to fall back on "final causes";[45] and nowhere is this clearer than in the 1706 edition of *Opticks*, in Query 28, written at a period when he was temporarily rejecting the hypothesis of an ether altogether. To the question How are we to explain attraction if we reject an ethereal medium, he replies with a counterquestion: But surely, "nature does nothing in vain"? And the subsequent context of that Query refers us to evidence of design in both animate and inanimate nature, ending with the comment that it most certainly "appear[s] from the phenomena" that there is a divine being, the source of the harmony of nature.[46] Now as Kant held later, this reference to God as the origin of design may be interpreted as a pseudo-constitutive object, which we place in the "idea" of design, to simulate what is in fact a *regulative principle* whereby nature is regarded *as if* it were designed in accordance with the systematic activity of scientific reason.[47]

This emphasis on support from harmony is also to be found in some of

[43] *Opticks*, p. 376.
[44] *Principia*, pp. 398–399.
[45] *Ibid.*, p. 546.
[46] *Opticks*, pp. 369–370.
[47] Kant, *Critique of Pure Reason*, pp. 556, 559, 561 (A681, A686, A688).

Newton's unpublished notes for some sections of the *Principia*, as has been emphasized recently by McGuire and Rattansi, where we find Newton relating gravitation to the general harmony of nature, appealing to the authority of the Pythagoreans' view of divinity, "inspiring this world with harmonic ratios like a musical instrument,"[48] an attitude which represents a considerable current of Neoplatonic ideas in seventeenth-century thought.

Providing a "rationale" for gravitation is, however, not the same thing as pronouncing it to possess physical reality—contrary to what some commentators have recently maintained.[49] To use a form of speech implied in Clarke and Bentley, to "spiritualize" gravitational force is not to physicalize it outright.[50] While "God in the beginning"—to use the language of Query 31—created the atoms and endowed them with certain innate qualities such as solidity, hardness, impenetrability and inertia together with the relevant "passive laws of motion"—making up, as we shall discuss presently, the "essence" of matter—"active principles" such as gravity, fermentation, and cohesion have a more ambivalent status. Sometimes Newton employs the phenomenalist route, when he says that he regards these principles only as "general laws of nature," whose "causes" are "not yet discovered."[51] But two pages previously, he has already referred to the "active principle" of gravity as "*the cause of* gravity,"[52] indicative of his ambivalent approach. What is really involved we can guess from the fact that no sooner has he said that "the causes of those principles are not yet discovered" than he again abruptly shifts his text to a discussion of "the counsel of an intelligent agent"—clearly a reference to analogy and teleology once more.[53]

To sum up Newton's procedure, we find that the architectonic component acts as a resolvent of the tension set up between the denial of attraction consequent to his conceptual analysis and the urge to provide a physicalist interpretation for the constitutive component (the axiomatic portion of *Principia*). And as Newton says there: to "discourse" of God "from

[48] J. E. McGuire and P. M. Rattansi, "Newton and the 'Pipes of Pan,'" *Notes and Records of the Royal Society*, 21 (1966), 108–143.
[49] Cf. A. R. Hall and M. Boas Hall, ed., *Unpublished Scientific Papers of Isaac Newton* (Cambridge: Cambridge University Press, 1962), p. 197. See also footnote 30, above.
[50] For Clarke, cf. *Rohault's System of Natural Philosophy* (London, 1723), p. 54n (Clarke's note). For Bentley, cf. *Eight Sermons* (Oxford, 1809), Sermon VII, pp. 227, 234, 238.
[51] *Opticks*, pp. 401–402.
[52] *Ibid.*, p. 399.
[53] *Ibid.*, p. 403.

the appearances of things," or as we might say, to provide such a teleological rationale, "does certainly belong to natural philosophy," as distinct from "experimental philosophy," i.e., our constitutive component.[54]

Here again, the history of subsequent philosophy can provide more explicit texts, though perhaps no timeless defense, for the attempt of the historian of science to interpret Newton's ideas. Newton's problem of providing a gravitational bond, without endowing bodies with a corresponding innate property of attraction, or without physicalizing a rational entity like attractive force, is profoundly generalized by Berkeley whose basic metaphysics of itself already leads to the denial of all "necessary connections" and "active forces" between physical things, the latter being regarded by him as no more than so many "passive ideas." Here, too, the missing link is provided by God's causal activity. But more: Berkeley is aware of a further problem we alluded to before and generalizes it: why interpolate a hidden mechanism (such as Newton's ether) if all change is really due to the direct action of a spiritual type? Berkeley's answer is classical: such a mechanism makes possible a rational and harmonious explanation of the phenomena in accordance with the laws of nature—i.e., explanation needs continuity; secondly (he adds), the production of the phenomena in accordance with the "rules of mechanics" is an expression of the "wise ends established" by the deity: a teleological principle.[55] So a preferred explanation form (here teleology) once again provides a rationale, where the nexus of *physical* continuity has been snapped, partly because it had come to be interpreted (in Berkeley, and after him, Hume) as a "metaphysical bond." Similarly, Maclaurin, the historian of Newton's "philosophy," who was profoundly influenced by Berkeley, *both* introduces an ether to account for gravity *and* operates with the general principle that all power and efficacy derives from God. And the problem of the apparent superfluousness of the mechanistic medium is explained in similar fashion by arguing that God does not act through immediate volition, but employs "subordinate instruments and agents" to "perform the purposes for which he intended them."[56]

III

Our reference to a "metaphysical bond" brings us back to the point whose discussion we deferred, when meeting the protest that attraction of

[54] *Principia*, p. 546.
[55] G. Berkeley, *Principles of Human Knowledge*, sections 62, 72.
[56] C. MacLaurin, *An Account of Sir Isaac Newton's Philosophical Discoveries* (London, 1775), pp. 408–409.

matter for matter *in distans* was inconceivable to Newton and Locke, and not of course just to them: there is a stream of writers reaching right down to the end of the nineteenth century who repeat the complaint, as for instance still expressed uncompromisingly in Stewart and Tait, who insist as late as the 1880's that it is "impossible . . . for a moment to admit the possibility of such action."[57] And we know from Faraday's writings that the idea, to quote Tyndall, "perplexed and bewildered him," and that he "loved to quote Newton upon this point," "over and over again" coming back to and quoting the Bentley passages already mentioned.[58]

As I showed before, all these scientists and philosophers are assuming here some sort of interpretation of the mathematical formalism of the theory. Further, they clearly assume that there is *some causal relation* holding between bodies subject to the law of gravitation. But up to now we have only considered those attempts which, when faced by the supposed difficulties involved in the concept of attraction, imagine either that we must look for an intervening mechanism or that we must limit ourselves to a phenomenological account; and the latter, again, either by confining our attention to the requisite laws of the system, with their "causes or reasons" unexplained, or by providing such a "reason" through a teleological model—all three evidently being solutions espoused by Newton and some of his successors. What is still outstanding is an investigation of the explicative component as such. For clearly it would be possible to resolve Newton's difficulties by readjusting the explication of the concept of matter, and—as soon became evident—by providing some clarification of the concept of causal action involved. This, as we have already seen, was Kant's approach, almost coincident in time with a similar attempt by Boscovich, though the conceptual ramifications involved do not emerge there as clearly.[59] Similar hints of such an approach we find later also in Faraday, where he distinguishes between "experimental philosophy" and "speculations" whose purpose is that of "rendering the vague idea more clear," one of his instances being the concept of action at a distance.[60] Indeed, his answer to the question "what are gravitation and solidity" is remarkably similar to Kant's, although there were of course many other contemporary influenc-

