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Some Philosophical Prehistory of General Relativity

As history, my remarks will form rather a medley. If they can claim any sort of unity (apart from a general concern with space and time), it derives from two or three philosophical motifs: the notion—metaphysical, if you will—of *structure in the world*, or *vera causa*;[1] the epistemological principle of the *primacy of experience*, as touchstone of both the content and the admissibility of knowledge claims; and a somewhat delicate issue of scientific method that arises from the confrontation of that notion and that principle. The historical figures to be touched on are Leibniz, Huygens, Newton; glancingly, Kant; Mach, Helmholtz, and Riemann. The heroes of my story are Newton and Riemann, who seem to me to have expressed (although laconically) the clearest and the deepest views of the matters concerned. The story has no villains; but certain attributions often made to the credit of Leibniz and of Mach will come under criticism.

I

It is well known that Leibniz denied, in some sense, to space the status of a *vera causa*. In what precise sense he intended the denial is perhaps less well known; indeed, as I shall soon explain, I myself consider that sense in some respects difficult if not impossible to determine. The fact that Leibniz characterizes space as not "real" but "ideal," or as an "entity of reason" or abstraction, by itself decides nothing; for he also tells us that the structure thus abstracted is the structure—or, as he puts it, the "order"—of the situations of coexistent things; furthermore, of the situations of all actual *or possible* coexistent things; or, again (in the fourth letter to Clarke), that space "does not depend upon such or such a situation of bodies; but it is that order, which renders bodies capable of being

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situated, and by which they have a situation among themselves when they exist together." It is abundantly clear from this and many other explicit statements, as well as from his scientific practice, that Leibniz regarded the attribution to bodies at an instant of ordinary geometrical relations—distances, angles, etc.—as having objective significance; that he held these relations to be subject to all the principles of Euclidean geometry; that he regarded geometrical distinctions (i.e., nonsimilar arrangements) as in principle discernible; that he considered these distinctions legitimate to invoke in laws of nature; and thus that the Euclidean spatial structure of the world at an instant was, for him, in the only sense I am concerned with, a *vera causa*.[2]

It is, then—of course—the *connection through time*, the problem of *motion*, that is seriously at issue. But on this issue certain of Leibniz's statements seem to face in several different directions. Let us consider the one that seems most radical. In his treatise on dynamics (*Dynamica de Potentia et Legibus Naturae corporeae*, Part II, Sec. III, Prop. 19) Leibniz states *as a theorem* what appears to be a general principle of relativity. He employs the phrase "equivalence of hypotheses"—evidently derived (and generalized) from the usage in astronomy, where the Ptolemaic, Copernican, and Tychonian "hypotheses" were in question: so "hypothesis" means "hypothesis about motion," or more precisely "choice of a reference body to be regarded as at rest." The proposition asserts the dynamical equivalence of hypotheses for any closed system of interacting bodies, "not only in rectilinear motion (as we have already shown), but universally." Unfortunately, this last expression is obscure: does "not only in rectilinear motion" refer to the reference body—which would give us our "general relativity"; or does it refer to the *interacting* bodies, and generalize from what Leibniz had "already shown," namely for *impacts*, to arbitrary interactions, allowing also *curvilinear trajectories*? The evidence offered by this and related texts seems to me to weigh almost equally for each alternative. A thorough discussion would hardly be appropriate here; but let me sketch some of the salient points.

First, one naturally wants to know how this remarkable theorem is proved. There are in fact two proofs, both very short. The first cites two previous propositions: the analogous result, already mentioned, "for rectilinear motions"; and a proposition stating that *all motions are composed of rectilinear uniform ones*. From these our proposition is said by Leibniz to follow. Now, the exact meaning of the second premise is not immediately clear—not even from its own proof; but it *is* quite plain that, *whatever* it means, it cannot justify an inference from Galilean to "general" relativity. A possible clue comes from a number of passages—in the *Dynamica* and in other writings—in which Leibniz asserts the nonexistence in the world of any true "solidity" or "cohesion," or anything in the nature of a real "cord" or "string." For he says, in several of these places, that if there were any such bond *it would not be true that all motions are composed of uniform rectilinear ones*: rather, there would be real *circular* motions. On the other hand, he says, in actuality all cohesion results from the "crowding in" of particles by an ambient medium; and when a body rotates, its particles not only strive to go off on the tangent, but *actually begin* to go off, and are then turned aside by the medium. Whether Leibniz means this "begin" in the sense of a minute finite segment, or of some kind of infinitesimal, seems uncertain; but in any case it is plausible to conclude that, in saying that all motions are composed of uniform rectilinear ones, Leibniz essentially means that *all interactions are by impact*; and this at least would justify the inference from impact to interaction in general.

Leibniz's second proof makes no appeal to any previous dynamical result at all. It rests upon the claim that a motion by its nature consists in nothing but a change in the geometric relations of bodies; hence "hypotheses" describing the same antecedent relative motions of the bodies are indistinguishable, and their results must be likewise indistinguishable. This does certainly look like an argument for a general principle of relativity. Of course, the argument is philosophical, not dynamical: that is, if one accepts it, one accepts a *constraint* or *condition of acceptability* for any proposed system of dynamics. Does Leibniz's own dynamics satisfy this constraint? Of course it does not! It is in fact very hard to conceive what the structure of a dynamical theory satisfying this constraint could be like. Let me try to be precise about the difficulty.

We may start from space-time—its structure and its automorphisms. Clearly, for Leibniz, *simultaneity* ("coexistence") has objective meaning; so, we may assume, does *ratio of time-intervals*; and I have already remarked that the Euclidean structure on space at each instant is to be presupposed. Leibniz's claim about the purely relative nature of motion seems to imply that this is all the objective structure there is; although I think we may concede (without now questioning its grounds) the *differentiable* structure of space-time as well. Now consider any smooth map of space-time onto itself that preserves simultaneity and ratio of time-intervals, and that restricts, on each instantaneous space, to a Euclidean automorphism.[3] Such a map is an automorphism of the entire Leibnizian structure, and should therefore be a symmetry of the dynamics: i.e., should carry dynamically possible systems of world-lines to dynamically possible systems of world-lines. This immediately leads to the crucial difficulty: take any time-interval $[t_1, t_2]$, and any time t outside this interval; then there exists a mapping of the sort described that is the identity map within $[t_1, t_2]$ but not at t. It follows that the dynamics cannot be such as to *determine systems of world-lines on the basis of initial data* (for the automorphism just characterized preserves all data during a whole time-interval—which may even be supposed to be infinite in one direction, say to include "the whole past"—but changes world-lines outside that interval). It must not be thought that this argument demonstrates the impossibility of a deterministic Leibnizian dynamics; the situation is, rather, that all of the systems of world-lines that arise from one another by automorphisms have to be regarded as objectively equivalent (i.e., as representing what is physically one and the same actual history). But the argument does show that a Leibnizian dynamics cannot take the form of a system of "differential equations of motion," for such equations precisely do determine the world-lines from initial data. Or to put what is essentially the same point in a more sophisticated way: in Leibnizian space-time the "phase," or instantaneous state of motion, of a system of particles cannot be represented by an assignment of 4-vectors to the world-points of the particles at that instant. In short, the basic conceptual apparatus for a cogent formulation of a dynamics satisfying this version of "Leibnizian relativity" would have to be significantly different from the structural framework we are used to. So far as I know, the appropriate concepts have never been defined. The principles of his dynamics, as Leibniz actually formulates them, uncritically employ the standard kinematical notions; Leibniz not only states that a moving body tends to continue its motion *uniformly in a straight line*, but emphatically declares that for a body free from contact with others to move continually in a circle would be a miracle, because contrary to the nature of body. It follows that *in his scientific practice* Leibniz treats the distinction between uniform rectilinear motions and all other motions as a *vera causa*, and so presupposes—in conflict with at least some of his statements of principle, and with the usual interpretation of his views—that there is in the world an objective

structure that supports this distinction.[4] (According to Reichenbach, Leibniz regarded the inertial structure of the world as the result of an interaction of bodies with the ether, and he cites as evidence Leibniz's discussion of rotation in the *Dynamica*. That suggestion is really not coherent: the ether, for Leibniz, acts dynamically; hence its actions themselves involve the inertial structure. And of the passage in the *Dynamica*, although it is very obscure and would call for a much fuller discussion than I can give here, one thing at least is plain: the effect it attributes to the ether is not any *inertial* behavior of bodies, but their cohesion.)[5]

II

Not only in contrast with Leibniz's obscurity, but by any standard, the dynamical writings of Huygens are models of clarity. In these writings, from quite an early date, relativistic considerations play a major role—most notably in his work on impact, where the principle of Galilean relativity is stated as one of the axioms and employed to far-reaching effect. As to the philosophical question of the nature of motion, we have, for instance, this statement, in August 1669: "According to me rest and motion can only be considered relatively, and the same body that one calls at rest with respect to certain bodies may be said to move with respect to others; and [I say] even [that] there is no more reality of motion in the one than in the other."[6] Again, at about the same time, he wrote that "the motion of a body may be at the same time truly equal and truly accelerated according as one refers its motion to other different bodies."[7] This statement clearly asserts the relativity of acceleration, as well as that of uniform motion.

But one must ask: what did Huygens mean by the relativity of acceleration? In his treatise on centrifugal force, Huygens calculates the tension of a cord by which a body is held at the edge of a rotating disk, by imagining an observer fixed to the disk and holding the cord. He remarks that if the cord were cut the body would move off with uniform velocity on the tangent, and calculates from this the trajectory as it would appear to the observer on the disk: this trajectory, one easily sees, is the *involute* of the circle bounding the disk, described by *uniform unwinding*; and Huygens shows that the involute, so described, leaves the disk at right angles, with a velocity of zero at the initial point and initial acceleration v^2/r (v being the circumferential velocity of the rotating disk, r its radius). He concludes that the tension of the cord that prevents this acceleration must be the same that would, e.g., support the same body on an incline down which it would otherwise slide with the same acceleration. This beautiful argument—which implicitly exhibits, in the later parts of the trajectory of the body, the effect not only of centrifugal but of Coriolis force—may be considered an application of the relativistic idea just quoted: the same body is in the one sense in uniform motion on the tangent, and, at the same time, in the other sense, accelerating from rest to traverse its curved path. But this species of relativistic consideration is not an example of the "equivalence of hypotheses," or invariance of the laws of nature. And Huygens seems to have been quite clear about this; for in one of his notes from nearly the same time as our passage on the relativity of acceleration we find this: "Straight motion is only relative among several bodies, circular motion is another matter and has its κριτηριον which the straight does not have at all."

This last statement expresses a view which, in the joint opinions of Huygens and Leibniz—in their correspondence in 1694—agrees with that of Newton in the *Principia*.[8] (In his *Dynamica*, by the way, Leibniz enigmatically remarks that that view *would be right* if there were any real solidity or real cords in nature.) But Huygens and Leibniz also agree, in that exchange, that the view in question, which Huygens himself had long held, is wrong; and Huygens says that he has only recently—within two or three years—arrived at a more correct understanding of the matter.[9]

What this more correct understanding was cannot be inferred from the correspondence. In particular, Huygens makes no explicit reference there—as Leibniz does—to the "equivalence of hypotheses." But there is a set of late manuscript notes by Huygens on the nature of motion that shed some light on the question. Unfortunately, there is a problem of dates that creates some uncertainty whether we do have here Huygens's final view: that, from the statement to Leibniz, should date from around 1691, whereas the position described in the manuscripts seems to go back three years further, to 1688.[10] On the other hand, Huygens remarks more than once in these fragments that he "had long considered that we have in circular motion a κριτηριον of true motion from centrifugal force," but considers so no longer; and this appears to be just the change of view he avows to Leibniz. In any case, what Huygens says in these several notes amounts to the following: *Only relative, not absolute, motion is real; but there can be relative motion with no change in the relative positions of bodies*. In particular, he says, this is the case in a rigid rotation: all

distances are preserved, and yet, e.g., the opposite ends of a diameter are in relative motion.

At first sight, this seems a very lame and disappointing argument; even a naïve blunder, confusing the proper concept of *relative motion*—as, say, change of coordinates with respect to a system of reference (or, more generally and more fundamentally, as *change of geometrical relations*)—with that of *velocity difference*. And I think this impression is not entirely wrong: insofar, namely, as Huygens believes that his new analysis squares with the epistemological argument for relativity of motion—the argument that the empirical content of the concept of motion can extend no further than to change of observed, hence relative, position.[11] But there is another aspect to Huygens's analysis, which seems to me to show remarkable—and instructive—insight.

In discussing *how circular motion is known*—is recognized in experience—Huygens says there are two ways. First, "by reference to bodies that are round about and relatively at rest among themselves and free"; and he adds that this is the case with the fixed stars—"unless they are fastened to a solid sphere as some used to believe"—so we know from observation of the stars that the earth rotates. Second, even if there are no such surrounding bodies, circular motion can be known by the centrifugal projection of bodies—as, he remarks, the observations of the behavior of pendulum clocks on voyages have revealed that "the earth flings bodies harder near the equator." We see, then, that no purist dogma is here maintained by him, that motion can "mean" only change of observed position; rather, the concept of motion is allowed to claim, as "empirical content," whatever accrues to it by virtue of the application to experience of a theory in which it appears. We also see that Huygens has by no means embraced the equivalence of the Copernican and Tychonian hypotheses, and by no means abandoned centrifugal force as a criterion of circular motion. But then, in what has his view changed at all? Is not the discussion just reviewed of criteria of the earth's rotation exceedingly close to the discussion in Newton's scholium on space, time, place, and motion?

