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Newtonian Space-Time*

I

MY REMARKS TODAY AND TOMORROW WILL BE BASED UPON A RATHER lengthy paper, written with several simultaneous objectives:

(1) To cast light upon the issues involved in a celebrated passage of intellectual history, and incidentally to clarify some of the purely historical circumstances;

(2) By elucidating those issues, to help furnish insight on related questions of current interest;

(3) To promote an attitude toward philosophical questions that was a prevalent one in the seventeenth century, that seems to me sound and admirable, and that seems not to be prevalent today.

The subject is Newton's doctrine of space and time; but I want to begin by outlining, in the language of present-day mathematics, not what Newton says about this, but what is in actual fact presupposed by the science of dynamics that we associate with his name.

The structure of space-time according to Newtonian dynamics—two formulations:

A. (1) The "world" W, the set of "events" (whatever physical interpretation the word "event" is to have), is totally ordered by the (asymmetric and transitive) temporal relation "earlier than": that is, the relation "neither is earlier," called "simultaneity," is an equivalence. The quotient of W by the relation of simultaneity is *time*; in other words, if by an "instant" we mean an equivalence-class of events under simultaneity, then time is the set T of all instants.[1]—There is furthermore intrinsic in W a structure that determines for T a particular notion of *ratio of intervals*; and this ratio satisfies a simple set of conditions, which taken together characterize T as a *one-dimensional real affine space* (or Euclidean line), whose affine structure is compatible with the ordering it inherits from W.

(2) There is intrinsically determined *for each instant* a notion of *ratio of distances* (among events belonging to that instant); and this ratio satisfies a rather intricate set of conditions, which can be summed up by the statement that *any class of simultaneous events is a subset of a three-dimensional Euclidean space* (the ratio of distances among events agreeing with the ratio derived from the Euclidean structure). If we assume further that at no instant do all events lie in a plane, then the entire three-dimensional Euclidean space of each instant is effectively defined by the structure postulated. The union of all these instantaneous spaces may be thought of as the *locus of all possible events*; it is called "space-time."

(3) The instantaneous three-dimensional spaces, which so far as (2) goes are entirely separate, form part of a larger connected structure: space-time is, intrinsically, *a four-dimensional real affine space*—that is, it possesses a a notion of "straight line," having the same properties in respect of intersection (and so also parallelism) as in a Euclidean space of four dimensions. (The straight lines of space-time are to be thought of as representing all possible *uniform rectilinear motions*.[2]) Moreover, the natural projection of space-time upon T (assigning to every possible event its "epoch" or "date") is an affine mapping; and the Euclidean structures of the instantaneous spaces are compatible with their affine structures as the fibers of this mapping.

The technicalities of this account need not too much concern those to whom they are unfamiliar; but it is important to remark that the structure required by (3), which I shall refer to as the "kinematical connection" of space-time, and which is essential in order to have a framework for the analysis of *motion*, cannot be derived as a consequence of (1) and (2): this structure is constrained by (1) and (2), but is far from being determined by them.—I have expressed the structure of Newtonian space-time as a set of conditions upon the world of events. Before giving a second version of this structure, it may be helpful to summarize the first still more succinctly in terms of space-time alone:

> Space-time is a four-dimensional real affine manifold, given together with: a one-dimensional real affine manifold ("time"); an affine projection of the former manifold onto the latter; and, on each fiber of this projection, a Euclidean metric compatible with the affine structure.

B. If S is a three-dimensional Euclidean space and T a one-dimensional affine space, the *Cartesian product* S×T—that is, the set of ordered pairs (s,t) whose first member, s, belongs to S, and whose second member, t, belongs to T—has, in a natural way, all the structure just postulated for space-time. But S×T has in addition a particular *spatial projection*, whose fibers define a preferred family of parallel straight lines or (briefly) a *time-axis*. Conversely, if in space-time we pick any direction transverse to the instantaneous spaces, we get a well-defined projection onto the space of any instant, having fibers parallel to that chosen direction; and this defines a representation of space-time as a product S×T. The fact that space-time has no preferred time-axis is called (in the present context) the "principle of Galilean relativity"; and the structure of space-time can be characterized as the structure obtained from S×T by applying the principle of Galilean relativity. In S×T we have something that can be called "enduring space": the spatial projection can be viewed as a mapping that assigns to every possible event its place, just as the temporal projection assigns to every possible event its date; in order words, for each point of space, the fiber of the spatial projection over this point—which is a line parallel to the time-axis—can be regarded as "the point, persisting through time." Applying Galilean relativity obliterates this structure: removing the time-axis, it leaves the projection on T but not that on S.

Now let us turn to Newton's famous scholium, in the *Principia,* on time, space, place, and motion.

Newton says that although kinematical notions are so familiar that he doesn't need to define them, their ordinary application gives rise to certain preconceptions or "prejudices" that he is concerned to remove. To this end he proposes to distinguish these notions into "absolute and relative, true and apparent, mathematical and common"; and he clearly intends the "absolute, true, and mathematical" notions to furnish the kinematical substratum of his dynamics. "Absolute, true, and mathematical time," which "of itself and from its own nature flows equably without regard to anything external," is just the structure T: the one-dimensional affine space, upon which space-time has a natural affine projection that preserves its chronological ordering. And "absolute space," similarly, is the structure S, but *identified with the set of lines parallel to the time-axis in* S×T—or in other words, *with a given mapping of the world of events upon it*: for Newton says that absolute space "in its own nature, without regard to anything external, remains always similar and immovable"—clearly implying that it has an identity through time.—On the other hand, *phenomena*, to which dynamics must be applied, and *measurements*, which afford the means of application, always involve the "relative" and "apparent" notions: "relative, apparent, and common time, is some sensible and external (whether accurate or unequable) measure of duration by means of motion, . . . such as an hour, a day, a month, a year"; and "relative space is some movable dimension or measure of the absolute spaces; which our senses determine by its position to bodies . . . ."

These distinctions are responsible for Newton's having had a rather bad press amongst philosophers since the late nineteenth century. The chief problem is how *phenomena*, which by definition bear directly upon the "apparent" notions, can give us any information about the so-called "true" ones; in other words, how Newton's "absolute, true, and mathematical" notions can have any empirical content. Newton indeed discusses this question in the scholium—most notably in the celebrated account of the phenomena in a spinning water-bucket—but his elucidation has usually been dismissed as inadequate.

The scholium ends with a striking remark:

> But how to collect the true motions from their causes, effects, and apparent differences, and conversely from the motions whether true or apparent to collect their causes and effects, shall be explained more fully in the sequel. For to this end have I composed the following treatise.

—Now, what does he mean by that? The intimation that the entire *Principia* was composed to explain how to determine absolute motion has been rejected by some with shocked and fervent rhetoric. But it seems to me that one does well, in a case like this, to read the book and see whether it does what the author says he intends it to do.

The central argument of the *Principia* is to be found in the first part of Book III, on the System of the World—that is, as we should say, on the solar system. The dominant question of natural philosophy in the seventeenth century was the question of the structure of this system, and in particular the question whether it is the earth or the sun that occupies its true fixed center. Newton succeeded in giving a very solid and decisive answer to the general question; and to the particular question, whether the earth or the sun is at the center, he gave an answer that was quite surprising: Neither. The argument upon which this answer was based is a most beautiful one, and repays careful study. Although Newton's exposition is in most respects extremely lucid, it conceals certain subtleties, and the reasoning is less straightforward than it has generally been taken to be. Let me summarize the salient points.

The premises of the argument, in Newton's formulation, are statements about astronomical phenomena, summing up the best data on the motions of the planets and their satellites essentially in the form of what we know as Kepler's laws (taken as characterizing the *relative* motions). Alongside these premises are the general principles of force and motion: the Laws of Motion that stand as "Axioms" at the beginning of the *Principia,* and a formidable battery of theorems derived from them in the preceding Books, especially Book I. (It is, by the way, these theorems that Newton specifically refers to as the "mathematical principles of philosophy.") And the argument is conducted in accordance with yet another class of premises or principles, which Newton in later editions calls "Rules of Philosophizing," although in the first edition they are simply listed as "Hypotheses" together with the astronomical premises. The presence of this third class of principles shows that Newton does not present his argument as simply a mathematical deduction from astronomical premises and principles of mechanics. It is also not a deduction from these together with the Rules of Philosophizing—the latter function as guiding principles rather than as premises or precise rules of inference. But in saying that the argument *conceals* subtleties I have in mind more than this: the reasoning has this extraordinary character, that *its conclusions* (so far from being logical consequences) *stand in formal contradiction to its premises*. For according to the theory of universal gravitation, which is of course the product of this argument, Kepler's laws (which were its premises) cannot give an exact representation of the motions of the planets.