[57] [B. Stewart and P. G. Tait], *The Unseen Universe* (London, 1885), p. 146.
[58] J. Tyndall, *Faraday as a Discoverer* (London, 1877), p. 82.
[59] Cf. Roger J. Boscovich, *A Theory of Natural Philosophy* (Cambridge, Mass.: MIT Press, 1966), especially part I, sections 16–31.
[60] M. Faraday, *Experimental Researches*, vol. 3, p. 408.

es: "Certainly not the weight and contact of the abstract [atomic] nuclei. [Rather] . . . one is the consequence of an *attractive* force, which can act at distances . . . and the other is the consequence of a *repulsive* force. . . ."[61]

Evidently such conceptual investigations are not purely a priori. They borrow heavily from the context of empirical and theoretical researches. Thus, Kant, seeking to repel the suggestion that bodies can only act when they touch one another, counters this with the question "What do we mean by touch?"; he answers this by noting that the required analysis (which—as he stresses—is empirical in nature) does not involve matters of pure geometry, but rather the fact that when we touch a body we *experience* a *force* of repulsion, which suggested that it was force that is the primary concept here involved.[62] As a consequence he espouses the solution—anticipated by some Leibnizians—of packing force (or action) into the very conception of matter, just as is subsequently done by all those who make field action the core of the whole phenomenon. But to do this, one had to break through a whole host of preconceptions concerning the concept of matter and of action, i.e., causation. Let us therefore survey some of the controversies that stemmed from the Newtonian approach. This will give us an opportunity to observe (at least in one instance) what is involved in conceptual explication.

For Newton, as we saw, matter is characterized by "passive laws" only; moreover, he imagines a closed set of "innate" properties, which he thinks of as its "essence," e.g., impenetrability, extension, mobility, inertia—but not of course gravity, which would endow matter with an active principle,[63] something which this generation was trying to avoid, perhaps because it seemed to reintroduce hylozoic conceptions. Its rejection is expressed in the strongest possible terms, as we have seen, by claiming it to be *inconceivable* that matter should possess attractive properties.

[61] *Ibid.*, p. 449.

[62] Kant, *Enquiry concerning the Clarity of Principles*, in G. B. Kerferd and D. E. Walford, trans., *Kant Selected Pre-Critical Writings* (Manchester: Manchester University Press, 1968), p. 20. Needless to say, this analysis was not accidental. The choice of the empirical element of force was suggested precisely by the importance which that concept had come to acquire in the Newtonian corpus. We see here clearly how empirical and theoretical developments come to affect the "meanings" of scientific terms.

[63] *Principia*, pp. 399–400; *Opticks*, Advertisement II. So instead of being "essential," gravity is said to be "universal"; *only* universal, because although it is a lawlike property, its magnitude varies, depending on the distance, and so is not a constant property (cf. p. 400), but still something, i.e., a universal property, because sanctioned by the constitutive and the architectonic components.

But in what sense "inconceivable"? Locke and Clarke occasionally suggest logical inconceivability, when they say that action at a distance involves a "contradiction,"[64] but we should not make too much of this, since the notion of "logical contradiction" is not at that time too sharply distinguished from other kinds of contradiction. People quite generally use "inconceivable" to refer not only to what is self-contradictory, but also to what clashes with possibilities to which not only traditional evidence but currently received modes of thought seem overwhelmingly opposed, a position which Kant's "metaphysical foundations" expresses admirably, if in a somewhat formal way. In fact, it is just because the situation is logically so fluid that we find such an extreme variety of positions vis-à-vis our concept. Altogether, these are too numerous and complex to deal with here in more than bare outline.

How then do the various schools of thought seek to avoid the putative difficulty of the conception of attraction at a distance, if we now discount the appeal to intervening media, as well as phenomenalist or teleological escapes?

There is first of all the school which includes such diverse figures as Locke, Maupertuis, and Whewell. All three agree in the belief in an "essence" of matter, which includes properties held to belong to matter in some sense "necessarily" or "primordially."[65] Locke and Whewell also seem to share the view that this necessity is in some way "transparent" to our intellect. And both Locke and Maupertuis contrast this with the case of gravity, which they regard as a property whose admitted possession by matter they hold to be "inconceivable." Surprisingly, however, none of them conclude that the possession is impossible. The pressure from the constitutive component, the "inductive success" of the theory, prevents this conclusion. Rather, they interpret "inconceivable" to mean "nonintuitive," not transparent to the intellect.[66] In another terminology, the gravity of matter is now viewed as a synthetic a posteriori truth.

[64] For Clarke, see H. G. Alexander, ed., *The Leibniz-Clarke Correspondence* (Manchester: Manchester University Press, 1956), Clarke's Fourth Reply to Leibniz, section 45, p. 53. Clarke says that attraction "without any intermediate means, is . . . a contradiction: for 'tis supposing something to act where it is not."

[65] Whewell, *Philosophy of the Inductive Sciences*, vol. 1, book I, chapter 4; book III, chapter 9, especially p. 258. Maupertuis, *Oeuvres*, I, 95, 101, speaks of "primordial properties," which are "regarded" as "essential properties of matter."

[66] For Locke, see Koyré, *Newtonian Studies*, p. 155, quotation from a letter to Stillingfleet; for Maupertuis, see I, 97–98: "The manner in which properties reside in a subject is always inconceivable to us" and "it is experience to which we owe our knowledge" of both the primordial and all other properties.

This conclusion will frequently appear under a skeptical guise, nowhere more than in Locke, who generalizes Newton's problem of how a body can act at a distance into the question of how it can act—or rather: how it can be seen, or be conceived, to act at all, by impact or otherwise, since "all matter is essentially passive."[67] And a similar escape is suggested by Maupertuis when he writes that "the manner in which the properties reside in a subject is always inconceivable for us. . . . Attraction is simply a question of fact. . . . It is in the system of the world that one must look whether a principle has an effective place in nature. . . ."[68]

Here the complaint has become metaphysically generalized. In the case of attraction, it might perhaps have been contended that the phenomenon itself cannot be "directly perceived" to take place; that it is only a theoretical interpretation; but not so for impact—unless you take a starkly phenomenalist stance, foreign to Locke's thinking. And so he writes that although "by daily experience" we observe the production of motion by impulse, the idea is totally "obscure and inconceivable": "the manner how hardly comes within our comprehension," so that we have only a very "obscure idea of an active power."[69] Here it becomes obvious that this is not just a difficulty engendered by the construal of matter as "passive," but an incipient critique of the notion of transient causality.