The answer is yes, the parallel between the two discussions is very close. There is only one difference: Newton believes that the circumstances reviewed prove the need for a concept of *absolute place* and *absolute motion*. Huygens says this is not so; he says that although centrifugal force *is* a criterion of circular motion, circular motion itself is *not* a criterion of "absolute" motion, *in the sense of change of absolute place*. Indeed, the concept of absolute motion, or absolute velocity, Huygens dismisses as a "chimera"; but he admits the objective significance of what he calls "relative motion," which amounts to (absolute) difference-of-velocities, even though this may be attended by no change in the relative positions of bodies. This, as we can see very clearly today, is precisely the right conclusion about the structure of the world as it is represented in Newtonian dynamics. In Newtonian space-time, a state of uniform motion is represented by a (nonspatial) *direction* in space-time; and all such directions are equivalent: for any two, there is an automorphism of space-time taking the one to the other; there is no "absolute motion" or "absolute rest." But a pair of such states, or directions, have what is analogous to an *angle* between them, and this can be measured by a (spatial) vector, the "velocity-difference." Of course neither Newton nor Huygens had available the mathematical language to express this as I have just done. It seems to me quite remarkably penetrating of Huygens to have grasped, and expressed in the language available to him (in which it sounds paradoxical), the heart of the matter: velocity a "chimera," velocity-difference real. And I think his insight not only remarkable, but instructive, when one considers how he arrived at it: not by standing upon philosophical dogmas, whether empiricist or metaphysical, about admissible concepts and theories; but by considering, for a theory known to have fruitful application, exactly how its concepts bear upon experience.

III

I said at the outset that Newton was a hero of the story. Clearly Huygens merits consideration: on the issue of the space-time structure of the world, I think he does see farther than Newton. Not that the distance between them is so very great: we know that Huygens had held Newton's view; we have seen how closely his late discussion parallels part of Newton's scholium. In point of fact, that late discussion of Huygens's was directly *stimulated* by Newton's *Principia*. In the latter, too, we find a clear statement of the principle of Galilean relativity for dynamical systems—and from this it follows, if we regard dynamics as the fundamental theory of physical interactions, that neither absolute place nor absolute motion is a *vera causa*. So when Huygens tells Leibniz that he is eager to see whether Newton will not revise his views on motion in a new edition of the *Principia*, we can take this as a reasonable expectation based upon a

perceived affinity of views. But Newton did not revise his opinion, and Huygens on this point stands alone until the threshold of the twentieth century.[12]

I do not, of course, wish to have my use of the word "hero" taken seriously; nonetheless, I do intend a serious point in continuing to sing Newton's special praise as a philosopher—and the point concerns the twin issues of "structure" and "knowledge-source." Newton has often been represented as rather crude in his philosophical views: his epistemology a crude empiricism; his metaphysics dogmatic and crudely realistic (with theological overtones); the epistemology and the metaphysics unresolvedly incompatible with one another. I believe this to be an erroneous representation, and have attempted on previous occasions to offer correctives.[13] What I wish to emphasize at present is that Newton's empiricism was indeed a radical one: he considered *all* knowledge of the world to be grounded in, and in principle corrigible by, experience. Thus he presents the laws of motion as propositions long accepted, and "confirmed by abundance of experiments"; he tells us that geometry itself is "founded in mechanical practice"; and when he offers theological remarks, he claims for them no *a priori* metaphysical status, and no sort whatever of justificatory force to ground propositions of natural science—quite the contrary, he concludes one set of such remarks with the words, "And thus much concerning God; to discourse of whom *from the appearances of things*, does certainly belong to Natural Philosophy."

What has made this radical empiricism appear crude and shallow is, I think, Newton's failure to discuss in detail the methods by which claims to knowledge are to be empirically grounded and tested, or the difficulties that are faced by an empirical "justification" of knowledge. Yet he does take up both these issues, in several brief but pregnant passages. On the first, for instance, the "Rules of Philosophizing" in Book III of the *Principia*, although they are certainly not definitively satisfying, do constitute a substantial statement; and they have the inestimable virtue of being immediately put into detailed application, in an argument upon which they provide a commentary, and which provides a substantial exemplification—and thus data for the explication—of them in turn. On the second issue—the *difficulties* of empirical justification—we have such a (terse) statement as this in the *Opticks*: "[A]lthough 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. . . ." This antedates Hume. But Hume's critique of induction is in effect an explication of Newton's first clause; and all attacks upon Hume's problem are in effect attempts to explicate the second. The statement itself, once again, does not *finally* satisfy; yet I do not think it unreasonable to call it farseeing and remarkably balanced—or deep and clear.

But, as I have already suggested, the aspect of Newton's empiricism that I consider most remarkable and most philosophically meritorious is its dialectical relationship to his views about the fundamental structure of the world. The notion of such a structure he took very seriously indeed; and he put forward, for the guidance of physical investigation, a general conception of the kind of structure to be sought—a general program for physical theory or explanation. Huygens and Leibniz, too, had such conceptions or programs—closely similar to one another, since they were variants of the prevalent view of the time, that all physical action is "mechanical" action by contact, through pressure or impact. Leibniz considers this conception to be a necessary consequence of the nature of body; [14] and Huygens repeatedly refers to it as defining *our only hope to achieve understanding in physics.* [15] Both Huygens and Leibniz rejected universal gravitation—that most famous of *verae causae* discovered by Newton—because they saw no way to accommodate it to their general conceptions of physical action: "[I]t cannot," says Leibniz, "be explained by the nature of bodies." [16] This mode of argument—the dogmatic refutative use, against a proposed theory, of structural or "metaphysical" preconceptions—is precisely what Newton objects to in his well-known deprecation of the appeal to "hypotheses" in experimental philosophy. Just because experience is (on the one hand) our only authoritative guide, and cannot (on the other hand) demonstratively establish general conclusions, we must *always* be prepared to *modify* any conclusion, including our most general conceptions about the structure of the world. In a certain sense, this methodological principle plays, for Newton, a role analogous to that of the structural principle of mechanical causation for Huygens: it is the basis of all our hopes for understanding in natural philosophy. To violate it—to "evade the argument of induction by hypotheses"—is to risk the stultification of inquiry. [17] I believe the case can be made, in historical detail, that Newton was in practice consistently faithful to this principle: that in all the controversies he engaged in, he *never argued against* a rival physical theory on the grounds of what could

be called a "hypothesis." [18] And his expositions of his own deepest physical conceptions are also characteristically in the spirit of the same methodological principle. I shall return to this point later, and shall cite two examples.

IV

Before advancing now from the seventeenth to the nineteenth century, I should like to make one slight remark about Newton and space-time—a remark that really bears more upon the technique of historical interpretation than upon Newton's theory. I have found that some historians are suspicious of the very use of a term like "space-time" in explicating Newton's thought, on the grounds that such a use is anachronistic. On one occasion, I cited a statement made by Newton in one of his most metaphysical-theological passages: "Since every particle of space is *always*, and every indivisible moment of duration is *every where*, certainly the Maker and Lord of all things cannot be *never* and *no where*"; and I suggested that to say that every particle of space *is always* and every moment of duration *is everywhere* is exactly to identify the "particles of space" or points with certain one-dimensional submanifolds of space-time, and the "moments of duration" or instants with certain three-dimensional slices or submanifolds. The suggestion was considered extravagant. It was, therefore, with a certain shock of delight that I recently noticed exactly the same reading of the same passage by no less a figure than Kant—to suspect whom of harboring such a notion as space-time might otherwise itself seem anachronistic. The Kantian statement occurs in a footnote in section 14 of Kant's inaugural dissertation (a footnote, by the way, to the striking remark that *simultaneity* is the most important concept following from that of time). In the midst of this note, Kant says:

> Although time is of only one dimension, yet the *ubiquity* of time (to speak with Newton) . . . adds to the quantum of what is actual another dimension. . . . For if one designates time by a straight line produced to infinity, and the simultaneous in any point of time by ordinate lines, the surface so generated will represent *the phenomenal world*, in respect of substance as well as accidents.

I am not, of course, suggesting that Kant's reading of Newton is to be taken as necessarily correct—any more than my own is necessarily so; yet I think that Kant's words, written within sixty years of Newton's, carry some persuasive force. But to put my point about interpretation and anachronism in a general form, it is this: For the avoidance of anachronism, it is *neither necessary nor sufficient* to restrict one's conceptual vocabulary to that of the period under discussion. To impose such a restriction is to inhibit flexibility of thought *without any important compensating guarantee against error.* It is an intellectual stratagem analogous to that of the shallow empiricism in science, that seeks security in rules for the construction of concepts, and achieves only a hobbling of theory.

V

In Mach, of course, we have a classic case of this abusive empiricism. It is a case that also exemplifies a characteristic tendency, a kind of Nemesis, of what we might call "hypercritical" philosophic theories—theories that lay down methodological standards or criteria which are actually impossible to practice. The tendency is to lose, at crucial junctures, basic critical control of the conceptual process. Such critical failure is to be seen in Mach whenever he engages on phenomenalistic grounds in polemic against a physical theory. For instance, Mach's opposition to the kinetic-molecular theory is based upon the fact that, as he puts it, atoms are "mental artifices." But what about perfectly ordinary objects? "Ordinary matter," Mach says, is a "highly natural, unconsciously constructed mental symbol for a relatively stable complex of sensational elements"; the only distinction he finds to the disadvantage of atoms is that of the "natural unconscious construction" versus the "artificial hypothetical" one.[19] To conclude, as Mach does, on the basis of this distinction, that *atomic theories* should eventually be replaced by some "more naturally attained" substitute[20] is very strange: not only is the argument at right angles to Mach's view of the "economic" objective of science, it actually accords a preference to the instinctive and unconscious over the conceptual and deliberate mental processes. And it in no way makes philosophically plausible the claim that atoms are more "artificial" than, for example, thermodynamic potentials.

In Mach's discussion of the issues of space, time, and motion, such loss of critical control occurs in an especially acute form. Let us consider Mach's critique of Newton's inference from the water-bucket experiment. Newton laid stress upon four stages of the experiment: two in which the rotational velocity of the water relative to the bucket was zero, and two others in which it was maximal. In each of these pairs, there was a case in

which the water surface was plane, and a case in which it was concave: namely, whenever the water was at rest *relative to the earth* its surface was plane, and whenever it was rotating relative to the earth its surface was concave. Newton argued that these observations indicate that not relative but absolute rotation is responsible for the dynamical phenomenon.

Prima facie, one may wonder why Newton put such stress on the relative motion of the water *with respect to the bucket*: do not the observed facts of the experiment naturally suggest that rotation with respect to the earth is the relevant circumstance? The commentators with whom I am acquainted have failed to notice that Newton's emphasis here is motivated, not by a mistaken estimate of the demonstrative force of this argument, but by a dialectical enagagement with *Descartes's* theory of motion—according to which, as it is presented in Descartes's *Principia Philosophiae*, the "true" or "absolute" motion of a body is that relative to *the bodies immediately touching* the given one. *This* notion the bucket experiment quite convincingly shows not to be the appropriate one to serve as the basis for dynamical theory. On the other hand, the question remains: does the bucket experiment show that dynamics requires an "absolute" notion of rotation? What rules out rotation relative to the earth as the appropriate fundamental concept?

Now, it is really quite plain that the bucket experiment does *not* establish this point. Yet this question, a natural one not only from Mach's point of view but intrinsically, is not the question Mach raises: he proceeds at once to the stars: "Try to fix Newton's bucket and rotate the heaven of fixed stars and then prove the absence of centrifugal forces." But why the fixed stars? Is this not—from the phenomenalist point of view especially—a very artificial view of the situation? (Doubtless the stars were not visible at all to Newton during the experiment!) Mach says, in a famous and true remark, that the world is given to us only once, and he concludes that it is "not permitted to us to say how things would be" if that world were other than it is; so, he says, we know only that centrifugal forces accompany those states of motion which are rotations relative to the fixed stars. But Mach does not make it a general rule for science that in every statement based upon experience there should appear a list of all the circumstances over which we have no control (the universe being given only once), in order to avoid seeming to claim that we know that the statement would continue to be true even if these things were otherwise.

Such a rule would not only grievously violate Mach's "economy of thought," it would make science impossible. So, again: if we need not bring the stars in everywhere, why here? And why the stars rather than the earth?

The answer to the second question is obvious: we know—essentially from Newton's dynamical analysis of the solar system—that, although the bucket experiment is insufficiently sensitive to show it, rotation relative to the earth will not do as the basic dynamical concept; for the earth itself has to be regarded as rotating. So Mach appeals to the stars because they provide his only recourse: Mach's epistemology teaches him—or he thinks it does—that the only differences among states of motion that can be taken for *verae causae* are differences among *relative* motions; and the only relative motion he finds that can be assigned responsibility for centrifugal force is that relative to the stars. This argument, however, is not really epistemological: it is metaphysical, and is quite akin to Mach's rejection of atoms: namely, it is a rejection of the *possible* reality of certain structures, although they are suggested by and serve for the systematic characterization of phenomena, on the grounds that they are not in some sense properly *constituted out of* phenomenal "elements."

But Mach's confusion on this issue can be documented more fully, and not just in the philosophy but in the physics. One avenue of approach to the point is this: Mach says that we are "not permitted to say" how things would be if the universe were differently arranged. But scientific theories ordinarily do permit all sorts of inferences about situations different from the actual one. Does Mach have a way to formulate a theory of motion that will not permit such inferences? More especially, does he have a dynamical theory that erases the distinction between a rotating universe and a nonrotating one? Or, again—to put the same question another way—it is agreed that we have *evidence*, within the frame of Newton's theory, that the earth is in rotation; does Mach exclude the possibility of finding analogous evidence about the system of the visible stars and galaxies?

To the first, most general question, the answer is a simple negative: Mach suggests no way to formulate a theory in which inferences about states contrary to fact cannot be made; and the very idea seems to contradict our usual notion of what we mean by a "theory." But on the more special issues about motion and the stars, the situation is more complicated. It has not, so far as I know, been noticed that in his discussion of Newton's theory of motion in *The Science of Mechanics*, in the space of

about four pages, Mach sketches or suggests *three entirely different physical theories*, each of which puts the subject on a footing satisfactory to him, but which are (as he does not seem to realize) quite incompatible with one another.