From a quite abstract point of view, one could describe this situation by saying that Newton found a simple general postulate which accounts for the premised phenomena in close approximation—and that he then chose to regard the general postulate as exact, in preference to the empirical laws of the phenomena. But this description does not do justice to Newton's argument. The astronomical phenomena that Newton starts from can be represented, according to his analysis, by supposing the major astronomical bodies to be surrounded by central acceleration-fields whose intensities vary inversely with the square of the distance from the center; this yields Kepler's laws exactly. Newton's demonstration of this fact led his greatest contemporaries, Huygens and Leibniz, to accept the inverse square law in astronomy; indeed Huygens, who had previously doubted that Kepler's laws were more than an empirical approximation to the planetary motions, was actually convinced by Newton's theory that Kepler's laws hold exactly! Huygens and Leibniz both, however, *rejected* the theory of universal gravitation, as theoretically objectionable and empirically unproved. Since Huygens and Leibniz were men of formidable intellect—and Huygens in particular a skillful and profound investigator of nature; and since on the other hand Newton's fantastic conclusion that *all bodies attract one another*—and how really extraordinary a conclusion this was, and even (compared with our ordinary experience of bodies) still is, only the dulling effects of what we call "education" can have succeeded in obscuring[3]—has proved to be entirely correct; there is *prima facie* reason to consider that there may be something both sound and deep in the method that led Newton along this path where his great contemporaries could not follow.

A detailed analysis of this matter would take me too far from my central topic, but I shall try to sketch the main outlines. Newton's argument can be described as having three principal stages. The first, and logically simplest, made the greatest impression at the time. It is the purely mathematical demonstration (given in Book I) that Kepler's laws for a system of bodies moving about a fixed body are *equivalent* to the statement that the accelerations of the moving bodies can be derived from a single inverse-square acceleration-field. (This result is essentially contained in Propositions I-III, Proposition IV Corollary 6, Proposition XI, and Proposition XV, of Book I. Its straightforward application to the astronomical case is given in Propositions I-III of Book III—although Newton's concise discussion of these Propositions touches upon some subtler points, which I pass over.) The second stage is also very clearly expounded, and is concentrated in Propositions IV-VI of Book III. Its conclusion, obtained through appeal to the Rules of Philosophizing, is that the accelerations of the heavenly bodies are due to the same *vis naturæ* or *potentia naturalis* that manifests itself in our daily experience as the *weight* of bodies; in other words, that the inverse-square acceleration-fields of the major bodies are *gravitational* fields that affect all bodies near them. The crucial step in this stage of the argument is the famous comparison of the moon's acceleration with that of falling bodies on the earth: Newton shows that the former is to the latter as the square of the earth's radius is to the square of the radius of the moon's orbit—so that if the moon were brought down to the earth, and its acceleration continued to increase according to the inverse square law, that acceleration would reach just the magnitude of the acceleration of falling bodies, and would therefore simply *be* what we should call the acceleration due to the moon's "weight." A full discussion would demand that more be said here, about the extrapolation involved in imagining the

moon brought down to the earth, and about the character of the link so established between the terrestrial phenomenon of weight and the astronomical motions, not only of the moon but of all the other bodies. However, the broad argument is clear, and all competent readers of the *Principia* found it convincing; up to this point, Huygens and Leibniz in particular gave full agreement.

The third stage of Newton's argument is in point of fact a little hard to locate in his text. Its conclusion is the law of universal gravitation; and this is essentially stated in Proposition VII of Book III and its Corollaries. But the proofs of these statements are very short, and consist largely in appeal to what has already been shown; one feels uneasily that something may have been smuggled in. Careful analysis finds two crucial points. The first is the question, how far the extrapolation involved in what I have called the second stage is to be carried, and how exact it is to be assumed to be. The question arises for the following reason:—One of the assertions of Newton's theory that Huygens dissents from is what the latter calls "the mutual attraction of the whole bodies." By this he means—in distinction from the mutual attraction of the small particles of bodies, which he rejects *a fortiori*—the attraction between the major bodies of the solar system. But since the second stage of the argument has concluded that *all* the major bodies are surrounded by inverse-square acceleration-fields, that is gravitational fields, which affect *all* the bodies about them, it would seem to *follow* that these bodies affect one another; that, for instance, the sun gravitates towards each planet, Jupiter and Saturn towards one another, and so forth. Huygens does not say how he is able to escape this conclusion; but there is really just one way he can, namely by refusing to extend the inverse square law arbitrarily far—i.e., by supposing the validity of that law restricted to some finite region, beyond which the gravitational field decays more rapidly and even goes to zero. There is reason to believe that this was Huygens's conscious supposition, made not just from skepticism of the reach of empirical generalization (for to *doubt the exactness* of such a generalization is very much less than to *believe an equally definite contrary statement*: here, for example, the statement that the acceleration of Saturn towards Jupiter is not what Newton thinks, but zero), but made on the basis of Huygens's own theory of the mechanical cause of gravity, which could hardly be reconciled with Newton's unrestricted linear superposition of gravitational fields.[4] For Newton, on the other hand, it is a fundamental principle of method to press empirical generalizations as far and as exactly as possible, subject to empirical correction; and to do so without regard for theoretical considerations of a speculative kind.[5]

We may thus say that the beginning of the third stage of Newton's argument consists in a sort of rigorous construction that he places upon the conclusion of the second stage. But the most critical point is Newton's application of the Third Law of Motion to any gravitating body and the body towards which it gravitates. Newton does not insist that what he calls the "centripetal force" on the gravitating body is truly an attraction, exercised by the central body; yet he argues as if the Third

Law required the force on the gravitating body to be coupled with an equal and opposite force on the central body. Huygens, who assumes the force of gravitation to be exerted by an ether, objects explicitly and with justice to this step, which roughly amounts to the assumption that even if an ether does enter the interaction, momentum is conserved separately amongst the *visible* bodies.[6] The step is critical, because it is by this argument that Newton concludes *all* gravitation to be mutual; concludes, therefore, that every particle of matter, having weight proportional (at a given place) to its mass, is also a center of gravitational force, with a strength proportional to its mass.[7]

So much, then, for our outline of the argument that led to the law of universal gravitation. Before connecting this with the questions that chiefly concern us, I should like to make two comments of a historical nature on the theoretical controversy that Newton's work aroused. The first remark is, that when Newton responds to criticism of the *Principia*, he appears to rest his case for universal gravitation, not upon the *argument leading to* the theory, but upon the *extremely detailed agreement of its consequences with observed phenomena.* Thus he writes to Leibniz on 16 October 1693:

> What that very great man Huygens has remarked on my work is acute. . . . But . . . since all the phenomena of the heavens and of the sea follow accurately, so far as I am aware, from gravity alone acting in accordance with the laws described by me, and nature is most simple; I myself have judged that all other causes are to be rejected. . . .

Among the points concerned in this detailed agreement were: the beginnings, at Newton's hands, of a satisfactory representation of the motion of the moon; the first explanation of the precession of the equinoxes; and the theory of the tides. (Newton's explanations of the precession and of the tides were explicitly rejected by Huygens.)—And the second remark is, that one particular point of detailed agreement—one indeed that would most probably have convinced Huygens at least of the mutuality of the astronomical forces (if not of the universality of gravitation)—was not established at the time Huygens read the *Principia.* I refer to the mutual perturbation of the orbits of Saturn and Jupiter near the conjunction of those planets. I have not found an account of the circumstances in any of the histories of mechanics or of astronomy that I have examined; but the second volume of Newton's collected correspondence contains a letter written to Flamsteed, the Astronomer Royal, in December 1684, inquiring about a possible deviation of Saturn's motion at its conjunction with Jupiter; and contains Flamsteed's reply, somewhat indecisive, but negative. The first edition of the *Principia* (published in 1687) discusses this perturbation without any claim that it is observable. But the second edition (published in 1713) describes the mutual perturbation as observable, and Saturn's deviation in particular as having "perplexed" astronomers. And in fact one finds, in

a notation by David Gregory dated 4 May 1694 (reproduced in Volume III of Newton's correspondence), the following:

> At Saturn's and Jupiter's most recent conjunction [their] Actions upon one another were manifest; indeed before the conjunction Jupiter was accelerated and Saturn retarded, after the conjunction Jupiter retarded and Saturn accelerated. Whence Corrections of the Orbits of Saturn and Jupiter by Halley and Flamsteed, which were afterwards perceived to be worthless . . . .