In the sequel there were two possible responses to all this. A prominent member of the first type of response was Mill. He accepts—but without Locke's skeptical undertones—the synthetic a posteriori status of the phenomenon of attraction, as he does of *all* empirical relations, and (commenting on Newton's letter to Bentley) indignantly rejects the charge of "inconceivability," which he simply puts down to the results of "past history and habits of our own minds,"[70] indeed labeling it a "fallacy of simple inspection." (In the same vein he also castigates as "fundamental errors of the scientific enquirers" most of the regulative ideas and maxims that we have mentioned before, such as principles of conservation, economy, simplicity.[71]) On the other hand, the price paid is the inordinate weight that has to be placed on induction pure and simple (part of the constitutive

[67] Cf. Locke, *Essay*, ii.21.4 and 23.28.
[68] Maupertuis, *Oeuvres*, I, 98, 103.
[69] Locke, *Essay*, ii.23.28.
[70] J. S. Mill, *System of Logic* (London, 1879), vol. 1, book II, chapter 5, paragraph 6, p. 275.
[71] *Ibid.*, vol. 2, book V, chapter 3, paragraph 3, pp. 319–320.

component), as a logical operation for which Mill has to claim some kind of absolute "validity."[72]

Not all the empiricists go that far. Hume for instance clamps certain constraints on causal action, such as the requirement of spatiotemporal contiguity, which in particular he applies to the case of action at a distance, where he insists that we should always "presume" that a "chain of causes" exists which links "distant objects" in contiguous fashion;[73] and he bestows considerable praise on Newton's "hypothesis" of "an ethereal fluid" as one such method of explanation.[74] Perhaps this "presumption" was also motivated by a conceptual point, for Hume held that "a vacuum and extension without matter" are strictly speaking "impossible to conceive."[75]

It is thus always the case that the weight of scientific reasoning will be found distributed in different directions. This brings us to the second response to the charge of inconceivability. This involves a growing realization that one may always modify a troubling concept and thereby remove the contradiction. Behind this there lies, however, a more general point, namely, that in the metaphysics of science (and metaphysics in general, for that matter) one cannot start with rigid definitions as in mathematics, but instead one needs to employ a more empirical and tentative method when approaching the basic concepts of a science.

A very interesting early instance of such a method can be found in the Newtonian John Keill's *Introduction to Natural Philosophy*. Though we need to start with definitions, he declares, a proper method construes such definitions not of an "intimate essence," in terms of "genus and difference," but as "descriptions" obtained from "acquaintance" with "bodies or their actions," or "deduced" from "some of their properties . . . discovered by" the "senses."[76] Thus, "solidity" must be defined, not in the old Aristotelian way of "impenetrability," or the "inability of two bodies to be in the same place at once," but as "that property of a body whereby it resists all other bodies" that press on it "on every side."[77]

Keill was among those influencing Kant, who took, as already noted, a

[72] *Ibid.*, book III, chapter 21, paragraph 2, p. 101.
[73] D. Hume, *A Treatise of Human Nature*, ed. L. A. Selby-Bigge (Oxford: Clarendon Press, 1946), book I, part III, section II, p. 75.
[74] D. Hume, *An Enquiry concerning Human Understanding*, ed. L. A. Selby-Bigge (Oxford: Clarendon Press, 1902), section VII, part I, p. 73n.
[75] Hume, *Treatise of Human Nature*, book I, part II, section IV, p. 40.
[76] John Keill, *An Introduction to Natural Philosophy* (London, 1720), Lecture I, pp. 7–8.
[77] *Ibid.*, Lecture II, p. 12.

precisely similar approach.[78] Displaying the "possibility" of a concept turns out to be a process whereby this is related to other basic concepts, both empirical and categorial; but the difference between this and the approach of empiricists like Mill consists in the sharp distinction between constitutive and explicative inquiry, the latter providing a special kind of "foundation," a bit like insisting that one's theoretical terms need to have an independent meaning before they can play their part in the construction of a scientific theory. In the case of action at a distance, the change of meaning might be summarized—as was done later by Stallo—not in the slogan that a body acts where it is—the old view—but rather, it is where it acts. For the "inconceivability" claimed for the concept of the action of matter, says Stallo, was an "incompatibility of new facts or views with our intellectual prepossessions"; not—as he argues against Mill—just a fallacious habit. And he adds that "a reconstruction of our familiar concepts of material presence" would of course lead to a new conception of matter, different from the Newtonian one of hard atomic particles separated from one another by empty space.[79]

The lack of interest shown by writers of this group in the "problem of induction" is testimony that the center of gravity of the weights determining the choice or acceptability of hypotheses has shifted toward the region of conceptual explication. In this way, the developments and disputes in the history of science illuminate the intentions of philosophers and methodologists, just as our understanding of *their* doctrines provides a new key for our grasp of some of the scientific disputes.

A summary of the structure of my argument so far may be helpful. I have distinguished three components that appear to determine the acceptability of scientific hypotheses, where each component evidently itself has a particulate structure:

1. Conceptual explication:
   intra-theoretical
   pre-theoretical
2. Constitutive articulation:
   formal presentation of system
   informal interpretation
3. Architectonic determination:
   regulative ideas and maxims
   preferred explanation types
   consilience

[78] Cf. note 62.
[79] J. B. Stallo, *The Concepts and Theories of Modern Physics* (London, 1885), chapter 9, p. 145.

Applying the schema above to the evaluation of the Newtonian conflict situation, I have distinguished between the original situation as it first presents itself to Newton and the subsequent attempts at mediation in order to provide a resolution of the conflict.

I. Original situation.
   1. Constitutive component: its informal interpretation yields an explication (intra-theoretical context) which suggests action at a distance.
   2. Conceptual explication: derived from pre-theoretical contexts and suggests "matter cannot act on matter at a distance."
II. Proposals for mediation.
   1. Manipulations of the constitutive component:
      i. formalist response: "we are concerned only with the calculus, the mathematical aspects."
      ii. phenomenalist approach: "we are concerned only with the kinematical and lawlike aspects of the phenomena."
   2. Recourse to the architectonic component:
      i. Alternative explanation types:
         a. hypothesis of an inter-phenomenal ether.
         b. conception of a supra-phenomenal or teleological setup: God as the "cause or reason" of gravity.
      ii. regulative ideas: the power of analogy.
   3. Reconstruction of conceptual component:
      i. reinterpretation of significance of "inconceivability" as "contingent" (synthetic a posteriori).
      ii. reinterpretation of concept of matter (borrowing from intra-theoretical context) as reducible to a center of repulsive and attractive forces (from "matter acts where it is" to "matter is where it acts").

IV

There is still left a question outstanding which I have briefly broached once or twice before. Suitable or "satisfying" conceptual explications, or the adoption of preferred explanation schemes, so it will be said, may lead one to *believe* in having got hold of the right theory, but they can hardly provide support *in fact*; that can only come from the constitutive factor, e.g., empirical confirmation. Such objections, however, clearly overlook a number of difficulties and facts. Facts: because it simply is the case that scientific theorizing has a lot to do with aiming to "satisfy" our quest for "understanding," and this evidently stands in need of our two other components. Difficulties: because the objection assumes both a simple view of scientific truth and an optimistic assessment of the nature of scientific inference, and of some transcendent "inductive foundation."

At this point it will be best to take the view that we cannot expect a solution of our problem if it is looked at through the spectacles of the "old problem of induction."[80] If instead we take such a position as espoused in Goodman's *Fact, Fiction, and Forecast* (to choose just one among many contemporary variants), our task can be viewed as that of providing satisfactory models for the explication of the conception of admissible "projections."[81] The provision of such models is to some extent a *descriptive* exercise, subject to certain conditions, e.g., that projections should be in accord with our normal "inductive intuitions," and that no paradoxes should arise. According to Goodman, the main condition that must be satisfied is that projected predicates should exhibit a certain degree of "entrenchment" with respect to normal inductive processes.

