cal one, he found radically different views on the nature of matter, each associated with a genuinely different epistemological stance. Could the two be reconciled by giving chemical principles particulate embodiment? Apparently, he soon came to accept the idea that they could, but each side of the equation presented him with its own peculiar difficulties. With the benefit of hindsight, we may hazard the guess that the principal stumbling blocks were two in number, one from each side: the mechanists' a priori assumption of a common matter and the chemists' inability to provide an accurate list of the differentiated types of matter with their elements and/or principles.

The recently isolated chemical papers of Newton provide a starting point for new studies of his development as a chemist, but here we have worked principally within a very brief time period in the 1660s, when he had begun to study chemistry but before he launched his exhaustive exploration of alchemy. Any final assessment of his development must take into account his encounters with both the "vulgar" and the "vegetable" chemistries: a program for the future.

2

The significance of Newton's *Principia* for empiricism

ERNAN McMULLIN

Even before Newton's *Principia* appeared in print in July 1687, those aware of its contents were quite certain of the fundamental significance of the work for the new science of mechanics. And the passage of time showed them to be right. The *Principia* did mark a turning point in the history of mechanics, indeed in the history of science itself. I want to argue here that it also had far-reaching consequences for epistemology, and specifically for the empiricist theories of science then current. In Newton's own estimation, his epistemology was of course a straightforwardly empiricist one. But the long-range effect of the *Principia*, we shall see, was to challenge classical empiricism in some fundamental ways.

When I speak of "classical empiricism," I have two theses principally in mind. The first of these has to do with *meaning*, specifically with how terms derive their meaning from experience. The second concerns the *warrant* appropriate to a scientific hypothesis. The first is associated especially with Locke, the second with Bacon. The challenge in both cases comes from the unitary character of the theory of motion proposed in the *Principia*. The terms in this theory derive their meaning not from separate experiences of the qualities the terms denote (as Locke would suppose) but from the theory taken as a whole. And the theory itself derives its warrant from its explanatory power when applied as a whole to our observations of moving bodies, not from inductive generalizations supporting each of its constituent "laws" separately, as Bacon's theory of science might have led one to expect.

In order that my claim not be misunderstood, its scope must be further clarified. I do not claim that the *Principia* was the *only* work of that time in which the shortcomings of these two empiricist theses could be said to have been presaged. The development, for example, of the "method of hypothesis" in which the weight of an hypothesis is assumed to be given by the number and variety of its verified consequences had already to some extent called into question the simpler notion of inductive generalization prescribed by Bacon. Even in the *Novum Organon* itself, it was difficult to see how the imperceptibly small corpuscles and their "latent processes" could be arrived at by a gradual ascent from experience in which each "level" is established inductively; i.e., by the perception of similarities among particulars. But the *Principia* posed a far sharper challenge to empiricism than any other scientific theory up to that time had done.

To put it this way might suggest that this challenge was perceived by Newton or at least by his contemporaries. This was not, in fact, the case. As we look back on the *Principia* today, *we* can see how seriously empiricism was compromised by the shift from Aristotle to Newton. But it took a long time for this to be grasped. Indeed, it has been only in our own century that the significance in this regard of the *Principia* and of the kind of science it announced, has become clear. Why it took so long and what finally brought about the realization would be material for another essay.

My theme is thus of a complex sort. It is neither straightforwardly historical nor straightforwardly philosophical. I am not arguing that the *Principia* was a turning point in epistemology in the way it was in science. It had little direct effect on epistemology at the time or, indeed, for long afterwards. Its importance in this respect was of a less direct kind. Latent within it were more complex notions of meaning and of theory-validation than classical empiricism could accommodate. The fact that it took so long for these notions to be fully discerned in no way diminishes their philosophical significance, nor the historical significance of the fact that it is in the *Principia* that we, from *our* vantage point, can first clearly espy their appearance.

AN EMPIRICIST STARTING-POINT

In an unfinished manuscript, *De gravitatione*,[1] Newton proposed to analyze the notion of body (or matter). He reminded the reader

[1] The manuscript is usually designated by its opening words: "De gravitatione et aequipondio fluidorum." It is published with a translation in *Unpublished Scientific Papers of*

that since God could perhaps bring about the appearance of matter in several ways, the analysis he would give should be taken as provisional, a description of a "kind of being similar in every way to bodies."[2] Suppose, he asked, God were to make a space impervious to bodies, "it seems impossible that we should not consider this space to be truly body from the evidence of our senses (which constitute our sole judges in this matter)." The only other additional property required is mobility. Such an entity would presumably also "operate upon our minds." These three properties, impenetrability, mobility, and the ability to act upon the human senses, are together sufficient to constitute body.[3] From them, other "universal qualities" of body (as he called them in Rule III of Reasoning in the *Principia*) can then be derived, such properties as shape, hardness, inertia, and even perhaps the quality of mutually gravitating towards other bodies.[4] He took it as obvious that such other qualities as color, taste, and smell do not inhere in the perceived object itself. A distinction between "primary" and "secondary" qualities had been an almost universal presupposition of seventeenth-century science. Newton did not develop it further, nor comment on it. From his early papers on light and colors to the last Queries he added to the *Opticks*, he simply took it for granted.

What constitutes a quality as primary? Newton would probably have agreed with his philosopher-friend, Locke, who devoted some attention to this issue. Primary qualities are "such as are utterly inseparable from the body, in what estate soever it be."[5] No matter how far matter be divided up, "each part still has solidity, extension, figure and mobility," even though the parts be so minute as to be no longer separately perceptible. But how do we *know* that color is not primary in this sense? Locke does not say. Indeed, he speculated about the possibility of a "microscopical eye," which would allow one to see the minutest parts of bodies.[6] But this

Isaac Newton, ed. A. R. and M. B. Hall (Cambridge: Cambridge University Press, 1962). The manuscript has no indication of the date of composition; the editors conclude that it is an early work from the mid-1660s.

[2] Ibid., p. 138.

[3] Ibid., pp. 139–40.

[4] He hesitated to call it an "essential" quality (because that would make matter essentially active), and settled, unsatisfactorily, for its being "universal" only. See chapter 3 of my *Newton on Matter and Activity* (Notre Dame, Ind.: University of Notre Dame Press, 1978).

[5] John Locke, *Essay Concerning Human Understanding*, ed. Peter H. Nidditch (Oxford University Press, 1975), book II, chap. 8, sec. 9.

[6] Ibid., chap. 23, sec. 12.

would, of course, entail that they have a quality analogous to color.