(The point of the last remark is that the deviations of the two planets from the courses predicted for them were not due to errors in the determination of their orbits, and could not be used to compute more accurate orbits, since what was observed was in fact a perturbation. Evidently neither Flamsteed, to whom Newton had addressed his inquiry several years before, nor Halley, Newton's good friend, had thought at first of the effect predicted by Newton.)

In any case, Newton's analysis begins from phenomenological and therefore relative motions, and leads to a comprehensive account of the forces of interaction in the solar system. But now if this *dynamical* account is accepted, it has implications beyond the phenomenological premises that led to it—indeed, as I have just explained, it even implies *phenomenological corrections* to those premises; and these implications of the dynamical theory concern in part what Newton calls the "true" or "absolute" motions. Newton's own exposition is characteristically clear and succinct. Following Proposition X of Book III we find (in the second and third editions):

> HYPOTHESIS I. *That the center of the system of the world is at rest.*
>
> This is acknowledged by all, while some contend that the earth, others that the sun, is at rest in the center of the system. Let us see what from hence may follow

I ask you to note that Newton makes no attempt here to argue the *truth* of this "Hypothesis"; he merely proposes to examine the consequences, in the light of his theory, of a common presupposition of the whole cosmological controversy. Then:

> PROPOSITION XI. *That the common center of gravity of the earth, the sun, and all the planets, is at rest.*

The proof is trivial: the center of gravity of an isolated system (which the solar system can be taken to be) is either at rest, or in uniform rectilinear motion; but if the center of gravity were in uniform rectilinear motion with non-zero velocity, there could obviously be no fixed point in the system at all (since the configuration of the system is at least roughly stable); therefore to uphold the Hypothesis we must suppose the center of gravity to be at rest.—But from this we have the conclusion:

> PROPOSITION XII. *That the sun is agitated by a perpetual motion, but never recedes far from the common center of gravity of all the planets.*
>
> . . . .

Corollary. Hence the common center of gravity of the earth, the sun, and all the planets, is to be esteemed the center of the world. For since the earth, the sun, and all the planets, mutually gravitate one towards another, and are therefore . . . in perpetual agitation, . . . it is plain that their mobile centers cannot be taken for the quiescent center of the world. If that body were to be placed in the center towards which all bodies gravitate most (as is the common opinion), that privilege ought to be allowed to the sun. But since the sun moves, that resting point is to be chosen from which the center of the sun departs least—and from which it would depart still less if only the sun were denser and larger and so were less moved.

If we compare the entire argument whose structure I have now described, with Newton's statement of his intention at the end of the scholium on time and space, I think we can only conclude that the performance corresponds very well with the promise: from motions, either true or apparent—and here more particularly from *apparent* motions—to arrive at the knowledge of their causes and effects; and from the causes, effects, and apparent differences, to collect the true motions.—The intricacy of the theoretical and empirical connections is worth emphasizing. Perhaps it has seemed puzzling that Newton speaks of arriving at the knowledge of the causes *and effects* of the motions. But in fact it is just as *an effect of the earth's true rotation* that Newon predicts its oblate shape—for which geographical evidence was still lacking; and it is only on the basis of this shape, known from the theory but not yet confirmed by direct measurement, that Newton is able—bringing to bear the full power of his theory of gravitation—to account for the precession of the equinoxes.

What can be said of the explicitly "hypothetical" element in Newton's elucidation of the true motions: the hypothesis that the center of the world is at rest? We know the answer: there is no way, on the basis of observations and of the principles of Newtonian dynamics, to establish the truth of this hypothesis; for Newtonian dynamics satisfies the principle of Galilean relativity, according to which one cannot distinguish a state of rest from any other state of uniform irrotational straight-line motion. We know this answer; and Newton did too, and formulated it quite clearly in the fifth Corollary to the Laws of Motion. In the light of this fact, and of the very carefully hypothetical form of Newton's statement about the center of the world, it appears to me that Newton's philosophical analysis of the kinematical presuppositions of his dynamics emerges as acute and *almost* unexceptionable. The qualification lies in this, that although he is clear that dynamics does not provide any way to distinguish motion from rest, Newton does not seem to have conceived the philosophical possibility that *that distinction cannot be made at all*; that is to say, that the spatio-temporal framework of events does not intrinsically possess the structure of the Cartesian product $S \times T$, but a weaker structure. One easily understands why Newton should not have conceived this possibility; even Poincaré, at the end

of the nineteenth century, could express the view that if rotation is real then motion must be real, and if acceleration is real then velocity must be real. But the more abstract point of view that mathematics has now made available allows us to see, today, that these considerations are specious, and that the true structure of the space-time of Newtonian dynamics, with its Galilean invariance, is the one I have already described, in which there is an absolute time but no absolute space—that is to say, a natural mapping upon T but none upon S. The point that is really crucial for kinematics is that within this structure *there is no absolute or intrinsic notion of velocity, but there is an absolute or intrinsic notion of velocity-difference*—and *therefore* of rotation and of acceleration.

If as I have said it is not surprising that Newton failed to conceive the possibility of such a structure for the space-time of the real world, I regard it as one of the astonishing facts of intellectual history that a contemporary of Newton's did conceive exactly this possibility, and maintain it as the truth. I mean Huygens, who expresses himself to this effect in a number of late manuscript fragments on the question of absolute and relative motion. These fragments have been repeatedly discussed, but they have not in my view been estimated at their true worth; partly because of a certain confusion in Huygens's philosophical conception, which somewhat obscures his exposition; but more, I think, because the true conceptual situation has not been well understood. No one else before Einstein matched Huygens's appreciation of the fundamental importance of the so-called "special principle of relativity." And when Einstein put the matter in its true light, it was in the context of electrodynamics rather than mechanics, therefore of Lorentz-invariance rather than Galilean invariance—a context which required a quite drastic revision of mechanics itself; and fairly soon the far deeper issues of general relativity emerged. As a consequence, rather little attention has been paid to the intrinsic relativity theory of Newtonian dynamics.

In conclusion today, I want to explain briefly what I have just said about velocity and velocity-difference in Newtonian space-time:—The instantaneous state of motion of a particle is represented, in the structure defined at the beginning of this lecture, by a tangent-vector to space-time, "normalized" by the requirement that its time-component be unity: namely, the vector with unit time-component that is tangent to the particle's space-time trajectory at the instant in question. If one asks for the *velocity* of the particle, one wants the "space-component" of this vector; and this is not a well-defined notion, unless a projection of space-time onto space is given—that is, the projection onto time being present from the start, unless space-time is endowed with the structure $S \times T$. On the other hand, if we consider two different states of motion (whether at the same time or at different times), the difference of their representative vectors will be a well-defined space-time vector *with time-component zero.* Such a vector, at any space-time point, is tangent to the instantaneous space in which that point lies; that is, it can be regarded in a natural way as a spatial vector. This is the velocity-difference vector, from which the absolute acceleration-vector of a trajectory can be derived. And the existence of this velocity-difference vector where no well-defined velocity vectors exist allows us to make perfect sense of Huygens's statement that when a body is in rotation its parts, although maintaining constant geometrical relations amongst themselves, are mutually in relative motion (by which he means that they have non-zero velocity-differences); and yet that one cannot say, or meaningfully ask, how much "real" motion any one of them has—this being, he says, "nothing but a chimera, and founded on a false idea."

In tomorrow's talk, I shall comment upon the philosophical background of Newton's views of space and time, and on some of the philosophical controversy to which those views have given rise, from his time to our own.

