Was Einstein Really a Realist?

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It is widely believed that the development of the general theory of relativity coincided with a shift in Einstein's philosophy of science from a kind of Machian positivism to a form of scientific realism. This article criticizes that view, arguing that a kind of realism was present from the start but that Einstein was skeptical all along about some of the bolder metaphysical and epistemological claims made on behalf of what we now would call scientific realism. If we read Einstein's philosophy of science in its proper late nineteenth- and early twentieth-century philosophical *context, we find that a kind of Duhemian underdeterminationist holism and conventionalism was more important to Einstein than either positivism or realism. And, reading his philosophy of science in its proper* scientific

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context, we find that his realism did not take the form of a commitment to a philosophical doctrine about the interpretation of scientific theories but was, instead, defined by a commitment to a very specific set of physical principles, foremost among them being the principle of spatiotemporal separability.

Introduction

Every movement needs a hero, a figure on whom the movement projects its aims and ideals, a figure who thereby becomes a model for emulation, a figure whose triumphs validate the movement's principles and aspirations. Truth matters little in such cases. And so it was that Einstein was first canonized as a positivist saint by the logical empiricist clerisy in the 1930s, only to be reconstructed as an early, lonely champion of realism during the realist reformation of the 1960s and beyond, when, in fact, neither description really fits. One worries that the making of history, especially history for a polemical purpose, always leads to this kind of abuse.

The error of making Einstein into some kind of positivist or operationalist is widely recognized, though the belief that he was a positivist in his early years—before he "grew up" and became a realist—still enjoys enough currency to require some discussion here. What I mainly want to focus on, however, is the more recent and still more widely accepted error—indeed, it has become something of a commonplace in the literature—that of regarding the adult Einstein as having anticipated late twentieth-century scientific realism. This interpretation of Einstein's philosophy of science is open to at least two different kinds of objections.

There is, first, the mistake of our going to Einstein's writings seeking there a position on something like the realist-instrumentalist debate. The problem is that the debate itself, and the very concepts of "realism" and "instrumentalism"—in the sense in which they are deployed in that debate—are inventions of the second half of the twentieth century. We commit the error of anachronism when we ask about his position in what was not his debate, but is our debate. If we read Einstein in his own, contemporary philosophical context, we find that what really mattered to him was far more the debate between conventionalists, especially of the Duhemian, holistic, underdeterminationist variety, and Marburg neo-Kantians. In this debate, he took a very definite stand, on the side of Duhem. Of course, there were points of view labeled "positivism" and "realism" at that time, but their connections with mid- and late twentieth-century views of the same name are complicated, to say the very least. Thus, the revisionist neo-Kantian critical realism of a Bruno Bauch or an Alois Riehl is no more than a distant cousin to the scientific realism of a Karl Popper or a Richard Boyd. Moreover, as we shall see, the evidence of Einstein's possible sympathy even with the realism of his day is, at the very best, ambiguous; he was certainly not the friend of any simple realism.

The second major objection to viewing Einstein as a scientific realist is that it misses what was really "scientific" in Einstein's attitude toward physical reality. As Arthur Fine has argued, Einstein tended to "entheorize" questions about such issues as realism and determinism, which is to say that, for Einstein, these were not straightforwardly metaphysical questions, to be answered by principled philosophical arguments. They were, instead, questions about the success of programs of investigation comprising realistic or deterministic theories (Fine 1986). Thus, if we want to pursue the question of the precise sense in which Einstein may have been a realist, we have to ask how Einstein *physicalizes* the concept of reality, we have to ask what counted for him as a realistic physical theory. And what we find, to our surprise, is that realism, for Einstein—he says, "I just want to explain what I mean when I say that we should try to hold on to physical reality"—came down to a commitment to a program of theories satisfying a principle of *separability*, according to which non-null spatiotemporal separation is a sufficient condition for the individuation of physical systems.[1] This is not at all the reality of late twentieth-century philosophical controversies about scientific realism, but it was what reality meant to Einstein, and it turns out to be a theme running through virtually all of Einstein's scientific work, from the time of the first quantum hypothesis paper of 1905 to his final musings on unified field theory in 1955, for what distinguishes field theories like general relativity is, for Einstein, their satisfaction of the separability principle, and what condemns the quantum theory, in Einstein eyes, is its outright denial of that principle.

What I am recommending then is that, instead of making Einstein after our own image, for the purpose of propagandizing on behalf of our own philosophical programs, we should do him the favor of reading what he had to say about the philosophy of science first in his own, contemporary philosophical context and, second, in the context

1. In this respect, as I explain below, I part company from Fine, who sees a form of determinism as more central to Einstein's physicalized or entheorized realism; see Howard 1991*a*.

of his physics. The Einstein that results from such a reading may not fit any simple, preconceived category, but he is bound to be a more interesting Einstein. There is a systematic unity in Einstein's epistemology, but the key to it is to be found not in any one philosophical system, but in the intimate connections between Einstein's philosophy of science and his work in physics.