It would seem that the real basis of the distinction, both in Locke and in Newton, is not that primary qualities can be shown to be universally possessed whereas secondary qualities can be shown not to be. Rather, an implicit reductionist claim is being made that the latter can, in principle, be explained in terms of the former.[7] The primary properties are precisely those which a science of mechanics requires in the objects to which it can properly apply. And this science could then, in principle (or so it is assumed), explain all other properties of bodies, whether secondary (pertaining to the effects on human sense organs) or "tertiary" (pertaining to effects producible on other entities).[8] Newton believed that a knowledge of the configuration of the minute parts of bodies and of the forces acting between them would prove sufficient to account for the colors of bodies, their chemical interactions, and their coherence.[9]

If such "universal" qualities as extension, mobility, impenetrability, and inertia are so foundational to the science of nature, it is important to know how our ideas of these qualities are first formed. Locke's answer is well known:

> Since the extension, figure, number and motion of bodies of an observable bigness may be perceived at a distance by the sight, it is evident some singly imperceptible bodies must come from them to the eyes, and thereby convey to the brain some motion which produces these ideas which we have of them in us.[10]

It may seem surprising that an empiricist like Locke would have deemed it "evident" that the origin of ideas was to be explained

[7] A further refinement could be added here: A property is *epistemically* primary if it plays an essential part in the reducing science; it is *ontologically* primary, if it exists in its own right. Recent discussions of reduction have made much of this distinction. I shall not need it here. See "Matter, Perception and Reduction," sec. 6 of the Introduction to my *Concept of Matter in Modem Philosophy* (Notre Dame, Ind.: University of Notre Dame Press, 1978), pp. 32–41.

[8] The hardness and solidity of perceptible bodies could presumably also be explained in this way. In draft-notes from his later years, Newton speculated about the extent to which apparently "solid" bodies might reduce to largely empty space, populated by corpuscles exerting strong forces of repulsion and attraction. Impenetrability might then cease to be primary though still universal. See A. Thackray, "Matter in a Nutshell," *Ambix*, 15 (1968), 29–63.

[9] Newton had, of course, no real evidence for this belief, and history would later show his confidence to have been premature. The inability of his mechanics to explain the colors of bodies was, indeed, one of the factors that would motivate the replacement of that mechanics with the conceptually much richer quantum mechanics.

[10] Locke, *Human Understanding*, book II, chap. 8, sec. 12.

in this way, since there could be no direct empirical warrant for the "singly imperceptible bodies," nor could any inductive support be provided for the claim that bodily motions are the cause of ideas. Yet these suppositions were evident within the context of the mechanical philosophy, to which Locke on the whole subscribed. His next step would not, however, be so easily justified:

> It is easy to draw this observation that the ideas of primary qualities of bodies are resemblances of them, and their patterns do really exist in the bodies themselves; but the ideas produced in us by these secondary qualities have no resemblance of them at all. . . . They are, in the bodies we denominate from them, only a power to produce those sensations in us; and what is sweet, blue, or warm in idea, is but the certain bulk, figure, and motion of the insensible parts in the bodies themselves, which we call so.[11]

There has been much discussion as to what Locke might have meant here by "resemblance."[12] It seems fair to say that he was postulating a likeness of pattern between, say, the particular extension of a perceived body and the "idea" of that extension as it exists in the perceiver's mind. But how about the idea of extension itself? Locke discussed how we make such particular ideas general. We abstract, he said, from the contingent circumstances of the particular idea, and form a general idea (to which the corresponding name is attached), which will serve as "general representative" for all the particular perceived extensions:

> Such precise, naked appearances in the mind, without considering how, whence, or with what others they came there, the understanding lays up (with names commonly annexed to them) as the standards to rank real existences into sorts. . . . Thus, the same color being observed today in chalk or snow, which the mind yesterday received from milk, it considers that appearance alone, makes it a representative of all of that kind, and having given it the name "whiteness," it by that sound signifies the same quality wheresoever to be imagined or met with; and thus universals, whether ideas or terms, are made.[13]

There are two operations of the mind here: the first, that by which the particular "ideas" (perceptions) are formed and, in the case of the primary qualities, actually mirror in some way the spe-

[11] Ibid., sec. 15.

[12] See, for example, J. W. Yolton, "The science of Nature," in *Locke and the Compass of Human Understanding* (Cambridge: Cambridge University Press, 1970), chap. 2.

[13] Locke, *Human Understanding*, book II, chap. 11, sec. 9.

cific patterns in the world they convey to the mind; and the second, that of abstraction, by which general ideas (like extension, solidity) are formed by abstracting from the specific features of the particular perceptions. There are strong analogies between this analysis and the classical Aristotelian doctrine of abstraction, but there are some important differences too. And Berkeley was soon to point out some of the more obvious difficulties in Locke's account.

Our concern here, however, is not with the antecedents nor with the specifics of Locke's epistemology. The above is intended only to suggest in broad outline what the standard empiricist view was in Newton's day regarding the *origin* of such ideas as extension or body. Newton himself had little to say on this topic. His references to sense perception (in *De gravitatione,* the papers on light and colors, and the *Opticks*) are consonant with the mechanical philosophy.[14] In his response to Hooke of 1672, he acknowledged his use of the "hypothesis" that colors are "modes of sensation, excited in the mind by various motions, figures, or sizes of the corpuscles of light, making various mechanical impressions on the organ of sense."[15] More than forty years later, he drafted a fifth Rule as a possible addition to the four Rules of Reasoning for a new edition of the *Principia:*

> Whatever things are not derived from objects themselves, whether by the external senses or by the sensation of internal thoughts, are to be taken for hypotheses. . . . And those things which follow from the phenomena neither by demonstration nor by the argument of induction, I hold as hypotheses.[16]

It seems likely, then, that the definitions of such terms as "body" and "hardness," over which he labored so much in the incessant drafts he made for revisions of the *Principia,* were for him quasi-inductive in nature. Individual bodies interact mechanically with the retina and produce "pictures, [which] propagated by motion along the fibers of the optic nerves in the brain, are the cause of

[14] See J. E. McGuire, "Body and Void and Newton's *De mundi systemate:* Some New Sources," *Archives History of the Exact Sciences, 3* (1967), 206–48, especially sec. 5.

[15] *Isaac Newton's Papers and Letters on Natural Philosophy,* ed. I. B. Cohen (Cambridge, Mass.: Harvard University Press, 1958), p. 119.

[16] A. Koyré, "Les *Regulae philosophandi,*" *Archives internationales d'histoire de sciences, 13* (1960), 3–14.

vision."[17] From this a perception of body is built up, and by something akin to generalization, the notion of body itself is formed.

THE "DEFINITIONS" OF THE PRINCIPIA

The importance of this view derives, of course, from the crucial role that definitions play in Newtonian science. He treated them as nonproblematic, as part of the axiomatic basis of the science. Even though they had proved so difficult to formulate satisfactorily, he nowhere gave the impression that they could introduce a "hypothetical" (in the sense of problematic) element into his mechanics. In the *De gravitatione,* he began: "The foundations from which this science may be demonstrated are either definitions of certain words, or axioms and postulates denied by none."[18] He then went on to assert: "The terms, 'quantity,' 'duration,' and 'space' are too well-known to be susceptible of definitions by other words." The implication is that there is no *need* to define them since everyone understands them in the same way. When he went on to discuss the notion of space (or extension), he noted that it can be grasped only by the understanding and not by the imagination alone; it is our understanding that tells us that "there exists a greater extension than any we can imagine."[19] But the imagination plays a role too, for instance, "as when we may imagine spaces outside the world, or places empty of body," when showing that space is not an accident of body.[20] Still, though imagination and understanding help us to grasp a notion like space, it is our perception of particular extended bodies that furnishes the material for the notion which can then be elaborated in whatever detail we choose.