II

It is not only in respect of the role played by Newton's kinematical conceptions in his positive work that I believe commentators to have erred: I think too that they have misconstrued Newton's chief reasons for making an issue of the distinctions he proposes, and for using the language he does to express them. We are accustomed to seeing Newton's doctrine of space contrasted with that of Leibniz, who held the essence of space to be *relational*: that is, he held that space is constituted essentially by the *spatial relations of bodies,* and he wanted to explicate those relations in terms of something more fundamental. The relationship between Newton's doctrine and Leibniz's is in fact of very great interest. But to understand Newton's scholium on time, space, place, and motion properly, it is necessary to realize that in it he was concerned to differentiate his theory not from Leibniz's but Descartes's. Establishing this difference was important to Newton for two reasons: Descartes's physics and cosmology constituted the most influential view in the scientific world at the time; and Descartes's mechanics was based upon a very confused semi-relativistic concept of motion, on which it would have been hopeless to build a coherent theory. In other words, the "prejudices" that Newton says his scholium was intended to remove were, in large part, those of the scientific community, and his aim was to establish a new technical terminology as the foundation for a coherent theory.—In part, however, the "prejudices" were of a theological character; and so we find at the climax of the scholium the following passage:

> . . . [I]f the meaning of words is to be determined by their use, then by the names time, space, place, and motion, their measures [i.e., the relative quantities] are properly to be understood; and the expression will be unusual, and purely mathematical, if the measured quantities themselves are meant. Upon which account, they do strain the sacred writings, who there interpret those words for the measured quantities. Nor do those less defile the purity of mathematical and philosophical truths, who confound real quantities themselves with their relations and vulgar measures.

The last two sentences are a characteristic example of Newton's condensed rhetoric. Not only do they state Newton's view of the proper way to read the Bible and the proper way to pursue philosophy, they also contain a twofold gibe at Descartes; for Descartes in his *philosophy* used a concept of motion as relative that allowed him to claim *theological orthodoxy* by maintaining that in the proper or philosophical sense the earth is to be regarded as at rest. And we also have here, incidentally, an example of the crudeness with which Newton's words have often been read: for the clause accurately translated by Andrew Motte as "they do strain the sacred writings," Florian Cajori, in his revision of Motte's version, changes to "those violate the accuracy of language, which ought to be kept precise"—thereby violating the accuracy of language, and ruining the sense of the passage.[8]

The evidence that Descartes was Newton's main philosophical target in the scholium is presented in detail in the paper I have mentioned. It is based upon a careful study of the text of the scholium, and a comparison with the second Part of Descartes's *Principia Philosophiæ.* The conclusion that I drew from this comparison I later found strikingly confirmed by an essay of Newton's, unpublished till 1962, treating substantially the same questions as the scholium, in a far more expansive style, in the explicit guise of a refutation of Descartes. I don't think it would be useful to pursue this matter today, because the details seem to me—like Descartes's physics itself—of fairly narrow historical rather than philosophical interest. My reason for emphasizing the point at all is that I believe it relevant for the philosophical assessment of Newton's discussion to understand that he was addressing himself to the clarification of an undeniably confused conceptual scheme.

But there is a passage in that recently published essay that has no counterpart in the scholium, and seems to me of considerable interest. It is a little prolix; I shall quote it with some excisions:

> Lastly, that the absurdity of this position [of Descartes's] may be made most clear, I say that it follows thence that a moving body has no determinate velocity and no definite line in which it moves. And all the more, that the velocity of a body moving without resistance cannot be said to be uniform, nor the line to be straight in which its motion is accomplished. . . .
>
> But that this may be clear, it is first of all to be shown that when some motion is finished, it is impossible according to Descartes to assign a place where the body was at the start of the motion. . . . For example, if the place of the Planet Jupiter a year ago be sought; by what criterion, I ask, can the Cartesian Philosopher describe it? Not by the positions of the particles of the fluid matter [that is, the ether], for the positions of these particles have changed greatly since a Jupiter a year ago be sought; by what criterion, I ask, can the Cartesian Philosfor the unequal influx of subtle matter through the poles of the vortices . . . , the Vortices' undulation . . inflation . . . and absorption, [all of which are processes described by Descartes in Part III of his *Principia,*] and other truer causes, such

as the rotation of the sun and stars about their own centers, the generation of spots, and the passage of comets through the heavens, change both the magnitudes and the positions of the stars so much, that perhaps they do not suffice to designate the place sought for but with an error of several miles—much less can the place be accurately described and determined by their help, as a Geometer would require. Truly there are no bodies in the world whose relative positions remain unchanged with the passage of time . . . . And so there is no basis from which we can now designate a place that was in time past, or say that such a place is any more to be found in nature. . . . And so as of the place of Jupiter a year ago, it is manifest according to Descartes's doctrine that, of the place of any moving body, not God himself (a new state of things holding) could describe it accurately and in a Geometrical sense; since on account of the changed positions of bodies it does not exist in nature any more.

Now since after any motion is completed the place in which it began . . . cannot be assigned nor any longer exists: that space traversed can have no length; and . . . it follows that the moving body can have no velocity; which is what I first wished to show. Further, what was said of the starting-point . . . should be understood similarly of all the intermediate places; and therefore, as the space has neither a starting-point nor intermediate parts, it follows that there was no space passed over, and thus no direction of motion, as I wished secondly to show. . . . So it is necessary that the determination of places, and therefore of local motion, be referred to some immobile being, such as is extension alone, or space so far as it is seen as truly distinct from bodies. And this the Cartesian Philosopher may more readily admit if he observes that Descartes himself had an idea of this extension distinct from bodies. . . . And that the gyrations of the vortices, from which he deduced the force of the ether in receding from their centers, and therefore the whole of his mechanical Philosophy, are tacitly referred to that generic extension.

I believe that if Huygens and Leibniz, who commented adversely upon the scholium in favor of a relativistic view of motion but never exchanged views on the subject with Newton, had been confronted with the argument of this passage, a clarification would have been forced that could have promoted appreciably the philosophical discussion of space-time—and of scientific concept-formation in general. When I say "promoted appreciably," I mean in fact to a point that has not become commonplace even today. Contrary-to-fact conditionals in history are of dubious worth, and the basis of my suggestion will perhaps appear somewhat subjective: in studying the writings of these three men—Newton, Huygens, and Leibniz—I have found striking, not only their intellectual power, but even more the extraordinarily high plane of discussion in which they engaged, from which the subsequent generations of scientific-philosophical controversy seem to me a serious falling-off. To put it as simply as I can, these were—with all due allowance for personal anfractuosities

—serious men, concerned with real problems of understanding the world even more than with pet ideas or the scoring of points in debate. And this seems, in point of fact, rare.

What Newton points out in the passage I have quoted is the need for what I called, in yesterday's lecture, a "kinematical connection," to allow one to discuss trajectories, velocities, and so forth. The point of central philosophical interest is that such a "connection" is indeed required for the formulation of the principles of mechanics, and that *it cannot*—as Newton quite clearly indicates—*be defined in terms simply of the spatial relations of bodies.* (For Newtonian space-time as I have presented it, this is just the remark that the mapping upon time and the Euclidean metric on each instantaneous slice of space-time do not determine a unique four-dimensional affine structure.) And this point affects the program of Leibniz quite as much as the fanciful cosmology of Descartes. Alongside the instantaneous geometry assumed (at the time) by everyone, and *whether or not* the spatial relations of that geometry can be explicated in terms of more fundamental properties and relations of bodies as Liebniz wished, it is necessary for mechanics to assume that further structure that has appeared in our discussion as the four-dimensional affine structure of space-time: this is the "immobile being" that Newton says is required. Huygens came closer than Newton did to understanding the mathematical structure of this being; but Newton perceived clearly its independent nature, in the conceptual framework of the mechanics which they shared, and Huygens did not. As for Leibniz, careful examination shows that his attempt to carry out within mechanics his program for the philosophical foundation of the theory of space and time miscarried disastrously—just because he never faced the issue posed by Newton in the passage above. For like Descartes, Leibniz postulated the primacy in nature of uniform rectilinear motion—even while maintaining a relativistic view of motion and (more radically) a relational theory of space; Leibniz indeed held uniform rectilinear motion to be "the natural motion" in so fundamental a sense that, according to him, any deviation from such motion in any other circumstance than the collision of bodies would constitute a miracle.

It is often claimed that the general theory of relativity has demonstrated the correctness of Leibniz's view. This is a drastic oversimplification. It is no more true in the general theory than in Newtonian dynamics that the geometry of space-time is determined by *relations among bodies.* If the general theory does in a sense conform better to Leibniz's views than classical mechanics does, this is not because it relegates "space" to the ideal status ascribed to it by Leibniz, but rather because the space—or rather the space-time structure—that Newton requires to be real, appears in the general theory with attributes that might allow Leibniz to accept it as real. The general theory does not deny the *existence* of something that corresponds to Newton's "immobile being"; but it denies the rigid *immobility* of this "being," and represents it as interacting with the other constituents of physical reality.

