# Some reflections on the structure of our knowledge in physics

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I want to use this occasion to make some remarks of a rather general kind about the character of our knowledge in physics. I do this with some diffidence: I run the risk that what I shall say may seem—may indeed be—largely platitudinous. But there are some points that seem to me important, even if obvious; and also seem, even if obvious, to be not widely recognized, or not held firmly in view, in current philosophical discussion; so I have decided that the risk is worth taking.

That already has something of the air of the introduction to a sermon; sanctimoniousness may be another risk. But let me nevertheless hazard a rough diagnosis of the reason why some things that are (in my view) true, important, and obvious tend to get lost sight of in our discussions. I think "lost sight of" is the right phrase: it is a matter of perspective, of directions of looking and lines of sight. As at an earlier time philosophy was affected by a disease of system-building—the *ésprit de système* against which a revulsion set in toward the end of the last century—so it has (I believe) in our own time been affected by an excess of what might be called the *ésprit de technique*. I see this as having two chief kinds of manifestation. One has to do with details: a tendency both to concentrate on such matters of detail as allow of highly formal systematic treatment (which can lead to the neglect of important matters on which sensible even if vague things can be said),[1] and (on the other hand), in treating matters of the latter sort, to subject them to quasi-technical elaboration beyond what, in the present state of knowledge, they can profitably bear. The second principal manifestation lies in the way we treat the efforts of our forebears and contemporaries: namely, we often discuss their work less in the hope of drawing instructive insight from it than as a source of doctrines to analyze, contrast, elaborate, or destroy—in any case, to serve as material for the further exercise of technique.

Of course I do not think that such is the deliberate practice of philosophers; nor do I intend to devote this talk to the presentation of an indictment. But let me say just a little more about the matter in general; for it seems to me to present certain instructive ironies.

[1] Wittgenstein's famous aphorism, "Was sich überhaupt sagen läßt, läßt sich klar sagen," although inspiring is unfortunately false; for the maxim he bases on it I would propose a more modest one: not "Wovon man nicht reden kann, darüber muß man schweigen," but "Wovon man nichts beleuchtendes zu sagen findet, darüber schweige man lieber!"

In the first place, what I have described can be characterized rather precisely as a species of scholasticism—which is about as far as may be imagined from what the advocates of a new spirit of philosophy intended to stimulate. In so far as the word "scholasticism," in its application to medieval thought, has a pejorative connotation, it refers to a tendency to develop sterile technicalities—characterized by ingenuity out of relation to fruitfulness; and to a tradition burdened by a large set of standard counterposed doctrines, with stores of arguments and counterarguments. In such a tradition, philosophical discussion becomes something like a series of games of chess, in which moves are largely drawn from a familiar repertoire, with occasional strokes of originality—whose effect is to increase the repertoire of known plays. This was especially unfortunate in the later middle ages, when (in particular in natural philosophy) potentially very fruitful new ideas were introduced—which, however, remained as mere curiosities among the opinions of the commentators on the physics of Aristotle.[2]

On the other hand, what I am speaking of can also be regarded as itself a kind of *ésprit de système*: "local," one might say, and technical, in contrast with the global and "romantic" mentality of the nineteenth-century "systems." Among the unfortunate results of such practice is the frustration of that hope which so signally characterized our predecessors earlier in this century: the hope for a cumulative and progressive philosophy, to the advance of which many workers would contribute in collaboration among contemporaries and development by successors. Of course, in reaction to that hope of our predecessors, it is now vigorously contended in some quarters not only that the hope for philosophy was a delusion, but that science itself lacks the cumulative and progressive character that had been presumed for it. These doctrines, so far as they concern science, seem to me absurd; I shall therefore not say very much on the subject—although I shall say a little, because the absurdity of such a view of science is one of the things I consider important even if obvious. But if it is conceded that science makes progress, it might still be questioned whether philosophy does or can. One of the main theses I want to defend, with examples, is that philosophy indeed *has* made *very important* progress, but that this is seriously obscured by what I have called problems of perspective.

Now, philosophical progress that is not recognized as such by practising philosophers is clearly progress of a precarious sort; so—acknowledging that my diagnosis may be incorrect—I hope it will seem at least forgivable, believing as I do, that I should come to you to proclaim that the sky is not falling.

The first serious platitude I want to present is this: If Wittgenstein's early standard of clarity is impossible to meet; if the hopes of the logical empiricists for a philosophy built up with the rigor and exactness of mathematics upon a basis that is—if not entirely secure epistemologically—at least entirely precise in both structure and content, have failed; and if nonetheless we do not wish to abandon the attempt to achieve such clarity as is possible, or wish to abstain from the use of rigorous techniques where they are fruitful, then there is an obvious rough distinction that we ought never to lose sight of in philosophical work: namely, what I shall just call the distinction between *presystematic*

[2]Cf. on this subject the very instructive account in Clavelin 1968, ch. 2; especially Clavelin's evaluation of the general character and limitations of the natural philosophy of the fourteenth-century schools of Oxford and Paris (pp. 121ff.).

and *systematic* considerations. Accordingly, I emphasize now that in speaking of the "structure" of our knowledge in physics, I am using the crucial words very broadly: I do not presuppose an exact notion of "structure," and in applying the vague presystematic notion to "our knowledge in physics," I am construing the word knowledge in a wide and ambiguous sense. The reflections I am proposing have as their object (a) our knowledge in physics as an *achieved result*: knowledge as *the knowledge we have of X*; (b) our knowledge as susceptible of *justification* or *defense*—that is, as involving a structure of "evidence" for its asserted contents; and (c) knowlege—science—as (to appropriate a word of Isaac Levi's) an *enterprise*: an activity aimed at increasing our knowledge in sense (a), by means appropriate to the constraints of (b). But, again, this is a presystematic description, and I neither promise nor threaten you with even a sketch of an actual *theory* under these three heads.