The process of definition seems, then, to be a matter of finding words for ideas antecedently grasped. Newton sought the authority of common usage, especially where there had been some dispute among philosophers (as in the case of "body" and "void," where he was at odds with both the Cartesians and Leibniz):

[17] Isaac Newton, *Opticks* (New York: Dover, 1952), p. 15. This edition is taken from the fourth edition of 1730.

[18] Ibid., p. 122. See R. S. Westfall, *Never at Rest: A Biography of Isaac Newton* (Cambridge: Cambridge University Press, 1980), pp. 411–420.

[19] Newton, *Opticks*, p. 134.

[20] Ibid., p. 132.

Body I call everything tangible in which there is a resistance to tangible things. . . . It is indeed in this sense that the common people always accept the word. . . . Vacuum I call every place in which a body is able to move without resistance. For thus the common people are wont to use the term. If anyone should contend that there are bodies which by touch are neither felt nor cause resistance, he would be disputing the grammatical sense of the word describing as "bodies" what ordinary people do not call bodies. And I would prefer to side with ordinary people, since they have assuredly the ability to give things names.[21]

But in the case of the most important definitions of all, those that preface the first book of the *Principia*, he did not commit himself entirely to common usage. After listing them, he concluded:

Hitherto I have laid down the definitions of such words as are less known, and explained the sense in which I would have them to be understood in the following discourse. I do not define time, space, place, and motion, as being well known to all.[22]

He recognized, therefore, an element of the *prescriptive* in these definitions. In the terminology popularized by Copi,[23] they were proposed as "precising" definitions, that is, they conform broadly to lexical usage but stipulate a more exact sense. Thus their warrant is partly what the "ordinary people" said, and partly Newton's own authority. Did this make the science built on them in any way provisional? Newton obviously did not think so. He insisted that he was "deducing" directly from the phenomena; that was, it seems no element of the "hypothetical" about the definitions he was using. Yet he labored with them through many drafts and rejected so many variant possibilities. How could he be so secure that he had "got them right"? And what did getting them "right" mean for him?

[21] Draft material for a set of definitions to be set before the Rules of Reasoning in the third edition of the *Principia*, but never actually incorporated. (The last phrase is lined out in the manuscript.) Printed as an Appendix to McGuire, "Newton's De mundi systemate" [see note 14], pp. 245–6. Translation mine. Original in the University Library Cambridge, Add. 3965, fol. 437ʳ. See also I. B. Cohen, *Introduction to Newton's Principia* (Cambridge, Mass.: Harvard University Press, 1971), pp. 37–8.

[22] *Principia*, Motte–Cajori translation (Berkeley: University of California Press, 1962), p. 6. He later distinguished between the absolute and the relative senses of these latter terms, and noted that "if the meaning of terms is to be determined by their use," it is in the *relative* sense they are to be understood. It would "do violence to language, which ought to be kept precise," to take these words in the absolute sense proper to mathematics only (p. 11).

[23] *Introduction to Logic*, 3rd ed. (New York: Macmillan, 1968), chap. 4, sec. 3.

Matter, motion, and force are the three generic concepts he worked with. He clearly assumed that they were drawn straightforwardly from ordinary experience. They designated real features of the world to which one could appeal with confidence. What he contributed is the extra "quantity of . . ." qualification, which transforms the definitions into something the "mathematician" can use.

Matter is still "stuff-in-general." But it drops entirely out of sight as a working concept in the *Principia*, and is replaced by "quantity of matter" or mass.[24] Instead of equating matter with extension, as Descartes had persisted in doing despite the obvious difficulties into which this led him, Newton went back to the standard pre-Cartesian way of understanding it as proportional to both extension *and* density. There are obviously "matters" of different density, i.e., which have more or less "stuff" in the same volume. How do we *know* that there is more or less "stuff"? Perhaps by noting that one could sometimes compress the same stuff into smaller and smaller volumes (a popular theme in medieval natural philosophy), and that the *mechanical* behavior of the compressed matter remains the same even when volume changes.

The first Definition of the *Principia* was thus the product of a long tradition of reflection on our everyday experience with bodies. It was grounded in this experience and could plausibly be thought of as an induction or generalization from it. If someone had questioned it, Newton's response would probably *not* have been to say that he was stipulating a special sense for the purposes of the work he was writing. He would very likely have adduced various features of our experience with bodies as evidence in support of the plausibility of taking the "stuff" of bodies to be both an invariant and to be quantifiable in the simple direct proportionality he proposed.

The second Definition might have seemed more in need of justification. From Aristotle's time onward, there had been periodic discussion as to how motion ought to be rendered quantitatively. Aristotle defined local motion as change of place, and represented it as a ratio of space covered to time taken. This gradually became the concept of velocity, understood as a single quantity, space divided by time. But in later medieval natural philosophy, the notion of motion as something communicated from one body to

[24] See the Introduction to McMullin, *The Concept of Matter in Modern Philosophy* [note 7], pp. 50–5.

another eventually suggested that another more complex concept would be needed. Descartes thought of "motion" as an invariant of the created universe. God gave motion to the matter of the world at its first creation and conserved this motion ever since. There is no *new* motion, despite appearances to the contrary. Clearly, the concept of velocity will not suffice once these considerations of invariance are introduced. Descartes thus took "motion" to be proportional both to velocity and to "size," and it was this notion that Newton took over and clarified.[25]

What makes this Definition more than a nominal one of a quantity (i.e., momentum) that is to be computed in a certain way from two other quantities, is the implicit assumption that it is an invariant of contact action. But this invariant is now rather removed from the ordinary perception of motion. It could hardly be regarded as an abstraction from such individual perceptions. But because it is set equal to the product of volume, density, and velocity, it may still seem innocuous enough in the eyes of the inductivist.

But this is no longer the case with the third notion, force. Newton defined three sets of force: inherent, impressed, and centripetal. The first, *vis insita:*

> is a power of resisting by which every body, as much as in it lies, continues in its present state, whether it be of rest or of moving uniformly in a straight line. This force is proportional to the body and differs from the inertia of the mass only in the manner of conceiving it. . . . A body exerts this force only when another force impressed upon it changes its condition, and the exercise of this force may be considered as both resistance and impulse.[26]

Vis insita is a notoriously confused concept, and a great deal of Newtonian scholarship has gone into the effort to untangle it. Our

[25] Since Descartes equated "size" with quantity of matter, and also used the phrase "force of a body to act" interchangeably with "motion," much clarification was needed. His stress on invariance *did*, however, lead him to one major discovery, the principle of inertia "that every body which moves tends to continue its motion in a straight line," where it is not only momentum but *velocity* that remains the same (since the body remains the same in "size"). See R. S. Westfall, *Force in Newton's Physics: The Science of Dynamics in the Seventeenth Century* (New York: Elsevier, 1971), pp. 57–72.