It is perhaps a natural tendency for commentators on intellectual history to emphasize oppositions. It is an unfortunate consequence of this tendency that issues which, in their own time, were vital and charged with the potentiality of honest interaction, come to appear as rigid and ritualistic as the duello. In the correspondence of Huygens and Leibniz, for example, on the question of absolute or relative motion, one finds the following things:—The correspondence is initiated by a critical remark of Huygens about a paragraph in Leibniz's notes on Descartes's *Principia*: Huygens interprets Leibniz as *objecting* to the view that motion is merely relative; and says that for his own part he holds just such a view, "undeterred by the reasoning and experiments of Newton in his Principles of Philosophy, which I know to be in error—and I am eager to see whether he will not retract this in the new edition of the book. . . . Descartes did not sufficiently understand the matter."—Leibniz in reply affirms his allegiance to a similar view, and says: "Mr. Newton acknowledges the equivalence of hypotheses"—i.e., the principle of relativity—"in the case of rectilinear motions, but in regard to Circular, he believes that the efforts the circulating bodies make to recede from the center or axis of rotation reveals their absolute motion. But I have reasons that make me believe nothing breaks the general law of Equivalence. It seems to me, however, that you yourself, Sir, were once of Mr. Newton's opinion in regard to circular motion."—And Huygens answers: "As for absolute and relative motion, I am amazed at your memory—namely your recalling that at one time I was of Mr. Newton's opinion in regard to circular motion. Which is so, and it is only 2 or 3 years since that I discovered what is truer."—Leibniz's closing reponse is: "When I said to you one day in Paris that it is hard to know the true subject of Motion, you answered that this was possible by means of circular motion, which gave me pause; and I remembered it when I read almost the same thing in the book of Mons. Newton. . . ."—Now, a feature of this correspondence that I find very striking is the circumstance that, in spite of the strength and the importance of their differences with Newton,—which are certainly not to be depreciated,—these two philosophical antagonists are very far from finding Newton's doctrine bizarre; so much so that one of them acknowledges having held the same view as Newton, independently, for a major part of his scientific career, and the other says that when he learned this view from the first it "gave him pause." To me, this circumstance appears quite irreconcilable with, for instance, the following statement of Reichenbach's, in reference to this very correspondence:

> [Leibniz and Huygens] feel themselves repelled by the spiritualistic way of thinking, derived from Henry More, of this physicist who, whenever he left the domain of his narrower specialty, became a mystic and a dogmatist.

And Reichenbach continues:

> They both know that the crude reification of space that Newton shares with the epistemologically unschooled mind in its naive craving for realism cannot possibly be right. . . . Huygens even expresses the conjecture that Newton will

> correct his views on the problem of motion in the new edition of the *Principia,* so sure does he feel in his rejection of the Newtonian absolutism.

There is no doubt that Huygens felt sure of his ground; but is not his expectation rather a testimony of his admiration for Newton's genius than of his contempt for Newton's "dogmatic mysticism"? And is not the basis of his expectation the fact that he sees Newton on the same road that he himself had traveled?—And note the tendency, from Newton's adjective "absolute," which he applies in its logical sense to concepts that are not "relative" to something,[9] to derive the epithet "absolutist" for Newton himself.[10] What this seems to me to show is just how dulling it is to view a struggle with real problems through the smoky air and accumulated dust of later sterile controversies.

One particular source of misjudgment about the philosophical issues of the seventeenth century, I think, is a confusion about the very word "philosophy." For instance, Newton's criticism of Descartes is that Descartes's several formulations of the fundamental meaning of the word "motion" are inconsistent, and that none of them can satisfy what for Newton is the crucial test of a *philosophical* conception of motion: namely that it make possible an adequate expression of the principles of dynamics. Descartes's own dynamics is in effect, according to Newton,—and in verifiable fact: the *Principia* of Descartes is a confused, but by no means an obscure book,—based on quite different ideas of place and motion than his "official" ones. We have seen Newton refer these ideas to what Descartes calls "generic" extension, and we have seen him appeal to the followers of Descartes to recognize the identity of this Cartesian generic extension with the space of Newton's own doctrine. His appeal concludes with the statement I quoted a few minutes ago: "And . . . the gyrations of the vortices, from which he deduced . . . the whole of his mechanical Philosophy, are tacitly referred to that generic extension."—Now E. A. Burtt says:

> When we come to Newton's remarks on space and time . . . he takes personal leave of his empiricism, and a position partly adopted from others, partly felt to be demanded by his mathematical method, and partly resting on a theological basis, is presented, and that in the main body of his chief work. Newton himself asserts that "in philosophical disquisitions," which apparently means here when offering ultimate characterizations of space, time, and motion, "we ought to abstract from our senses, and consider things themselves, distinct from what are only sensible measures of them." This is surely a peculiar observation from a philosopher of sensible experience.

I hope it is enough to place this quotation alongside the previous discussion, to make plain its sad inadequacy. But what seems to me almost unbelievably obtuse is Burtt's interpretation of what Newton really meant by "philosophical disquisitions." Apparently he forgets the very title of that "disquisition," Newton's "chief work," of which the scholium with its characterizations of space and so forth is so small a part; namely, *Mathematical Principles of Natural Philosophy*. Burtt's work deserves respect, as a pioneering study of the philosophy of seventeenth-century science. Nevertheless, taking the words of a man like Newton to mean "apparently" what one thinks they might be likely to mean,—without paying very careful attention to the evidence that indicates what they did in fact mean,—and on such a basis finding in his utterances all sorts of inconsistency, inadequacy, and unintelligibility, ought to be regarded as shocking; in fact it is shockingly common. Burtt's summary judgment is that: "In scientific discovery and formulation Newton was a marvellous genius; as a philosopher he was uncritical, sketchy, inconsistent, even second-rate." I do not think this judgment is of any value.

The words that Burtt quotes are neither peculiar nor obscure, unless they are read, anachronistically, with connotations they acquired in later metaphysical discussion. For example, Newton doesn't mean by "things themselves" what Kant means by "Dinge an sich": he means what Kant would call "Gegenstände der Erfahrung," objects of experience; and the statement that "we ought to abstract from our senses, and consider things themselves, distinct from what are only sensible measures of them," has nothing to do with a departure from empiricism in any sense in which "empiricism" applies to Newton at all. "Geometry," Newton tells us, "is founded in mechanical practice," and is indeed a part of mechanics; but geometry is also founded upon the use of terms in a precisely controlled way—a way that, while learned from experience, does certainly "abstract from our senses." Newton would never have agreed with Hume that a finite volume contains a finite number of points (because there is a minimal quantum of what can be perceived).

The general situation, with respect to the question of the empirical content of the kinematical notions that Newton calls "absolute, true, and mathematical," appears to me to be this:—These notions are part of Newton's *theoretical apparatus*. We have come to know that the critique of the empirical content of theoretical notions is indeed of great importance in science; but that it cannot be managed by simple translation of theoretical terms into some kind of "observation language." In this respect Newton's use of the "absolute" kinematical notions should be regarded as of the same class with his use of such theoretical notions as "force" or "attraction" or "gravitation." His philosophical investigation of the system of the world led Newton to conclude that things themselves all mutually attract one another, although this attraction amongst the objects around us is quite inaccessible to our ordinary sensible measures. The same investigation led to the conclusion that the sun bobs about and the earth turns. Is the conclusion that all things attract of a different kind from the conclusion that the earth turns? Improved technique did of course allow the former conclusion to be tested, and a hundred years after the *Principia* Cavendish measured gravitational forces in the laboratory. But in the scholium Newton discusses this precise question about the second conclusion: how "absolute" motion can be determined empirically; and one might suggest that Foucault's pendulum experiment, for instance, was as striking a confirmation of Newton's theory of rotation as Cavendish's torsion-balance experiment was of his theory of gravitation. Of course, both were confirmations of theories for which strong confirmation did already exist: for gravitation, the planetary perturbations and the precession of the equinoxes; for rotation, the earth's equatorial bulge—and, one perhaps ought to add (although it was not to my knowledge adduced at the time), the direction of propagation of the weather, which is a sensible phenomenon of impeccable qualifications.