With regard to the structure of our achieved knowledge in physics, there is a point that struck me with great force many years ago, in the course of my own attempts to learn something of the subject. To present it to you, it will be useful to refer to an early attempt of Carnap's to give a schematic view of the structure of physical knowledge; I quote from his retrospective description in the Schilpp volume:

> In an article on the task of physics [1923][3] I imagined the ideal system of physics as consisting of three volumes: The first was to contain the basic physical laws, represented as a formal axiom system; the second to contain the phenomenal-physical dictionary, that is to say, the rules of correspondence between observable qualities and physical magnitudes; the third to contain descriptions of the physical state of the universe for two arbitrary time points. From these descriptions, together with the laws contained in the first volume, the state of the world for any other time-point would be deducible, ... and from this result, with the help of the rules of correspondence, the qualities could be derived which are observable at any position in space and time. (CARNAP 1968, p. 15.)

This is familiar logical-empiricist doctrine of the earliest vintage, and it foreshadows much of what continued to be Carnap's view of the matter (of course, the implied Laplacian determinism would not have been maintained after the development of quantum mechanics); in particular, the crucial distinction between the "observational" and the "theoretical" could not be more emphatically posed than in this image of the separate volumes. Carnap immediately remarks: "The distinction between the laws represented as formal axioms and the correlations to observables was resumed and further developed many years later in connection with the theoretical language."

The issue now familiarly associated with that distinction is that of the "theory-dependence" of observations. As a subject for philosophical commentary, this issue continues to present virtually limitless opportunities; and I have felt the temptation to expatiate on the matter here to some degree. But I ask myself, how much profit is now to be gained from such discussion? The matter has been very widely treated. One may hope to put a point more trenchantly than has been done before, perhaps even to

[3]The reference is to the article CARNAP 1923.

find a new turn of argument; but hardly, by subtle technical analysis, to effect a real transformation of the subject.

Instead of something subtle, I want to suggest something crude. In Carnap's Platonic myth of the three volumes of physics, consider what the first and second volumes might look like. I submit that there is no difficulty at all in envisaging the first. Carnap says that it is to contain "the basic physical laws, represented as a formal axiom system." I should not wish to insist on the notion of logical formality, which seems to me to have been overemphasized by the logical empiricists; so let me just substitute the phrase, "a mathematical system." It would be inappropriate to demur that we do not possess a mathematical formulation of all "the basic physical laws"—or even a unified mathematical formulation of all the basic physical laws we know—because Carnap is explicitly presenting what I have just called a "myth": an image of "the ideal system of physics." The first volume is conceived simply as having the form of a treatise on theoretical physics. There are many such treatises in existence, some of them very good indeed; and it is even possible to learn branches of theoretical physics by reading them. This is what I discovered in my student days: I had a strong desire to gain a real understanding of the theory of relativity, both special and general, and after some frustrating attempts came to try Weyl's great work on the subject, *Raum-Zeit-Materie* (WEYL 1923)—from which I had earlier been deterred by a remark I had read of Leon Chwistek's, to the effect that Weyl's book was spoiled by an objectionable philosophy (CHWISTEK 1948, p. 3). The book was a triple revelation for me: it put the physical principles of the special and general theories of relativity—and also, as a preliminary, those of Maxwellian electrodynamics—in what seemed to me an astonishingly clear light; it opened my eyes to a new perspective on mathematics; and, in the process (in view particularly of the fact that the idiosyncrasies of Weyl's philosophy in no way obstructed these clarifications), it altered my conception of what the philosophy of physics could be. At any rate, this is one example among several in my experience of a book that reasonably resembles Carnap's ideal first volume (of course restricted to a more modest scope), and that succeeds not only as a systematic formulation but even as a pedagogical instrument.

When we turn to Carnap's second volume, the situation is drastically different. Carnap says the "phenomenal-physical dictionary" it contains is to make it possible to derive, from the data in the third and the laws in the first, *"the qualities . . . which are observable at any position in space and time."* But nothing remotely like this exists, for however restricted a domain of physics. I shall return to the point; but for now I should like to consider a less demanding alternative to that dictionary: granted that it is possible to learn the principles of parts of theoretical physics from books in which those principles are presented in a systematic mathematical framework, is it analogously possible to learn corresponding parts of *experimental* physics? My own experience has been that it is at the least very much harder. My belief is that it is, in practice today (that is, with the help of the existing literature), very nearly impossible: I have never found a single book on experimental physics comparably instructive with those I have found on physical theory. My suspicion is that it may be impossible even in principle. It is hard, but possible, to learn theory by self-study from books; it is surely much harder to learn experimental techniques without a teacher to help one acquire skills; but what I suspect to be impossible is to learn the principles of experiment without *actual* experience with the relevant *instruments.*

That may seem banal; but what strikes me is that it stands in odd contrast with our clichés about the theory-dependence of observation. In a famous passage Duhem said that, in the case of physics, "it is impossible to leave outside the laboratory door the theory that we wish to test" (DUHEM 1954, p. 182). Technical assistants, however, can be taught to perform experiments, and to report the results of those experiments in usable form, without teaching them the theories those experiments are designed to test. In any case, my point is that, whether or not bringing the theory inside the laboratory door is necessary, it is certainly very far from sufficient.

Now it seems to me that this has a rather interesting consequence, not only for the logical empiricist view of the structure of physical knowledge, but for post-positivist views as well: in a certain sense, in my opinion (here and elsewhere too), the critique of logical empiricism by its opponents has fallen off center. My own view is that in the rough sense Carnap was willing to adopt from the time he abandoned the more primitive versions of the empiricist thesis, there is no great difficulty in defining an "observational" vocabulary: an "observation-language" in Carnap's sense is the language in which we ordinarily conduct the business of daily life, and the only theory it is dependent upon is the theory that there are ordinary objects[4] with such properties as we habitually ascribe to them. There are also systems of concepts of the sort that constitute the framework of fundamental physical theories; so, referring again to my example, I may say that a book like *Raum-Zeit-Materie* demonstrates the existence of theoretical vocabularies distinct from the observational. Thus I argue, on the basis of these crude and banal considerations, that Carnap was right to make and to emphasize this distinction. I also believe that his philosophic career consists to a considerable degree in a series of genuinely instructive attempts to do better justice to the character of the distinction. But I think too that there was a fundamental bar to success along any of the routes Carnap essayed. For he always assumed that "the observation language" is more restricted than, and included in, a *total* language that *includes an observational part and a theoretical part, connected by deductive logical relations.* And this, I think—I do not say by virtue of some basic principle I can identify, but simply, at the present time, *de facto*—is not the case: there is no department of fundamental physics in which it is possible, in the strict sense, to *deduce* observations, or observable facts, from data and theory. So I suggest that the principal difficulty is not that of how to leave the theory outside the laboratory door, but that of how to get the laboratory inside the theory.