[26] My translation. The Motte–Cajori translation is faulty here in several respects, notably in having *vis insita* act only when another force *endeavors to* change the body's condition. The phrase "endeavors to" does not occur in the Latin. For a discussion, see "*Vis inertiae*," chap. 2.3 of my *Newton on Matter and Activity* [note 4].

concern here is only with the status of the Definition itself as a definition. It makes an empirical-sounding claim about the world, that a body tends to continue in the "state" in which it is. This had been denied by Aristotle and had only gradually begun to make its way as an hypothesis in later medieval natural philosophy, prompted mainly by some ingenious thought-experiments. It requires one to abstract from the everyday conditions of nature, in which bodies do *not* tend to continue in their state of motion. The fact that they do not must be capable of being explained in such a way that the "Definition" still holds good. Thus, there is an implicit reference to the frictions and resistances ("impressed" forces), which are held to explain away the actual nonuniform motions of bodies.

Can this sort of counterfactual claim be thought of as inductive? It could be said that Newton was generalizing from our experience of "impediments" to uniform motion. But will this motion, for example, be rectilinear or circular? Galileo had much difficulty with this question and never quite resolved it. And what precisely is to count as an "impediment"? This is no longer a matter of simple generalization. The idealization practiced by Galileo and Newton seems to require a different and much more complex form of inference.

The epistemic status of the Definition becomes even more problematic when one considers the explanation it offers for continuance in the state of motion or rest. The continuance is said to be due to an internal "force," proportional to the mass of the body. How do we know such a "force" operates? Not by observing it, but by postulating that something like it must be operating to account for "resistance" to change of motion.

Newton was in a quandary here. In an addendum to the Definition which he planned for the third edition of the *Principia*, he wrote:

> I do not mean Kepler's force of inertia by which bodies tend toward rest, but the force of remaining in the same state whether of resting or of moving.[27]

But why should remaining in the same state require a force? And what in the end is meant by calling it the "force of inertia"? If

[27] In Newton's own interleaved copy of the second edition. See Cohen, *Newton's Papers and Letters* [note 15], Introduction, pp. 27–9.

matter is by its nature inert (as Newton insisted it is), how can a force be *inherent* in it, even one that only operates when an outside force affects the body? Later Newtonians eventually cut this cord that still bound Newton to Aristotle; they simply eliminated *vis insita* from their mechanics. But Newton himself was still struggling with the need to explain why inertial motion continues, and thus he postulated this phantom cause.

The third Definition is obviously not just a definition. It proposes a certain sort of motion as the norm, and it proposes to explain why such motion occurs as it does. As has often been noted, the first Law is already implicit in it. And when Newton said that *vis inertiae* is exerted only when external forces act to change a body's motion, and that this exercise can be considered as impulse insofar as "the body, by not easily giving way to the impressed force of another, endeavors to change the state of that other," one can immediately see that both the second and third Laws are also implicated.

The epistemic distinction between the Definitions and the Laws is at best, therefore, a hazy one. The former presuppose the latter just as much as the latter do the former. The former stipulate word-usage, but then so do the latter. The latter draw in a very general way on our experience of bodily motions, but so do the former. The Definitions cannot be singly derived by inductive generalization from observation (in the way in which Boyle's Law, say, might be said to be). But neither can the three Laws of Motion. The inverse-square law of gravitational force (if interpreted as a *description* of gravitational motion and not as an *explanation* of it) comes closest to being an empirical law after the Boyle model.

When force is taken to be an *explanation* of change of motion (as it clearly is in the fourth Definition and the second Law), we have left the realms of inductive science well behind, as Newton's critics were not slow to point out. Newton wanted it both ways. The fifth Definition speaks of bodies being "drawn" by centripetal forces, and later Propositions (e.g., LVII) speak of bodies "attracting" one another. The implication is that such forces and attractions are something more than a redescription of the motions they bring about, that they are real *causes* of the motions. In the eighth Definition, Newton backed away from this: "I here design only to give a mathematical notion of those forces, without considering

their physical causes and seats." He went on to enjoin the reader not to assume that because he spoke of centers as "attracting," that he attributes forces "in a true and physical sense" to them.

But if the sun cannot, in the light of the Definitions, be said to attract the earth in a "true and physical sense," has the motion of the earth been explained? If (as the fourth Definition suggests), force "consists in the action only," does this compel us to the further question: What is the cause of the action? Or has the causal question in some sense been answered by specifying the "force"? Newton hoped to deflect these questions by insisting that the *Principia* is restricted to "mathematical notions" only. But does not such a restriction reduce the second law to a *definition* of force, making it nothing more than an abbreviation for the product of mass and correlative acceleration in certain sorts of motion? What exactly is this "mathematical notion" of force anyway?

I am not proposing to address these questions here; they have been the starting point for much recent Newton scholarship.[28] It is sufficient to raise them, however, in order to underline the enigmatic character of Newton's concept of force. As it appears in the Definitions and Laws, it cannot be understood either by reference to the usage of "ordinary people" or to our everyday experience of the behavior of bodies. Nor is it a product of generalization; rather, it is a construct of an ambiguous and puzzling sort.

THE NETWORK EFFECT

How, then, is the meaning of the key terms in the mechanics of the *Principia* being specified? Through a complex network of interconnections that draws them all together in a single web. Mass is related to density and volume. How is density obtained? Ordinarily, through a knowledge of mass and volume. Force is measured by the acceleration it produces in a given mass, so mass can be determined in this way also. Weight can be measured by the extension of a spring, and the ratio of mass to standard mass can be obtained. And so on. These threads that link each mechanical term to all the others are familiar to every student of physics to-

[28] See, for instance, Westfall, *Force in Newton's Physics*, chap. 7; McMullin, *Newton on Matter and Activity* [note 4], chap. 4; Anita Pampusch, "Isaac Newton's Notion of Scientific Explanation," Ph.D. dissertation, Ann Arbor Microfilms, 1971).

day. And it was in the *Principia* that they were first specified sufficiently exactly to enable the system as a whole to be applied to concrete cases.

The specification was not *entirely* exact, of course. We have already seen something of the confusions that surrounded *vis insita*. And there was the fact that Newton in the second Law made force proportional to change of motion (instead of to rate of change of motion), thus making impulse rather than force the key explanatory concept. And there were the "absolutes" of time, space, and motion, which would leave later Newtonians with some intractable puzzles. Nonetheless, Newton built well, and his successors had for the most part only to adjust the network slightly here and there, to bring the mechanics of the *Principia* close to the formal idea of a conceptual system in which every syntactic relation is precisely specified.