Of course, we all bring an agenda to the telling of history, an agenda that inevitably influences the way we tell that history. What I am calling for, then, is not some impossible objectivity, but rather more honesty about one's agenda and greater sincerity in one's efforts to put oneself in the place of the historical actors, knowing full well that it is impossible ever completely to relocate oneself in the past. It may be impossible "to tell the whole truth and nothing but the truth" about historical events, but at the very least we can try to be aware of our biases and agendas, and then try to avoid the distortions they threaten to introduce.[2] Philosophers of science probably have an especially difficult time learning to do history in a manner sensitive both to contemporary context and to the historian's own possible biases, because we labor under the burden of having been taught that the philosopher's task is "rational reconstruction," the implication being that, in any historical episode, we today are guaranteed to be able to do a better job of explicating what the real issues were than the historical actors themselves. Some modesty is called for here.

"The Reality of Atoms": The Myth of Einstein's Early Antirealism

Gerald Holton's classic paper "Mach, Einstein, and the Search for Reality" simultaneously demolished the myth of Einstein the positivist and established the myth of Einstein the realist. The paper begins thus: "In the history of ideas of our century, there is a chapter that might be entitled 'The Philosophical Pilgrimage of Albert Einstein,' a pilgrimage from a philosophy of science in which sensationism and

2. What are my biases? I have long been sympathetic to the kind of Duhemian holism I claim to find in Einstein's philosophy of science. But the influence of Duhem on Einstein came as a complete surprise to me. I first noticed the holism in Einstein's epistemology many years ago in reading the Einstein-Schlick correspondence and those of Schlick's epistemological writings that formed the background for Einstein's discussions with Schlick (see Howard 1984, 1993). The earliest progenitor of the present article, drafted in 1983, said nothing about Duhem, but quite a bit about the holism in Schlick's epistemology prior to 1922. I first stumbled on the connection to Duhem (via Friedrich Adler; see below) only in 1989 (see Howard 1990*a*); the "smoking gun," the remark quoted below from Einstein's electricity and magnetism lectures in Zurich, surfaced only a little over a year and a half ago.

empiricism were at the center, to one in which the basis was a rational realism" (Holton 1968, p. 219).

In outline, Holton's story of the pilgrimage is the following. From at least the time of his first reading of Mach while a student at the Eidgenössishe Technische Hochschule in 1897 or 1898 until the time of the consolidation of general relativity, around 1915, Einstein's philosophical thinking betrayed the clear influence of a Machian, phenomenalist positivism with its attendant ontological scruples. This influence was supposedly already manifest in Einstein's famous argument, in the first special relativity paper of 1905, to the effect that there is no objectivity in judgments of distant simultaneity, because an observer can have no immediate awareness of two spatially distant events (Einstein 1905*d*, pp. 892–95). But, in the wake of his final formulation of the general theory of relativity in 1915 (Einstein 1915*a*, 1915*b*, 1915*c*, 1916*a*), Einstein came to realize that general relativity's ontology of point-events was incompatible with Machian, positivist ontological scruples, and so he converted to a kind of realism that would be more hospitable to the ontology of general relativity. The extent of Einstein's conversion to realism is supposed to be demonstrated by what is now a frequently quoted remark in a letter from Einstein to Moritz Schlick, then the leader of the Vienna Circle, of November 28, 1930:

> Generally speaking, your presentation does not correspond to my way of viewing things, inasmuch as I find your whole conception, so to speak, too positivistic. Indeed, physics *supplies* relations between sense experiences, but only indirectly. For me *its essence* is by no means exhaustively characterized by this assertion. I put it to you bluntly: Physics is an attempt to construct conceptually a model of the *real world* as well as of its law-governed structure. To be sure, it must represent exactly the empirical relations between those sense experiences accessible to us; but *only* thus is it chained to the latter. [Einstein Archives, document 21-603][3]

There is more than a little truth in Holton's account of Einstein's philosophical development, but it still misleads more than it enlightens, and this both with respect to Einstein's alleged early positivism and his alleged later realism. The source of the difficulty is that, look-

3. As quoted in Holton 1968, p. 243; my translation. Items in the Einstein Archives at the Hebrew University of Jerusalem are hereafter cited as EA followed by their numbers in the control index.

ing back from the perspective of the late 1960s, such an analysis employs a simple pair of categories—either positivist or realist—that does not accurately reflect the variety and complexity of the contending points of view in Einstein's day and so misses most of the subtlety, nuance, and originality of Einstein's position. Einstein himself warned us that his philosophy of science could not be so simply characterized. Replying, in 1949, to some critical essays on his philosophy of science by Victor Lenzen and F. S. C. Northrup, Einstein wrote:

> The reciprocal relationship of epistemology and science is of noteworthy kind. They are dependent upon each other. Epistemology without contact with science becomes an empty scheme. Science without epistemology is—insofar as it is thinkable at all—primitive and muddled. However, no sooner has the epistemologist, who is seeking a clear system, fought his way through to such a system, than he is inclined to interpret the thought-content of science in the sense of his system and to reject whatever does not fit into his system. The scientist, however, cannot afford to carry his striving for epistemological systematic that far. He accepts gratefully the epistemological conceptual analysis; but the external conditions, which are set for him by the facts of experience, do not permit him to let himself be too much restricted in the construction of his conceptual world by the adherence to an epistemological system. He therefore must appear to the systematic epistemologist as a type of unscrupulous opportunist: he appears as *realist* insofar as he seeks to describe a world independent of the acts of perception; as *idealist* insofar as he looks upon the concepts and theories as free inventions of the human spirit (not logically derivable from what is empirically given); as *positivist* insofar as he considers his concepts and theories justified *only* to the extent to which they furnish a logical representation of relations among sensory experiences. He may even appear as *Platonist* or *Pythagorean* insofar as he considers the viewpoint of logical simplicity as an indispensable and effective tool of his research. [Einstein 1949, pp. 683–84]

If Einstein appears to the "systematic epistemologist" to be an "unscrupulous opportunist," it is only because the epistemologist fails to read Einstein's philosophy of science in its proper historical and scientific context. Einstein does have a coherent philosophy of science, but it is not simply a kind of realism, or a kind of positivism.