The most elaborately presented of these three theories, and the least satisfying, is essentially an attempt to make precise and general the idea of taking the cosmic masses as defining the basic dynamical reference frame. I shall not attempt to present this Machian theory, which is given in chap. ii, sec. vi, parag. 7 of the book, in any formal detail, but shall only comment upon the trouble with it. Mach's formulation is sketchy and loose, and his exact meaning a little hard to determine. The basic idea, however, is, not to refer explicitly to the fixed stars (or galaxies), but just to take successive determinations of the average relative positions and motions of *all* the surrounding bodies over various solid angles and out to increasingly great distances, and to use the limits of sequences so obtained to define the dynamical variables.[21] Difficulties of formulation aside, this strategy has a quite basic defect. First, it *cannot* be made general: that the sequences involved converge, and that the limits taken in different directions fit together into a coherent geometrical and kinematical framework, are special assumptions about the cosmic geography. If these assumptions fail, the theory just collapses. Second, the special assumptions involved, since they *are* cosmological (or cosmographic), go far beyond anything for which convincing empirical evidence is available. Finally, the theory really rests upon the assumption that Newtonian dynamics itself is correct; it is entirely parasitic upon the latter theory, and is merely an attempt to *reformulate* it in a way that refers only to relative motion. Since the device by which this is to be accomplished is the one I have described—in effect, the positing and then the exploitation of a very special cosmic geography—the theory does indeed imply that there is no difference between a universe globally at rest and a rotating one; but this is achieved, not through any theory of inertial structure as an effect of interaction, but only by the special assumptions I have mentioned—which are tantamount, so far as this question is concerned and from the wider Newtonian point of view, to the mere arbitrary exclusion of an average rotation of the universe from the range of envisaged possibilities. It is hard to see in this theory propounded by Mach an exemplification of anything like what has subsequently been discussed under the rubric of "Mach's Principle." And, what is ironical above all, in the interest of purging Newtonian dynamics of an allegedly nonempirical component, Mach has been led to put forward a theory which must be regarded as on an *empirically weaker footing* than Newton's own—since Mach's theory is equivalent to the *conjunction* of Newton's and of special cosmological assumptions. In short, I submit that this is a clear case of ideology out of control.

But in quite sharp contrast to the foregoing theory is a recurring remark of Mach's, to the effect that the true and complete principle of inertial reference systems is contained in Newton's fifth Corollary to the Laws of Motion—i.e., the principle of Galilean relativity.[22] This remark is a little vague; but it clearly represents a very different point of view from the one I have just been discussing—in particular, it makes no reference to the cosmic masses, and is perfectly compatible with a Newtonian rotational state for these (or, for that matter, with there being no global steady state at all). The most natural interpretation of this Machian view is the following: The laws of mechanics are to be construed as asserting that the relationships they express hold *for some kinematical reference frame*; Corollary V tells us how all such frames may be obtained from any one of them; beyond this, no identifying or "individuating" mark of a distinguished reference system is given.[23] This point of view is precisely the appropriate one for Newtonian dynamics; and it rests, as Mach entirely fails to notice, not indeed upon absolute space, but nonetheless upon "absolute uniform motion" as a *vera causa*—*not* explicated through phenomena of relative motion.

Mach's third theory—or rather suggestion; for it does not amount to a theory, or even to the sketch of a theory—is presented in a series of detached remarks, all of which point out not only that (according to Mach's general caveat) *we have no way of knowing, and ought not to presume*, how things would be if the universe were differently arranged, but that, positively, *we must be prepared for possible surprises in novel circumstances*. The most striking remark of this sort is the famous one in which Mach says we cannot know how the bucket experiment would turn out if the sides of the bucket were several leagues thick.[24] This is a *very* different thing from the analogous comment about putting the stars into rotation about the bucket—because in this case, unlike the one of the stars, we are not obviously beyond the hope of obtaining information to decide the issue. So this third point of view, in contrast to the other two, raises the prospect of a *possible revision* of physical theory, on the basis of

new discoveries; although it neither suggests the actual content of a revised theory, nor in any way provides evidence for the belief that new discoveries will in fact lead to revision: it simply points out the possibility that this may happen. What has made it appear to readers of Mach (most notably, to Einstein) that evidence has been given that it is somehow "right" or "desirable" for a rotating hollow mass to induce centrifugal forces on stationary bodies inside it, is the occurrence of this suggestive physical speculation in the confused context of the conflated epistemology and metaphysics I have just been describing.

In summary, then: of the three views, the first implies that there is no difference between rotation of the stars about the earth and rotation of the earth beneath the stars. It is equivalent to the statement that the universe is Newtonian, with the stars, on the average, in an inertial state; and would be defeated by evidence against this assertion. The second implies that there *is* a difference between those two cases, and leaves the issue of rotation of the stars open to empirical investigation. The third suggests that there *may be* no difference between the two cases, because *moving masses may induce "inertial" effects*. This is of course the suggestion that caught the imagination of Einstein.

In connection with the second of the Machian positions we have considered, a question arises: Why does Mach single out Newton's fifth Corollary for praise? The *sixth* Corollary to the Laws of Motion goes further: it states that the internal motions of a system of bodies are unaffected by any *accelerated* motion shared by the bodies of the system. Why does Mach not take this up, as a more far-reaching embodiment of the sole relevance of *relative* motion? I am unable to answer this—unless by echoing Mach's own phrase about Newton, that he "was correctly led by the tact of the natural investigator" (or of the good interpreter). For despite Corollary VI, absolute acceleration is in Newton's theory a *vera causa*. But how is it so, despite Corollary VI? A superficial answer is that, for Newton, an acceleration requires a force to produce it, and conversely an unbalanced force requires an acceleration. To this, the proper objection can be made that Newton's dynamics gives us no independent general criterion of the presence or absence of force; thus it is merely glib to cite force as "the criterion" of acceleration. The solution of the puzzle, however, is the following: Newton indeed does not provide us with what a naïve empiricism used to demand—something like an "operational definition" of force or of acceleration. But his dynamics imposes upon dynamical systems the general condition that *all the forces in such a system occur in action-reaction pairs*. A shared acceleration, common to all the bodies of a system, cannot come from such forces: it can come, if at all, only from some source outside the system in question. Thus Newton's theory affords a way of assigning kinematical states up to a Galilean transformation, *on condition that one has succeeded in accounting completely for the relative motions by a system of action-reaction pairs*, and on the further condition that there is no reason to suspect the system in question to be subject to an outside influence imparting equal accelerations to all its members. Newton had the good luck to find such a system: namely, the solar system; and the skill to effect its thorough dynamical explication. That explication is the explicit basis upon which Newton rests his determination of the "true" or "absolute" motions.[25] Mach, when he places his emphasis upon the fixed stars as reference bodies, has failed to notice that this choice in no way suffices to decide—on the basis of seventeenth-century data—whether the earth's center, or the sun's, or some other point, is to be described as fixed;[26] Newton, however, was in a position to assert that *neither* earth nor sun can be at rest, but that the center of mass of the solar system—which is never far from the sun—is "either at rest or moving uniformly in a right line."[27]

Newton's Corollary VI, however, is not in the *Principia* for mere ornament. Newton needs it to establish that, to a first approximation, the system of a planet and its satellites can be treated as an *isolated* gravitating system if one ignores the shared orbital motion around the sun. Thus we have a case—treating the sun's gravitational field in the region of the system in question as essentially homogeneous—of what Einstein was to call the "principle of equivalence." Indeed, Newton's sixth Corollary (which deals with a homogeneous field of acceleration), and Huygens's discussion of centrifugal force (which deals with an inhomogeneous one), together adumbrate the principle, exploited so fruitfully by Einstein, that the dynamical states and behavior of bodies in no way distinguish between, on the one hand, a certain kinematical state, and, on the other, a second kinematical state implying the same distances and rates of change of distance, together with a suitable applied field of force. It is a little surprising that Mach, with his relativistic view of motion and his interest in seventeenth-century mechanics, did not at all notice these things.

I said, however, at the outset, that my story has no villains; and I particularly do not wish to give the impression of using Mach as a

whipping-boy. I believe he deserves better. I do think that his epistemology was faulty, and his application of it confused. But—despite what I have earlier characterized as "ideology"—the honesty of his mind seems to me beyond dispute; his critique of basic concepts, however defective, has been stimulating for philosophical analysis, for historical interpretation, and—not least—for physical theory; and the same can be said of the sheer physical speculation which, in the guise of critique, appears in his suggestion that a whirling container might induce a field of centrifugal force. I should like to close this lengthy and rather critical section on Mach with two quotations that should leave a pleasanter flavor. The first is from Einstein: "I see Mach's greatness," he wrote, "in his incorruptible skepticism and independence." The second is from Mach, commenting upon experiments designed to detect possible induced centrifugal force fields (about which he was in fact quite skeptical): "[W]e must not," he said, "underestimate even experimental ideas like those of Friedländer and Föppl, even if we do not yet see any immediate result from them. Although the investigator gropes with joy after what he can immediately reach, a glance from time to time into the depths of what is uninvestigated cannot hurt him."

VI

I shall have to abbreviate my discussion of Helmholtz and Riemann; let me try to reduce it to bare essentials. Helmholtz was evidently led to questions about the foundations of geometry by way of his studies in the physiology of visual perception, and the associated questions about the genesis of our perceptions and conceptions of space; and then he was further stimulated by the posthumous publication, in 1867, of Riemann's *Habilitationsvortrag*.[28] The work that resulted was of substantial philosophical interest, and of rather deep mathematical interest. In its latter aspect, as completed and improved some eighteen years later by Sophus Lie, it led to the celebrated group-theoretical foundation of Euclidean and non-Euclidean geometry. Helmholtz's leading idea was that *all our knowledge of space comes from observation of the properties of* (approximately) *rigid bodies*, and therefore that the general properties of space should be deducible from the conditions that must be satisfied by bodies in motion if they are to qualify as "rigid." These conditions can be expressed more advantageously in terms of the motions themselves, directly; that is—since Helmholtz assumes that any rigid motion can be extended, conceptually, through all of space—as assumptions about the group of those mappings of space upon itself that take each figure to a congruent figure: the group of "congruence transformations." The execution of this program led to a twofold result, in relation to the theory of space presented in Riemann's essay: first, the "Pythagorean" metric, postulated by Riemann in an avowedly arbitrary way as just the first and simplest case to consider, is *derived* by Helmholtz from his basic assumptions; second, Helmholtz is led to a far more drastic restriction: for of all the structures that Riemann's theory allows, only those of uniform curvature satisfy Helmholtz's postulates.[29]

Helmholtz's view of his contribution, in its relation to Riemann's theory, is sharply expressed in the title of his basic paper: Riemann's essay, of course, was called, "On the Hypotheses Which Lie at the Basis of Geometry"; Helmholtz's is, "On the Facts That Lie at the Basis of Geometry." Riemann says, in his introduction, that the postulates of Euclidean geometry, as well as those of the (ordinary) non-Euclidean geometries, are "not necessary, but only of empirical certainty—they are hypotheses; one can thus investigate their probability—which, of course, within the limits of observation, is very great—and subsequently judge the reliability of their extension beyond the limits of observation, both on the side of the immeasurably great and on the side of the immeasurably small." The implication of Helmholtz, in substituting "facts" for "hypotheses," is that by reducing the theory to its more fundamental presuppositions he had narrowed the range of open possibilities, and in particular had eliminated the "hypothetical" character of the postulates of "Pythagorean" metric and constant curvature.

Helmholtz, of course, was wrong. It is childishly easy—after the fact—to point out the source of error. It is true that we arrive at our notions about congruence, distance, etc., from manipulations and observations of bodies; and that the general notions we form are connected with the behavior of *rigid* bodies, which we do conceive as conforming to Helmholtz's postulates. But it does not follow from this—either from the psychological facts of the genesis of our spatial notions, or from the mathematical relationship of Helmholtz's postulates to metric geometry—that these postulates, or the geometry they entail, must hold strictly in the world. It follows at most that they hold in some sense "approximately" (as Riemann says: they have a high degree of assuredness or probability, within the limits of observation). The question then arises,

does it make sense to ask what the "exact" state of affairs is? One may guess that Helmholtz thought something like this: that since the very notion of length is based upon that of congruence, and congruence is based upon the motion of rigid bodies, *either* the basic spatial relationships of one of the geometries of constant curvature must hold, *or* geometry will just break down altogether and our question about the "exact state of affairs" will be devoid of meaning.

VII

Riemann's *Habilitationsvortrag* is one of the most marvelous documents in the history of the human intellect. It is about fifteen pages long; contains almost no formulas; is singularly lucid, and yet so dense with ideas that I am tempted to say that to understand it is to be a wise and a learned man. It is primarily a work of mathematics, and the richness of its mathematical content would merit a very extensive commentary. But I have to confine myself to two or three points that bear upon physics and the philosophy of physics; and I shall first take up Riemann's discussion of the matter just adumbrated in connection with Helmholtz. Here is what he says—lightly paraphrased:[30]

> If one presupposes that bodies exist independently of place, then the measure of curvature is everywhere constant [—this of course is Helmholtz's point of view]; and it follows from the astronomical measurements that [that measure cannot be much different from zero]. But if such an independence of bodies from place does not obtain, then one cannot infer the measure-relations in the infinitely small from those in the large; in that case the measure of curvature can have an arbitrary value at each point in three directions, if only the total curvature of every measurable part of space does not differ noticeably from zero. . . . Now the empirical concepts in which the spatial measure-determinations are grounded—the concept of the solid body and of the light-ray—appear to lose their validity in the infinitely small; it is therefore very well conceivable that the measure-relations of space in the infinitely small are not in accord with the presuppositions of [ordinary] geometry—and one would in fact have to adopt this assumption, as soon as the phenomena were found to admit of simpler explanation by this means.

Notice that Riemann has made a remark that one would not have expected Helmholtz, of all people, to ignore: our basic source of spatial information is not bodies only; light also plays a fundamental role. But both of these physical structures do in fact break down in the small. When

Riemann says this about solid bodies, he undoubtedly has in mind the atomic constitution of matter—which he is thus not disposed to dismiss as an "artifice" or as irrelevant. When he says the same about the light-ray, he is of course referring to diffraction. We know a great deal more now about the breakdown of ordinary physical conceptions in the very small; and clearly we have not yet learned all there is to know about it. Riemann's statement of the case was quite remarkably on target.[31]

But what about the objection that if the empirical basis of our geometric knowledge gives way, we are left with no sensible conceptions at all—no concept of a structure to be investigated?