The terms Newton presented in the Definitions prefacing his work are not "observational terms" in the sense in which empiricists, early and late, attempted to define these. They may sound like them. And Newton may have thought that they *did* satisfy the empiricist criteria (though he might have admitted to doubts about "force"!). But there can be no question about their tight and total interconnection. They simply cannot be related one by one to specific "properties" or "observables," somehow discriminable in advance.

Once this be admitted, a second difficulty arises for the strict empiricist. The system of the *Principia* involves all sorts of theoretical presuppositions, some of them very general (e.g., as to how time and length should be measured), some quite specific (e.g., regarding the nature of inertial motion). Because the application of individual terms to the behavior of bodies implicitly involves the network as a whole, it must also implicitly involve the presuppositions underlying the system viewed as a physical theory.

When the system is taken formally (or "mathematically," as Newton would put it), there are no presuppositions; everything is on the same level. The syntactic relations specifying the system may, for instance, have active gravitational mass equal to passive gravitational mass. This then becomes one of the "givens," and from it computations can be made. It is only when the system is considered as a physical theory, as an explanation of real motions, that this way of relating the two masses becomes a *presupposition*,

something assumed, for which the evidence offered is less than conclusive. To restrict consideration to the formal system, to the mathematically expressible syntax, as Newton continually tried to do in the *Principia*, has the effect of eliminating the crucial epistemic distinctions between definitions, empirical laws, and theoretical presuppositions. It also allows one to lay aside (as Newton well knew) the really difficult semantic issues involved in interpreting one's causal terms physically. But these issues are crucial to the success of the theory as an explanatory account of real motions.

Fifty years ago, the logical positivists attempted to reconstruct an empiricist theory of science, and in doing so relied heavily on a distinction between "observation terms" and "theoretical terms." Since the former were assumed to be related directly and unproblematically to a class of operations or observations, the distinction served to provide (or so it was hoped) a firm starting point for scientific inference. The later collapse of this distinction in the face of the criticisms of Quine and others is well known.[29] Quine based his criticisms on a general account of language, not on an analysis of a specific scientific theory. But he could well have focused on mechanics, the paradigm science for the positivists, since (as Wittgenstein had already hinted in the *Tractatus*) it exemplifies the network model more clearly than does any other theory.

THE WARRANT OF THE PRINCIPIA

There is one further way in which the *Principia* serves effectively to undermine the classical empiricist ideal of science, despite appearances to the contrary. We have seen that the linking of words to world in the *Principia* is warranted, not in a one-to-one way by means of notions like observation or abstraction, but through the conceptual system taken as a whole. This is, of course, at odds with the empiricist view that the scientist begins by singling out the appropriate observable qualities, and then proceeds to build up inductive generalizations relating them one to another. In this view, the evidence for a generalization such as Hooke's Law would be a

[29] Quine's "Two dogmas of empiricism" (*Philosophical Review*, *60*, [1951], 20–43) was the most influential criticism, and launched the "network" model of meaning which is now widely accepted in accounts of physical theory. Many features of this model can already be found in the works of earlier writers, such as Duhem and Collingwood.

specific set of observations. One would then build the science, generalization by generalization (as Bacon advised) until the first principles (that is, the highest generalizations) were ultimately discovered. This is not what Newton did, and the warrant for the *Principia* (and hence for the use of each technical term in it) must be recognized as being of quite a different sort.

Did Newton recognize this? It does not seem so, and indeed it would have been quite remarkable if he had. He struggles to adapt his thought to the two principal models of science of his day, but there are hints of a third model also. The few passages in which he discusses method are so well known that an apology is needed for quoting some of them again. My intent here is not to deal with the very difficult issue of Newton's method in the detail this would require, but only to make two limited points, that the actual warrant[30] for the *Principia* is not what Newton appears to have thought it to be, and second, that it is at odds with the empiricist ideals of science, both classical and contemporary.[31]

Let me begin with Newton's best-known declaration. When Cotes was preparing the second edition of the *Principia*, he queried Newton regarding the epistemic status of the Laws of Motion. Newton's response was that the Laws "are deduced from phenomena, and made general by induction which is the highest evidence that a proposition can have in this philosophy."[32] And he added this passage to the General Scholium with which the *Principia* concludes:

[30] Obviously, the notion of "actual warrant" raises many sensitive philosophical issues in regard to scientific rationality and its historical dimension. Some of these will be touched on briefly below, but for the most part, they cannot be dealt with in the space at my disposal here. See McMullin, "The Rational and the Social in the History of Science," in *Scientific Rationality: The Sociological Turn*, ed. J. R. Brown (Dordrecht: Reidel, 1984), pp. 127–63.

[31] It may be noted that these theses are flatly contrary to those defended by R. M. Blake in "Newton and Hypothetico-deductive Method," in *Theories of Scientific Method*, ed. R. M. Blake, C. J. Ducasse, and E. H. Madden (Seattle: University of Washington Press, 1960), pp. 118–43. Blake argued that Newton had grasped the essentials of scientific method as we understand them today, that he exemplified these consistently in the construction of the *Principia*, and that the method he both proposed and used is "inductive," or "hypothetico-deductive" (he equated these two labels). Blake published this piece at a time when the hold of logical positivism was still strong. It is both instruction and warning to see him read Newton so neatly into this perspective.

[32] *The Correspondence of Isaac Newton*, ed. A. R. Hall and L. Tilling (Cambridge: Cambridge University Press, 1975), *5*, 386–7.

> Hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame (feign) no hypotheses. For whatever is not deduced from the phenomena is to be called an hypothesis, and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy, particular propositions are inferred from the phenomena, and afterwards rendered general by induction. Thus it was that the impenetrability, the mobility, and the impulsive force of bodies, and the laws of motion and of gravitation, were discovered.[33]

Before commenting on this puzzling text, it is worth noting that in the very next paragraph Newton spoke of "a certain most subtle spirit which pervades and lies hid in all gross bodies, by the force and action of which the particles of bodies attract one another"; it also accounts for cohesion, electrical attractions and repulsions, the behavior of light, and the operations of the senses. He allowed that we are not yet "furnished with that sufficiency of experiments which is required to be an accurate determination and demonstration of the laws by which this electric and elastic spirit operates." Newton apparently saw no difficulty about juxtaposing his condemnation of "hypothesis" with this flat assertion of the existence of the "subtle spirit," so that we must be careful (as many recent commentators on Newton have reminded us)[34] not to interpret the *"hypotheses non fingo"* as a flat exclusion of hypothesis from science.

Nonetheless, Newton did insist, many times over, that in the "experimental philosophy" he espoused, basic propositions like the Laws of Motion must be "deduced" from the phenomena and then generalized by induction. Presumably, he meant "deduced" to be taken in the weaker sense of "derived" (since one obviously cannot *deduce* a general law from phenomena). But can the Laws be properly regarded as the product of induction/generalization? In his comment to the first Law, Newton mentioned projectiles, tops, planets, comets, and the effects of resistance on their motions, implicitly suggesting an "induction" of sorts. But the Law refers to a counterfactual state where a body is acted upon by no

[33] General Scholium to book III of the *Principia*, p. 547.