What is wrong with the claim that Einstein was, in his early years, an antimetaphysical, phenomenalist, Machian positivist? After all, Einstein did read Mach with enthusiasm: "In my younger years ... Mach's epistemological position ... influenced me greatly, a position that today appears to me to be essentially untenable" (Einstein 1946, p. 21).[4] Moreover, the critique of distant simultaneity in the 1905 relativity paper (Einstein 1905*d*, pp. 892–95), as well as the discussion of the asymmetry in classical electrodynamics between the cases of the moving magnet and the moving conductor in electromagnetic induction, both cases leading to the same observable consequences (pp. 891–92), is couched in a vocabulary suggestive of something like a verifiability criterion of meaningfulness. The answer is that there are many things wrong with this interpretation.

To begin with, Einstein was writing on more than just relativity theory in the years around 1905. In his papers on capillarity (Einstein 1901, 1902*a*), the foundations of statistical physics (1902*b*, 1903, 1904), Brownian motion (1905*a*), molecular dimensions (1905*b*), and the quantum hypothesis (1905*c*), Einstein betrays no doubt about the reality of the atoms and molecules constituting the fundamental ontology of the kinetic theory of matter. Quite the contrary, Einstein himself believed that one of the chief contributions of these investigations was their proof of the reality of atoms: "My principal aim in this was to find facts that would guarantee as much as possible the existence of atoms of definite size. ... The agreement of these considerations with experience together with Planck's determination of the true molecular size from the law of radiation (for high temperatures) convinced the skeptics, who were quite numerous at that time (Ostwald, Mach), of the reality of atoms" (Einstein 1946, pp. 45, 47). We should remember that, at the same time he was reading Mach, Einstein was also feeling even more strongly the influence of Ludwig Boltzmann, whose *Gastheorie* (Boltzmann 1896*a*, 1898) provided the essential background to his own work on kinetic theory and statistical physics.[5] Boltzmann was famous at the time for his public defense of atomism, being the chief critic of Wilhelm Ostwald and Georg Helm in the energetics controversy.[6]

4. Einstein evidently read both Mach's *Wärmelehre* (Mach 1896) and *Mechanik* (Mach 1897) around 1897 or 1898; for the dating of Einstein's reading of Mach, see Einstein 1989, p. 43, n. 18. Holton cites a number of remarks similar in tone to the one cited here.

5. For Einstein's study of Boltzmann, see Einstein 1989, pp. 41–55, and the annotations to the reprintings of Einstein 1902*b*, 1903, and 1904 in Einstein 1989.

6. For the debate between Boltzmann, Helm, and Ostwald, see, e.g., Boltzmann 1896*b*, 1896*c*, 1897*a*, 1897*b*; Helm 1896, 1898; Ostwald 1895, 1896. A helpful survey of the controversy is found in Deltete 1983.

A careful reading, especially of Einstein's papers on the foundations of statistical physics, reveals that the influence of Mach and Ostwald was being felt.[7] It was not, however, in the form of any doubt about the reality of atoms, but in the form of a caution about prematurely investing these atoms with any properties other than those necessary for the purpose at hand. Thus, in these early statistical papers, the assumption of an atomic theory of the constitution of matter amounts to nothing more than the assumption that the systems under investigation possess a finite number of degrees of freedom. And, in the papers on the quantum hypothesis, Einstein adds the assumption that, in a system of material particles, the joint probability for two particles occupying specific cells of phase space must factorize, an assumption necessary for the derivation of the Boltzmann principle relating entropy and probability.[8] Nothing more is assumed about the nature of material atoms. It was thus not an ontology of unobservables that troubled Einstein; it was merely an ontology that was richer than it need be.

What, then, about the apparently verificationist vocabulary of the 1905 relativity paper? The answer is that one finds anticipations of verificationism only if one goes looking for them. Someone reading the paper in the years after about 1929, someone who wants to claim the example and the authority of Einstein to legitimate emergent Vienna Circle logical empiricism (or its operationalist American cousin), can bend the words of the 1905 relativity paper to fit. But if one approaches the paper with no such prior agenda, it takes on a rather different aspect. The discussion of simultaneity speaks of an "observer," but not in any epistemologically loaded way. If this discussion is informed by any philosophical viewpoint, it is more the conventionalism of Henri Poincaré than the phenomenalism of Mach. Einstein says that a measure of time valid throughout the rest frame can be established only if one "stipulates *by definition*" the equality of the times required for a light signal to go from one stationary observer

7. There is good evidence of Einstein's having read Ostwald's *Lehrbuch* (1891, 1893) during his student days at the Eidgenössishe Technische Hochschule. See the letter from Einstein to Ostwald, March 19, 1901 (Einstein 1987, document 92), and the letter from Einstein to Mileva Marić, December 10, 1901 (Einstein 1987, document 97; see also the editorial notes, "Einstein on the Nature of Molecular Forces" and "Einstein on the Foundations of Statistical Physics," in Einstein 1989.