For Goodman, "entrenchment" was supposed to be a guarantee that such admissible predicates would yield lawlike projections, lawlikeness being thus linked in an essential way with entrenchment.[82] Goodman never offered a very satisfactory account of what might constitute "entrenchment," but the reference to "lawlikeness" provides us with a useful bridge to what has preceded in this essay. As we have noted, for Kant what I have called "conceptual explication" is equivalent to providing "metaphysical foundations." Now his view on the ultimate function of, as well as limitations on, "metaphysical foundations" is well expressed in one of his *Reflexionen* where he writes that the understanding can provide no more than the "ground of lawlikeness" of empirical laws, and not the "ground of particular laws," i.e., their empirical foundation. And he adds: "All metaphysical principles of nature are no more than grounds of lawlikeness."[83] This gives us a hint for a formal bridge between the metaphysics of nature (in the sense of explication) and entrenchment. Our conceptual and architectonic criteria could then be viewed as providing those

[80] Some recent writings have laid a similar stress on supplementary components; e.g., G. Schlesinger, *Method in the Physical Sciences* (London: Routledge and Kegan Paul, 1963); R. Harré, *Matter and Method* (London: Macmillan, 1964); *The Anticipation of Nature* (London: Hutchinson, 1965); Gerald Holton, "Presupposition in the Construction of Theories," in H. Woolf, ed., *Science as a Cultural Force* (Baltimore: Johns Hopkins Press, 1964).

[81] Nelson Goodman, *Fact, Fiction, and Forecast* (London: University of London, 1954), pp. 52ff.

[82] *Ibid.*, pp. 74, 75, 77, 95.

[83] *Kant's Schriften* (Ak. ed.), XVIII, 176 (No. 5414).

constraints which would assure a degree of "entrenchment" for the terms of a theory, it being understood that these criteria are only necessary but not sufficient conditions; for quite evidently we always require the check from the constitutive criterion.

Here a new objection will arise. It will be said—as I have indeed noted a moment ago—that these criteria are the result only of an empirical, and hence descriptive, survey of methodological activities arising out of the context of historical episodes. And it would then seem that such an account provides insufficient grounds for ascribing the character of "rationality" to the scientific procedure defined thereby. Now insofar as this expresses once again a complaint by an "old-fashioned" inductivist against the "new view of induction," I will not discuss that any further, since it would lead us here too far afield. But one might perhaps suggest that insofar as Kantian explications link up (however weakly) with categorial principles, such a procedure offers us at least a *simile* of rationality, with the further assumption that such a foundation has a raison d'être because it offers us "a way round the world." And the same remark would go for the architectonic principles, where they are viewed as heuristic methods without which no science would ever get off the ground. (Not for nothing did Kant regard them as "demands of reason"!)

But here a more substantial objection may be brought forward: such transcendental structures may perhaps enable us to construct a possible world, but they are no guarantee that they catch the sense of the "actual world." Once again, I will overlook the concealed old-fashioned inductivist note in this objection. Rather, what it suggests is that there are certain criteria, for instance, falsification, which seem more directly designed to facilitate an approach to the actual (observational and inferential) structure of nature. Now, partly, there lies behind this the consideration that to the falsification schema there corresponds a logically valid formula of reasoning (*modus tollens*), whereas the same cannot be said of, say, the Peircean schema of abduction, to which corresponds in fact only an invalid formula (the so-called "fallacy of affirming the consequent").

Now the correct comment on this seems to me that such contentions involve a confusion between a methodological schema and a logical formula. As has become abundantly clear in recent years, falsification as such (however much surrounded by additional provisos) is no guarantee of arriving at any "true" conclusions, including instantaneous rejections. What is correct is that the existence of apparently falsifying circumstances forces us to reconsider our ideas on a subject; but it is at best only one among many considerations. Certainly, we should not be tempted to slide from the compulsiveness of the logical formula into a belief in any conclusiveness of its application in a scientific (i.e., empirical) field.

Corresponding remarks apply to some of our other criteria. As my case study shows, conceptual explication does not move in a logical vacuum. For instance, we noted that the Kantian explication of matter borrows heavily from the suggestiveness of the Newtonian theory, i.e., the constitutive component.

It must be admitted, however, that there are obvious differences in degree of "depth" with which our different criteria attach to the final result, the construction and adoption of a theory. I purposely included "preferred explanation-type" among these criteria (with the prominent example of teleological explanation) because such a criterion seems particularly apt to provoke the objection of arbitrariness and historicity. But if the reader will reflect carefully over the whole complex situation, he will realize that the extent to which the different criteria express rational schemata varies only by degrees. And if it is thought that the existence of a formal correspondence (as in the case of falsification) should bestow the ceremonial title of rationality on a procedure, then the considerations here advanced suggest that—with the proviso of differences of degree in depth—the same privilege may be accorded to the other criteria as well. And it is for this reason that in this essay I have as a first step attempted a somewhat more systematic enumeration of, and comparative assessment of the different tensions between, such criteria when "at work" in an actual historical context.

COMMENT BY LAURENS LAUDAN

Gerd Buchdahl's paper is an extremely interesting one; more to the point, it is probably an extremely *important* one. With considerable skill and subtlety, he shows how inadequate—and how irrelevant to actual science—have been those philosophies of science which argue that the major (if not the only) determinant of scientific choice is empirical success. He says explicitly what many of us have felt but never expressed so clearly; namely, that the evaluation and assessment of a theory occur on several levels simultaneously, and that empirical adequacy (what Buchdahl calls the "constitutive" vector) constitutes only one of the many levels on which a theory can be debated and criticized. Thereby, he provides a rationale for the oft-observed, but generally unexplained, fact that although theories are (as Feyerabend, Kuhn, and Lakatos argue) "born refuted," they are nonetheless "rationally" accepted and compared. Buchdahl points to the kind of strategies and ploys which scientists can and do resort to when confronted with anomalies, criticisms, and exceptions. He shows that scientists can have reasons, apart from Kuhnian collective commitment, for retaining a theory in the face of contrary empirical evidence. He also indicates why a theory which is perfectly sound empirically may nonetheless be felt to be unsatisfactory and even unacceptable, which empiricists have never been able to explain. The fact that theories are challenged and defended at these different levels—the architectonic, the explicative, and the constitutive—reveals the question of scientific choice to be a much more complicated and intricate affair than is commonly thought. As Buchdahl points out vis-à-vis Newton, attacks and weaknesses on one level can often be compensated for, or at least "neutralized," by strengths at another level (and, of course, vice versa). Thus, even if Newton could not show that action at a distance was intelligible,[1] he could at least show that it suited the "analogy of nature," and that it was a fact about the world: that, in Buchdahl's language, it had an architectonic and constitutive foundation.

I do not have space here to consider the very perplexing question whether nonempirical arguments at the architectonic and explicative level are methodologically justified or whether (as some intransigent empiricists would maintain) they should have no place in the appraisal of scientific theories. What I do want to explore here is the suitability of these Buchdahlian vectors for explaining the kind of appraisals which scientists *have in fact used*, whether justifiably or not. Putting it another way, I want to consider briefly the usefulness of Buchdahl's schema to the historian of science who wants to understand the nature of scientific debate.