Riemann's stand on this issue is the following—expressed by him in almost lapidary prose, without circumlocution, with the greatest simplicity; yet, for some reason, apparently not often appreciated at its true worth: Our conceptions about spatial structure, and most particularly about structure *in the small*, are essentially bound up with our whole theoretical understanding of physical interaction. "Upon the exactness with which we pursue the phenomena into the infinitely small," he says, "essentially rests our knowledge of their causal connection. . . . The questions about the measure-relations of space in the immeasurably small therefore are not idle questions." From this position it follows, (1) that we must not take a too narrow view of what empirical sources may be used to obtain spatial information: if the ordinary sources of such information break down in the very small, this is an indication that we should be prepared for something possibly quite new and surprising; and empirical information bearing upon possible spatial revision may come from *any* source relevant to fundamental physical theory itself. It also follows (2) that it is appropriate to think of space in a way that has, after all, something in common with the point of view of Leibniz: not with the somewhat rigid dogma that spatial properties are nothing but relations among bodies, but with the broader view that our spatial notions, insofar as they are brought to bear in physics, have their significance only as structural aspects of a more embracing structure: that of physical interaction itself. This is how I understand one most important remark of Riemann's that does seem to me obscurely phrased: his famous statement that if "the reality that lies at the basis of space" is not a discrete manifold, then "the ground of its measure-relations must be sought . . . in binding forces that act upon it." Setting aside the issue of discrete versus continuous, the essential point seems to me this: By "the reality that underlies space," Riemann means that aspect of the real structure of the world which we express in terms of spatial concepts. That the ground of the measure-relations is to be sought in *binding forces* expresses partly the general principle that the full physical meaning of the spatial structure comes from its role in physical interaction, but also the more special point that our ordinary middle-sized spatial knowledge does derive from our experience of solid bodies, and that *solid bodies have to be understood, on a more fundamental view, as equilibrium configurations*. This is the same point that Einstein makes when he says that it is wrong, in a fundamental sense, simply to postulate that objects of some kind "adjust" to the metric field—that, roughly, the metric is what we read from meter-sticks—because how an object behaves in relation to the field ought to be derived in the theory *from the object's physical constitution*. Notice that this applies to Newtonian physics quite as well as to any other; and nothing in Riemann's statement implies that it will in fact be necessary to give up Newton's physics—he implies only that it *may* be necessary to do so.

This brings me to the third main consequence of Riemann's view. It is that, since the possible sources of information that might bear upon spatial structure are as wide as physics itself, one cannot hope to foresee in detail what will eventually prove relevant. What one can do—specifically, what the mathematician (I think one should say, the philosophical mathematician) can do—is to explore as well as he can the conceptual possibilities. Here is how he puts it:

> The decision of these questions can only be found by proceeding from the traditional and empirically confirmed conception of the phenomena, of which Newton has laid the foundation, and gradually revising this, driven by facts that do not admit of explanation by it; such investigations as, like that conducted here, proceed from general concepts, can serve only to ensure that this work shall not be hindered by a narrowness of conceptions, and that progress in the knowledge of the connections of things shall not be hampered by traditional prejudices.

VIII

It is with this last passage from Riemann that I should like to compare two of Newton's characterizations of his own most basic physical results, and his program for the development of physics. The first occurs near the end of the *Opticks*, after a summary statement of Newton's fundamental principles: the laws of motion, and the notion of *forces of nature*, among which he mentions "that of Gravity, and that which causes Fermentation, and the Cohesion of Bodies." "These Principles," he says, "I consider, not as occult Qualities, . . . but as general Laws of Nature, . . . their Truth appearing to us by Phaenomena, though their Causes be not yet discover'd. For these are manifest Qualities, and their Causes only are occult. . . . To tell us that every Species of Things is endow'd with an occult specifick Quality by which it acts . . . is to tell us nothing: But to derive two or three general Principles of Motion from Phaenomena, and afterwards to tell us how the Properties and Actions of all corporeal Things follow from these manifest Principles, would be a very great step in Philosophy, though the Causes of those Principles were not yet discover'd: And therefore I scruple not to propose the Principles of Motion above-mentioned, they being of very general Extent, and leave their Causes to be found out."

The second Newtonian characterization appears in the Preface to the *Principia*. Having explained what the work—in particular, the third book—accomplishes, namely the establishment of the theory of gravity and the derivation from it of "the motions of the Planets, the Comets, the Moon, and the Sea," he continues thus: "I wish we could derive the rest of the phaenomena of Nature 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 each other and cohere in regular figures, or are repelled and recede from each other; which forces being unknown, Philosophers have hitherto attempted the search of Nature in vain. But I hope the principles here laid down will afford some light either to that, or some truer, method of Philosophy."

IX

I have been discussing a few of the strands which—with others as well—were drawn together by Einstein in creating the general theory of relativity. A philosophically satisfying account of the process by which Einstein accomplished that great work has, I believe, never been given. In taking up these pieces of prehistory, I hope to have helped to clarify some of the issues—and some of the confusions and obscurities—that formed the background of Einstein's achievement. I have tried also to suggest—I hope persuasively—a certain general philosophical attitude

toward science and its history. Of science, it is the view that, through all the complexities and perplexities of epistemological analysis, all of science is most fruitfully thought of as having one great subject: the structure of the natural world; that to understand a scientific theory is to understand what it says about that structure; but that the touchstone of the genuineness of a structural claim is its connection with experience. Of the history of science, my view is correspondingly old-fashioned: where some see "incommensurable" theories, I see, from the seventeenth century up to today, a profound community of concerns, and a progressive development that has involved both cumulative growth and deepening structural understanding. It is partly a matter of emphasis. Einstein unquestionably effected a conceptual revolution. Such a revolution—I will not say that it was foreseen, but its possibility was foreseen by Riemann. And this work of Riemann's and of Einstein's was entirely in the spirit of Newton's hopes for physics. Indeed, I doubt that a history of three hundred years has ever more gloriously crowned the wishes of a man than the past three hundred years have crowned the expressed hope of Newton: that his principles might afford some light, either to his own or some truer method of Philosophy.

Notes

1. The term *vera causa* is to be taken here, not as bearing a technical sense, but rather as presystematic (a "commonplace"). This term appears to have entered the literature on scientific method in Newton's first "Rule of Philosophizing" (*Principia*, Book III): "We ought to admit no more causes of natural things, than such as are both true and sufficient to explain their phenomena." Newton's phrase was taken up and commented upon, with animadversions variously favorable and unfavorable, by (for example) Whewell, Mill, and Peirce. The sense in which it has been generally understood is expressed as follows by Dewey (*Logic: the Theory of Inquiry* [New York: H. Holt, 1938], p. 3): "Whatever is offered as a hypothesis must . . . be of the nature of a *vera causa*. Being a *vera causa* does not mean, of course, that it is a *true* hypothesis, for if it were that, it would be more than a hypothesis. It means that whatever is offered as the ground of a theory must possess the property of verifiable existence in *some* domain. . . . It has no standing if it is drawn from the void and proffered simply *ad hoc*." (The word "standing" is particularly suggestive here; I have wondered whether there might not have been a prior usage of the term *vera causa* in jurisprudence, to signify a *case* with standing in court—that is, with a prima facie claim upon the court's attention.)

It should be noted, however, that my own emphasis is principally upon a connection the reverse of that asserted by Dewey: I am concerned primarily, not with "true causality" as a credential for admission into a theory, but with the inferences concerning what are and are not "true causes" that may be drawn from a theory taken as already adopted.

2. To relate this sense to the metaphysics professed by Leibniz is a formidable task; but some remarks bearing upon the problem are perhaps in order. First, in Leibniz's ontology the only true "beings" ("substances"; "beings capable of action") are the *monads*, whose states are states of *perception*, whose (exclusively internal) processes are governed by *appetition*, and which together—all mutually adjusted in a harmony pre-established through God's

will—constitute the *kingdom of final causes* or *of grace* (cf. the *Monadology*—e.g., in Gottfried Wilhelm Leibniz, *Philosophical Papers and Letters*, ed. Leroy E. Loemker [2nd ed., Dordrecht-Holland: Reidel, 1970], pp. 643ff., paragraphs 14, 15, 18, 19, 79, 87; and *The Principles of Nature and of Grace, Based on Reason*, *ibid.*, pp. 636ff., paragraph 1). At the same time, in a sense which it is not at all easy to explicate, each monad is associated with a particular body (cf. *The Principles of Nature and of Grace, Based on Reason*, paragraph 4); and Leibniz sometimes refers to the monad and its body together as a "corporeal substance" (see letter to Bierling, 12 August 1711, quoted by A. G. Langley in his edition of Leibniz's *New Essays Concerning Human Understanding* [3rd ed., La Salle, Ill.: Open Court, 1949], p. 722). Bodies themselves are not substances, and thus not, strictly speaking, capable of action; they are only *phenomena*. But they are none the less real; Leibniz dismisses Berkeley with some scorn (letter to des Bosses, 15 March 1715; Loemker, p. 609): "We rightly regard bodies as things, for phenomena too are real. . . . The Irishman who attacks the reality of bodies seems neither to offer suitable reasons nor to explain his position sufficiently. I suspect that he belongs to the class of men who want to be known for their paradoxes."

The realm of "well-founded phenomena" comprising bodies and bodily processes is the *kingdom of efficient causes* or *of (corporeal) nature*. Notwithstanding the incapacity of bodies for "action," this kingdom can be considered as if it were autonomous; and its autonomous concordance with the kingdom of grace is a manifestation, according to Leibniz, of the pre-established harmony of the monads. In the words of the *Monadology* (paragraphs 79 and 81):

> Souls act according to the laws of final causes through their appetitions, ends, and means. Bodies act according to the laws of efficient causes or the laws of motion. And the two kingdoms, that of efficient and that of final causes, are in harmony with each other.
>
> In this system bodies act as if there were no souls . . . , and souls act as if there were no bodies, and both act as if each influenced the other.

There are in this striking analogies to Kant (whose metaphysical schooling was of course of Leibnizian derivation), even in terminology (e.g., "kingdom of final causes" = "kingdom of ends"); in particular, Leibniz's "real" or "well-founded" corporeal phenomena are precisely equivalent to Kant's "objects of (external) experience," and constitute, just as with Kant, the subject matter of natural science. The "substantial" foundation of these phenomena involves most crucially, according to Leibniz, the notion of *active force* or *power*; he accordingly coined the word "dynamics," i.e., the theory of power, for the basic science of corporeal nature; and he gave to his major treatise on the subject the title "Dynamica de Potentia et Legibus Naturae corporeae"—that is, "Dynamics, on Power and the Laws of Corporeal Nature."

The central difficulties in an attempt to find a coherent formulation of Leibniz's metaphysical principles and of their connection with the principles of his physics can, then, be summarized as follows:

(1) How are we to understand the relation of bodies to the monads?
(2) What role and what status have space and time in this system?
(3) How can Leibniz's concept of force perform the function he claims for it as the metaphysical foundation of bodily phenomena—i.e., their foundation *in the kingdom of final causes* or realm of monads? How, that is, can a conceptual transition be effected between the two realms?

A sketch of answers to these questions, with brief indications of textual evidence, will be essayed here.

To begin with point (2): Space and time are (to use Leibniz's term) "orders"—i.e., systems of structural relations—of *everything that exists*; in particular (and here we have a very sharp divergence between Leibniz and Kant) of the (*noumena* or) monads—even including God.

(That spatio-temporal relations affect monads is, e.g., explicit in the letter to de Volder of 20 June 1703; Loemker, p. 531: "I had said that extension is the order of possible coexistents and that time is the order of possible inconsistents. If this is so, you say you wonder how time enters into all things, spiritual as well as corporeal, while extension enters only into corporeal things. I reply that . . . every change, spiritual as well as material, has its own seat, so to speak, in the order of time, as well as its own location in the order of coexistents, or in space. For although monads are not extended, they nevertheless have a certain kind of situation in extension, that is, they have a certain ordered relation of coexistence with others, namely, through the machine which they control." This passage, to be sure, is restricted by the next sentence to *finite* substances; but the application to God is assured by, e.g., the discussion of God's "immensity" and "eternity" in the fifth letter to Clarke, paragraph 106; Loemker, p. 714: "These attributes signify . . . , in respect to these two orders of things, that God would be present and coexistent with all the things that should exist.") Because they are systems of relations, space and time are "beings of reason"; the reality that underlies them is a certain collection of affections of the monads taken severally (cf. letter to des Bosses of 21 April 1714; Loemker, p. 609: "My judgment about relations is that paternity in David is one thing, sonship in Solomon another, but that the relation common to both is a merely mental thing whose basis is the modifications of the individuals"). But what modifications of the individual monads constitute the "basis" of the spatial relations? The answer, presumably (since the only states of monads are perceptive states), is: *their own spatial perceptions*. The pre-established harmony must then ensure a concordance among the spatial perceptions of all the monads, which can be expressed in the formula that *the monads have perceptions of their spatial relationship to one another*.

If this is correct so far, it makes prima facie good sense to identify bodies either with (a) certain *collections of monads* (not necessarily, for "the same body," the same monads at all times), or with (b) certain *correlated perceptions*—appearances or phenomena—of (i.e., internal to) all the monads (taken severally). For each of these identifications will endow bodies with spatial relations; and the first has some conformity with Leibniz's assertion that each monad "has a body," the second with his characterization of bodies as "phenomena." The second identification is in effect that made by Kant: bodies, as objects of experience, are constituted by a connection of perceptions "in consciousness in general." The first contains, by contrast, what Kant would call a "transcendent"—and therefore an illegitimate—judgment. But for Leibniz *both* of these identifications are possible, for one can appeal to the harmony to guarantee compatibility between them; and the double identification agrees very well with a number of passages in which Leibniz speaks almost interchangeably of "aggregates" and "phenomena." For instance, to de Volder, 20 June 1703 (Loemker, pp. 530–531): "[I]n appearances composed of aggregates, which are certainly nothing but phenomena (though well founded and regulated), no one will deny collision and impact." And: "[S]ince only simple things are true things, and the rest are beings by aggregation and therefore phenomena [etc.]." And again: "[A]n internal tendency to change is essential to finite substance, and no change can arise naturally in the monads in any other way. But in phenomena or aggregates every new change arises from an impact according to laws prescribed partly by metaphysics, partly by geometry. . . ."