8. This assumption anticipates the separability principle that we will later find to be essential to Einstein's physicalized or entheorized conception of realism; see below. For more on the history of this principle in Einstein's early writings on the quantum hypothesis, see Howard 1990*b*.

to another and back again (Einstein 1905*d*, p. 894).[9] And the talk of the observational indistinguishability of the moving magnet and moving conductor in electromagnetic induction is equally lacking in the kind of epistemological significance that a positivist might want to find there; after all, we are talking about observing a current, which hardly qualifies as a Machian "element of sensation." What is really bothersome to Einstein is the fact that classical electrodynamics treats these cases asymmetrically—in the first case an electrical field is created in the vicinity of the moving magnet, in the second case there is no field, but rather an electromotive force in the moving conductor—when, from the point of view of the relativity principle, it should make no physical difference whether it is the conductor or the magnet that moves.[10]

And yet, as we saw above, Einstein said that, in his younger years, Mach's epistemological position influenced him greatly. What was this influence? Here we come up against a larger problem, which is that Mach's own works are themselves also too often read from the perspective of the later appropriation of Mach as the precursor of Vienna Circle logical positivism.[11] This means that one tends to overemphasize Mach's phenomenalism and to misconstrue his opposition to empirically ungrounded speculative metaphysics as a wholesale repudiation of any role whatsoever for theory or for unobservables in science. Contemporary readers, Einstein included, read Mach in a somewhat different way, with the emphasis in a different place.

As a measure of how differently Mach was read in the decade of the 1910s, consider the fact that many of Mach's contemporaries saw a significant community of purpose between Mach's writings on epistemology and scientific method and those of a thinker whose views are now thought to be incompatible with Mach's, Pierre Duhem. Comparisons with other period thinkers, such as the Marburg neo-Kantian, critical idealist, Paul Natorp, would be equally enlightening,

9. Poincaré makes a similar point about the equality of time intervals in Poincaré 1902, p. 111. Einstein had evidently read Poincaré 1902 (probably in the German translation, Poincaré 1904) before this section of Einstein 1905*d* was written; see Einstein 1989, p. xxv and p. 307, nn. 4, 9.

10. Michael Friedman (1983, pp. 22–25) is guilty of a similar willful misreading of some epistemologically pregnant passages in Einstein's first major summary of the newly established general theory of relativity (Einstein 1916*a*), seeing in them anticipations of verificationism. For a critique of Friedman's analysis, see Howard 1991*b*, p. 212–14, n. 25.

11. Remember that the members of the Vienna Circle organized themselves into the "Verein Ernst Mach" in 1928. For the attitude toward Mach of the members of the circle, see, e.g., Hahn, Neurath, and Carnap 1929 and von Mises 1938.

but the comparison with Duhem will prove particularly helpful, given Duhem's influence on Einstein, which will be discussed below.

We now regard the epistemological holism of Duhem's *La Théorie physique: son objet et sa structure* (Duhem 1906) to be radically incompatible with the atomistic reductionist phenomenalism that can be found in such works as Mach's *Analyse der Empfindungen* (Mach 1886, 1900). The idea—attributed to Mach—that science can admit only those concepts constructible out of the elements of sensation suggests that each concept and, consequently, each proposition has its own determinate empirical content, an idea that is incompatible with the view—attributed to Duhem—that it is only whole sets of hypotheses, and never an individual hypothesis, that are put to the test of experience. This tension was, to be sure, noted at the time, at least by Duhem's German translator, Friedrich Adler (see Adler 1908, p. vi). But, for the most part, thinkers of that day tended to downplay the significance of such a tension, seeing Duhem and Mach as being both representatives of what Abel Rey dubbed the "new positivism" (Rey 1907, pp. 392 ff., as cited in Frank 1949, p. 21), united by a common antimetaphysical orientation (see Frank 1949, pp. 25–28).

Philipp Frank, for example, described Duhem as the "most important representative of the Machian line of thinking in France" (Frank 1917, p. 66), and Adler wrote of Duhem's *La Théorie physique:* "The elimination of all metaphysics constitutes the fundamental tendency of the work, and the principle of economy of thought, which Mach first formulated, is consistently maintained" (Adler 1908, p. vi). Duhem had some kind things to say about Mach's *Mechanik* in a 1903 review of the French translation (Duhem 1903), and Mach reciprocated, first in correspondence,[12] and later in print, as in his foreword to Adler's German translation of *La Théorie physique* (Mach 1908), a translation that he helped to promote.[13] Most interesting, however, are the footnotes concerning *La Théorie physique* that Mach added to the second edition of his *Erkenntnis und Irrtum* (Mach 1905, 1906). In one, the supposedly antitheoretical Mach commends the characteristically Duhemian thesis of the theory-ladenness of observation: "Claude Bernard advises us to disregard all theory in experimental investigations, to leave theory at the door. Duhem rightly objects that this is impossible in physics, where experiment without theory is incomprehensible. . . . In fact, one can only recommend that attention be given to

12. See, e.g., a letter of Mach to Duhem, May 15, 1904 (reprinted in Hentschel 1988, p. 78), where Mach thanks Duhem for the review.