Well, how *do* we do it? For of course we do put theory and experiment in relation to one another; otherwise it would be impossible to test theories, and impossible to apply them. It would also, I should add, be impossible to *understand* a theory, as anything but a purely mathematical structure—impossible, that is, to understand a theory *as* a theory of physics—if we had no systematic way to put the theory into connection with observation (or experience). This has been taught us not only by the philosophers we usually call empiricists, but also (for instance) by Kant: "Gedanken ohne Inhalt sind

[4]In delivering this address, I interpolated here, with a gesture at the apparatus in question, the words: "including such objects as microphones"—anticipating an objection that might be raised, and indeed was raised during the discussion period, concerning this point; see the Supplementary Note at the end.

leer, Anschauungen ohne Begriffe sind blind" (KANT 1781/7, p. 51/75). So it might be asked of me—and I did in fact ask of myself—how I succeeded in learning any *physics* from Weyl's book.

The short and simple answer is that Weyl first of all connects his exposition of the new theories he expounds with older physical theories I already knew something of, and secondly describes—I shall say "schematically," and return to comment on this word later—a few experiments that bear critically upon the theories he is developing. But that reply is not very instructive, without some indication of (a) how this is done at all, in view of the difficulties I have claimed lie in the way of drawing logical inferences between theoretical statements and observational ones, and (b) how—or to what extent—it suffices to establish "physical understanding" (Kantian *Inhalt*) for a theory. To enrich the discussion of all this, I want to turn to a much earlier physical theory—not just to an earlier "paradigm," but to what may be called the grand archetype of all that we call physics: namely, the theory presented in the first and third books of Newton's *Principia.* I am going to try to say, in brief compass, what this theory (roughly speaking, in our own terms, the conjunction of Newtonian mechanics and Newtonian theory of gravitation) *is,* as a theory of *a mathematical structure discernible in the world of phenomena, of observations, of experience*—and to do so in a way that adheres to the basic conceptual framework introduced by Newton himself; and also to say something about how both what I have just called the "conceptual framework," and the theory formulated within it, were discovered—or "invented"—by Newton. (Thus I mean to touch upon another aspect of the "structure" of physical knowledge: the question of its *advancement*, or knowledge as an *enterprise.*)

Newton tells us in the preface to the *Principia* that he is proposing in it a certain "method of philosophy" (that is, of natural philosophy: of physics). This method consists in investigating the phenomena of nature—in particular, of motions—with a view to determining what Newton refers to both as "the forces of nature," and "the natural powers"; and it involves the working hypothesis that all natural phenomena result from the action of such forces. Newton says: "[A]ll the difficulty of philosophy seems to consist in this, from the phænomena of motions to investigate the forces of Nature, and then from these forces to demonstrate the other phænomena." He goes on to say that in the first two books—whose character he describes as "mathematical"—general propositions are developed to facilitate this end, and that in the third book, containing the theory of gravity, he gives *an example* of such investigation. And he adds, in an expanded statement both of the program he is advocating and of the standing he attributes to it in philosophy:

> I wish we could derive the rest of the phænomena 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. (NEWTON 1729, vol. 1, third and fourth pages of the Author's Preface [pages unnumbered].)

I have quoted this passage so often that to do so may seem a mannerism on my part; but it continues to strike me, in its clarity, economy, and what I may call its *philosophical truth of method,* as not only instructive but a shining example.

The passage does however demand some explication. Let me call your attention to one phrase that deserves to be puzzled over. Newton says that he suspects the phenomena of nature all depend upon "certain forces, *by* which the particles of bodies, *by some causes hitherto unknown,*" are urged either towards or away from each other—thus what we call "central forces." But is there not one "by" too many here? Should not *the forces themselves* be called the causes? What sense does it make to speak of a force "*by* which, *by some cause,* bodies are impelled"? Or as an alternative, may it not be appropriate to drop the perplexed notion of cause altogether, expecting the theory itself (including its empirical interpretation, however such interpretation is managed) to give an adequate explication of the *systematic, technical* concept of "force," without any need to cloud positive science by such a metaphysical notion as "cause"?

Hold that question for a while in suspension; I want first to describe how, as I see it, Newton actually proceeded in the development of his theory, and to give an account of the actual system he propounds. (Of course this must be in significant measure speculative; and within the constraints of such a paper as this, necessarily sketchy.)

At the time of the investigation that gave us the *Principia,* Newton had good reasons for regarding *accelerations* as of critical significance in the interactions of bodies.[5] In fact, nearly two decades earlier Newton had already understood this point well enough to motivate a calculation of the acceleration of the moon, a derivation from Kepler's so-called third law of the implied relation among accelerations (assuming uniform circumferential speeds in circular orbits), and a comparison, on the basis of his result, of the moon's acceleration with that of falling bodies on the earth (cf., e.g., WESTFALL 1980, pp. 151-152). But in 1684 he did something very much more far-reaching.[6] By a purely mathematical, kinematical, analysis he demonstrated, in effect, that the so-called three laws of planetary motion of Kepler[7] are equivalent to the following pair of propositions:

1. Each of the bodies in any one system (that is: planets around the sun; satellites around a planet) has, at each instant of its motion, an acceleration that is a function

[5]Notably: Galileo's propositions (by then well-confirmed) about the motion of bodies in free and oblique fall, and of projectiles; the more elaborate application of Galilean principles to constrained motion under the influence of weight in Huygens's great work on the pendulum clock; the successful application of notions derived—once again—from Galileo's theory of fall to the phenomena described as "centrifugal force." (For a brief account of Huygens's investigations as providing significant background for Newtonian mechanics—a background cited by Newton in his scholium to the laws of motion in the *Principia*—see STEIN 1990a, pp. 20–26.)

[6]For a fairly detailed analysis of Newton's argument for universal gravitation see STEIN 1990b.