We have, thus, a hypothesis in answer to question (1) above. As to question (3), a general answer is easy to give (although not an answer that will relieve Leibniz's doctrine of obscurity, or defend it against Kant's criticism of all pretensions to "transcendent" knowledge). Leibniz seeks, among the descriptive parameters of corporeal phenomena (motions), one that is suitable as a *measure* of "power" or "force"; and he argues for the suitability of the quantity he calls "living force" (*vis viva*)—or, over an interval of time, of a quantity he calls "moving action," which is in effect the time-integral of living force. Unfortunately, the connection of these physical quantities (and of the laws he proposes for them) with the monadic metaphysical realm is merely claimed, rather than convincingly explicated (much less established), by Leibniz. (For the best account of the notions themselves, and of the conservation laws suggested by Leibniz, see the French paper "Essay de Dynamique sur les

Loix du Mouvement," in Leibniz, *Mathematische Schriften*, ed. C. I. Gerhardt [Halle: H. W. Schmidt, 1860; photographically reprinted, Hildesheim: Georg Olms Verlagsbuchhandlung, 1962], vol. VI, pp. 215ff.; English translation as Appendix V in Langley's version of the *New Essays* cited above. This paper contains an impressive anticipation of the general principle of the conservation of energy, including some discussion of the problem of inelastic impact. Cf. also, on the same general subject and for some account of the metaphysical connections as Leibniz views them, the essay *Specimen Dynamicum*; English translation in Loemker, pp. 435ff.) Leibniz does, however, make one pregnant suggestion: inspired by Fermat's derivation of the optical law of refraction from a principle of *minimality*, he suggests that the basic laws of the "kingdom of efficient causes" may be derivable from a principle of "final causes" in the form of an extremality principle ("principle of the optimum"). (See *Specimen Dynamicum*; p. 442 in Loemker; and *Tentamen Anagogicum: an Anagogical Essay in the Investigation of Causes*; Loemker, pp. 477ff. It should be noted that Leibniz did not—so far as I know—apply the principle of extremality to his quantity of "moving action": this step was apparently first made by Maupertuis, and first put in a satisfactory form by Euler.)

If this rough account of Leibniz's general doctrine is accepted, it will be seen that within it the spatial structure occupies a critical position, and in some critical ways an ambiguous one. This structure may be said to manifest itself on three levels (which, to press the Kantian analogue, correspond respectively to the Transcendental Aesthetic, the Transcendental Analytic, and the Transcendental Dialectic): first, spatial qualities belong, "subjectively" in Kant's sense, to the perceptions of each monad—they are *phenomena* (and it is of this aspect that Leibniz is presumably speaking when he says: "I can demonstrate that not merely light, heat, color, and similar qualities are apparent but also motion, figure, and extension"—see "On the Method of Distinguishing Real from Imaginary Phenomena"; Loemker, p. 365). Second, spatial relations hold *objectively* among bodies (and, despite Leibniz's assimilation, in the passage just quoted, of extension, figure, and motion to the "secondary qualities," he remained committed to the Cartesian view that only motion can make a real difference among bodies—cf. Appendix II, pp. 44 and 45). It is in view of this circumstance that Leibniz sometimes characterizes space and time, like bodies, as "well-founded phenomena." (That he does so is a remark for which I am indebted to Arthur Fine; cf. Leibniz's letter to Arnauld of 9 October 1687; Loemker, p. 343: "[M]atter . . . is only a phenomenon or a well-founded appearance, as are space and time also.") Finally, as we have seen, spatial relations hold, according to Leibniz, among the monads themselves. Yet it should be noted that this is so only in an indirect or derivative sense. Leibniz's monads, as he puts it, "have no windows": each is a world to itself, characterized in a fundamental sense only by what is internal to it, its "perceptions" and its "appetition." Only through the harmony can the monads be said to perceive, or be related to, one another; and the harmony is a harmony among the perceptions, i.e., the phenomena. Since this is just what constitutes the realm of corporeal nature, it is after all in this realm that space or extension primarily occurs; and accordingly, in the letter to de Volder quoted from above, monads are said to have spatial relations to one another "through the machine which they control." So, in the letter to des Bosses of 16 June 1712 (Loemker, p. 604), Leibniz can say the following (italics added here): "I consider the explanation of all phenomena solely through the perceptions of monads functioning in harmony with each other, with corporeal substances rejected, to be useful for a fundamental investigation of things. In this way of explaining things, space is the order of coexisting *phenomena*, . . . and *there is no spatial or absolute nearness or distance between monads*. And to say that they are crowded together in a point or disseminated in space is to use certain fictions of our mind when we seek to visualize freely what can only be understood."

We come thus to the conclusion that in their *central* sense, for Leibniz, *spatial relations are objective relations among bodies*; and that whatever is to be regarded as "real" or objective about space must be expressible in terms of such relations. However, a critical problem remains; for this view of what is objective about space is hardly reconcilable with

Leibniz's claim that "force" has absolute metaphysical standing, and that the phenomenal manifestation of this absolute force is *vis viva*. The conceptual incoherence shows itself, for example, in the manifest ambivalence of Leibniz's position on absolute versus relative motion in his correspondence with Huygens (see Appendix I below, and cf. also notes 4 and 5 below).

What I wish most to emphasize, in concluding this rather lengthy but hardly adequate note, is that the substance of the discussion in the main text does *not* depend upon the resolution of these intricate issues of the metaphysical interpretation of Leibniz. In particular, what Leibniz's physics implies about the "real structure of the world" is a question that can be approached through a philosophical analysis of that physics; and the compatibility of the result with his metaphysical principles—or with some proposed interpretation of those principles—can be taken as one measure of the success of (the proposed interpretation or of) Leibniz's philosophical program. This point of view, although it is not without its own hazards, seems to me fruitfully applicable to many philosophers who have given serious attention to natural science (and seems to me to have been, on the whole, neglected by historians of philosophy and of science).

3. It is not quite clear whether "automorphism" (for the Euclidean structure, what is usually called "similarity") or "isometry" ("congruence") is the more appropriate notion here. From a general point of view, it should certainly be the former, since isomorphic structures are structurally indiscernible. But Leibniz seems to have conceived of Euclidean metric relations—and not just ratios of distances, but distances themselves—as having a kind of absolute (monadic) basis (cf. *The Metaphysical Foundations of Mathematics*; Loemker, pp. 666–667: "In each of the two orders—that of time and that of space—we can judge relations of *nearer to* and *farther from* between its terms, according as *more* or *[fewer]* middle terms are required to understand the order between them"); and this suggests that Leibniz would choose congruence.

4. "In conflict with *some* of his statements of principle"—namely, those asserting the "equipollence of hypotheses" for more than just uniform rectilinear motions. (I have suggested that Leibniz may have intended the generality here to apply only to the interacting bodies, not to the reference systems. In this case, however, his strictures against Newton—so far as the dynamical issue goes, setting the metaphysics aside—are unwarranted: for, as Leibniz recognizes, Newton admits the dynamical principle of Galilean relativity; and this is absolutely general so far as the interacting bodies are concerned.)

At the opposite extreme, we have a collection of statements in which Leibniz maintains that motion—"or, rather, force" (i.e., *vis viva*—kinetic energy)—must have a determinate subject. *These* statements of principle are quite compatible with the view that rectilinear motion is distinguished; but, so far as physics is concerned, they are essentially equivalent to the theory of absolute space, and are hard to reconcile with *any* version of the "equipollence of hypotheses." The editors of vol. XVI of the *Oeuvres complètes de Christiaan Huygens*, published by the Société hollandaise des Sciences (The Hague, 1929), suggest a resolution of this problem by cutting the knot (p. 199, n. 8 of the cited volume): "Remarquons que chez Leibniz il faut toujours faire une distinction entre le point de vue du métaphysicien et celui du physicien. La force absolue et le 'mouvement absolu véritable' . . . peuvent exister sans que (suivant Leibniz) le physicien puisse les apercevoir." To me this seems a counsel of despair, making nonsense of Leibniz's philosophy. After all, for Leibniz the identity of indiscernibles is a fundamental *metaphysical* principle; how, then, for him, can physically indistinguishable hypotheses be metaphysically distinct?

5. Reichenbach tells us that Leibniz "would argue . . . that the appearance of centrifugal forces on a disk isolated in space proves its rotation relative to the ether and not relative to empty space" (*The Philosophy of Space and Time*, trans. Maria Reichenbach and John Freund [New York: Dover, 1958], p. 212); and then comments (*ibid.*, n. 2): "This view is not precisely formulated by Leibniz, but it may legitimately be extrapolated from a passage in his *Dynamics* (Gerhardt-Pertz, *Leibnizens mathematische Schriften*, VI, 1860, p. 197) and also from his defense of the relativity of motion in the exchange of letters with

Clarke." I have been unable to find in the letters to Clarke any passage bearing such a construction as Reichenbach suggests; and the page cited in Gerhardt is in the midst of an article on the cause of gravity—an article that bases itself upon Huygens's theory of weight as *an effect of the centrifugal force of a type of vortical motion of the ether*, which is surely not to the point! I should guess that Reichenbach meant to refer either to the *Dynamica* or to the *Specimen Dynamicum*, both of which address the issue of apparent deviations from the "equipollence of hypotheses." In both these works, such apparent deviations are attributed to hidden interactions of some kind with an ambient medium (see, e.g., the last sentence of the long first paragraph under Proposition 19 of the passage from the *Dynamica* given below in Appendix II, p. 42). In the cited passage from the *Dynamica* it is clear beyond a doubt, from the immediate context and from the following Proposition 20, that the hidden interactions in question are those responsible for cohesion. A careful reading of the closely related passage in the *Specimen Dynamicum* (see Loemker, pp. 449–450) reveals the same; except that in this passage gravity is cited as another effect of hidden interaction, and the inhomogeneity—in particular, the varying direction—of the gravitational field is named as the reason why projectile motion on a moving ship is not, when considered with extreme precision, "phenomenologically" the same as on a ship at rest.

The connection Leibniz repeatedly asserts between the equivalence of hypotheses and the nature of cohesion (cf., besides the passages already referred to, the last two sentences of the selection from a letter to Huygens given in Appendix I(d), p. 41) is surely a crux for any interpretation of his dynamical relativism. What seems to me the melancholy truth of the matter is that *no* interpretation resolves this crux: that it stands as evidence of a fundamental confusion in Leibniz's thought on this subject. I have suggested, as alternative constructions of Leibniz's "general" principle of equivalence, that the generality may apply either to the *interactions* or to the *reference systems*; and I have suggested that the former (and more restricted) version has the merit of making sense of Leibniz's first argument for this principle. Supporting evidence for this reading of Leibniz is found in a passage in the *Specimen Dynamicum* (Loemker, p. 445) in which, having praised Descartes for maintaining the purely relative character of motion, Leibniz criticizes him for failing to infer from this principle "that *the equivalence of hypotheses is not changed by the impact of bodies upon each other* and that such rules of motion must be set up that the relative nature of motion is saved, that is, so that the phenomena resulting from the collision provide no basis for determining where there was rest or determinate absolute motion before the collision." Here the statement that the equivalence "is not changed by the impact" clearly means only that *Galilean* relativity—the freedom of choice as to "where there was rest or . . . motion before the collision"—is not broken by collisions. (Descartes's laws of impact grossly violated Galilean relativity.) If this passage is compared with Leibniz's phrase to Huygens (Appendix I(b) below, p. 40) that he has reasons to believe that nothing, including circular motion, "breaks the general law of equivalence," the reading under discussion gains considerable plausibility. *But this reading shatters upon our crux*. For it is explicitly demonstrated by Newton in the *Principia* that Newtonian dynamics satisfies Galilean relativity without qualification; and forces of attraction, or inextensibility of cords, or perfect rigidity, make no difficulties in the matter. So if the reading in question is the right one, Leibniz's statements about cohesion and solidity can only be taken to show that he had failed to understand some of the basic elementary arguments and theorems of Newton's mechanics.

The second interpretation—that Leibniz professed an honest principle of "general relativity"—seems in better accord with his philosophy, and (as we have seen) with his second argument for the equivalence of hypotheses. On the other hand, the difficulties with this interpretation are formidable, as we have also seen; and it, too, fails to solve our crux. I have attempted to put the notion of general relativity and Leibniz's conception of cohesion from impact by ambient particles into intelligible connection in the following way, which may be of some interest although I cannot claim that it finally succeeds: Leibniz says (Appendix II below, p. 42) that if there were true circular motion (as ordinarily conceived) the general equivalence of hypotheses would be violated by the occurrence of

centrifugal force on a rotating disk. Could he have believed that, on his view of the nature of cohesion, "rotating reference systems" are without strict physical meaning—because, in that view, there are *in principle* no rigid bodies? Of course, this is hardly a tenable position—for whether the earth, for example, is or is not rigid, one can adopt it as a kinematical reference body. Could, then (to approach something more cogent), Leibniz have reasoned as follows?—If the earth, for instance, or Newton's famous bucket, were a strictly rigid body, then the two systems, earth at rest and earth rotating (or bucket at rest and bucket spinning), would be kinematically equivalent but (as the phenomena of centrifugal force, or Newton's experiment of putting water in the bucket, show) dynamically inequivalent; so in this case the general principle of equipollence of hypotheses would indeed be broken by circular motion. In fact, however, because there is no perfect rigidity, *the two systems in question here are not even kinematically equivalent*: the rotating earth has—necessarily—*a different shape*, that is a different internal geometrical configuration, from that of a nonrotating earth; and the same will be true of the bucket *or of any other body*. Therefore, rotating systems are not examples of dynamically inequivalent *but kinematically equivalent* ones, and the principle of equipollence is unbroken. It seems to me indeed possible that Leibniz's reasoning was something like this; it is, at any rate, as close as I can come to a solution of the crux I have posed. Yet it is a highly problematic solution. On the basis of the texts known to me, it is (to use Reichenbach's term) a rather fargoing "extrapolation." It leaves unresolved the other difficulties that have been discussed, and therefore fails to absolve Leibniz of serious confusion. And so far as the crucial issue itself is concerned, it falls short in two ways: First, although this line of argument depends upon there being no true *rigidity*, it does not in any clear way exclude some sort of true *elasticity* of bodies. And second, for a viable theory to be developed along such lines, it would be necessary to give rules for *determining kinematically*, on the basis of the configurations and changes of configuration of bodies and their parts, those states that are to be taken as "inertial"; but of the existence of such a problem—to say nothing of its solution—there is not a word in the writings of Leibniz. These two defects are not necessarily fatal to the proposal, as an interpretation of Leibniz, for we have found no way to avoid attributing to him a defective theory; but it can claim nothing like the satisfactory status argued for in section II below, in the interpretation there offered of the position of Huygens. We are left, for Leibniz, in an ambiguity of confusions.