13. For more on the relationship between Mach and Duhem, see Howard 1990*a* and Paty 1986.

whether or not the experimental result is on the whole compatible with the assumed theory. Cf. Duhem (*La Théorie physique*, pp. 297f)" (Mach 1906, p. 202, n. 3). And, in another, the Mach who is supposedly an epistemological atomist commends Duhem's epistemological holism: "Duhem (La Théorie physique, pp. 364ff) explains that hypotheses are not so much *chosen* by the researcher, arbitrarily and at will, but rather *force* themselves *upon* the researcher in the course of historical development, under the impress of facts that are gradually becoming known. Such a hypothesis usually consists of a whole complex of ideas. If a result then arises, e.g., through an 'experimentum crucis', that is incompatible with a hypothesis, then for the time being one can only regard it as contradicting the *entire complex of ideas*. On this latter point cf. Duhem, l.c., pp. 311f" (Mach 1906, p. 244, n. 1). If Mach himself can thus endorse those very doctrines of Duhem that we now regard as most incompatible with Machian positivism, and if their contemporaries also saw them as engaged in a similar enterprise, then perhaps we misunderstand how Mach intended his position to be interpreted and how others, at the time, did interpret it.

What Mach and Duhem were most eager to oppose was neither hypothesis, nor theory (certainly not all theory), nor even the postulation of unobservables as part of the ontology of one's science. What they were most eager to oppose was, instead, the intrusion of empirically ungrounded, a priori metaphysics in science. Duhem's motive in doing so was to save metaphysics for the theologians; he was a devout Catholic, still fighting the medieval battle over the roles of reason and faith.[14] Mach's motive was to remove metaphysical impediments to the progress of science. *This* was the Mach who impressed so much the young theoretical physicist, Einstein.[15]

There were, of course, differences between Mach and Duhem. Thus, Mach, who tended to see deep theory as a sanctuary for a priori metaphysics in science, was inclined to insist that each individual scientific concept possess impeccable empirical credentials, whereas Duhem was more generous, promoting his epistemological holism as a way of securing a larger role for theory, since it required only whole sets of hypotheses, and not individual concepts or propositions, to pass an empirical test. But these differences really came to the fore only after the German translation of *La Théorie physique* appeared in 1908 (recall that the translator, Adler, mentioned them in his preface),

14. For more on this side of Duhem's thinking, see Jaki 1984 and Martin 1991.

15. For a thoughtful defense of the "revisionist" reading of Mach as a friend of theory in science, see Wolters 1987, 1988.

and especially after the publication in 1909–10 of the famous exchange between Mach and Max Planck, in which Mach, by contrast with Planck, came to appear more radically antitheoretical and antirealist than he may have appeared to readers of his earlier writings.[16] It was certainly not Mach's antirealism or his hostility to theory that attracted the young Einstein when he read the *Mechanik* and the *Wärmelehre* in 1897 or 1898.

Consider what Einstein wrote in his 1916 obituary for Mach. Einstein praises, first and foremost, Mach's "independence of judgment." And when it comes to an assessment of Mach's legacy, Einstein writes:

> Concepts that have proven useful in ordering things easily achieve such an authority over us that we forget their earthly origins and accept them as unalterable givens. Thus they come to be stamped as "necessities of thought," "a priori givens," etc. The path of scientific advance is often made impassable for a long time through such errors. For that reason, it is by no means an idle game if we become practiced in analyzing the long commonplace concepts and exhibiting those circumstances upon which their justification and usefulness depend, how they have grown up, individually, out of the givens of experience. By this means, their all-too-great authority will be broken. They will be removed if they cannot be properly legitimated, corrected if their correlation with given things be far too superfluous, replaced by others if a new system can be established that we prefer for whatever reason. [Einstein 1916*b*, p. 102]

Mach is not prized here for an opposition to deep theory or an ontology of unobservables. Nor is he prized for a phenomenalist epistemology. The only mention of a connection between concepts and experience here is not an epistemological one, but a genetic one, the question being how individual concepts have "grown up" (*herauswachsen*) out of experience, not how they might individually be validated in experience. What Einstein prizes is Mach's method of historical conceptual criticism, as illustrated by the example of Mach's critique of the Newtonian conception of absolute space and, specifically, his criticism of Newton's bucket experiment, the kind of critique

16. See Mach 1910; Planck 1909, 1910; see also Adler's defense of Mach (Adler 1909).

that would obviously have appealed to a younger Einstein in his thinking about special relativity.[17]

If Mach's style of historical conceptual criticism was the aspect of his "epistemological position" that chiefly influenced Einstein in his "younger years," what was it about Mach's position that Einstein later so vigorously repudiated as "essentially untenable"? It was precisely the more niggardly attitude toward deep theory that distinguished Mach from Duhem. Einstein himself explained why Mach's position was untenable: "For he did not place in the correct light the essentially constructive and speculative nature of all thinking and more especially of scientific thinking; in consequence, he condemned theory precisely at those points where its constructive-speculative character comes to light unmistakably, such as in the kinetic theory of atoms" (Einstein 1946, p. 21). And he made much the same point twenty-four years earlier, at the April 6, 1922, session of the Societé française de Philosophie:

> For Mach, there are two points of view to distinguish: on one hand there are the immediate givens of experience, things that we cannot touch; on the other hand, there are concepts which, by contrast, we can modify. Mach's system studies the existing relations between the givens of experience; for Mach, science is the totality of these relations. That point of view is horribly wrong; in short, what Mach has made is a catalogue, not a system. . . . This limited view of science led him to reject the existence of atoms. Probably were he still with us today he would change his opinion. I would like to say, however, that on this point, that concepts can change, I am in complete agreement with Mach. [Societé française de Philosophie 1922, pp. 111–12]

But we should not conclude that, in thus distancing himself from more objectionable features of Mach's epistemology, Einstein is simultaneously confessing and condemning his own youthful errors. Because it was, again, not Mach's phenomenalism that caught the attention of Einstein the university student, but rather Mach's critical attitude toward received concepts that harbor an a priori metaphysics. We should also not conclude, therefore, that, in turning away from Mach's positivism, Einstein was turning toward some kind of realism conceived as an antithesis to a Machian antirealism.