[7]*Not* so called by Newton: he does refer the "harmonic law" for the primary planets to Kepler; and he records the law of areas for the primary planets, and the harmonic law for the satellites of Jupiter (in the second edition, also for those of Saturn) among the results established by astronomical observations (without explicit reference to Kepler). As for the law of ellipses, Newton does not admit this at all into his catalogue of results secured by observation.

of *position relative to the central body* alone: namely, having its direction towards that central body, and with a magnitude that varies inversely as the square of the distance from the central body. (In particular, then, in each such system there is a well-defined *field of acceleration,* and the acceleration is independent of any special characteristics of the particular planet or satellite.)

2. In the course of the motion, each body remains within a bounded distance from its center of accelerations.[8]

This result is easy to understand, and it is familiar: "Ah, yes!" the sophomore will say, "the law of gravitation—*I* know that; so *that's* how Newton got it!" But of course no: this is not the law of gravitation at all; it is only how Newton *began* to "get it." What he did next (and had already thought of in the 1660's) was to apply his result to the moon, on the assumption that if the position of the moon were made to vary *away from its actual orbit,* it would continue to "explore," as it were, the same inverse-square acceleration-field about the earth that it does in its actual orbital motion; and in particular, he calculated what that acceleration would be at the surface of the earth. The result agreed well with the observed acceleration of falling terrestrial bodies (or rather, with the value of that acceleration derived by Huygens from his careful observation of pendulums). And Newton concluded that the acceleration of falling bodies and that of the moon are effects of "the very same force" (NEWTON 1729, vol. 2, p. 217 [in the proof of Prop. IV, Book III]), or (as he puts it elsewhere) that they are manifestations of one and the same "active Principle" or "general Law of Nature" (NEWTON 1730, p. 401). Since the effect familiar in terrestrial bodies is called that of "weight"—"gravity"—this "active principle," "natural power," or "force of nature" is called by Newton the force of gravity.

Still, that is only a word: what *is* the alleged "principle" or "general law of nature"? Two things are clear enough from what I have already rehearsed: (1) If the line of thought is correct, then the principle in question must assign to any body subject to it, under given relevant circumstances, an acceleration that is independent of that particular body, and depends only upon its geometrical situation (relative to those " relevant circumstances"); for this characteristic, inferred for the *existing* motions from the observed phenomena, has already been assumed in arriving at the identification of the force on the moon with terrestrial weight. (2) By the same token, it is clear that the principle must involve accelerations directed towards certain centers, with magnitudes that vary inversely with the square of the distance from those centers.

None of this is especially subtle (although of course the mathematical analysis that underlies it was pathbreaking for both its methods and its results); and the conclusion that Newton had uncovered a new "principle" of the kind so far characterized was greeted at the time with acclaim and no serious controversy. But for Newton that was not the

[8]Besides compressing the formulation of this result, I have drawn certain inferences that Newton does not make explicit, but does make use of (and which can be justified on the basis of his theorems).

The second of the two propositions above is needed to exclude the case of open orbits (parabolic or hyperbolic)—to exclude them, that is, not (of course) from occurring in nature, but from the scope of Kepler's laws.

last or the most crucial step. At this point I want to indulge in some speculation about his state of mind.

As I see it (judging both from the actual sequence of propositions and arguments in the *Principia,* and from the evidence of the circumstances surrounding the development of the work), Newton asked himself two interrelated—perhaps not clearly distinguished—questions: (a) *What can be the cause* of such an effect as these inverse-square centripetal accelerations, affecting both the planets and all earthly bodies? (b) If there is a principle that certain bodies are affected by inverse-square accelerations directed towards certain other bodies as "centers"; and if among those centers are the sun, the earth, Jupiter, and Saturn, and among the bodies affected are all the planets (towards the sun), all the satellites (towards their planets), and all terrestrial bodies (towards the earth); can one arrive at any conclusion as to *which bodies in general* are centers of such acceleration, and which bodies in general are subject to it—towards which centers?

It is at this point, I should argue, that the new method of philosophy was born. Let me contrast Newton with Huygens. Huygens read the *Principia,* admired it enormously, but thought that just here Newton went wrong. Huygens himself had a ready answer to the questions I have just put. To the first—what can be the cause?—his answer was that "the philosophy of the present day" (which is to say, "modern physics") teaches us that the causes of all natural effects are to be sought in the impinging of matter upon matter (cf., e.g., HUYGENS 1690, pp. 2–3); and so a motion of some kind of ambient medium must be conceived that can give rise to a pressing of bodies towards a center. He had already proposed a theory of weight based on such a hypothesis; and now concluded from Newton's results that one must investigate further just what kind of motion of the medium could produce an inverse-square variation of the force. To the second question, his answer was much simpler: there is, he said, absolutely no evidence that there are any other centers of such acceleration than the ones already identified—except that one will naturally generalize, and say that every star and every planet is a center of the sort. Thus it is precisely about the stars and the planets that one should assume the existence of ambient matter in such a state as to produce this effect.

What Newton did reflected, in contrast, what might be called a respectful skepticism about the demands of "the philosophy of the present day." He had devoted most of the second Book of the *Principia* to an analysis of the behavior of fluid media and of bodies moving through fluid media; and had concluded that there are insuperable obstacles to any attempt to reconcile the observed motions of planets, satellites, and comets, with the existence of any such medium of appreciable density occupying the interplanetary spaces. He did not say that he quite despaired of a "mechanical explanation"—i.e., one in terms of the impinging of bodies upon one another—of the astronomical phenomena; but he did say that one ought not to build positive conclusions in physics on the demand that such explanations be forthcoming. So he reflected upon the situation without any regard for the mechanical "hypotheses." On the other hand, he allowed himself an extremely bold—one might almost say, a reckless—use of a principle he had extracted from results of work in mechanics by several investigators, including Huygens and himself. This is the principle of the conservation of momentum, discovered by Huygens as a true substitute for the false principle of conservation enunciated by Descartes, but never placed by Huygens in a central theoretical position; a principle to which Newton, emphasizing as he did *acceleration* as a fundamental parameter of natural processes, gave the form (actually somewhat more restrictive) of the third law of motion. In particular, Newton argued thus: If body $A$ is subject to (here using my own term rather than Newton's) an "acceleration-field" directed towards body $B$, of magnitude dependent only upon position, then what Newton calls the "motive measure" of the force on $A$ is the product of its *mass* by this acceleration—thus the "motive force" (at a given position) is proportional to the mass of the body acted upon. According to the third law of motion, there must be an equal and opposite motive force on something—in the ordinary formulation, which is Newton's own, on whatever *exerts* that force on $A$. What does? This Newton explicitly, repeatedly, emphatically says he does not claim to know. And yet he takes the step—this is what I have "almost" called reckless—of asserting that there is an equal and opposite force exerted *upon the "central" body B.*