[34] See, for example, I. B. Cohen, "Hypotheses in Newton's Philosophy," *Physis*, *8* (1966), 163–84; A. Koyré, "L'hypothèse et l'experience chez Newton," *Bulletin société française de philosophie*, *50* (1956), 59–79.

forces. Can such a claim be regarded as properly inductive? And it mentions impressed forces – but *their* measure requires one to turn to the *second* Law. So that the two cannot be separately evaluated.

In his comment to the third Law, Newton reminded us that "if a horse draws a stone tied to a rope, the horse will be equally drawn back to the stone."[35] Is this observation really derived straightforwardly from the phenomena? And can it be generalized to yield the third Law? Surely it could have been argued (and indeed it *was* argued by some of Newton's predecessors) that action and reaction cannot be equal in cases such as these, for if they were, no motion would occur. When Newton used this everyday experience of horse and stone in the way in which he did here, he relied on a mature mechanical insight – and implicitly assumed a knowledge of the other two Laws. The simple observation of such occurrences as horses drawing stones will never furnish sufficient grounds for the third Law, expressed as it is.

It is surely significant that Newton did not attempt to substantiate each Law in the normal inductive way (as Boyle, for example, did his claim about the inverse relationship of pressure and volume of a mass of gas), other than by citing vague generalities about projectiles and tops. There is *some* element of induction here, of course, a plausible generalization from mechanical experience which makes it sound antecedently likely that mass, for example, should be measured by the product of density and volume. But it is hard to see how the basic insights relating force to *rate of change in motion*, rather than to the more intuitive *motion* of the Aristotelian tradition, could be justified by such an appeal alone.

It seems fair to conclude, then, that the warrant for the *Principia* cannot be taken to be a strictly inductive one in which the system is built up, proposition by proposition, on the basis of generalizations from experience. The Newtonian system stands or falls as a unit; whatever notion of warrant we develop must incorporate this insight.

There is a second strain in Newton's thinking that might seem more promising in this regard. In the Preface to the *Principia*, he wrote:

[35] *Principia*, p. 14.

I offer this work as the mathematical principles of philosophy, for the whole burden of philosophy seems to consist in this: from the phenomena of motions to investigate the forces of nature, and then from these forces to demonstrate the phenomena.[36]

He described the propositions of the first two Books as "mathematically demonstrated." They enabled him (he told us) in the third Book to:

derive from the celestial phenomena the forces of gravity with which bodies tend to the sun and the several planets. Then from these forces, by other propositions which are also mathematical, I deduce the motions of the planets.[37]

The deductivist tone is unmistakable. One might easily come to think of the *Principia* as a work of applied mathematics, to be evaluated by the correctness of its deductions and their utility for practical ends, as well, of course, as by its elegance and clarity. And the *Principia* indeed was, as we know, regarded in this way in the tradition of rational mechanics that derived from it in the eighteenth century.

Newton had, as we have already seen, one special reason for wanting the *Principia* to be taken mathematically rather than physically:

I here design only to give a mathematical notion of those forces, without considering their physical causes and seats. . . . I use the words "attraction," "impulse" or "propensity of any sort to a centre," promiscuously and indifferently, one for another, considering those forces not physically but mathematically, wherefore the reader is not to imagine that I anywhere take upon me to define the kind or the manner of any action, the causes or the physical reason thereof . . .[38]

Though *force* was his central explanatory concept, Newton was never comfortable with it. His critics were quick to urge that to explain in terms of a force of attraction was not to explain at all, because it was not at all clear what forces are or where they are to be located. In addition, to postulate a force of attraction inherent in all matter would make matter essentially active, and this ran

[36] *Principia*, pp. xvii–xviii. [37] *Principia*, p. xviii. [38] Definition VIII, *Principia*, pp. 5–6.

counter to one of Newton's most fundamental metaphysico-theological principles, namely, that matter is by its nature inert.[39] In the abundant draft-material from Newton's later years, we find him returning again and again to this problem, but he was never able to find a satisfactory solution to it. His "official" position, as it appears in the *Principia*, is thus as guarded as he could make it. To treat forces "mathematically" is to restrict oneself to their effects, the accelerations they produce, and to leave all other "physical" issues aside. So when he said: "To us it is enough that gravity really exists and acts according to the laws we have explained,"[40] the existence of gravity appears to reduce to the claim that bodies accelerate in the way the inverse-square law prescribes.

Much more could be said about the several ways Newton related the "mathematical" and the "physical," but our interest in the topic here is only due to the emphasis he was led to give to the "mathematical" character of the *Principia* in consequence. The empirical hardly intrudes in the first two Books, except for an occasional scholium, and, of course, the claim that the Definitions and Laws are inductively grounded.

There is, however, the fourth Rule of Reasoning in Book III, which allows that later exceptions *may* be found to inductive generalizations. But the intent of the Rule is to exclude idle hypotheses that rely on the bare possibility of the generalizations' being inaccurate; thus its emphasis is rather on the assertion that inductions are to be taken as "accurately or very nearly true" unless we have reason not to do so.

A more significant reservation is expressed in the *Opticks:*

> Although the arguing from experiments and observations by induction be no demonstration of general conclusions, yet it is the best way of arguing which the nature of things admits of, and may be looked on as so much the stronger, by how much the induction is more general. And if no exception occur from phenomena, the conclusion may be pronounced generally. But if at any time afterwards any exception shall occur from experiments, it may then begin to be pronounced with such exceptions as occur.[41]

[39] See McMullin, *Newton on Matter and Activity* [note 4], chap. 2.
[40] *Principia*, General Scholium to book III, p. 547.
[41] *Opticks*, p. 404.

So induction does not amount to demonstration. But a very general induction is claimed to be a close approximation to the truth; even if empirical anomalies are found, the original generalization may still be asserted, only that the exceptions now have to be mentioned. Obviously, Newton had no thought that the inductive claim could be overthrown, only that it might have to be qualified in some way. Thus, the fallibilism here is of a limited sort; it is that appropriate to an empirical law, like Boyle's Law. Is it appropriate to the Laws of Motion? Hardly, because the notion of progressive approximation does not apply so well here. Newton himself gave us a clue in this regard. In a Scholium regarding centripetal forces in Book I, he remarked:

> In mathematics, we are to investigate the quantities of forces with their proportions consequent upon any conditions imposed; then when we enter upon physics, we compare those proportions with the phenomena of Nature that we may know what conditions of those forces answer to the several kinds of attractive bodies.[42]

What happens in "physics," then, is that the laws of force are adjusted to the observed motions. The *provisional* aspect of mechanics would, then, be restricted to the various laws of force attributed to the "several kinds of attractive bodies." The inverse-square "law" would thus have a rather different epistemic status from the three "Laws" of Motion. Newton never explicitly discussed the possibility that the more basic Laws might also have to be adjusted. They are presented as conceptual relationships of an intuitively satisfactory sort. Did he think them to be definitive in their formulation (as his later followers often tended to do)? It is hard to know, one way or the other. But he certainly gave no indication in the *Principia* that he thought them open to further revision.