17. Bear in mind that here, as elsewhere, Einstein gives equal credit to Hume as an influence in his thinking about special relativity.

"The physical world is cock-a-doodle-doo":
The Myth of Einstein's Later Conversion to Realism

According to Holton's account, Einstein became a realist after 1915 because he realized that this was required by the event ontology of general relativity. It is true that Einstein believed in the reality of space-time events. But, as we have seen, he believed just as much, from the very beginning, in the reality of the atoms and molecules constituting the ontology of the kinetic theory. Does this mean that he was a realist from beginning? It all depends on what one means by "realism." For, while he believed in the reality of atoms and the space-time manifold, he was never a realist in the specific sense in which that term is employed in the late twentieth-century philosophical literature.

To understand how Einstein could believe in the reality of atoms and space-time events without being a realist requires our understanding something about the intellectual and scientific contexts in which Einstein's mature philosophy of science developed. And the first thing to note is that, in the eyes of Einstein and his contemporaries, a debate between realists and positivists was not the most important controversy of the day. The more important debate between the late 1890s and the mid-1920s, especially for thinkers concerned with the epistemology of relativity theory, was that between the different species of conventionalists and the several varieties of neo-Kantians.[18] Einstein was keenly interested in this debate, for by the late 1910s and early 1920s many of the neo-Kantians had emerged as serious critics of general relativity, with its challenge to the a priori status of Euclidean geometry. He read widely and carefully in the relevant literature, and the position he adopted, a version of Duhemian holism, became the crucial defining feature of his mature philosophy of science.

We now know that many of the essential elements of Einstein's mature position were already in place as early as 1910 (five years before the completion of general relativity), this as a result of his reading of Duhem's *La Théorie physique*. His views were reinforced and refined through his discussions with Moritz Schlick between 1915 and 1921 (before Schlick's move to Vienna). And his philosophy of science took on its more or less final form in the course of his parting of the ways

18. Among the conventionalists one should include, along with Poincaré and Duhem, such thinkers as Eduard Le Roy and Hugo Dingler; among the neo-Kantians, at least the Marburg critical idealists, such as Hermann Cohen, Paul Natorp, and Ernst Cassirer, critical realists, such as Alois Riehl and Bruno Bauch, and adherents of Hans Vaihinger's "als ob" (as if) philosophy. For a more complete catalog of both groups, see Hentschel 1990.

with Schlick (and Hans Reichenbach), between 1921 and 1930. But to understand Einstein's attitude toward realism, we must begin with his study of Duhem.

It was only four years ago that the fact and significance of Einstein's reading of Duhem first came to light (see Howard 1990*a*). The story, in brief, is this. In the autumn of 1909, when Einstein returned to Zurich from his position at the patent office in Bern to take up his first regular academic position at the University of Zurich, he became the upstairs neighbor of his old friend and fellow student Friedrich Adler, who just happened to have been the other candidate for the position Einstein was then assuming.[19] Adler was an ardent supporter of Mach, and one year earlier, partly at Mach's instigation, Adler had published the German translation of Duhem's *La Théorie physique* (Duhem 1908). During the eighteen months that they were neighbors (Einstein moved to the Charles University in Prague in March 1911), they spent long hours in conversation "about questions whose place is generally not understood by the majority of other physicists" (letter from Adler to his parents, October 28, 1909; quoted in Ardelt 1984, p. 166; see also Seelig 1960, p. 165, and letter from Adler to Einstein, March 9, 1917 [EA 6-001]). And it was during this time that Einstein no doubt first read Duhem.

What impressed Einstein from the very start was Duhem's epistemological holism, which Adler had highlighted in his preface to the translation of *La Théorie physique* as marking one of the chief differences between Mach and Duhem (Adler 1908, p. vi). Einstein rightly saw such holism as the key to a kind of empiricism more accommodating to hypothesis and deep theory than was Mach's positivism, inasmuch as it required the empirical validation not of every individual scientific concept and proposition, but only of whole bodies of theory. How quickly Einstein assimilated this central Duhemian thesis is evident from the notes for his lectures on electricity and magnetism during the 1910/11 winter semester at the University of Zurich. The question is how we can assign a definite electrical charge everywhere within a material body, when points on the interior are not accessible to test particles, which a Machian positivist might think necessary for meaningful, empirically controllable talk of such charge distributions. Einstein tells his students:

19. Adler was the son of Viktor Adler, one of the founders of the Austrian Social Democratic party. He was imprisoned during World War I for assassinating the Austrian Minister-President, Count Stürgkh, but was released after the war and went on to play an important role in Austrian political affairs. See Ardelt 1984 for more details.