This postulate, together with some simple qualitative considerations, led Newton inescapably, in a very few steps, to the law of universal gravitation: that is, to the conclusion that the answer to question (b) above is that *all* bodies are subject to the "principle" of gravitation and that *all* bodies are centers of gravitational acceleration-fields. (That the "motive measure" of the force is directly proportional to the mass of the body acted upon is equivalent to the proposition that the "accelerative measure" is independent of that body; that the motive force is also proportional to the mass of the gravitational center follows from the third law of motion—so that, as Newton emphasizes, there ceases to be any difference in status between the two bodies concerned: one is dealing with an *interaction,* whose participants enter it *symmetrically.* That the force is inversely proportional to the square of the distance is a conclusion already reached; and the law is formulated in a way that is complete *and perfectly general* [cf. the fuller discussion in STEIN 1990b].)

This is not to say that the gravitational principle is *established* by the argument I have outlined; only that it is *found*—"invented," as the seventeenth century would say—by that argument. More is found as well: namely, the terms of the Newtonian program; it remains to put that into a systematic framework.

But I have not yet given Newton's answer to question (a): what can be the *cause* of such an effect as this—a universal attraction between all particles of matter? Huygens, believing in the necessity of "mechanical" causes, thought it clear that *nothing* could cause such an effect; and, seeing no evidence for universal gravitation, rejected it on those grounds. Newton's answer is rather subtle. He says: "The cause of gravity is what I do not pretend to know."[9] But he is certainly interested in the question; he adds that he wants more time to consider it, and of course he did eventually publish a speculation about it (NEWTON 1730, pp. 350ff. [Book 3, Query 21]). He amplifies his view most significantly near the end of the long final Query in the third Book of the *Opticks,* in his fullest discussion of his conception of a force of nature. He had been attacked for reintroducing, in his theory of gravity, the much-deplored "occult qualities" of the scholastics, by assuming an "occult" cause of the universal attraction. His answer is that he regards his own "active Principles" or forces "not as occult Qualities, . . . but as general Laws of Nature . . . ; their Truth appearing to us by Phænomena, though their

[9]Letter to Bentley, 17 January 1692(o.s.)/3(n.s.); see TURNBULL 1961, p. 240.

Causes be not yet discover'd"; and concludes:

> To tell us that every Species of Things is endow'd with an occult specifick Quality by which it acts and produces manifest Effects, is to tell us nothing: But to derive two or three general Principles of Motion from Phænomena, and afterwards to tell us how the Properties and Actions of all corporeal Things follow from those 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-mention'd, they being of very general Extent, and leave their Causes to be found out. (NEWTON 1730, pp. 401–402.)

There is evidence—which I find convincing—that Newton had in fact abandoned all belief in the traditional "mechanical" causation as ultimate. If I am right, the correct reading of his words here is this: When we have found such principles of motion as Newton's program envisages, and as he has given an example of in the law of gravitation, we have *(ipso facto)* discovered something about what one calls "causation." In any given case, such a principle may have underlying deeper (e.g., "mechanical") causes; and this is a proper subject of inquiry. Then again, it may not; for the ultimate cause (in Newton's view)—"the very first Cause, which certainly is not mechanical"—is the direct legislative action of God (whose laws are self-executing): which we may immediately translate, simply, as "the ultimate constitution of nature." And Newton's words in the preface, referring to his hope to "afford some light either to that, or some truer, method of philosophy," bear two interrelated meanings: first, he hopes that the principles laid down in the *Principia* will facilitate the investigation of nature, either by the means he has suggested or by some modification in which those principles are still of use; second, he hopes that those principles help to explicate the constitution of nature, whether or not they prove adequate to its *ultimate* constitution.

As to the principles themselves, the mathematical structure they involve is reasonably clear (although there are certain important ambiguities, as I shall explain in a moment). Newton found it impossible to codify dynamical theory without presupposing the structures he calls "absolute time" and "absolute space." So we have—in our own terms—the four-dimensional manifold of space-time, given with the product structure: $\mathbf{S} \times \mathbf{T}$, where $\mathbf{S}$ is a three-dimensional Euclidean space and $\mathbf{T}$ a one-dimensional affine space. We also need to posit a further "space," which I shall call $\mathbf{B}$, the set of "bodily points"; this must have the structure of a measure space—the measure, following Newton, we call *mass.* The postulate that all the phenomena of nature depend upon *configurations and motions of bodies* takes the form of the assumption that the entire history of nature is represented by a mapping from the Cartesian product $\mathbf{B} \times \mathbf{T}$ into $\mathbf{S}$: the *kinematical history.* There is a problem about the finer specification of the space $\mathbf{B}$ (for example, to demand that $\mathbf{B}$ have the structure of a differentiable manifold is appropriate to some classical contexts, but highly inappropriate to others). Newton himself considered it "probable," he tells us, that matter consists ultimately of *rigid indivisible particles*; this implies that $\mathbf{B}$ is a disconnected topological space, each of whose connected components has the structure of a compact three-dimensional metrically Euclidean manifold with boundary, and that the kinematical mapping is, for each instant $t$ of time, isometric.

A related subtlety concerns the further requirements to be placed on the kinematical map. Newton would not suppose this map to be everywhere smooth with respect to the time as argument—for he would expect occasional (although, he tells us, very infrequent) *impacts* of the rigid fundamental particles. At any rate, it is immediate that wherever this mapping *is* smooth, it determines (for each bodily point at each such instant of time) the associated velocity and acceleration vectors; and—subject to suitable conditions on the structure of **B** and of the kinematical mapping as a function of its body-point as well as its time argument—it is also clear that one will be able to define a motive force (or force-density, or force-*measure*: most generally, a "force-distribution") over such points of **B** at such times.