6. *Oeuvres complètes de Huygens*, vol. VI, 1895, p. 481.

7. *Ibid.*, p. 327.

8. It will be seen presently that some qualification is required here. The quoted statement of Huygens is not altogether explicit; it is argued below that that statement, taken quite literally, continued to "express Huygens's view" even when his view changed and no longer agreed with Newton's.

9. See Appendix I below for the pertinent passages of the Huygens-Leibniz correspondence. I have previously commented on these matters, and especially on Huygens's theory of relative motion, in my paper "Newtonian Space-Time"; see Robert Palter, ed., *The Annus Mirabilis of Sir Isaac Newton* (Cambridge, Mass.: MIT Press, 1970), pp. 258ff.

10. Appendix III below contains translations of three of these notes of Huygens, from the *Oeuvres complètes*, vol. XVI. The piece published as No. III of this set (*ibid.*, pp. 222–223) is assigned by the editors to the year 1688, since, itself undated, it occurs in a manuscript between pages dated 27 March 1688 and 8 November 1688 respectively. It is tempting (and not impossible) to construct an argument suggesting that No. III is an earlier note than the others here translated, and that their contents involve a significant advance, which may have been made at a date that accords with the statement to Leibniz; but this would be skating on rather thin ice. It is at least as plausible that Huygens, in writing to Leibniz, used the phrase "*2 ou 3 ans*" in the imprecise sense of "a few years." The best reason for believing that these notes represent the view Huygens speaks of in his letters to Leibniz is that they quite certainly do represent a late change in his conception of motion; that their contents are fully compatible with his statements in the letters; and that it is somewhat unlikely that he should have experienced a further fundamental change of views before the letters to Leibniz and

yet not have included in them a hint of the nature of the change—but unlikely to the second order that, if this *were* true, there should remain no manuscript evidence of it. (It was clearly Huygens's habit to ruminate, in his private notebooks, over his theoretical conceptions, and to preserve his notes. For instance, the editors of vol. XVI of the *Oeuvres complètes* tell us that they are printing only a representative selection of the papers dealing with the same new conception of relative motion; and they print eight such pieces.)

11. Cf., e.g., below, Appendix III(a), last sentence (p. 46), and Appendix III(b), pp. 47–48.

12. I had originally written "until the twentieth century"; but I have learned from an article of Jürgen Ehlers—"The Nature and Structure of Spacetime," in J. Mehra, ed., *The Physicist's Conception of Nature* (Dordrecht-Holland: Reidel, 1973), p. 75—that Ludwig Lange advanced essentially this view in 1885; see his paper "Ueber das Beharrungsgesetz," *Berichte über die Verhandlungen der königlich sächsischen Gesellschaft der Wissenschaften zu Leipzig*, Mathematisch-physische Klasse, vol. XXXVII (1885), pp. 333ff.

13. See my paper, already cited (n. 7 above), "Newtonian Space-Time"; and "On the Notion of Field in Newton, Maxwell, and Beyond," in Roger H. Stuewer, ed., *Historical and Philosophical Perspectives of Science*, Minnesota Studies in the Philosophy of Science, vol. V (Minneapolis: University of Minnesota Press, 1970), pp. 264ff.

14. Cf., e.g., the following (already quoted in part), from the letter to de Volder of 20 June 1703 (Loemker, p. 531): "[I]n phenomena or aggregates every new change arises from an impact according to laws prescribed partly by metaphysics, partly by geometry. . . . So any body whatever, taken by itself, is understood to strive in the direction of a tangent, though its continuous motion in a curve may follow from the impressions of other bodies." Also the third letter to Clarke, paragraph 17 (Loemker, p. 684): "I maintain that the attraction of bodies, properly so called, is a miraculous thing, since it cannot be explained by the nature of bodies." (Cf. also the fourth letter to Clarke, paragraph 45; and the fifth letter, paragraphs 112 *et seq.*)

15. See, e.g., his *Traité de la Lumière*, chap. 1, p. 3 of the original edition; *Oeuvres complètes*, vol. XIX (1937), p. 461: "[In considering the production of light and its effects, one finds everywhere] that which assuredly manifests motion [of some matter]; at least in the true Philosophy, in which one conceives the cause of all natural effects by mechanical reasons. Which it is necessary to do in my opinion, or else to renounce all hope of ever understanding anything in Physics."

16. Cited above, n. 14.

17. For the quoted phrase, see *Principia*, Book III, fourth "Rule of Philosophizing."

Of the several passages in which Newton expresses his views about "hypotheses," the one that I consider the clearest and most rounded occurs in a relatively obscure place in his early optical correspondence (letter to Oldenburg for Pardies, 10 June 1672; H. W. Turnbull, ed., *The Correspondence of Isaac Newton*, vol. I [Cambridge: University Press, 1959], p. 164): "The best and safest way of philosophizing seems to be, that we first diligently investigate the properties of things, and establish them by experiments, and then more slowly strive towards Hypotheses for their explanation. For *Hypotheses* should only be fitted to the properties that call for explanation, not made use of for determining them—except so far as they may suggest experiments. And if one were to guess at the truth of things from the mere possibility of *Hypotheses*, I do not see how it would be possible to determine any settled agreement in any science; since it would be always allowable to think up further and further Hypotheses, which will be seen to furnish new difficulties."

18. This is not, of course, to say that Newton was never wrong, or that he was never influenced in his judgment by his theoretical leanings. He was, for example, wrong about the law of double refraction and wrong in asserting the impossibility of correcting the chromatic aberration of lenses; and both these errors arose from too hastily adopted theoretical conclusions. My contention is, rather, that Newton never argued against a rival theory either on the grounds that the phenomena might be otherwise explained, or on the grounds that the theory itself did not admit of "explanation" in some suitable sense—as (for the first part) Hooke attacked Newton's theory of light on the grounds that an explanation in

terms of waves was also possible, Huygens his theory of "mutual" gravitation on the grounds that pressure by an ether might produce gravitation towards a center with no reciprocal action upon that center; and (for the second part) Huygens was reserved towards Newton's theory of the composition of white light, Huygens and Leibniz very adverse to the theory of universal gravitation, on the grounds in each case that a "mechanical" explanation seemed impossible.

19. Ernst Mach, *The Science of Mechanics*, 6th English ed. (La Salle, Ill.: Open Court, 1960), pp. 588–589; *The Analysis of Sensations*, revised English ed. (New York: Dover, 1959), p. 311.

20. *Mechanics*, pp. 588–589: "[T]he mental artifice atom . . . is a product especially devised for the purpose in view. . . . However well fitted atomic theories may be to reproduce certain groups of facts, the physical inquirer who has laid to heart Newton's rules will only admit those theories as *provisional* helps, and will strive to attain, in some more natural way, a satisfactory substitute."

21. See *Mechanics*, pp. 286–287. (The third sentence of paragraph 7 is mistranslated; it should say that the alterations of the mutual distances of remote bodies are proportional *to one another* [not "to those distances"]—i.e., if in two time-intervals, I and II, bodies A and B have distance-changes Δr_{I} and Δr_{II}, and if bodies C and D have distance-changes Δs_{I} and Δs_{II} in these same intervals, and if these bodies are all "remote from one another," then $\Delta r_{\mathrm{I}}:\Delta r_{\mathrm{II}} = \Delta s_{\mathrm{I}}:\Delta s_{\mathrm{II}}$.)

22. See *Mechanics*, pp. 284–285, 293, 340; and Preface to the Seventh German Edition, p. xxviii. (This view seems to be the one Mach held to most firmly; note that it is, for example, restated in the last sentence of the Preface to his last edition.)

23. That this is Mach's intention seems plain from a passage added in a late edition of the *Mechanics* (p. 339), in which Mach suggests that, rather than "refer the laws of motion to absolute space," we may "enunciate them in a perfectly abstract form; that is to say, without specific mention of any system of reference." The import of this last phrase may seem obscure; but the sequel makes clear that it implies in effect an *existential quantification* over reference systems: Mach says that this course "is unprecarious and even practical; for in treating special cases every student of mechanics looks for some serviceable system of reference."

24. *Mechanics*, p. 284. (Thus all three views appear in pp. 284–287.)

25. For a discussion of this point, see my paper (already cited—n. 7) "Newtonian Space-Time."

26. Two astronomical phenomena provide optical evidence of the earth's annual motion relative to the fixed stars: the aberration of starlight and stellar parallax. Aberration was discovered in 1725 by James Bradley, and was explained by him in 1727 (the year of Newton's death). Stellar parallax, sought for since (at least) the time of Tycho Brahe, was not detected until 1838 (by F. W. Bessel).

27. Proposition XI of Book III of the *Principia*, which states "that the common center of gravity of the earth, the sun, and all the planets, is immovable," is asserted under the hypothesis—expressly so labeled—"that the center of the system of the world [i.e., solar system] is immovable": in other words, that there exists a point, definable by its geometrical relations to the earth, sun, and planets, which is immovable. Newton's argument for Proposition XI is that, by the Laws of Motion, the center of gravity must be either at rest or in uniform rectilinear motion; but if the center of gravity has a constant velocity different from zero, there clearly cannot be any fixed point in the system at all; so the hypothesis requires that the center of gravity be at rest. He immediately infers (Proposition XII) that the sun itself is in continual motion. Of course, the whole argument assumes that the solar system is dynamically closed. On this point Newton comments further, in his subsequent, less formal work *The System of the World*, section 8 (see *Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His System of the World*, ed. Florian Cajori [Berkeley, California: University of California Press, 1946], p. 558): "It may be alleged that the sun and planets are impelled by some other force equally and in the direction of parallel lines; but by such a

force (by Cor. VI of the Laws of Motion) no change would happen in the situation of the planets one to another, nor any sensible effect follow: but our business is with the causes of sensible effects. Let us, therefore, neglect every such force as imaginary and precarious, and of no use in the phenomena of the heavens. . . ."

28. See the introductory paragraphs of Helmholtz's paper "Ueber die Thatsachen, die der Geometrie zum Grunde liegen," in his *Wissenschaftliche Abhandlungen*, vol. II (Leipzig: Johann Ambrosius Barth, 1883), pp. 618ff.

29. Some further comments about the theorem of Helmholtz and Lie seem to be called for:

(1) It was known to Riemann, and clearly stated in his *Habilitationsvortrag*, that "free mobility" of figures "without distension" is possible, in a connected manifold with Riemannian metric, if and only if its curvature is uniform (but see (3) below for a qualification of this statement). What is distinctive in the result of Helmholtz is, therefore, not the *sufficiency* of the condition "Riemannian metric of uniform curvature," but the *necessity* of the condition "Riemannian metric," for such free mobility.

(2) However, the sense of this "necessity" requires elucidation (for I have found, in discussing the point with colleagues, that the mathematical facts are less well known than I should have supposed). The work of Lie and his successors has shown that there are several theorems, not just one, "of Helmholtz type"; what they have in common is the following: One defines a certain condition, C, upon a manifold, involving a class of mappings (to be called "rigid displacements"); and proves that if the condition C is satisfied, then *there is an essentially unique Riemannian structure on the manifold for which the already postulated "rigid displacements" are precisely the isometries of the Riemannian metric.* ("Essentially unique" here means unique up to a constant positive factor—i.e., up to the choice of a "unit of length.") The clause in italics is, therefore, a necessary condition for the condition C to hold.