So far we have examined two possible types of warrant for the mechanics of the *Principia*, inductivist and deductivist. I mentioned earlier that there are hints of a third possibility in the *Principia* itself. In his Preface to the second edition, he mentioned that the theory of fluid resistance in Book II was "confirmed by new experiments," and that the theory of comets in Book III "was

[42] *Principia*, p. 192.

confirmed by more examples." The notion of empirical confirmation suggested here is a clue worth following.

When the mechanics of the *Principia* is taught today in an elementary physics course, it is usually said that its main warrant was the successful deduction of Kepler's Laws in Book III and of the motion of the pendulum in Book II. Did Newton think this? If he did, he gave little sign of it. It is instructive in this regard to look at the six "Phenomena" with which he opened Book III, "The system of the world." These had been listed as "Hypotheses" in the first edition, and mingled with the Rules of Reasoning; they were evidently to be taken as additional propositions needed in order to extend the work of Books I and II to the system of the world. The "Phenomena" list a sample of the major known astronomical regularities: Kepler's Laws and the periods of the satellites of Jupiter and Saturn. (Others, e.g., in regard to the nodes of the moon, are introduced later in the book.)

Proposition II, for instance, asserts that the forces acting on the planets obey an inverse-square law of distance. The text simply says that this is "manifest" from Phenomenon IV (Kepler's third Law) and Corollary VI to Proposition IV in Book I. It adds that this conclusion is also "demonstrable with great accuracy" from the fact that the aphelion points of the orbits do not move; even a very slight departure from the inverse-square law would bring about a large and perceptible motion of these points. What he was trying to establish, then, was precisely *which* law of force applies to our planetary system. He did not take it for granted that it would be inversely as the square of the distance; this was regarded as contingent and needing empirical determination. But there is no indication that the axiomatic section of Book I, constituting his mechanics proper, is in any need of confirmation.

Proposition XIII asserts that the planets move in ellipses, with the sun in one focus. "Now that we know the principles on which [these phenomena] depend, from them we deduce the motions of the heavens *a priori*."[43] The principles are assumed to be *known*, though the exact form of the laws of force in given cases has to be ascertained empirically. Newton went on to say that the planetary orbits are not *exactly* elliptical. "But the actions of the planets upon one another are so very small that they may be neglected." He

[43] *Principia*, p. 420. The phrase used in Latin is "a priori." Its use here ought not be taken to suggest an "a priori" status for the principles in the later Kantian sense of this phrase.

was thus implicitly *correcting* Kepler's empirical results on the basis of his theory, although of course what he was stressing was the *agreement* (within the limits of observational accuracy of the time) of the calculated and the observed orbital figures.

The most detailed calculations of Book III are devoted to the lunar motions and to cometary orbits. No one before Newton had managed to explain either of these phenomena in terms of a broader theory. He concluded his treatment of the first: "By these computations of the lunar motions I was desirous of showing that by the theory of gravity the motions of the moon could be calculated from their physical causes."[44] Here, for once, he allowed that he was deducing from physical causes. But although some of the results he obtained "exactly agree with the phenomena of the heavens,"[45] his calculations were in general much more fine-grained than the astronomer's observations: "The mean motion of the moon and of its apogee are not yet obtained with sufficient accuracy." Thus more long-term observation was needed, for "the theory of the moon ought to be examined and proved from the phenomena."[46] "Proving the theory from the phenomena" here presumably means testing the applicability to the lunar motions of the inverse-square law, as well as the adequacy of the analysis of the different forces acting on the moon.

Newton's treatment of comets is especially interesting in this regard. After a series of propositions and lemmas detailing how the orbits of comets are to be calculated, he headed the next section: "Example," and went on "Let the comet of the year 1680 be proposed."[47] There follows page after page of observational detail gleaned from astronomers all over Europe. He ended with a table comparing the *observed* positions of the comet over five months with the *computed* positions (based on the assumption that the comet's orbit is a highly elongated elliptical one) and found a satisfying degree of agreement. He ended on a triumphant note:

> The observations of this comet from the beginning to the end agree as perfectly with the motion of the comet in the orbit just now described as the motions of the planets do with the theories from whence they are calculated, and by this agreement plainly evince that it was one and the same comet that appeared all that time, and also that the orbit of that comet is here rightly defined. . . . And the theory which justly corre-

[44] *Principia*, p. 473. [45] *Principia*, p. 463. [46] *Principia*, p. 477. [47] *Principia*, p. 507.

sponds with a motion so unequable, and through so great a part of the heavens, which observes the same laws with the theory of the planets, and which accurately agrees with accurate astronomical observations, cannot be otherwise then true.[48]

Here, quite clearly a warrant was being claimed of a different sort from the inductivist and deductivist ones. What made the theory of the comet true in Newton's eyes was that it predicts correctly over a wide variety of dynamic contexts and that it coheres with the successful theory of planetary motions. This is *not* an inductive warrant, because there is no question of working upward from generalizations about cometary motions. This is a properly *retroductive* warrant.[49] The observed results are explained by a theory that introduces hypothetical conceptual elements (like *force*); the theory is confirmed by the range of accurate predictions it makes possible.[50]

Newton was speaking only of the theory of the comet here, not of his theory of mechanics as a whole. He did not, it would appear, claim a specifically retroductive warrant for the broader dynamic theory, perhaps because he did not think it was needed. But the remarkable agreement between theoretical prediction and observed regularities, which is apparent in the treatment of the pendulum in Book II[51] and in the astronomical analysis of Book III, *does* constitute a retroductive warrant of a convincing kind. Newton seemed to treat them as "problems to be worked" rather than as potential confirmations. It is as though he already *knew* that the dynamics was correct, and now he was going to *apply* this knowledge.

What one takes to be the "real warrant" for the *Principia* depends, of course, on one's own theory of science. There is much

48 *Principia*, pp. 515–19.

49 Peirce's term. I prefer it to "hypothetico-deductive," which was used by the logical positivists in a similar but rather more restricted way.

50 As well as by the novelty of the predictions it makes possible, and by its fertility over time. The notion of retroductive warrant is much more complex than simply "getting the predictions right." See, for example, McMullin, "The criterion of fertility and the unit for appraisal in science," *Boston Studies in the Philosophy of Science*, 39 (1976), 395–432.

51 Where his only remark on the evidential value of his results is the laconic: "and by experiments made with the greatest accuracy, I have always found the quantity of matter in bodies to be proportional to their weight" (Corollary VII to Proposition XXIV, p. 304). Did this constitute in his own mind a *test* of the second Law?

induction in Newton's science, there is a persuasive appeal to simple mechanical experience, there is the coherence of a tight mathematical system. But it is, in the end, the fact that the dynamics of the *Principia* predicted so variously, so well, and for so long, that would today count most in its favor. No theory before it came even close to illustrating the power of retroduction so well.