Are the experiments of Cavendish and of Foucault genuinely parallel cases? Against this it may be urged that the evidence of Cavendish is rather direct: the experiment detects, by the observed displacement of the bar of the balance—which puts the wire into torsion—the existence of a torque upon the wire when the test masses are brought close together. On the other hand, the Foucault experiment only shows a kind of geographical fact (like the motion of the weather): the plane of oscillation of the pendulum rotates, at a rate that depends upon terrestrial latitude; the experiment does not and cannot show that this precession of the plane of oscillation is due to the earth's state of motion. The difference is real; but it is important to understand in what it consists, and in what it does not consist. In the scholium, Newton suggests how one might go about testing whether an observed force is due to rotational motion: namely, by applying a torque to vary the rotational state, and observing how the force responds—in particular, whether variation in one sense increases the force and variation in the other sense diminises it. In the present case, the torque would have to be applied to the earth, so as to change its state of rotation; the experiment is therefore, unfortunately, technologically unfeasible. On the other hand, the limitation *is* purely technological; and as a matter of fact, a partial substitute for Newton's test will soon be quite feasible, namely a Foucault experiment on the surface of the moon, to verify that the precession there differs from that on the earth to the extent demanded by the difference in rotational velocities (and, of course, the difference in gravities). So we see that an extension of Foucault's experiment would allow us to verify that the force there observed depends upon the state of rotation of the system, just as the experiment of Cavendish shows the force observed in that experiment to depend upon the distance between the two bodies and upon their masses.

Nevertheless a question remains. Suppose the extended Foucault experiment indeed shows that the force varies with the earth's state of rotation. How does this establish that the particular state corresponding to null force is, as Newton asserts, the state of null "absolute" rotation?

Now, this is obviously in large part a question about words and meanings. If one denies—on whatever grounds—that "absolute" space and motion can be well-defined notions at all, then one obviously cannot accept *any* experimental result as confirming that the earth possesses a certain quantity of "absolute" rotation. But it is surely nonsense to deny the *definability* of "absolute" motion, since prior to definition—or to some stipulation of conditions to be satisfied—the mere constitution of a word confers no logical attributes whatever upon it. One can with reason deny only the intelligibility of some proposed definition, or the realizability of some proposed set of conditions.

I make these rather pedantic and trivial remarks, because that part of the philosophical controversy—and especially in recent times—that has tended to fasten upon the adjective "absolute" as a deprecable one, is fairly infected with disregard of these simple considerations. Let me give two elementary examples; a little later I shall discuss a third and more substantial one.

The first example is from H. G. Alexander's introduction to his edition of the Leibniz-Clarke correspondence. After recounting the phenomena and the theoretical considerations on which Newton bases his discussion of absolute space and time, and concluding that "only a frame of reference with respect to which the earth is rotating . . . is an inertial frame," Alexander says:

> One would like to believe that this is all Newton meant when he introduced the concepts of absolute space and time. Such an interpretation is supported by his equating the distinction between absolute and relative space and time, with the distinction between "mathematical and common" space and time. This recalls the distinction made in his preface . . . between geometry and mechanics. There geometry is said to be concerned with ideal straight lines and circles even though "artificers do not work with perfect accuracy." Similarly one might interpret the scholium as saying that space and time are ideal entities which it is helpful to consider in theory, although they may not exist in reality.
>
> Unfortunately it is easy to cite other passages in which Newton treats absolute space and time as real even though perhaps unknowable. . . . [T]hese show that Newton thought he was doing more than just identifying the set of frames of reference with respect to which the laws of dynamics would take the simplest form.

The second example is from a paper by Stephen Toulmin:

> . . . [O]ne need no more assert that Newton was committed by his theory of dynamics to the objective existence of a cosmic substratum called "absolute space" than one need say that a geometer . . . is committed to the quasi-material existence of an invisible network of geometrical entities interpenetrating the world . . . . What matters for Newton's dynamics is that his theory—which includes, of course, the distinction between inertial frames and others—should have a physical application. . . .
>
> I question, therefore, whether the *objective existence* of absolute space was or need have been the central issue for Newton. Its "reality" or applicability, as a concept, is a different matter. . . . It is in this respect—in taking Newton to be concerned not with the objective existence of a hypothetical entity called Absolute Space so much as with the applicability to the world of nature of two kinds of spatial and temporal concepts, which he labels "absolute" and "relative"—that I chiefly contest the received interpretation of the scholium on space, time, and motion.

In both of these passages one observes what I should characterize as a loose and uncontrolled manipulation of verbal distinctions. What exactly do these authors *mean* by "ideal entities which it is helpful to consider in theory," or by a notion or theory that "has a physical application",—as opposed to entities that "exist in reality," or to "the objective existence of a cosmic substratum"? If the distinction between inertial frames of reference and those which are not inertial is a distinction that has a real application to the world; that is, if the structure I described yesterday is in some sense really exhibited by the world of events; and if this structure can legitimately be regarded as an explication of Newton's "absolute space and time"; then the question whether, in addition to characterizing the world in just the indicated sense, this structure of space-time also "really exists," surely *seems* to be supererogatory. It is quite true that the discussion of these questions became involved with issues of logical and metaphysical terminology, and of theology: Is space a substance, an attribute, or something other than these? Is space "ideal" or "real"? Is God in space, or is space an idea of God, or neither? And so on. And a careful study of the philosophy of a man who discusses such questions ought to take account of his discussion of them. But when two philosophers disagree about whether, for instance, space is real or ideal, their disagreement—this is a point one would hope had become commonplace in the era of linguistic analysis—is at least as likely to be based upon different usages of the terms "real" and "ideal" as upon different views about space. Unless one undertakes a critical study of this usage, as neither Alexander nor Toulmin does, comment upon such issues ought to be eschewed; to speak of a "cosmic substratum" or of "the objective existence of a hypothetical entity called Absolute Space," as if such terms had a clear meaning and moreover a clearly objectionable one, is just sophomoric.

It is important to be very clear about one point: the notion of the structure of space-time cannot, in so far as it is truly applicable to the physical world, be regarded as a mere conceptual tool to be used from time to time as convenience dictates. For there is only one physical world; and if it has the postulated structure, that structure is—by hypothesis—there, once for all. If it is not there once for all—and that is what the evidence today indicates overwhelmingly, of the structure of Newtonian space-time—then it is not there at all; although of course it may still be (as the evidence also indicates, overwhelmingly) that a structure is there that approximates, in some sense, to the postulated one. On this point—the "reality" of space and time as an objective framework of the phenomenal universe (although as regards their mode of being he regards them as "ideal" things)—Leibniz can be quite as forceful as Newton:

> . . . [S]pace and time taken together constitute the order of possibilities of the

one entire universe, so that these orders—space and time, that is—relate not only to what actually is but also to anything that could be put in its place, just as numbers are indifferent to the things which can be enumerated.

Newton, on the other hand, is by no means so far as one might be led to suppose from Leibniz's view that the essence of space and time is in some sense relational. Leibniz's phrase, that space is "an order of situations," which occurs in his correspondence with Clarke, and which Clarke finds absurd, actually appears verbatim in Newton's scholium. And the following remarkable passage occurs in the essay of Newton's from which I have quoted before:

> . . . [J]ust as the parts of duration have their individuality from their order, so that (for instance) if yesterday could change places with today and become the later of the two it would lose its individuality and be no longer yesterday but today: So the parts of space have their individuality from their positions, so that if any two could exchange their positions, they would thereby exchange their individualities, and each would be converted numerically into the other. By their mutual order and positions alone are the parts of duration and space understood to be just what they are in fact; nor have they any other principle of individuation besides that order and those positions, which therefore they cannot change.

We clearly do not have here a "crude reification of space"—or an "epistemologically unschooled mind in its naive craving for realism." The idea formulated by Leibniz as the principle of the identity of indiscernibles is obviously a familiar one to Newton; and he bases upon it a view of the standing towards one another of the parts of space that is strikingly similar to Leibniz's. Not identical, however: the relations that constitute space and give its parts their individuality are according to Newton *internal* relations; that is to say, he is content to postulate the entire structure of space, without attempting to derive it from or ground it in the relations of non-spatial entities.

But I want to come back to the Foucault experiment—or for that matter the water-bucket experiment—and to the question whether such phenomena do provide an adequate criterion for applying the notion of "absolute" rotation.