I have been asked whether this result truly establishes "necessity" that a manifold with condition C *be* Riemannian in structure—or whether it merely shows that such a manifold "admits" a suitable Riemannian structure. The issue is perhaps in part merely verbal; but these points should be noted: (a) The *essential uniqueness* stated in the theorem means, in effect, that if one is "given" a manifold M with "rigid displacements" satisfying the condition C, one is *ipso facto* "given" a Riemannian structure upon that manifold. (b) In ordinary Euclidean geometry, axiomatized with the help of the relation of "congruence," a metrical structure is determined in no other sense than this: the length or distance, satisfying the Pythagorean theorem, is *definable* (up to the choice of a unit) *in terms of the axiomatized relations*, in precise analogy to the Helmholtz-Lie situation. (c) What is perhaps the central thing, from the point of view of Helmholtz, is this: A careful reflection on the proof(s) of the Helmholtz-Lie theorem(s) will show that the Riemannian structure whose existence and (essential) uniqueness is thereby established is *the* metrical structure that is obtained through the "ordinary, standard procedures of measurement" using "freely mobile rigid bodies" as measuring instruments. (d) It might be asked, beyond this, whether there exist, under the stipulated conditions, besides the (essentially unique) Riemannian metric, still other—non-Riemannian—metric structures for which the "rigid displacements" are the isometries. The answer to this question depends in part upon the generality allowed to the notion of a "metric." If one only demands satisfaction of the standard "metric space" axioms for a distance-function, then there are indeed non-Riemannian metrics admitting the same isometries as the Riemannian one (and, in the Euclidean case, *there exist "non-Pythagorean" metrics whose isometries are exactly the Euclidean congruences*). This follows easily from the fact that if d is a distance-function on a set, and if f is an arbitrary *monotonically increasing* and *concave* real-valued function on the non-negative real numbers, with $f(0) = 0$, then the composite function $f \circ d$ is again a distance-function (and has the same isometries as d). But in the spirit of differential geometry, a natural specialization of the notion of a metric is obtained as follows: First, for an *arbitrary* metric space, one can define the notions of a "rectifiable curve" and of the "length" of such a curve in a straightforward way. It is then

always true that the length of a rectifiable curve from p to q is greater than or equal to the distance from p to q. Let us call a *metric* "rectifiable" if for each pair of points p and q there exists a rectifiable curve from p to q whose length is as close as one likes to the distance from p to q—i.e., if the distance from p to q is the *greatest lower bound* of the lengths of curves from p to q. (Thus "rectifiability of the metric" entails that the metric space in question is pathwise connected.) Now we can assert that, under "Helmholtz-Lie conditions" C on a connected manifold, *the only* RECTIFIABLE *metrics whose isometries are the "rigid displacements" are the Riemannian metrics, unique up to a unit of length, given by the theorem of Helmholtz and Lie.*

(3) In the older literature of geometry, authors were notoriously careless of the distinction between "local" and "global" structures; and this leads to some difficulties in our present context. Thus in the work of Helmholtz and Lie—and, indeed, in all the later work known to me on the "Helmholtz-Lie problem"—the notion of "rigid displacement" is taken to mean a certain class of mappings *of the whole manifold onto itself*. But if the notion is so construed, it is not true that uniform Riemannian curvature is a sufficient condition for free mobility under rigid displacement: there are required, in addition, certain "global" conditions on the manifold (finiteness of the fundamental group, and metric completeness). Moreover, from the quasi-epistemological point of view from which Helmholtz began, based upon considerations of spatial *measurement*, this "global" construction of the notion of rigid displacement is inappropriate: for purposes of measurement, one does not carry around the whole space—one carries around only a small measuring body. It therefore seems worthwhile to give here a strictly local form of the Helmholtz-Lie theorem (which does in fact single out all and only the Riemannian manifolds of constant curvature).

We suppose given, then, a connected n-dimensional differentiable manifold M, and a class R of mappings (the "rigid displacements"), each of which is *a diffeomorphism into* M, *defined on a domain* (i.e., connected open set) in M. These are required to satisfy the following conditions:

(a) "Local character" of rigid displacement: A diffeomorphism f defined on a domain U is in R if and only if for each p in U there is a neighborhood V of p such that for any domain W contained in V the restriction of f to W is in R.

(Intuitively: a diffeomorphism f is a rigid displacement on U if and only if it is a rigid displacement on all small enough parts of U.)

(b) "Group-like properties" of the set of rigid displacements: If f, defined on U, is in R, then f^{-1} (defined on the domain $f(U)$) is in R; if f, defined on U, and g, defined on $f(U)$, are both in R, then so is the composite mapping $g \circ f$ (defined, again, on U).

To facilitate formulation of the requirement of "free mobility," it is convenient to introduce the notion of a "flag" on the manifold M: namely, a sequence $a = (a_0, a_1, \ldots, a_{n-1})$, with a_0 a point ("0-dimensional subspace") of M, a_1 a 1-dimensional subspace of the tangent space to M at a_0, a_2 a 2-dimensional subspace of that tangent space *containing* the 1-dimensional subspace a_1, and so forth up to the $(n-1)$-dimensional subspace a_{n-1} (which contains all its predecessors). The flag $a = (a_0, a_1, \ldots, a_{n-1})$ is called a flag "at the point a_0." The set of all flags on M has itself a natural manifold-structure (of $n(n+1)/2$ dimensions), and the set of all flags at a fixed point a_0 is a submanifold (of $n(n-1)/2$ dimensions).

(c) "Condition of free mobility": For every pair of points (p, q) of M, there exist domains U, V—neighborhoods of p and q respectively—such that:

(i) "Freedom" and "rigidity": For every flag a at p and every flag b at a point in V, there exists a unique $f_{(a,b)}$ in R, defined on U, under which a is carried to b.

(Intuitively: *some* ["small enough"] body at p [namely, U] can be moved freely everywhere near q, and rotated every which way; but given the "where" and the "way"— specified by the flag b—the displacement is rigidly determined.)

(ii) "Continuity": For a fixed flag a at p, the mapping $(b, v) \rightarrow f_{(a,b)*}(v)$ (with b a flag at a point in V, v a tangent vector at a point in U, and $f_{(a,b)*}$ the "induced" mapping on the tangent bundle) is continuous.

(In effect, this can be construed as saying that if b' is near b, the rigid displacement that takes a to b' is near the one that takes a to b, where "nearness" of the differentiable maps is measured by their effect both upon points and upon tangent vectors.)

This completes the exposition of our "local Helmholtz-Lie condition C"; the conclusion now follows, as already stated in (2): that there exists an essentially unique Riemannian metric on M, such that a diffeomorphism f of a domain U into M is in R if and only if f is a Riemannian isometry.

The original proof of this theorem—in its weaker, therefore *easier*, "global" version—proceeded by a rather formidable induction on the dimension of M, using properties of the projective spaces. With more modern techniques, however, a conceptually rather straightforward proof is possible, and a sketch of the proof will now be given: We first choose a fixed point p, and take $q = p$ in (c) above. Fixing also a flag a at p, we write g_b instead of $f_{(a,b)*}$; then g_b is a nonsingular linear transformation on the tangent space at p, and by (c) (i) and (ii) the mapping $b \rightarrow g_b$ is a 1-1 continuous map of the flag-manifold at p into the Lie group of all such transformations. Since the flag-manifold at a point is compact, so is its image set in the linear group. But using (a) and (b), one easily shows that the image set is a *subgroup* of the linear group, and is therefore itself a *compact Lie group*. By the standard technique of "averaging" or "integration" over the action of the group, one can then define a *positive-definite quadratic form on the tangent space at p, invariant under all "rigid rotations"*; and it follows easily from (c) (i) that this form is unique up to a constant positive factor. Finally, the rigid displacements "from p to q" can be used to "transport" this form over the entire manifold, giving *a Riemannian structure invariant under all the (local) rigid displacements*; and with this, since "essential uniqueness" follows at once from the construction, the proof is complete.

(4) Our formulation of the condition of free mobility in (3) depended essentially upon the *differentiable* structure of the manifold M; for only this structure gives us the notions of "tangent space" and "flag." Lie gave a second version of the (global) Helmholtz theorem, in which free mobility is expressed without such an "infinitesimal" construction. Nevertheless, Lie's second theorem also involves assumptions of differentiability, because these were presupposed in the very foundations of his theory of "continuous groups of transformations," which provided the tools for his attack upon Helmholtz's problem.

The motive of eliminating all explicit differentiability conditions from the Helmholtz-Lie foundations of geometry has played a noteworthy role in the subsequent history of mathematics; for it was the direct inspiration of the celebrated "Fifth Problem" of Hilbert—the problem *to what extent assumptions of differentiability can be dispensed with in the theory of Lie groups*. This was fifth in the list of twenty-three problems posed by Hilbert at the International Congress of Mathematicians in Paris in 1900; its complete solution—showing that the differentiability assumptions in question can be dispensed with *entirely*—was obtained (after a number of preliminary advances by a number of illustrious mathematicians) in 1952 by Andrew M. Gleason, Deane Montgomery, and Leo Zippin. Hilbert himself introduced this problem in explicit connection with the Helmholtz-Lie foundations of geometry (see his *Gesammelte Abhandlungen*, 2nd ed. [Berlin: Springer-Verlag, 1970], vol. III, p. 304); and when, in 1902, in a paper "Über die Grundlagen der Geometrie" (*Mathematische Annalen* 56 (1903), pp. 381–422; also published as Anhang IV in his book, *Grundlagen der Geometrie*, 7th ed. (Leipzig: B. G. Teubner, 1930), Hilbert gave an axiomatization of geometry in two dimensions on the basis of the group of motions without any assumptions of differentiability, he expressed the view that this work "answers, for the special case of the group of motions in the plane, a general question concerning group theory, which I have posed in my lecture 'Mathematical Problems,' Göttinger Nachrichten 1900, Problem 5." The generalization of this geometrical result to n dimensions finally became possible as a consequence of the results of Gleason, Montgomery, and Zippin—cf. the article of Hans

Freudenthal, "Neuere Fassungen des Riemann-Helmholtz-Lieschen Raumproblems," *Mathematische Zeitschrift* 63 (1955–56); so the solution of Hilbert's group-theoretic problem did lead to the geometrical result he had hoped for.

30. Departures from strict translation occur only in the material within brackets.

31. But it should be noted that in one important point Riemann's anticipation has not proved correct: despite great efforts to account for the structure of the physical world "in the small" with the help of the space-time curvatures, no satisfactory account of this kind has been achieved; i.e., we have no evidence of strong fluctuations of curvature in regions of microscopic scale, averaging out on the scale of ordinary bodies, such as Riemann foresaw. Instead it is after all "in the large" that we have come to know phenomena which "admit of simpler explanation" through the assumption that "the measure-relations . . . are not in accord with the assumptions of [ordinary] geometry."

Appendix

For convenience of reference, there are added here translations of passages from the writings of Huygens and of Leibniz which may not otherwise be easily accessible.

I. From the Correspondence of Huygens and Leibniz.

(a) From a letter of Huygens to Leibniz, dated 29 May 1694:

I shall not touch this time on our question of the void and of atoms, having already been too lengthy, against my intention. I shall only say to you that I have noticed in your notes on des Cartes that you believe it *to be discordant that no real motion is given, but only relative*. Yet I hold this to be very sure, and am not checked by the argument and experiments of Mr. Newton in his Principles of Philosophy, which I know to be in error; and I am eager to see whether he will not make a retraction in the new edition of this book, which David Gregorius is to procure. Des Cartes did not sufficiently understand this matter.

(b) From a letter of Leibniz to Huygens, dated $\frac{12}{22}$ June 1694:

As to the difference between absolute and relative motion, I believe that if motion, or rather the moving force of bodies, is something real, as it seems one must acknowledge, it is quite necessary that it have a *subject*. For, a and b moving towards one another, I maintain that all the phenomena will occur in the same way, in whichever of them one posits motion or rest; and if there were 1000 bodies, I remain convinced that the phenomena could not furnish to us (nor even to the angels) an infallible criterion for determining the subject of motion or its degree; and that any of them could be considered by itself as being at rest; and this I believe is all that you ask. But (I believe) you will not deny that in truth each has a certain degree of motion—or, if you will, of force—notwithstanding the equivalence of hypotheses. It is true that I infer this consequence, that there is in nature some other thing than what Geometry can there determine. And among many arguments of which I make use to prove that, besides extension and its variations (which are purely geometrical things), it is necessary to recognize something higher, namely force, this one is not the least. Mr. Newton recognizes the equivalence of hypotheses in the case of rectilinear motions; but in respect of the circular ones, he believes that the effort of circulating bodies to increase their distance from the center or axis of circulation manifests their absolute motion. But I have reasons that make me believe that nothing breaks the general law of equivalence. It seems to me nevertheless that you yourself, Monsieur, were formerly of the sentiment of Mr. Newton with respect to circular motion.

(c) From a letter of Huygens to Leibniz, dated 24 August 1694:

. . . As to what concerns absolute and relative motion, I am amazed at your memory—that you recall that I used to be of Mr. Newton's opinion in regard to circular motion. Which is so, and it is only 2 or 3 years since I have found what is truer—from which it seems that you too are now not far, except that you would have it, when several bodies are in mutual relative motion, that they have each a certain degree of veritable motion, or of force; in which I am not at all of your opinion.

(d) From a letter of Leibniz to Huygens, dated $\frac{4}{14}$ September 1694:

. . . When I told you one day in Paris that one would be hard put to it to know the veritable subject of motion, you answered me that this was possible by means of circular motion, which gave me pause; and I recalled it in reading almost the same thing in the book of Mr. Newton; but this was when I already believed that I saw circular motion to have no privilege in this respect. And I see that you are of the same opinion. I hold therefore that all hypotheses are equivalent, and when I assign certain motions to certain bodies, I neither have nor can have any other reason than the simplicity of the hypothesis, believing that one may take the

simplest (all things considered) for the true one. Having thus no other criterion, I believe that the difference between us is only in the manner of speaking, which I seek to accommodate to common usage as much as I can, *salva veritate*. I am even not far from your own, and in a little paper that I sent to Mr. Viviani and which seemed to me suited to persuade Messrs. of Rome to license the opinion of Copernicus, I accommodated myself to it. Nevertheless, if you have these opinions about the reality of motion, I imagine that you must have opinions about the nature of bodies different from the customary ones. I have on this subject very singular views, which seem to me demonstrated. . . .

II. From Part II, Section 4 of Leibniz's *Dynamica*.

Proposition 19.

The Law of Nature that we have established of the equipollence of hypotheses—that a Hypothesis once corresponding to present phenomena will then always correspond to subsequent phenomena—is true not only in rectilinear motions (as we have already shown), but universally: no matter how the bodies act among themselves; but provided that the system of bodies does not communicate with others, i.e., that no external agent supervenes.

[Note: The explicative material that follows the colon might belong instead to the clause set off by dashes: i.e., it may either (as put above) amplify "universally," or further explain the notion of "equipollence of hypotheses" itself; the Latin is entirely ambiguous on this point.]