Buchdahl has taken a major step in this direction himself, with his treatment of the debate (between 1687 and 1850) concerning action at a distance and force. His case study already testifies to the explanatory power of Buchdahl's categories of analysis. But I think it also points to an ambiguity in Buchdahl's classification of components which I want to discuss here.

As Buchdahl makes clear, the strongest thing Newton's theory had going for it was its enviable empirical success, its confirmed explanatory

[1] I shall qualify this remark below.

power. On the "constitutive" level, therefore, the theory of gravitation had no serious rivals. Where the major problems arose, as everyone knows, was in the interpretation of action at a distance. Was it the case that bodies could really act where they were not, or was gravity a direct result of the will and action of God, or was gravitation rather an appearance to be explained in terms of kinematic collisions between ether particles and macroscopic bodies? These options, as well as others, were open to Newton, and to his critics and followers, and most of the debate about action at a distance from Newton to Faraday was an attempt to decide between such alternatives.

Most of the criticisms to come from Newton's contemporaries were of the following kind: Action at a distance is occult, unintelligible, mysterious, and unmechanical. (All more or less synonymous![2]) If Newton was to make his theory acceptable, claimed his critics, he must show how gravity could be reduced to, and explained in terms of, impacts, collisions, or other species of *contact action*.

Before we turn to look at Newton's attempts to deal with this criticism, I think we should consider what Buchdahl makes of the debate at this preliminary stage. On his view, the Leibnizian and Cartesian demand for a mechanical explanation of gravity is an argument exemplifying the *constitutive* component. (See his page 217.) I disagree fundamentally with Buchdahl here, and I think our differences are more than verbal ones. Let me try to explain why. Suppose that someone had argued (as Euler would forty years later) that Newton's gravitational theory gave inaccurate predictions for the motions of the moon, or for the perturbations of Venus. Such arguments clearly challenged the empirical adequacy and explanatory power of the theory of gravitation and thus belong to the constitutive component. Equally clearly, such arguments were fundamentally different *in kind* from the mechanist's argument against Newtonian theory. The latter did not draw attention to empirical difficulties, but to conceptual ones. It did not charge that Newtonian theory is empirically inadequate but that it is intellectually inadequate. It is too obvious to require elaboration that these cases are very different and that lumping them together

[2] That these terms were seen as more or less identical in meaning is confirmed by that famous passage in the *Leibniz-Clarke Correspondence* where Leibniz chides Clarke (and, by implication, Newton) as follows: "That means of communication [of attraction] (says he [Clarke]) is invisible, intangible, not mechanical. He might as well have added, inexplicable, unintelligible, precarious, groundless and unexampled. . . . 'Tis a chimerical thing, a scholastic occult quality." H. G. Alexander, ed., *The Leibniz Clarke Correspondence* (Manchester: Manchester University Press, 1956), p. 94.

under the "constitutive component" artificially coalesces two very different kinds of criteria of choice under a single head. But what is the alternative?

One might at first think that the mechanists were criticizing Newton's theory at the architectonic level, because Buchdahl has defined the architectonic level to include everything from "theological conceptions" to "maxims of simplicity and economy." When the mechanical philosophers charged that action at a distance was occult and unintelligible, their claim was (I believe) that Newton had not shown *how* actions at a distance could be reduced to the ontological categories of the mechanical or corpuscular philosophy. These categories were thought to specify exhaustively the kinds of things which made up the world. Unless Newton could show that action at a distance was deducible from (or reducible to) the kinds of entities postulated by the mechanical philosophy, then his theory was prima facie unacceptable. This is clearly a kind of criticism very different from what might be drawn out of considerations of simplicity or teleology. Indeed, it is sufficiently different in character to make it positively misleading to lump them both together under the same head. But it might still be said that Buchdahl's nomenclature is sufficient, and that the mechanists' critique falls under the explicative component. I am doubtful of this too; for, with the notable exception of Locke, none of the mechanistic opponents of action at a distance were confronted with a problem of meaning which required explication. On the contrary, they knew *precisely* what action at a distance meant, but wanted no part of it. When Leibniz wrote to Clarke that "a body is never moved naturally, except by another body which touches it and pushes it . . . any other kind of operation on bodies is either miraculous or imaginary,"[3] he was not saying that he did not know the meaning of action at a distance. He was claiming rather that he knew what it meant but believed it did not happen *in rerum natura* because "all the phenomena of bodies must be explained mechanically."[4]

Thus, the charge that gravity was occult is no more regulative or explicative than it is constitutive. I think the solution is to be found in a *quartum quid*. There is, in short, a fourth important criterion of choice which, for want of a better term, I shall call the "metaphysical" or "reductive" component. The reductive component generally specifies a set of entities, qualities, and modes of interaction which are regarded as both real and

[3] *Ibid.*, p. 66.
[4] Letter to Arnauld, 1686, *Die Philosophischen Schriften von G. W. Leibniz*, ed. C. I. Gerhardt (Berlin, 1875–90), II, 78.

basic. These privileged entities are viewed as "unexplained explainers" (to use Sellars's language) and a scientific theory or hypothesis is adequate (at the reductive level) if and only if it can be expressed or explained in terms of the privileged ontological categories. Thus, when the mechanical philosophers criticized the *Principia* because it did not show how gravity could be explained by impacts or collisions, their criticism involved not the explicative or constitutive but the reductive component.

We can now return to consider the question posed earlier; namely, how did Newton deal with the charge that his theory was unintelligible, because not reducible to the categories of the mechanical philosophers? As we have seen, Newton's critics realized that his theory had its major strength at the constitutive level, and its greatest weakness at the reductive level. Assuming (for the moment) that our four vectors are exhaustive of the kinds of criteria for theory acceptance, we can see that Newton had a number of alternative stratagems open to him, most of which he utilized at one time or another:

1. He could proclaim himself a proto-positivist, dismissing the importance of architectonic, explicative, and reductive components, and say it is a sufficient test of his theory that it is empirically well corroborated.

2. He could admit that the reductive argument against gravitation was a compelling one, and seek to eliminate actions at a distance by explaining them in terms of a mechanical ether.

3. He could argue that action at a distance *was* intelligible and that, in terms of a new nonmechanist ontology, distance forces are primary. This would be a counterargument (as opposed to a concession) at the level of the reductive component.[5]

4. Conceding that he has some minor difficulties at the reductive level, Newton could produce arguments for his theory taken from the architectonic and explicative levels.

Shrewd strategist that he was, Newton on occasion falls back on each of those ploys to defend himself. Thus, his famous disclaimer against making hypotheses, especially mechanical ones, has Newton adopting pose 1.[6] Haughtily dismissing the mechanistic reservations of his critics, he tells

[5] Still another example of a counterargument at the reductive level would consist in showing that contact action was unintelligible and incoherent. Although Newton himself does not take this option in his public writings, he does in unpublished manuscripts, as do several of his followers including Boscovich.