But that concept of motive force, which differs from a purely kinematic concept only in that it involves the mass as a coefficient, is not Newton's central notion of force; the framework so far described does *not* define a Newtonian mechanical system. (Indeed, the third law of motion has made no appearance yet.) To complete the account requires a new concept, which carries its own—"extra-systematic" if not presystematic—marginal gloss: namely, the concept of what Newton calls a "force of nature." His own way of putting it (only a little paraphrased) is this: the motive measure of the force on a body at a given time is the resultant of a system of what he calls "impressed forces" (the set may be infinite—even continuous, so that the "resultant" becomes an integral); each of these component impressed forces is the *exercise* upon the body in question of a "force of nature" (this is the connotation of his term "impressed force": the "impression" *upon* a body *of* a "natural power"). But the forces of nature are to be known through *general laws* of nature (as Newton says, an example of this is given in the third Book of the *Principia*—the first example of the kind ever discovered, the "invention" and "proof" of which, I am suggesting, is what motivated Newton to elaborate this conceptual framework itself). And these laws—the search for them is the proposed "method of philosophy"—are to take the form of *laws of interaction* between *pairs* of bodies, in which each body enters symmetrically in the sense of the third law.

So the specification of a Newtonian system requires the specification of the structure of the space **B** of bodily points and the specification of a *set* of laws of interaction of the indicated type; the motive force-distribution associated at any instant with the actual motion is to be the vector-sum of all the impressed force-distributions at that instant. Newton hopes that a small set of laws of interaction will suffice for an account of all of nature: "To derive two or three general Principles of Motion from Phænomena, and afterwards to tell us how the Properties and Actions of all corporeal Things follow from those manifest Principles, would be a very great step in Philosophy."

Now, something a little odd has happened in my own argument. We were considering the question of how to get the laboratory (or observatory)—the phenomena—into the theory. My discussion of Newton has largely been a discussion of how Newton got *from* phenomena *to* theory. I have described the theoretical framework—corresponding, as I put it, to Carnap's "first volume"—and have provided an extra-systematic commentary on the development, the motivation, and thus in a certain sense the intended "meaning," of this abstract theoretical structure (which itself is constituted by certain spaces and certain mappings or functions). The dialectic, one might say, has moved from phenomena to pure forms; and this seems, as I have said, to be opposite to the direction we were concerned with.

A closely related point is that we left Newton's theory of gravitation in a peculiar condition. In describing Newton's own path to that theory, I said that he took a very questionable turn, and that the argument by which he *found* the law of universal gravitation is not an argument that genuinely *establishes* the law—or establishes that there is any principle at all of *universal* attraction. (I hope it is clear that Huygens's objections were very sane and reasonable.) How, then, does—or did—Newton's theory really get "established"?

Again I shall have nothing startlingly new to tell you about this. Let me first confront the radical objection that the theory did not get established—that no theory ever gets established. You will not expect me to settle this issue; and I do not want to quarrel over mere words: in this case, what "established" should mean. I want to take for granted that we do not now believe Newton's theory of gravitation to be a correct general theory; that we *do* now believe that there is a "universal principle" of gravitation (and, indeed, we place it among the "fundamental forces"); and that we also believe—with enormous confidence—that Newton's theory is a very accurate approximation for a very wide range of applications, in which indeed it is our only usable theory: applications that include planetary astronomy (the recent solar eclipse occurred right on the dot) and such space-travel as has so far been accomplished. (By the way, having studied Newton, I was very much struck at the time of the first moon-landing in 1969 that after nearly three centuries Newton's experiment of bringing, not indeed the moon itself, but at least a piece of the moon, down to the earth's surface and weighing it was to be performed. Of course the whole space program relies crucially upon the law of gravitation in the management of space vehicles. And—despite, let me say, the views either of Popper or of anti-cumulativists—no one then thought that gravitational theory was being put to the test: no one at the time had any doubt at all that the law of gravitation was going to work properly; all anxiety concerned either the adequacy of the engineering or the firmness of the moon's surface.)

Next a point bearing on the question of the theory and the laboratory: it is hardly possible to maintain that the theory of universal gravitation was established by testing it in the laboratory. Cavendish's experiment, for example, can certainly not be regarded as having established the theory. To be sure, it was impressive *confirmation* of Newton's theory when Cavendish was able to demonstrate the existence of an otherwise unsuspected force between two bodies; but that the force in question was gravitational in nature—in origin—in other words, was "caused" by the same principle that is responsible for weight—could not conceivably have been even surmised from the experiment in the absence of the theory (here indeed the theory could not be "left outside the laboratory door"!). (One should add that the experiment also made a capital contribution to the *content* of theory by allowing a determination of the gravitational constant—on the *assumption,* of course, that the Newtonian theory was correct and that the Cavendish force was gravitational.)

Obviously, then, the really central evidence must be astronomical; and this, I suppose, as I have said, is no great surprise. It would be of some interest to consider just how astronomical evidence—which, after all, consists in observations of rather special, mostly rather large, bodies—could possibly support such an astonishing proposition as Newton's (we forget, I think, just how extravagant a proposition it is, because we have been taught from childhood that it is so—it becomes even a kind of mark of our enlightenment to believe it). But I have to paint here with a broad brush, so I set this question aside, and just consider how any evidence at all gets connected with the abstract mathematical framework I have sketched—once again apologizing for the obviousness of the answer I shall give.