One must beware—as I hope I have already implied—of the tyranny of words. How we use them is certainly not indifferent, and to assume that it is conduces to loose thinking—as in the case of Descartes, who slides the words "motion," "motion in the common sense," and "motion in the proper sense," about, to a degree that vitiates his dynamics. But how we use them is also not imposed upon us by some fixed standard, and much unclarifying dispute over theoretical doctrines has been occasioned by one party's assumption that some word used by the other party *must* have a certain meaning, which is not the meaning the second party intends. Words face in two directions, towards men and towards things; they are instruments of communication and of thought, and must be used in a way that respects both the receptivity of the audience and the structure of the things they refer to. When from a consideration of the nature of things we are led to propose a change or refinement in the use of words, the way to make the change is never uniquely determined by the subject. To judge such a proposal fairly one must attempt to divorce prejudice, to examine what the *proposed* meanings are and whether they have been (or can be) made clear, and to examine propositions formulated in the new terms in order to see whether they are true and instructive statements about the things they refer to. Otherwise criticism risks the shallowness of those who maintain that a poem must rhyme.

The proper question about Newton's doctrine, therefore, is not whether space, time, and motion really are as he takes them to be, but whether his definitions make sense and whether the things he says are correct statements about the things he means. The water-bucket experiment and Foucault's pendulum make *something* visible. In Newton's terms this something is called "absolute rotation." It is when this is correctly understood that Foucault's experiment can be seen as fully analogous to Cavendish's. The latter is not designed to show—nor can it be shown, for the proposition makes no sense—that the force detected in it should be called "gravitation"; and it is not designed to determine the cause of that force. What it shows is that a force occurs just as predicted by Newton's theory, in which that force is called "gravitation" and is said to be associated universally with the masses and distances of bodies. Just so Foucault's experiment exhibits the occurrence of a force predicted by Newton's theory, in which that force is said to be associated universally with what in the theory is called the "absolute rotational velocity" of a body.

Reichenbach makes a very interesting mistake about this (and this is the third example I promised earlier). He asks us to imagine, reflecting upon a famous comment of Mach's, that the universe is in a sense given twice: that there are two world-systems, each with its "earth" and its "star-sphere," vastly far apart but still accessible to observation; and that each earth is at rest with respect to the other star-sphere but rotates with respect to its own. According to Mach, he says, if centrifugal forces appear on the one earth they ought to appear on the other as well. Suppose that it proves otherwise, and more particularly that centrifugal forces are observed on the earth E_1, whose fixed-star heaven is F_1, but not on the earth E_2, whose star-sphere is F_2. Reichenbach says that according to Newton these observations would "establish absolute space," and would compel us to recognize that E_1 and F_2 are in rotation while F_1 and E_2 are at rest. But Reichenbach objects that the opposite interpretation—E_1 and F_2 at rest, F_1 and E_2 in rotation—can still be maintained. With this interpretation, he says, we introduce the general principle: if the fixed-star shell F rotates relative to absolute space, it produces a gravitational field on its earth E; so E_1 but not E_2 is subject to this gravitational attraction, which is the same as what on Newton's interpretation is called "centrifugal force." Both interpretations distinguish between states of motion which are indistinguishable from the point of view of purely relative kinematics,—since the systems (E_1, F_1) and (E_2 F_2) are indistinguishable from this point of view, and yet by hypothesis show different phenomena,—but they make the distinction differently. And Reichenbach concludes that if the observations agree, as supposed, with Newton's prediction against Mach's, then "*there exists absolute space, but its state of motion cannot be determined.*"

There are actually two mistakes in this. The first is that Reichenbach's alternative interpretation is just not tenable in the form he proposes. We have only to suppose a third system (E_3, F_3), in which the earth and the stars are at rest with respect to one another and to F_1 and E_2. According to Newton, E_3 and F_3 are then at rest, and there should be no centrifugal force. But according to Reichenbach's alternative, since F_3 (like F_1) rotates, there should be a gravitational attraction upon E_3. If we assume that the facts support Newton, Reichenbach's alternative would have to be modified in a rather complicated way. The general situation, so far as this point is concerned, is the following:—*However* one formulates the principles of Newtonian dynamics, these principles do in fact allow one to single out a *unique* class of "inertial" or "Galilean" reference systems, and thereby a *unique* rotational state, which is the state that Newton calls *absolute rotational rest.* If one chooses to formulate the dynamics in terms of some other choice of a state of rotational rest, the formulation is possible although a little involved; but what is of greatest systematic interest is the fact that one could *deduce* the arbitrary character of the choice, and one could still single out that unique rotational state (whether or not it be called "absolute rest") that Newton points to. This first mistake, then, concerns the form and substance of the detailed theory. The second is the mistake in critical procedure that I have just been arguing against. To make the point most sharply, I ask you to suppose that the structure of Newtonian dynamics were a little different from what it is, namely that in some way it singled out *two different* unique states of rotation. (By "different unique states" I mean not just that the pair of states is distinguished from all other states, but also that each of the two states is uniquely characterized as distinct from the other.) Suppose that Newton had chosen, for whatever reasons,—esthetic, theological, economic, psychiatric,—to call one of these two states, explicitly identified, "the state of absolute rotational rest." Then Reichenbach's statement, "There exists absolute space, but its state of motion cannot be determined," could be sharpened to mean that we can actually determine two definite states, but cannot tell which of them is the state of rest. And this would be wrong. For Newton would (we assume) have told us what *he means* by rest—how *he intends* to use the word—and this meaning on our supposition does single out a definite state. The only way to interpret Reichenbach's assertion as correct would be by taking it to mean that there is also another way in which the term "absolute rest" might have been used, in this suppositious theory. But this, while in the case contemplated an interesting remark—and one that might induce us to modify the terminology—could not be construed as saying that any part of the Newtonian formulation is wrong. If, on the other hand, the Newtonian theory singled out a pair of states, but afforded no intrinsic way to distinguish between them, then the statement that "there exists absolute space" would be of dubious content, but

the statement that "its state of motion—if it exists—cannot be determined" would be true, not just in the sense that *there is nothing that forces us to use a word in one way rather than another,* but in the sense that *there is no way to state unambiguously in terms of the theory how the word is to be used.*—The actual case of Newtonian dynamics corresponds to neither of these alternatives, when we consider only the concept of rotation: there is singled out within the theory one and only one unique rotational state, and while we are free to call this the state of absolute rotational rest or to refrain from using that term, if we do choose to use it at all, and to base our use upon the theory, there is really no other way. It is when we consider translation as well as rotation that we are led, as Huygens was, to deny not just the *determinability* but the *existence* of absolute space; for what Newtonian dynamics singles out is a *class* of states of motion, within which it allows no intrinsic distinctions to be made; but this class has the peculiarity that all its members have *the same rotational character.*

I should, then, summarize Newton's general position roughly as follows:—In philosophical disquisitions, one ought to rely neither upon immediate appearances, nor upon the common usage of words that play a crucial role; for common usage is not always adequate to the formulation of requisite principles. Now the science of dynamics has to deal with the phenomena of motion, and requires precise notions in order to build mathematical demonstrations. And the principles of dynamics, already discovered by earlier investigators and applied successfully to many phenomena, distinctly require a view of motion and therefore of place and space that cannot be explicated in terms simply of the geometrical relations among bodies. Therefore the only philosophical procedure is to adopt that conception of space and motion on which alone dynamics can be based—which implies, in particular, that (as in Newton's investigation of the solar system) considerations of *force* as well as of change of relative position must be brought to bear in order to determine the true state of motion or rest of bodies.

Viewed so, Newton's analysis of the notions of space and time is, but for the one shortcoming I have mentioned, a classic case of the analysis of the empirical content of a set of theoretical notions.

I should like to add a comment on the connection of Newton's views of space and time with his theology. Burtt emphasized this connection, and pointed out the affinity of Newton's theological ideas with those of Henry More (an affinity already noted by Leibniz). The observation is certainly both valid and interesting. But the corollary that one often finds asserted (for instance by Burtt in the passage I have quoted before), that Newton's doctrine of space, time, and motion, is based in part on his theology, is a *non sequitur*—and for the attempt to understand and evaluate that doctrine, a red herring. There is no serious reason to suppose that Newton, who rejected the doctrine of the Trinity on the basis of a critical analysis of texts, would have adopted a notion of space as the foundation of his mechanics because that notion formed part of the theology of Henry More. Newton's intellectual interests were far ranging; he desired, as Keynes has so vividly told us, to read the riddle of the Universe; but he was not an enthusiast: his attempts upon that riddle were passionate and possibly obsessive, but they were also very careful, accurate, and critical. It is surely more plausible that More's theology was (in part) acceptable to Newton because its conceptions agreed with those required by mechanics (as the conceptions of the philosophy and theology of Descartes did not). That is what Newton himself tells us, in effect; the celebrated passage on God in the General Scholium to Book III of the *Principia* concludes: "And thus much concerning God; to discourse of whom from the appearances of things, does certainly belong to Natural Philosophy." In the scholium on space and time, on the other hand, he tells us that we ought not to base our philosophical conceptions upon the authority of sacred texts, since these speak the language of ordinary discourse, not of philosophy.