[6] Recall the classic passage from the penultimate paragraph to the General Scholium: "hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in *Experimental Philosophy*."

them that gravitation is a fact, whatever its cause might be,[7] and intimates that this is all his *Principia* requires or presupposes. At other times, however, he will opt for strategy 4, and it is one of the strengths of Buchdahl's analysis that it gives a coherent reconstruction and rationale for Newton's references to teleology, the analogy of nature, "nature doing nothing in vain," and so forth. We can now see that these are attempts to answer the charge of unintelligibility by falling back on an architectonic justification.

But what about options 2 and 3? These are both courses of action at what I have called the reductive level. Moreover, it is difficult to imagine Newton's having seriously pursued both options, for there is an obvious tension between them. In case 2, Newton would have to concede that the mechanists were right, and modify his system so that all apparent actions at a distance were viewed as the result of contact forces. In case 3, however, Newton would undercut the mechanists' argument by offering a new ontology of forces, where action at a distance is a "respectable" concept.

There are very great exegetical problems here, and it would be silly to act as if Newton unequivocally went one way or the other. I have no doubt that Buchdahl would agree with me on this point. Even granting the textual difficulties, however, I think that there is an interpretation of what evidence we do have which is rather different from Buchdahl's. With certain qualifications, Buchdahl maintains that Newton took what I have called option 2. Being himself more or less a mechanical philosopher, Newton—on Buchdahl's view—shared the general doctrine that *actio in distans* was inconceivable; accordingly, he offered a number of etherial hypotheses in hopes of explaining away action at a distance. Thereby, he sought both to silence his critics and to ease his own conscience.

Buchdahl's account, shared to a greater or lesser degree by Newtonian scholars such as A. R. and M. B. Hall, J. E. McGuire, Joan Hawes, and I. B. Cohen, seems to me to be a dubious reconstruction. I am inclined to suggest that Newton generally preferred option 3, thereby conceding little or nothing to the mechanical philosophers. The basic question at issue here, put in its most general terms, is this: Why, from 1675 until the last set of Queries to the *Opticks* in 1717, did Newton persistently return to the theme of an etherial explanation for gravity? Buchdahl, in consort with Cohen, McGuire, and the Halls, answers: because Newton was convinced that action at a distance was unintelligible and saw in a mechanical

[7] In Query 31 of the *Opticks*, for instance, Newton insists that attractive forces such as gravity are "manifest Qualities, their truth appearing to us by Phenomena, though their Causes be not yet discovered."

ether a device for rendering such actions "intelligible." But the supporting evidence for this answer is ambiguous at best. As Buchdahl himself points out, and as I shall argue in more detail elsewhere, almost all of Newton's many etherial hypotheses depend *essentially* on actions at a distance. In 1717, for instance, *the particles of Newton's ether act at a distance, not only on one another, but also on larger bodies*.[8] This is scarcely the kind of ether calculated to quiet the doubts of a mechanist who can only "conceive" contact action! Given the structure of Newton's various ethers, almost all of them depending on particles acting at a distance, it simply cannot be maintained that Newton's etherial speculations were designed to eliminate distance forces. But there is still the famous Bentley letter, and Buchdahl goes to some length (see especially his note 30) to argue that that document, if no other, establishes that Newton was fundamentally opposed to action at a distance. And in order for me to establish that Newton preferred option 3, I must discuss that letter briefly here.

The relevant portion of the letter goes as follows: "It is inconceivable, that inanimate brute Matter should, without the Mediation of something else, which is not material, operate on, and effect other Matter without mutual Contact . . . that one Body may act upon another at a Distance thro' a *Vacuum*, without the Mediation of anything else, by and through which there Action and Force may be conveyed from one to another, is to me so great an Absurdity, that I believe no Man who has in philosophical Matters a competent Faculty of Thinking, can ever fall into it."[9] As Buchdahl allows, there are two very different interpretations possible here. One reading (which Buchdahl supports) has Newton arguing that it is impossible for one bit of matter to act on another without acting through an intervening *material* medium. On this view, the letter to Bentley is a forceful expression of the inconceivability of action at a distance. But there is (at least) one other interpretation, viz., that bodies *can* act at a distance on one another but only if there is an intervening nonmaterial medium. The clarification of this issue would require much more space than I have here and would include a discussion of the technical term 'brute matter' as well as what Newton might mean by a nonmaterial medium. It is sufficient, for my case, to point out that the overwhelming objection to Buchdahl's interpretation is that it does not fit with Newton's behavior when dealing with distance forces. As far as short-range forces are concerned, Newton *never* offers etherial or mechanical explanations, being perfectly content to deal with distance forces as basic.[10] Moreover, as we have seen, in dealing with long-range forces where Newton does postulate an ether, it is almost always an ether governed by distance forces. Whatever Newton might have written to Bentley in 1692, his scientific works (and their apologetics) show no symptoms of a man especially uneasy about action at a distance. Nor is this merely to be explained by a lack of imagination on Newton's part. From the 1690's onwards, there were several well-known and genuinely mechanistic ethers for explaining gravity (e.g., Fatio's[11]) which Newton could have adopted if he had really wanted a mechanistic hypothesis. The fact that he did not endorse these, and that his own hypotheses were invariably nonmechanical, is reasonably convincing evidence that he took option 3.

[8] See especially Queries 21, 28, and 31 to the *Opticks*. The Halls seem to have completely missed the significance of this point. They could not be more incorrect when they state that "any hypothetical aether that Newton was prepared to entertain in order to facilitate the explanation of natural forces was itself a *mechanical* fluid." *Unpublished Scientific Papers of Sir Isaac Newton* (Cambridge: Cambridge University Press, 1962), p. 207.

[9] *Four Letters from Sir Isaac Newton to Doctor Bentley* (London, 1756), pp. 25–26.

But Newton was not one to close any option prematurely, and he always allows for the possibility that someone may find a mechanistic explanation for gravity. Thus, in his Queries to the *Opticks*, he frequently reiterates that he is not ruling out of court the possibility that gravity is due to the action of a genuinely mechanical ether. But this is no more than a possibility as far as Newton is concerned, and it is manifestly not one which Newton thinks worth pursuing himself. While preferring to work exclusively with forces acting at a distance, and insisting that doing so is coherent and intelligible, Newton is not so confident an opponent of the mechanical philosophy (in the Cartesian-Boylean sense) as to deny altogether the possibility of a mechanistic analysis of these distance forces.

But one crucial question still remains. If Newton's various etherial hypotheses were not devices to reduce action at a distance to a mechanical model, then why did Newton persistently return to etherial theories of one kind or another? If he felt actions at a distance were genuinely acceptable, then why did he regularly try to explain gravitation in terms of the action of an ether? The answers to these questions are many-sided. Perhaps it is sufficient to point out here that Newton found the ether to be a concept

[10] This point is clearly established by Joan Hawes in her as yet unpublished work on Newton.

[11] A detailed discussion of the "mechanical" ether of Fatio de Duillier may be found in B. Gagnebin, "De la cause de la pesanteur. Mémoire de Nicholas Fatio de Duillier présenté à la Royal Society le 26 Février 1690," *Notes and Records of the Royal Society of London*, 6 (1949), 106–160.

of high explanatory value, and relied on it increasingly as he found more and more kinds of phenomena which it seemed to explain. As he makes clear in the Queries to the *Opticks* and in his many unpublished manuscripts, the ether could explain phenomena as diverse as (1) the transmission of heat through a chamber void of air (Query 18); (2) the damping of a motion of a pendulum in a "vacuum" (Query 28); (3) various properties of light, particularly, the "fits of easy reflection and transmission" (Query 19); (4) the transmission from the sense organs to the brain (Query 23); and several other phenomena. It is not surprising that Newton, having already filled space with an (unmechanical) ether for purposes of optics, thermometry, etc., should speculate whether this ether might be also responsible for gravitational attraction.