This is demonstrated from prop. 16 [note: there is no Proposition 16(!) —Proposition 17 is evidently intended; or rather, all propositions printed by Gerhardt with numbers greater than 16 should have their numbers reduced by 1, so that the present one should be Proposition 18], namely that all motions are composed of rectilinear uniform ones, for which the thing is so by prop. 14. But the same is demonstrated in another way from the general Axiom, that of those things whose determinants cannot be distinguished, the determinates cannot be distinguished either. And so, since in the cause or antecedent state the diverse hypotheses cannot be distinguished, namely insofar as the bodies are carried by free rectilinear motions, they clearly cannot be distinguished either, in any way, in the effects or subsequent states; nor, therefore, in collisions or any other events, even if some motions are perhaps converted from rectilinear to circular through the cohesion or solidity of bodies, or through restraining cords. Since, therefore, all motions—even circular or other curvilinear ones—can arise from preceding rectilinear uniform motions, changed into curvilinear ones perhaps by thrown cords; and since a motion once given, no matter how it was first produced, ought now to have the same outcome as another that is in all ways like it, even though otherwise produced; therefore in general Hypotheses can be distinguished in mathematical rigor by no phenomena ever. Universally, when motion occurs, we find nothing in bodies by which it could be determined except change of situation, which always consists in relationship. Therefore motion by its nature is relative. And these things are understood with Mathematical rigor. However, we ascribe motion to bodies according to those hypotheses by which they are most aptly explained; nor is a hypothesis true in any other sense than that of aptness. Thus, when a ship is borne on the sea in full sail, it is possible to explain all the phenomena exactly, by supposing the ship to be at rest and devising for all the bodies of the Universe motions agreeing with this hypothesis. But although no mathematical demonstration could refute this, it would still be inept. I remember, indeed, that a certain illustrious man formerly considered that the seat or subject of motion cannot (to be sure) be discerned on the basis of rectilinear motions, but that it can on the basis of curvilinear ones, because the things that are truly moved tend to recede from the center of their motion. And I acknowledge that these things would be so, if there were anything in the nature of a cord or of solidity, and therefore of circular motion as it is commonly conceived. [But] in truth, if all things are considered exactly, it is found that circular motions are nothing but compositions of rectilinear ones, and that there are in Nature no other cords than these laws of motion themselves. And therefore if ever the equipollence of hypotheses is not apparent to us it is because sometimes all events are not apparent, on account of the imperceptibility of the ambient bodies; and often some system of bodies seems not to be communicating with others, although the contrary is the case.

Moreover (what is worth mentioning), from this single principle, that motion by its nature is relative and therefore all hypotheses that once agree produce always the same effects, it would have been possible to demonstrate the other laws of Nature expounded so far.

Proposition 20.

The solidity or cohesion of the parts of bodies arises from the motion or tendency of striking of one body against another.

For (by prop. 17) all motions are rectilinear uniform ones compounded together. But if the solidity of bodies comes from anything but composition of motions, rotation too will derive from something other than composition, namely from that very necessity by which it follows from the hypothesis of solidity. And so indeed if a straight line that is corporeal or endowed with density and is solid, LM, is struck simultaneously in its extremities L and M, with equal respective force of contrary motions AL, BM, by bodies A and B, it is necessarily, by the advance of the bodies, put in rotation about its midpoint N; but in this way matter near L or M tending to recede from the center N will be retained solely by the solidity of the body, not by contrary impressed motion; and, therefore, this circular motion does not consist in a composition of rectilinear ones, unless we explain that solidity by a certain motion of pressing. The same is shown from prop. 19, which we have demonstrated not only from prop. 17 but from another different ground; and from this conversely prop. 17, together with the present 20, would (in a certain regress) be demonstrated from prop. 19 in another way than above. Doubtless, since it is shown in prop. 19 through the relative nature of motion that hypotheses are indiscernible, it cannot be known whether some particular body is rotated; but if we posit solidity, and therefore rotations not derived from the composition of rectilinear motions, a criterion to discern absolute motion from rest is given. Indeed, let body ACB rotate about its center C, near the row of points ADB [which points are themselves disposed in a circle about C as center], and now suppose the solidity of the body to be dissolved so that its extreme part A is separated by the breach of the connection: it will go along the straight [tangent] line towards E, if the motion of the body was a true one [note: "*versus*," in Gerhardt's text here, is an obvious error for "*verus*"]; if it was merely apparent, part A will stay with the remainder of the body ACB, notwithstanding the dissolution of the connection. And so we should possess a necessary ground of discerning true motion from apparent, against prop. 19. And this will not be avoided, unless the solidity of the body ACB arises from a pressing in of the bodies around it. Since, then, in this way all motions are rectilinear, and no other rotation has come about than a certain determinate composition of rectilinear motions; and since in purely rectilinear motions, speaking absolutely and of geometrical necessity, hypotheses cannot be discerned from one another (by prop. 19); it follows that they cannot be discerned in rotations either. But let us show more distinctly in what way a certain rotation about a center and a pressing in of bodies would arise from the sole impression of rectilinear tendencies. Indeed, let the *mobile* A be going in the direction and with the speed represented by the indefinitely small elementary straight line ${}_1A_1\alpha$; but let the tendency of the surrounding bodies be continually driving the *mobile* A towards the center C, so that it always keeps the same distance from the latter (namely because otherwise the present motion of the surrounding bodies is disturbed), . . . [there follows here a straightforward account (only slightly obscured by some notational errors in the letters referring to Leibniz's diagram) of uniform circular motion as the result of a suitable combination of tangential velocity and a continual "pressing in" (treated as a sequence of very small impulsive forces) towards the center; concluding:] And so from motion that is *per se* uniform rectilinear, but changed into circular by an added *tendency towards a center*, there arises a circulation also uniform; which is noteworthy, and agrees with experiments. We have therefore explained the conversion of rectilinear motion into circular by compositions of rectilinear tendencies—on which basis alone the equipollence of Hypotheses can be satisfied.

It is certain that the cause of cohesion is to be explained from these things that we understand of bodies—such as are magnitude, figure, rest or motion. But besides motion there is nothing that makes a boundary in a thing.

For let there be a body ABC, whose part AB, struck by a blow coming in the line DE, does not leave BC in its former place but moves with it; the reason of this *dragging* is sought. And for instance if we wish to reduce it to *pushing* by conceiving certain hooks of the body AB to be inserted in handles of the other body BC, or if we imagine certain ropes or fibrous webs or other tangled textures, we have accomplished nothing; because it is asked in return what, then, connects the parts of the fibers or hooklets. But contact alone, or rest of one beside another, or common motion, surely does not suffice; for it cannot be understood why one body drags another from this alone, that it touches it. And universally we understand no reason why a body is moved except this, that two bodies cannot be in the same place, and hence if one is moved then those others also must be

moved into whose place it enters. We have demonstrated the same in this place from the laws of Nature. And just as, from the law that change cannot be by a jump, we have shown all bodies to be flexible, or Atoms not to be given; so from the posited general law of Nature, that phenomena must proceed in the same way whatever hypothesis is made concerning the subject of motion, we have shown solidity to arise in no other way than from the composition of motions. If indeed some derive the solidity of bodies from the pressure of the air or ether, on the analogy of two polished tablets which are separated with difficulty, then although this is in some ways true, yet it does not explain the first origins of solidity or cohesion; for there remains the question of the very solidity or cohesion of the tablets. Since, therefore, a mass of matter cannot be discriminated except by motion, it is manifest that the ultimate grounds of the solidity of both the larger and the smaller ones must be sought in this alone.

III. From the Notes of Huygens on the Nature of Motion.

The following are among the notes published by the Société hollandaise des Sciences, in vol. XVI of the *Oeuvres complètes de Christiaan Huygens*, under the heading of "Pièces et Fragments concernant la Question de l'Existence et de la Perceptibilité de 'Mouvement Absolu'."

(a) No. III, assigned to the year 1688 on the basis of its position in the manuscript; from the Latin:

All motion and rest of bodies is relative. Nor without mutual reference of bodies can something be said or understood to be moved or to rest.

For they err who imagine certain spaces unmoved and fixed in the infinitely extended world—whereas that immobility cannot be conceived except with reference to a resting thing.

But the parts of a body can be moved with reference to one another (which is called whirling motion), preserving their distance on account of a bond or an obstacle: on account of a bond, as in the case of a top or the composite of two bodies connected by a cord; on account of an obstacle, as in the case of water swirled in a round vessel.

In this motion the parts tend to recede from one another or from a point defined with reference to them, and this with the greater force the greater is their relative motion. Whence, moreover, judgment can be made of the quality of this relative motion, when it cannot be made from change of distance.

Bodies which are moved with reference to one another are moved truly.

Between two bodies motion is produced by impelling either of them. And the same motion can be produced, whichever of the two is impelled; even though a smaller force is needed if the smaller of the two is impelled.

Any body continues its once received speed with reference to others, which are regarded as at rest, uniformly and along a straight line with reference to those other bodies.

Of rest we have no idea except through relation of bodies.

(b) No. IV, no date determined; from the French:

It must therefore be understood that one knows that bodies are mutually at rest, when being free to move separately, and in no way bound or held together, they maintain their mutual position. Thus if several balls are put on a smooth table and if each remains motionless in its place on the table, then they are at rest among themselves and with respect to that table. I have said that they must be free to move separately because they might maintain their place, being bound together or attached to the table, and yet be in motion among themselves—which may seem strange; but it is in this that the nature of circular motion consists, which occurs when two or more bodies, or the different parts of a single body, are impelled to move in different directions, and their separation is prevented by the bond that holds them together—so that it is relative motion among these bodies or among the parts of a single body, with continual change of direction, but with constancy of distance on account of the bond.

As when two balls A and B, held together by the thread AB, and being mutually at rest (which is judged, according to what has been said, by their rest in relation to other bodies that are free to move and that yet maintain their own position and distance)—if A is pushed towards C and B towards D, the lines AC and BD being perpendicular to AB and in a single plane and the impulsions equal [and oppositely directed (Huygens's diagram shows C and D on opposite sides of the line AB)], then these bodies will move in the circumference of a circle of diameter AB, to wit with respect to the bodies among which A and B were previously at rest. [Note: the anacoluthon is in the original.] Thus A and B will have motion among themselves, that is to say in relation to one another, yet without their mutual position or distance changing.

Without one's being able to say how much the one and the other have

of that motion which one commonly calls veritable, and without there being this veritable motion at all—it being nothing but a chimera, and based on a false idea.

It is the same with a single body, e.g. a wheel or globe; except that in the parts of such a body there are all sorts of different directions, not only in parallel lines as here. Now this circular motion is known either by relation to neighboring bodies that are mutually at rest and free; or by the centrifugal force that causes the tension of the thread that binds 2 bodies together—and so their circular motion would be known even if these other bodies did not exist at all. Or else, if there is only one body that rotates, the rotation causes the projection of some bodies that one might place on it; as, if it were a turning table, balls that one put on it outside the center would promptly flee and leave it. And in rotating water in a circular vessel it causes the elevation of the water toward the edges.

One knows by this that the fixed stars are mutually at rest and have received no impulsion at all to go around, because [if they had] they would separate—unless they are stuck in a solid sphere as some people used to believe. Consequently the Earth has received that [rotatory impulsion]. As one knows in another way by the clocks—that is to say, that the earth flings off more strongly toward the Equator.

Now in the circulation of 2 bodies bound by the thread AB one knows that they have received impulsion which has produced their mutual relative motion or direction; but one cannot know, by considering them alone, whether they were pushed equally, or whether only one was pushed. For if A alone had been pushed, the circular motion and the tension of the thread would have followed all the same, although the circle would then have a progressive motion with respect to the other bodies at rest.

That I have therefore shown how in circular motion just as well as in free and straight motion there is nothing but what is relative—in such a way that that is all there is to know [*connoitre* (H.'s orthography)—i.e., detect or recognize] about motion, and also all that one has any need to know. . . .

They say, we cannot perhaps know in what motion consists, but know only that a body which has received impulsion is moved. I reply that since we have the Idea of motion no otherwise than from change of situation of some body, or of its parts (as in circular motion), toward other bodies, therefore we are unable to imagine motion except by conceiving that change of situation to occur; because motion cannot be conceived to which the idea of motion does not conform. [Note: the last sentence is, in the original, in Latin—which lends it an aspect of enhanced formality.]

(c) No. VIII, no date determined; from the Latin:

Motion is merely relative between bodies.

It is produced by impression in either of them or in both; but, motion once effected, it cannot be discerned in which of them impression has been made. Indeed, absolutely the same effect results from either impression.

True and simple motion of any one whole body can in no way be conceived—what it is—and does not differ from rest of that body.

I long believed that a κριτηριον of true motion is to be had in circular motion, from centrifugal force. For indeed, as to other appearances, it is the same whether some disk or wheel standing next to me is rotated, or whether, that disk standing still, I am carried about its periphery; but if a stone is placed on the circumference it will be projected if the disk is turning—from which, I considered, that circumference is now to be judged to be moved and rotated truly, and not just relatively to something. But that effect manifests only this: that, impression having been made in the circumference, the parts of the wheel have been impelled in different directions by motion relative to one another. So that circular motion is relative [motion] of parts excited in contrary directions but constrained on account of a bond or connection. But can two bodies whose distance remains the same be moved relatively to one another? Indeed, in this way: if increase of distance is prevented. Contrary relative motion [then] truly obtains in the circumference.

It can be discerned whether a straight rod is moved freely and all in one direction (or is at rest, for that is the same thing), or whether its parts have received the impression of contrary motions. . . .

Most consider motion of a body true when it is carried from a determinate and fixed place in cosmic space. Wrongly. For since space is infinitely extended on all sides, what can be the definiteness or immobility of a place? Perhaps they will declare the fixed stars in the Copernican system to be at rest. They are indeed unmoved among themselves; but, taken all together, in respect of what other body will they be said to rest, or how will they differ *in re* from bodies most rapidly moved in some

direction? Accordingly a body can neither be said to rest nor to be moved in infinite space, because rest and motion are merely relative.

Rightly enough Descartes, article 29 of the second part. Except that he says the same force and action is required whether AB is carried from the neighborhood of CD or the latter from the neighborhood of the former. Which is then indeed true when AB is equal to CD, but otherwise not at all. Wrongly, too, he defines motion of a body as relative to those immediately touching it. For why not likewise those farthest away?