NOTES

* Two lectures, based upon a longer paper written for the Newton conference at the University of Texas (November 10–12, 1966). A shorter talk based upon that paper was delivered at the conference; the present version was presented at The Rockefeller University, February 2 and 3, 1967.

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[1] This represents a fundamental point of difference from the space-time of special relativity, in which there is an asymmetric order-relation "earlier than" for which the ordering is *not* total: the relation "neither is earlier" is not an equivalence, and therefore the definition of time as a quotient-set of W fails.

[2] Straight lines that lie in an instantaneous space can be so regarded only in an extended sense, as representing "infinitely fast motions."

[3] Huygens, for instance, wrote: "I am astonished that Mr. Newton has taken the trouble to construct, upon such an improbable and audacious hypothesis, so many Theorems and as it were a complete theory of the actions of the heavenly bodies. I mean his hypothesis that all the little particles of diverse bodies attract one another, in reciprocal squared ratio of the distances." That this remark, despite Huygens's own rejection of the improbable and audacious hypothesis," is not ironical but expresses honest admiration, is clear from the sequel: "He may have been led to his theory . . . by the book . . . by Borelli . . .; but he was unable to penetrate the true foundations as did Newton—who had the advantage of knowing the measure of centrifugal force by the Theorems I have given."

[4] It is somewhat ironic that just after stating his dissent from "the mutual attraction of the whole bodies," in his *Discours de la Cause de la Pesanteur,* Huygens expresses his acceptance and his admiration of Newton's theory that the astronomical force is gravitation, and remarks —somewhat wistfully, one feels—that while he had himself long supposed the underlying cause of weight to act near the sun as well as near the earth: "I had not at all extended the action of weight to such great distances, as from the Sun to the Planets, nor from the Earth to the Moon; because the Vortices of M. Des Cartes, which formerly had seemed very probable to me and which I had still in mind, stood in the way." Thus he shows regret at having failed to explore the possibility Newton did explore so successfully, for the sake of a theory he formerly inclined towards but now rejects; and does so just when he has dissented, under the influence of his new theory, from the further conclusions that Newton was led to.

[5] This is precisely the content of the fourth Rule of Philosophizing (added in the second edition of the *Principia*): "In experimental philosophy, propositions collected by induction from phenomena ought to be held for true, either accurately or very nearly, notwithstanding contrary hypotheses, until other phenomena occur, by which they are rendered more accurate or subject to exceptions.—This is to be done that the argument of induction be not destroyed by hypotheses." An equally forceful statement of this rule occurs in the *Opticks*: ". . . Analysis consists in making Experiments and Observations, and in drawing general Conclusions from them by Induction, and admitting of no Objections against the Conclusions, but such as are taken from Experiments, or other certain Truths. For Hypotheses are not to be regarded in experimental Philosophy. And 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 upon as so much the stronger, by how much the Induction is more general. And if no Exception occur from Phaenomena, 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."

[6] It seems that this irrelevance of the ether in the net gravitational interaction, on Newton's theory, contributed (alongside the argument he ordinarily adduces of the extreme rarity of the interplanetary medium) to Newton's doubt whether an ether causes gravity.—On the subject of this doubt, concerning which a number of comments were made at the Texas Newton conference, I have one small remark to add. Consider the following statement made by Fatio de Duillier, in a letter of 30 March 1694:

> Mr. Newton is still undecided between these two opinions. The 1st, that the cause of Weight is inherent in matter by an immediate Law of the Creator of the Universe: & the other, that Weight is produced by the Mechanical cause that I have discovered. . . .

While Fatio is not a reliable witness to the degree of favor shown by others towards his own hypothesis, the tolerance towards an ether theory of gravitation that he attributes to Newton is consistent with other statements of Newon's public attitude at the time. What I chiefly wish to suggest, however, is that although Newton clearly was then leaning away from an ether theory of gravitation, none of the statements of this leaning indicates a rejection of ethers altogether in physics. One should distinguish skepticism about the ether as a cause of gravitation from skepticism about the existence of hidden elastic fluid media affecting *some* physical interactions. I am not convinced that Newton ever showed serious doubt on the latter point. (Note the statement in Corollary 2 to Proposition VI, Book III—in all editions of the *Principia*—that the particles of an ether too must have weight. This obviously places in an awkward position and theory of weight as the *effect* of an ether; but on the question of the existence of an ether of some kind, it is at least neutral.)

[7] The lines of the astronomical argument are somewhat obscured by the fact that it is the Third Law, rather than the induction already made, that Newton invokes in Corollary 1 of Proposition V to establish the mutuality of the astronomical forces. This use of the more vulnerable of the two available arguments may have contributed to Huygens's dissent on the point.

[8] I am indebted to Professor I. B. Cohen for the remark that Cajori's transgression was not executed single-handed. In revising Motte's translation, Cajori made use of that of Robert Thorp. The latter rendered the clause "vim inferunt sacris literis, qui voces hasce de quantitatibus mensuratis ibi interpretantur" (third edition; first identical, save that there is no comma and "Sacris" appears for "sacris") as "those violate the accuracy of language, which ought to be kept sacred, who interpret these words for the measured quantities"; Cajori merely changed "sacred" to "precise."—This is an edifying history of the corruption of a text; its moral is *Caveat lector!* It is really hard to understand how Cajori, confronting Newton's Latin text and the versions of Motte and Thorp, made his choice.

[9] For instance, what Newton calls "absolute quantity" of the earth's gravity is a property of the earth itself, which we call the earth's "gravitational mass"; in contrast with the "ac-

celerative quantity" of the earth's gravity, which varies from place to place and is a property of a point in a given position relative to the earth—the intensity of the earth's gravitational field at that point; and in contrast with the "motive quantity" of the earth's gravity, which depends both upon the place and the body acted on—the weight, or attraction exerted upon the body by the earth.

[10] Reichenbach refers to Newton as "the great dogmatist of absolutism"; Mach (whose treatment of Newton is quite sympathetic) mentions "his metaphysical liking for the absolute"; and Hermann Weyl—who usually writes pure gold—calls him "Newton, the absolutist."

DUDLEY SHAPERE

The Philosophical Significance of Newton's Science

IN A FAMOUS PASSAGE IN THE PREFACE TO THE FIRST EDITION OF HIS *Principia,* Newton declared that:

> 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 other phenomena. . . . I wish we could derive the rest of the phenomena of Nature [besides those dealt with in this work] by the same kind of reasoning from mechanical principles, for I am induced by many reasons to suspect that they may all depend upon certain forces by which the particles of bodies, by some causes hitherto unknown, are either mutually impelled towards one another, and cohere in regular figures, or are repelled and recede from one another.[1]

Newton's statement is characteristically cautious: the "burden of philosophy" is not described categorically; it only "*seems* to be . . . ." Again, he only *suspects,* on the basis of "many reasons," that his view is correct—phenomena "*may* all depend upon certain forces," etc. And finally, at the end of the paragraph from which the quoted passage is taken, he notes the possibility that there may be "some truer method of philosophy." In spite of these qualifications, however, and in spite of some more specific uneasiness which, as we shall see later, Newton himself (to say nothing of his contemporaries) felt about the adequacy of this statement as a description of the whole, ultimate burden of philosophy—despite all this, the passage is important for understanding the logic behind the greater part of Newton's own scientific (or "philosophical") reasoning, as well as the problems and approaches of a whole tradition of succeeding thinkers. For in many ways, this passage defines those problems and approaches—that tradition. Besides prefacing a monumental example of "philosophy," reaching specific conclusions, in terms of the motions and forces of particles, about a vast body of "phenomena of Nature," and thus providing "many reasons" for suspecting that the approach may prove successful in other domains, this statement of "the whole burden of philosophy" lays down a program for further work. It establishes at once an ideal or goal of scientific investigation—a picture of what a completed science would look like—and a set of categories in terms of which the attempt to reach that goal should be made. It provides, that is, a statement of the terms in which proper possible ultimate explanations are to be formulated. The phenomena of nature—all of them—are to be approached and explained in terms of "forces" directed radially toward or away from "particles" which (to add a gloss to