If I am right, the account Buchdahl has given of Newton's defense of his system needs to be modified in certain respects. For instance, he sees Newton as unable to answer the action-at-a-distance objection head-on and, consequently, "escaping" (the term is Buchdahl's) to the architectonic level to give a rationale for his theory. While I agree that the architectonic component *is* there and is important, I believe that Newton responded to the reductive argument of the mechanists in its own terms. Action at a distance, on his view, is intelligible and defensible per se, without falling back on arguments about design, the harmony and tenor of nature, and the like. The latter were of course used to strengthen the case for action at a distance, but they were not the only weapons Newton had at his disposal.

Needless to say, none of my remarks undermines Buchdahl's general theses about the different kinds of criteria governing the choice of scientific theories. On the contrary, by modifying his criteria slightly I hope I have strengthened those theses. There is much else in Buchdahl's paper which deserves serious discussion and elaboration. I am confident that my reply will be only the first of a steady stream of reactions to his extremely suggestive essay.

COMMENT BY HENRY SMALL

The message of Gerd Buchdahl's paper for the historian of science is clear: do not overlook the role of philosophical criteria in directing the course of scientific change. This is certainly sound advice, since historians of science have often stressed the empirical criteria at the expense of the

philosophical, which are usually less visible. But having identified such criteria in historical cases, where do we then proceed?

It appears that the fundamental question which has not been answered is how do the criteria derive their strength; why is the force to conform greater for some than for others? That such a hierarchy exists is evident from studies of how scientists deal with anomalies. In modifying theory there are some assumptions which they are less willing to compromise than others.

But the relative strengths of criteria cannot be understood if they are considered in isolation from the idea system in which they play a part. The problem is to determine how this strength depends on the position of a criterion within the system. The solution will, I think, reveal the deep-lying psychological basis of science: not as irrational commitment to criteria, but as commitment based on the perception, rather than on the logical analysis, of a complex structure of ideas.

REPLY BY GERD BUCHDAHL

In order not to run into lengthy disquisitions, I will take Dr. Laudan's very perceptive comments more or less in the order in which they appear. His contentions will enable me to clarify certain things and to elaborate on others which obviously needed further elucidation.

Dr. Laudan imputes to me the contention that "the Leibnizian and Cartesian demand for a mechanical explanation of gravity is an argument exemplifying the *constitutive* component." Here we need to distinguish between the *considerations that lead to* the formulation of a theory employing such and such explanatory principles, and the *form of* that theory itself. What I say on the page referred to is that the forms of Newton's theory and those of his continental rivals differ, but it is obvious that the considerations determining for instance the Leibnizian formulation (involving propositions concerning the ether) arise out of conceptual (or, as Laudan would have it, "metaphysical") explications that differ from Newton's. What in fact I meant to emphasize was that Newton's conception of matter (hostile, as I take it to be to action at a distance) did not at that stage infiltrate the *form* of the theory, whereas Leibniz's conception did dominate the latter's formal presentation. The formulation of the conceptions belongs to the explicative component but that of the theories to the constitutive.

This brings me to the question of the appropriateness and sufficiency of the notion of "conceptual explication" (see Laudan, page 233). I think it will be clear from the context of my paper that this notion is intended *at least* to stand for Laudan's "metaphysical" or "reductive" component, for it will be fairly evident that "conceptual explication" in *my* use of that term is the offspring of a group of mental operations and argumentations of which Descartes's and Kant's are given as examples, and which by Kant, for instance, were admittedly labeled "metaphysical foundations." As I use this notion, it covers a very broad spectrum. It yields for instance a position about what are the "essential properties" of matter, what is the essential nature of motion, and so on. I might easily have chosen the *term* 'metaphysical explication' but I deliberately avoided this for a number of reasons. "Metaphysics" is a hard-worked concept. That God exists, man is free, and his soul immortal were typical metaphysical expressions according to the Kantian corpus. But Kant's "metaphysical" construal of the concept of matter as capable of exerting repulsive force which can be represented through vectorial construction is clearly a very different piece of "metaphysics," just as are those inquiries into the "deep structure" of language represented by Kant's transcendental principles (in the *Critique of Pure Reason*), called by him sometimes "the transcendental part of the metaphysics of nature" (cf. *Metaphysical Foundations*, page 140), or by some of the writings of Wittgenstein (*Tractatus*), Strawson, and Chomsky in our time. Still more appropriately, one might say that a notion like Locke's "simple idea," or indeed the whole theory of "ideas" as interposed between man and the world of "real things," is a more typical piece of "constructive metaphysics."

Against this, I needed a term that was rather more general and that would denote metaphysical considerations and constructions perhaps, to be sure, but that would also remind us in the end of the far looser and more tentative nature of the intellectual processes here in question, and which eventually lead to decisions about the scientific concepts involved. "Metaphysical" is a notion at once both too "constructive" and "existentialist" and too much reeking of putative achievement and conclusiveness.

Not to stray too far from the context of my paper, consider for instance Kant's construal of metaphysics in *Enquiry concerning the Clarity of Principles*, referred to on page 222 (see also footnote 62). There Kant attempts a contrast between the procedures of "mathematics" and of "metaphysics" (defined as "the first principles of our knowledge"). In mathematics

we begin with "explanations" or "definitions," because the meaning of the signs employed is already certain. In metaphysics we can never begin in this way. The reason given is that in mathematics "I have no concept at all of my object until it is given by the definition." By contrast, and this is the really crucial point, "in metaphysics I have a concept given to me already, *although it is a confused one*. My duty is to search for the clear, detailed and determinate formulation of this confused concept" (*Enquiry concerning the Clarity of Principles*, pages 14–15; and cf. my reference to the influence on Kant by Keill, page 225 of my paper). In the example of the method which Kant here recommends (and to which I allude briefly in the paper), he notes that there is no need to "dispute the correctness" of the Newtonian law of gravitation. What *is* necessary still, partly in order to secure this law against false imputations on the part of metaphysicians —an allusion to the denial of action at a distance on "conceptual" grounds —is to inquire into the meaning of notions such as "being at a distance," "touch," "resistance," "impenetrability," "force" (*Enquiry concerning the Clarity of Principles*, page 20). These kinds of "investigations into meaning" (as distinct from "truth") have become a commonplace in our day through the writings of Moore and Wittgenstein. To be sure, initially there may be *a sense* in which we "know precisely what action at a distance meant" (Dr. Laudan), but as the debates of several centuries show, in *another sense*, namely, the sense intended by Kant when he says that "in metaphysics I have a concept given to me already, although it is a confused one," further "clarification" may still be called for, of the kind which for instance William Whewell likewise included under the notion of "explication of conceptions" and "clarification of ideas." It was in this sense that I meant to urge that a conceptual investigation was required into the notions of "matter," and of "action" and "causation," in order to placate those who continued to insist that the action of matter on matter was "inconceivable."