Let me underscore the point that there can be no thought of *deducing observations* within that framework. To do so in the strict sense, one would need to have a physical theory of the actual observer, and to incorporate it into the Newtonian framework. I certainly do not want to say that there is a reason "in principle" why such a thing can *never* be done, for *any* possible (future) physical framework; but everyone knows that Newton could not do it, and that we—in the best versions of our own physics—cannot do it. Even waiving the theory of the observer, it is clear that all astronomical observations are intermediated by *light*; therefore, to deduce anything like observations, one would have to include the theory of light within the framework. Moreover, the light traverses the earth's atmosphere, and is usually received through a telescope; so we need the theory of atmospheric refraction and the theory of the instrument also—we are in the vicinity of the problem of the systematic treatise on experimental physics. In actual fact, the experimental physics is treated separately as a discipline in its own right, that is partly an art: an affair of both knowledge and manipulative and perceptual skill. But the possibility of connecting this art with the theory is closely connected with a certain possibility *within* the mathematical structure that is the theoretical framework: using a word I have introduced earlier, the possibility of representing experiments, and of representing the observer, "schematically." Kant used the word "schematism"—in a way I confess to finding rather obscure—for a process that intermediates between concepts of the highest abstractness (his "pure concepts of the understanding") and sensible contents; my use of it here is vaguely similar (but I hope *not* obscure): where Kant speaks of "schematizing the category to the manifold of intuition," I want to speak (as it were conversely) of "schematizing the observer within the theory"; but the intention is analogous: to secure empirical content—content within experience—for an abstract structure.

Fancy talk, but a simple idea that will be found perfectly familiar. One represents the observer within the spatio-temporal framework by a world-line (or a system of world-lines). Putting in—for the gravitational theory of the solar system—the world-lines of the planets and satellites, as calculated from suitable initial data, one can then determine at each instant all the relevant angles between lines drawn from the observer to the bodies of the system (including, if the theory is properly handled, with the earth represented as an extended body and its rotation treated systematically, lines from the observer to terrestrial landmarks). As a first approximation, such lines are treated as lines of sight. With more sophistication on the observational side, the results are turned over to the experts in observational astronomy, who will take such account as they are able to of atmospheric refraction, of aberration of starlight, and so on. But so far as the fundamental theory is concerned—or rather, so far as *mathematically defined structures and rigorous arguments* are demanded—the "schematic" representation of observers, experiments, and observations, is, I believe, as far as we know how to go.

Let me suggest a few further reflections upon and consequences of all this.

First, in the account I have given, the distinction between what is purely mathematical and what is not has certainly played a central role—closely related to the distinction I have argued we really do need between observational and theoretical "languages." That there is indeed such a distinction, and that it is of fundamental importance, is one of those things that seem to me quite obvious. But it is often denied. It is denied, for instance, by Quine, who (in contrast to Carnap) sees here a "continuum" within which no sharp boundaries can be drawn. This, I submit, is simply wrong. Newtonian mechanics, *in its application to the empirical world,* is a theory that gives very good results in a very wide domain, but that can no longer be defended as correct without restriction both as to domain and as to degree of precision. On the other hand, Newtonian "rational" mechanics, *as a purely mathematical theory,* stands on an unshaken footing and continues to offer a field for useful and deep rigorous investigations. That is the distinctive nature of mathematics, *qua* mathematics: it is, as such, *not* about the given, natural world.

Second, important modifications both of Newtonian space-time theory and of Newtonian dynamics are possible *within classical physics*: It is well known that the product structure of space-time as $\mathbf{S} \times \mathbf{T}$ is demonstrably *inappropriate* to the theory—that it can and should be replaced by the structure of a four-dimensional affine manifold, with an affine "time-projection" and a three-dimensional Euclidean structure on the associated space of vectors with time-projection zero. (Of course one then has to change the description of the kinematical mapping: it goes from $\mathbf{B} \times \mathbf{T}$ into space-time, commuting with the projection onto $\mathbf{T}$.) Within that structure, velocity vectors are no longer definable; but acceleration vectors are, and therefore also motive forces—the reformulation of the theory is unambiguously determined, and its correctness is *demonstrable* from the *original* formulation (with the help of the principle of Galilean relativity—proved by Newton as a theorem).

As to the other modification, it is even more familiar: A whole series of classical investigators, including Lagrange, Gauss, Hamilton, and Jacobi, found alternative ways of formulating the dynamical law of a Newtonian system. These formulations are not all equivalent; rather, they all generalize a certain common domain, and generalize it in different directions. And the generalizations have very important physical significance; for example, the Maxwellian electromagnetic field is not representable as a Newtonian system, but is representable as a Lagrangian or Hamiltonian one. But of course, Lagrange and Hamilton were consciously building upon and transforming Newton's principles. The result is a transformation of the concept of a "natural power" or "force of nature": such a force is now to be given, not by a law of motive force characterizing action-reaction pairs, but (for instance) by a Hamiltonian function. This surely deserves some recognition as a remarkable fulfillment of Newton's hope: that his principles might "afford some light, either to [his own], or some truer, method of philosophy." The fulfillment becomes all the more remarkable when one considers that, although Newtonian forces have little place in our own most fundamental physics, Hamiltonian and Lagrangian functions—or operators—are at the heart of those theories.

This brings me to another general point. It has been my experience that many philosophers balk at what they may think of—perhaps quite justly—as the rather "Platonic" notion of "general principles—or laws—of motion" as having in some sense a kind of "reality" and even "efficacy": "What can it mean," I have been asked in connection with Newton, to talk—as Newton quite explicitly does—of a 'force of nature' *as* a *law* of nature?" It is surely important to note that that is exactly the way physicists do talk today: when one says that, at the fundamental level, there are "four forces" (or fewer than four, in the light of the unifications that have been made or proposed), that has nothing to do with Newtonian "impressed motive forces," but it has everything to do with laws of nature, forms of interaction, Hamiltonians. It might be rejoined that this is an interesting sociological fact about physicists, but that it cuts no philosophical ice. I said in another context that I do not claim to give reasons of philosophic principle, but to call attention to what seem to me obvious but important *facts* that deserve philosophers' attention; and this is another one. It is not just a question of how physicists talk: it is a question of what, *de facto,* in the history of physics to date, has tended most to *persist as stable* (and, it appears, reliable) in what we think we know about the world. One of the things that have persisted is the more or less far-reaching and more or less precise (but, in application, always approximate) correctness of theories as applied to domains of phenomena; another is the "forms"—more precisely, certain aspects of the forms—that are characterized by what Carnap called "frameworks."