And now I can return finally to the main theme, and draw a particular moral from the discussion above of the warrant on which the *Principia* rests. Retroduction works for the theory as a *whole*. It is the dynamics as a *whole* that is confirmed by the results of Book II and Book III. Because of the interconnection of the elements of the theory, all of them are implicit in each deduction, in principle at least.

We are now very far from the classical empiricism which Newton inherited, and within which he struggled to clarify his procedures. Not only is the meaning of terms like "mass" and "force" determined by the theory as a whole rather than by abstraction from our experience of matter or of effort, but this meaning is "justified" by the success of the theory of which it is a part. We are not to seek separate inductive support for each of the three Laws; we are not to worry about the supposed circularity of defining mass partly in terms of density. The dynamics functions as an explanatory network, each node playing its part. But it is the network as a whole that is tested against experience by means of prediction, not the separate elements of it.

One further consequence of this is worth noting. If the retroductive warrant of a theory begins to fail, if anomalies multiply, what may happen is not the improvement of approximation (as the inductivist tradition would have had it), but the *replacement* of one theory by another. The earlier theory may not just be corrected; it may be refuted. The reason, note, lies in the difference between the inductive and the retroductive type of warrant.

There is much difference between philosophers of science as to whether (and if so, in what sense) one should say that Newtonian mechanics has been refuted. It is still in general use, so that there is an understandable temptation to hold that it is a limiting case (for low velocity, low mass, etc.) of relativistic mechanics, thus holding true as a sort of approximation. But this will not do (as Feyerabend, in particular, has stressed) because some of the basic assumptions of the older mechanics turn out to be not approximate but wrong. Space and time, for instance, turn out not to be related as Newton thought they were. As a *predictive* system, Newtonian mechanics is still perfectly adequate for most purposes. But as an *explanatory* account, it is no longer acceptable. And the notion of "approximation" does not apply to explanations in the way it does to empirical laws.

CONCLUSION

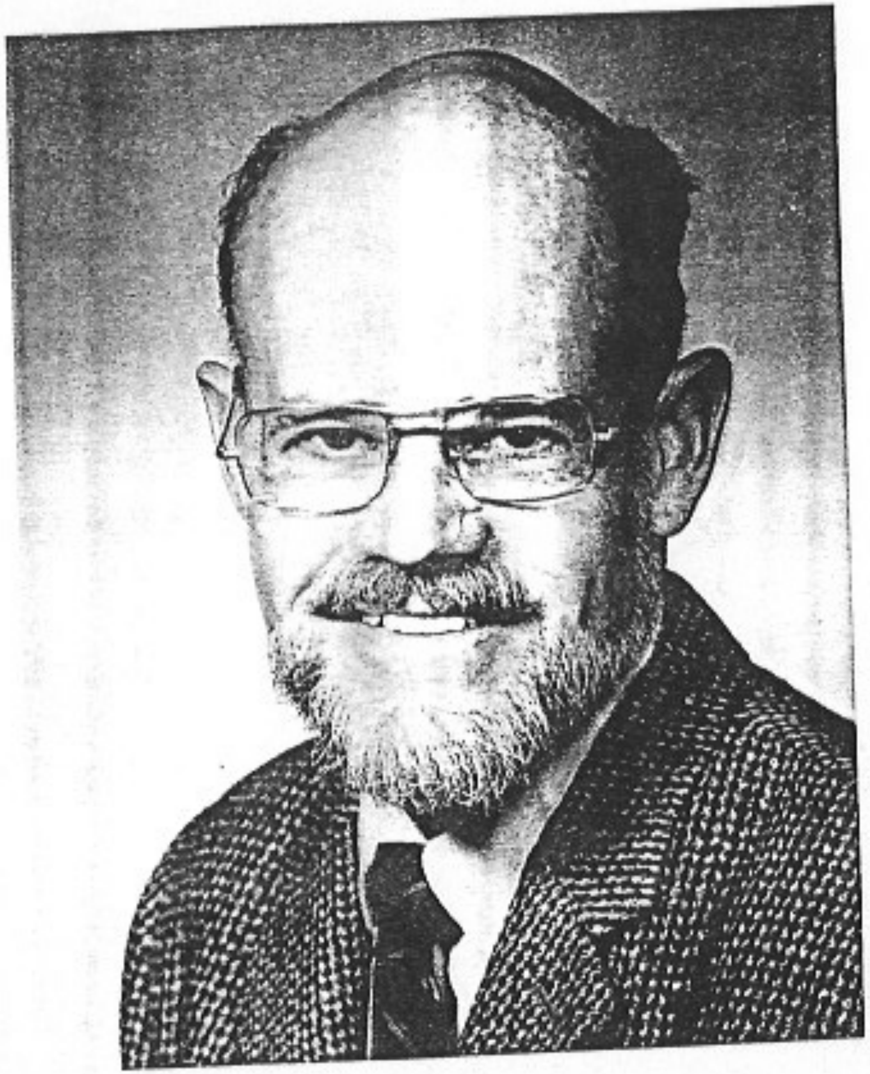
The kind of revolution that relativistic physics represents is the final challenge to the older empiricism. It is, indeed, a challenge to epistemology at a wider level, because basic notions like objectivity and truth come under pressure and a realistic account of knowledge is imperiled.

It would be silly to suggest that all of this was implicit in the original *Principia*. And Newton's conservative inductivism ("pronounced with such exceptions as may occur") would hardly have prepared him for what happened in 1905. My point is that the *Principia* was the starting point for a set of developments that moved epistemology permanently away from the empiricist maxims that were taken for granted when the *Principia* itself was composed.

There had, of course, been physical theories before Newton's. And the notion of language as a network, to be understood and to be evaluated in a holistic way, is not restricted to physics; it can be nicely illustrated by such philosophical disputes as the medieval one about the unity of substantial form (where the apparently "observational" claims turn out to be quite theory-laden). But the *Principia* holds a special position in the development of this epistemological insight, nevertheless, because it tied the conceptual elements together in a more explicit way than had been done before, a way so explicit and so precise in fact that the network could be almost completely formalized. Meaning and assessment were thus shifted to the system as a whole; the older empiricist notions of abstraction and induction (which related to single terms and single generalizations) no longer would suffice.

It is a commonplace that great works contain more than their creators know. This is true in science as elsewhere. And it is true in science, not only in the sense that the great work contains much that only later scientific research will make explicit, but also in the

sense that the creator may well be unaware of the significance of *how* he is proceeding. He intuitively "does it right," but when he tries to tell *how* he did it, or how more generally it *should* be done, he falls far short of the real achievement.



Richard S. Westfall.

Religion, science, and worldview

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Edited by
MARGARET J. OSLER
The University of Calgary

and

PAUL LAWRENCE FARBER
Oregon State University

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