Questions into the "nature" of causation and the "causal tie," for instance, are frequently framed in terms of alternative expressions, as either "what do we mean by causal action?" or "what is that entity?" or "what is the ontological significance of the claim involved in causal statements?" Already in Hume we get this dual approach. Hume begins by assuming that we know, in one sense, what is meant by causation, i.e., spatiotemporal contiguity, etc., as well as the existence of a necessary connection. But then he begins to ask questions about the genesis of the idea of necessary connection and to discuss this question by looking for the quasi-metaphysical entity ("impression") corresponding to the "idea" of necessary connection, an investigation at the same time not infrequently couched in the language of "what do we *really mean* when we claim the existence of a causal connection?" So my "conceptual explication" involves drawing attention both to the existence of the particular "reductive components" or "unexplained explainers" of Dr. Laudan's, and (where the occasion is appropriate) to the philosophical analysis of these reductive elements, on the lines indicated.

I agree with Laudan that there are "great exegetical problems" in trying to reconcile Newton's invocation of the ether hypothesis of 1717 with his teleologically based construal of gravitation as "basic," i.e., of reconciling the "inter-phenomenal" with the "supra-phenomenal" escape routes. One answer to the problem is that Newton felt tentative toward all these solutions and that therefore they may all be regarded as "queries" in the Newtonian sense of that term; and the notion of "query" is indeed intended to remove the need for ultimate consistency. To this we may add the consideration that Newton offered the ether hypothesis of 1717 as a didactic device, its professed aim being to stop anyone imputing to him a belief in the physical reality of action by matter on matter. Such a suggestion makes sense of course only if we grant my contention that providing an architectonic (teleological) rationale was not meant to yield a "realist" interpretation of the concept of attractive forces. And as I note, it was certainly not impossible for *some* of the writers during this period (e.g., MacLaurin) to reconcile a teleological approach with a supposed need to supplement Newton's theory with an ether hypothesis, a hypothesis possessing—as Laudan notes—"high explanatory value" in regions where we were dealing with an unfamiliar phenomenon, i.e., action at *great* distances.

Now I grant (and I note in my paper) that Newton does speak of attractive forces, and even employs this idea in his ether hypothesis of 1717. What we cannot do, pace Laudan, is to ignore—in the face of this—the doctrine of the Bentley letters and the explicit invocation against gravity as an essential property of matter (in the *Opticks* of 1717, already alluded to). To fall back on the conception of a "nonmaterial medium" or of gravity as "a direct result of the will and action of God" only increases the mental fog which undoubtedly prevailed in Newton's time—my Kantian interpretation of this "teleology" as a regulative device was meant to clear the fog a little (cf. page 218 of my paper)—and it is even true that the very

notion of an "ether" is not always so clearly mechanistic in character as to make it impossible to believe that it may not have served at times to close the gap between apparent inconsistencies. My teleological interpretation of Newton's intentions has at least the merit of making the best possible case for an admittedly confused and intermediate stage between straightforward acceptance of a phenomenological theory, on the one hand, and a realist interpretation of attraction, on the other.

I *do* note that Newton couples his views about short-distance forces with a reference to nature's being "very consonant and conformable to herself." Now this may just be a way of linking the acceptability of magnetic and electric forces with that of gravitational force. But this leaves the question open as before, since the nature of these forces in respect of being ultimate distance forces in the realist sense of that term was still an unsolved problem too. Moreover, as I meant to imply with my citation of the passage just referred to, the allusion to the "consonance of nature" evidently links the whole thing to Newton's teleological approach to large-distance gravity once again. Their existence, one and all, testifies to the harmony of nature, a conception through which Newton seeks, as I have tried to show, to escape from the suggestion of the informal interpretation of the constitutive component that gravity is basic in a realist sense. Indeed, it is just because Newton's interpretation focuses on the harmony of nature that we should expect him to fill in the *details* by recourse to ether explanations in the variety of fields Laudan notes on page 237, a fact which would provide a final and decisive proof that Newton's teleological approach implied to the end a nonrealist interpretation of forces in general.

So I am unrepentant. I can see no sense in regarding Newton as accepting action at a distance as "intelligible" (Laudan), in the face of his avowed denials, unless we hold that for the most part as a practicing scientist he held this view but on occasion felt scruples which he then equally temporally sought to assuage by his provision of the architectonic escape route. But perhaps ultimately Newton did fail to be as consistent as logicians with their tidy minds may try to make out. It is not unusual to find people holding inconsistent views when these are sufficiently "unclear" (in Kant's sense) and in process of change, battling against received tradition and subject to stress from new developments. It is precisely then that alternative explications come to the surface. And I would not therefore wish to give the impression that I hold my own views to be "right," and Dr. Laudan's mistaken. Such simple juxtapositions are not possible in a field where historical evaluation and logical appraisal do mutual battle with one another.

I now turn briefly to the question posed by Mr. Small: where does the exposition of such a finer structure of appraisal of criteria eventually get us? I am slightly uneasy about the tenor of this question, for it suggests that criteria like the ones here put forward are being viewed as means for the discovery of hypotheses, or at least as judges guiding instant decision-making processes with respect to acceptability. I would not want to claim exhaustiveness for my list. Moreover, since when applied it may lead to contradictory advice, and hence conclusions, it clearly cannot operate mechanically. My very case study shows that elements of structure may display mutual oppositions and tensions, perhaps indicative of the existence of warring "paradigms," though one may also interpret this as an indication that there never are any self-contained paradigms, and that the tensions involved in the struggle to get a new theory accepted are deeper and frequently contain more general aspects including low-lying philosophical assumptions which may cut across centuries of debate.

One of my general intentions had in fact been to add further considerations in favor of our growing awareness that existing purely "inductivist" discussions of the foundations of science are insufficient as grounds for the explication of the "logic of science." And I have purposely cast my net of criteria widely and have intentionally included criteria that may seem at first sight excessively historicist, subjective, almost arbitrary, in order to highlight the problem of "rationality" of scientific "inference."

This has the initial result of making any desired solution even more seemingly hopeless than our experience with purely inductivist theories of scientific inference might suggest. But, on the other hand, I have chosen my criteria (or components) so as to make it possible to indicate some relevant connections with purely philosophical discussions which have usually not been thought to relate any longer to modern inductivist problems—witness the threads of connection drawn between "entrenchment" and "lawlikeness" and "metaphysical foundations" (conceptual explication) toward the end of my paper.

Beyond this I would be the first to admit that a more systematic presentation of criteria is desirable. For instance (as Dr. Laudan also hints) the list of criteria lumped together under the "architectonic component" is too all-embracing. On the other hand, the interconnections between the different components are not always clear; thus a glance at my summary

at the end of section III of my paper shows the constitutive component as "suggesting" action at a distance on the basis of an "informal interpretation" of the formalism of the theory (I.1); a sort of "conceptual explication" matched against the *ex cathedra* explication I.2, displaying the contrary conclusion. I do not think, however, that excessive tidiness in one's classificatory schemes is necessarily a virtue. Such schemes can never be regarded as more than mnemonic devices, reminding us of the underlying critical debates which necessarily must contain more "ragged edges" and more ambiguous crosscurrents than any such schemes can explicitly display.