In fact, if we ask, say, of the physics of the end of the seventeenth century, what of all it told us about the world we can still regard as "true" or as having proved itself "real," the answer is—I use the word yet again—striking: Not Newton's hard particles, not Leibniz's material continuum, not Huygens's ether—indeed, hardly anything to which most philosophers would accord "ontological" status. (In particular, of course, not "space." If one reflects on what quantum field theory has told us about the characteristics of the only thing *in the physical world* that can be regarded as "empty" or "pure" space, its difference from anything earlier centuries conceived is startling indeed.) And of fundamental processes: no impacts of atoms, no pressures of continuous media, no immediate and instantaneous actions at a distance—indeed, no instants at all! And yet, although Huygens's ether has gone, the "form" of the propagation of light to which he contributed a first crude sketch is still discernible in the theory of electromagnetic waves, and through that theory—again transformed—in the quantum theory of electrodynamic and optical processes. One could go on in this vein; I hope I am right in thinking that the point really is obvious, and only needs to have attention called to it; I must come to an end, and there are still some things to say.

First, I want to mention the issue of the "incommensurability" of theories. That is a metaphorical term; in an appropriate interpretation, the doctrine may be true. But in any case, one does *compare* theories—as, of course, the analysis first developed by the Greek geometers allows one to compare *magnitudes* that are (technically) called "incommensurable"; and in so far as forms discernibly persist through the transformation of our theories, such comparisons form a most vital part of science. If we make the assumption that the human race will survive for another millennium, and in circumstances conducive to the advance of knowledge, then I should predict with great confidence—not that quarks and leptons will continue to be regarded as the most basic particles (I don't predict the contrary—I am perfectly agnostic); not that quantum field theory, or general relativity, will retain its fundamental role (on *this* point, I would hope very much for a radical advance)—but that the forms of these theories will be clearly *discernible in,* clearly *related to,* the structures of whatever theories supersede them.

The second point has to do with a special bearing of the crude account I have given upon the "structure" of our knowledge in the sense of *epistemology*: it is the simple remark that our understanding of our own relation to the world is mediated by our ability to place ourselves, however "schematically," within our conception of the course of nature. And it is a very interesting exercise, within the successive frameworks of Newtonian space-time (*without* absolute space—so that "geometrical" relations hold *only among simultaneous events,* and space is as it were constantly evanescent) and special relativity (in which, by contrast there is no such thing at all as simultaneity), to consider the epistemology of "geometrical knowledge." It is possible not only to see interesting parallels, as well as contrasts, between the two accounts, but to draw rather instructive conclusions about the way in which our "intuitions" of space and time relate to—and presumably result from—our experience of the "real" physical structures. (For a discussion of this point, see STEIN 1991, pp. 155–162.)

One would like to say a similar thing about quantum mechanics—particularly in respect of what it tells us about the structure of *causation,* and our "intuitions" of causation. But this we cannot do. In *this* theory, we just do not know how to "schematize" the observer and the observation. This is a quick way to characterize what I regard as still the basic unsolved philosophical problem of "interpreting" the theory: on a previous occasion, I have expressed the view that "the difficulties [quantum mechanics] presents arise from the fact that *the mode in which this theory 'represents' phenomena* is a radically novel one" (STEIN 1989, p. 59). In other words, here the difficulty of getting the laboratory inside the door of the theory is of a new—and I think still not understood—order.

And on that unresolved dissonance I close.

SUPPLEMENTARY NOTE:

Two questions raised during the discussion at the Congress deserve to be noted.

One of these concerned the point made in the paper concerning the place of "observation" within a theory: it was asked whether, instead of the notion there sketched of the "schematized observer," one could not as well—and in closer accord with traditional (e.g., logical empiricist) terminology—speak of an "idealized" theory of the observer.

To this suggestion I have no serious objection; and I hope it is apparent in the paper itself that I acknowledge a great debt to the logical empiricists, and especially to Carnap, for helping me to clarify my own thoughts about physics. But it has to be understood that the "idealization" involved is an idealized theory *of the observer* in, so to speak, a Pickwickian sense. For instance, in the astronomical example given above, the observer is represented ("ideally" or "schematically") merely by a space-time *locus*. The observational astronomer will infer something about the manipulations to make of the telescope in order to point it so as to receive light from a particular star or planet—but this inference is not one that can be made *within* the theory that incorporates the "idealized observer," because the manipulations in question are not even *describable* in the language of that theory—if they were, a good part of the "idealization" would have been removed ("de-idealized"). Moreover, it should be noted that to be able to infer, even "ideally," that under certain circumstances an observation *will be made,* one would have to include in the ideal theory terms that distinguish conscious from unconscious states of the observer, open from closed eyes, directions of looking, etc. (and noted, in particular, that "ceteris paribus" is not an expression that lends itself to deployment in the context of mathematical argumentation!). Thus unlike, say, the "theory of ideal gases," which does include notions such as temperature, volume, and pressure, central to the study of actual gases, the "theory of idealized observers" would perforce omit those notions that are crucial to the characterization of actual observations. Once this point is well understood, the choice of the word "schematized" or "idealized" is immaterial.

The second question concerned the conjunction of my remark that "technical assistants ... can be taught to perform experiments ... without teaching them the theories those experiments are designed to test," and the closely related claim, expressed just afterwards, that "an 'observation-language' in Carnap's sense is the language in which we ordinarily conduct the business of daily life, [etc.]." It is of course true that technical assistants—and the expert experimenters they assist—need to be masters of a technical discipline, which will include a vocabulary unknown to most of us in "ordinary life." In referring at that point of my talk to the microphone as an example of such an "ordinary object" (with "ordinary properties" that we habitually ascribe to them), I had just this consideration in mind. For what the experimenter needs to be expert in is how to recognize and use the relevant *instruments*; and these—with their properties (including what might be called quirks: their idiosyncrasies and the pitfalls involved in using them, the "other things" that are not always "equal")—*become* familiar (and even "ordinary") in the course of training and use. The microphone is an example of an instrument that has become familiar to most people in the course of the past century or so, although no such thing existed a century and a half ago. But the training and familiarization required for expertness in experimental physics today typically does *not* require a deep study of fundamental physical theories; and, conversely, most theorists today would be lost in a laboratory. (Note that to say this is not to take a stand on the question of the degree of specialization, in experiment or theory, that is desirable in the education of a physicist. But the state of affairs that *actually obtains* clearly has implications for the structure of the knowledge we *actually have* in physics.)

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