> We have seen how experience led to the introduction of the concept of electrical charge. It was defined with the help of forces that electrified bodies exert on each other. But now we extend the application of the concept to cases in which the definition finds no direct application as soon as we conceive electrical forces as forces that are not exerted on material particles but *on electricity*. We establish a conceptual system whose individual parts do not correspond immediately to experiential facts. Only a certain totality of theoretical materials corresponds again to a certain totality of experimental facts.
>
> We find that such an el[ectrical] continuum is always applicable only for representing relations inside of ponderable bodies. Here again we define the vector o[f] el[ectrical] field strength as the vector of the mech[anical] force that is exerted on a unit of pos[itive] electr[ical] charge inside a ponderable body. But the force thus defined is no longer immediately accessible to exp[eriment]. It is a part of a theoretical construction that is true or false, i.e., corresponding or not corresponding to experience, only *as a whole*. [Einstein 1993, p. 325]

This point of view on the empirical content of theories corresponds exactly to that developed in chapters 6 (secs. 8 and 9) and 7 of *La Théorie physique*.

At first glance, it might appear that Einstein's enthusiasm for Duhemian holism, with its hospitality toward hypotheses and deep theory, is evidence of a growing sympathy for realism. But there is another side to Duhemian holism that suggests otherwise. For, on Duhem's view, the fact that theories are tested as wholes, and not piece by piece, means that any body of empirical evidence can always be accounted for by any one of several different, empirically equivalent theories. This is the point of Duhem's famous critique of the Newtonian conception of the *experimentum crucis*. If a theory is seemingly refuted by a piece of experimental evidence that disconfirms one of the theory's predictions, one can either replace the theory by a new one, or one can save the old theory by making suitable changes in one or more of the auxiliary hypotheses or boundary conditions required either for the characterization of the experimental setup or for the derivation of the prediction. The result is at least two different total bodies of theory, each of which is consistent with the experimental findings. Our choice of one from among this multiplicity of empirically equivalent theories has the status of a convention.

It is this underdeterminationist conventionalism—a consequence of Duhemian holism—that blocks a quick identification of Duhem's openness toward hypotheses and deep theory as a kind of scientific realism. The problem is that, since there will always be a multiplicity of theories compatible with any body of experimental evidence, each of these theories can equally well lay claim to being a true description of the reality that the physicist is thought to be investigating. And how can there be more than one true description of what ought to be one reality?

The standard answer is, of course, that, with the progress of inquiry, we gradually eliminate the alternatives, so that, in the infinite long run, inquiry converges to a single, final, true theoretical description of the real. But, quite apart from the fact that, in the long run, everyone is dead, the convergentist answer to Duhemian underdeterminationism derives its plausibility only from the frailest of analogies to the mathematical notion of a convergent sequence. It is not obvious that a sequence of theories is like a sequence of numbers. If, at *every stage* in the history of inquiry, one encounters a multiplicity of empirically equivalent theories, then it is not clear why, suddenly, in the infinite long run, that multiplicity should disappear. It seems equally likely that, in the long run, inquiry could take us in any one of many different directions, any one of which would "work" just as well as any other.

This very issue—the tension between Duhemian underdeterminationism and a straightforwardly realistic interpretation of scientific theories—was explored in some detail in the correspondence and discussions between Einstein and Moritz Schlick in the years between 1915 and 1921. As far as can yet be determined, the acquaintance between Einstein and Schlick began in December of 1915, when Schlick sent to Einstein his just-published essay on the philosophical implications of relativity theory (Schlick 1915).[20] Einstein, who had just spent several grueling months putting the finishing touches on general relativity, read it immediately and responded enthusiastically in a letter of 14 December 1915: "Yesterday I received your essay and I have already studied it through completely. It is among the best that have

20. It is possible that Schlick and Einstein had met earlier, since Schlick, who had taken a Ph.D. in physics under Max Planck in Berlin in 1904, was studying philosophy at the University of Zurich at the time of Einstein's return there in October 1909 until his departure for his first academic position at the University of Rostock in 1910; the likelihood of their meeting at that time is increased by the fact that they had several common acquaintances, including the physicists Paul Hertz and Max von Laue. For more on the possibility of an earlier meeting, see Howard 1991*b*, p. 205.

until now been written about relativity. From the philosophical side, nothing appears to have been written on the subject that is at all so clear" (EA 21-610). Over the next six years, there was a philosophically rich correspondence between Schlick and Einstein, interrupted by occasional personal meetings that seem also to have featured the same shared interest in questions about the philosophical implications of relativity. After 1921, Einstein and Schlick begin to grow apart, for reasons to be discussed below. But before that Einstein had the highest opinion of Schlick's abilities as a philosopher, personally promoting Schlick's candidacy for positions at better universities, including his successful candidacy at Kiel in 1921, and arranging for the 1920 English translation of Schlick's influential monograph, *Raum und Zeit in den gegenwärtigen Physik* (Schlick 1917, 1920*a*, 1920*b*).[21]

At the heart of Schlick's 1915 essay is a point of view about the relationship between theory and world—a position reiterated in every one of Schlick's epistemological writings during this period, including his *Allgemeine Erkenntnislehre* (Schlick 1918)—that recalls Duhem's underdeterminationist conventionalism:

> The totality of our scientific propositions, in word and formula, is in fact nothing else but a system of symbols *correlated* to the facts of reality; and that is equally certain, whether we declare reality to be a transcendent being or merely the totality and interconnection of the immediately "given." The system of symbols is called "true," however, if the correlation is completely unambiguous [*eindeutig*]. Certain features of this symbol system are left to our arbitrary choice; we can select them in this way or that without damaging the unambiguous character [*Eindeutigkeit*] of the correlation. It is therefore no contradiction, but lies, rather, in the nature of the matter, that under certain circumstances, several theories may be true at the same time, in that they achieve indeed a different, but each for itself completely unambiguous designation of the facts. [Schlick 1915, p. 149]

And here is how Schlick put the same point in the concluding section of his *Raum und Zeit in den gegenwärtigen Physik,* about which Einstein wrote, in a letter to Schlick of May 21, 1917, "the last section, 'Relations to Philosophy,' seems to me excellent" (EA 21-618):

> It is, however, possible to indicate identically the *same* set of facts by means of *various* systems of judgments; and consequently there can be various theories to which the criterion of truth ap-

21. For further details on Einstein's relationship with Schlick, see Howard 1984.

> plies in the same way, and which then do justice in equal measure to the observed facts and lead to the same predictions. They are just different systems of symbols that are correlated to the same objective reality, different modes of expression that reproduce the same set of facts. [Schlick 1917, p. 62]

Schlick's way of formulating the view that several different theories can equally well account for the same set of facts differs from Duhem's formulation, largely because it assumes Schlick's own, earlier, semiotic conception of the truth: The relationship between a proposition and the fact or reality it aims to describe is like the relation between a sign and what it signifies; and a proposition is true if it is correlated unambiguously to that fact or reality (Schlick 1910). Just how great a difference this implies between Schlick and Duhem is unclear. If the difference between two empirically equivalent theories is no more than that between different modes of expression, two different signs for the same signified, then one might conclude that it is a difference that does not count for much, that the two theories are mere linguistic or translational variants of one another, united by their expression of a common content. By contrast, one has the impression that Duhem sees such difference going much deeper, perhaps to the level of the theories' deep ontologies. Precisely here is where Einstein will make one of his most significant contributions to the discussion.

Schlick's position is also distinguished by his rather original "logical" conception of simplicity, according to which the simpler theory is the one with fewer "arbitrary" elements. The arbitrary components of a theory reflect a human choice, not a physical reality, for "a theory represents reality only to the extent that it is determined just by the objective facts." And simpler theories are then to be preferred over more complicated ones, because "then we are certain to stray at least no farther from reality than required by the limits of our knowledge" (Schlick 1915, pp. 154–55). Schlick was confident that one of the competing theories will always emerge as the simplest, so that while experience may not determine our choice of a correct theory, experience plus the criterion of simplicity always will (Schlick 1915, pp. 149–50; 1917, p. 62).

Schlick's many discussions of underdetermination elicited from Einstein a series of thoughtful commentaries in which he endorsed the idea that experience alone does not unambiguously determine our choice of a theory. Einstein agreed that simplicity must be our guide in choosing among empirically equivalent theories, but, eventually, he came to doubt that simplicity is an objective criterion. And he was especially cautious about what underdetermination implied for a theory's claim to represent a unique deep reality.

A major theme in the aforementioned last section of *Raum und Zeit in den gegenwärtigen Physik* is Schlick's claim that the event ontology of general relativity should be construed just as realistically as Mach would have us construe the elements of sensation (Schlick 1917, pp. 58–60). Schlick leaves no doubt about the philosophical tendency of such a claim:

> It appears to me to be an arbitrary, indeed dogmatic stipulation if one lets only the intuitive elements and their relations count as *real*. . . . This often unsharp and shifting opposition between concepts that designate the real and concepts that are only auxiliary constructions is ultimately not sustainable, and we avoid it through what is certainly a permissible assumption, that every concept that is actually useful for describing nature may also be viewed in the same way as a symbol for something real. I believe that, in the striving for ultimate epistemological clarity, this assumption need never be abandoned, and that it makes possible a well-grounded, coherent world view that satisfies the intellectual demands of the "realists," without, nevertheless, giving up any of the advantages for which one justifiably praises the positivistic world view. [Schlick 1917, pp. 60–61]

Remember, by the way, that this was written before Schlick's move to Vienna; this is the Schlick who was the true student of Mach's antagonist, Max Planck, the Schlick who at this time still consistently identified himself as a realist.

Schlick's argument that the Machian elements of sensation and space-time events should be seen as equally real was the only point of philosophical substance in Schlick's *Raum und Zeit in den gegen wärtigen Physik* that Einstein chose to discuss in their correspondence from this period. And, instead of simply agreeing with Schick, he suggested that one needs to distinguish two different senses of "reality." In his previously cited letter to Schlick of May 21, 1917, he wrote:

> The second point to which I want to refer concerns the reality concept. Your view stands opposed to Mach's according to the following schema:
>
> Mach: Only impressions are real.
>
> Schlick: Impressions and events (of a phys[ical] nature) are real.

> Now it appears to me that the word "real" is taken in different senses, according to whether impressions or events, that is to say, states of affairs in the physical sense, are spoken of.
>
> If two different peoples pursue physics independently of one another, they will create systems that certainly agree as regards the impressions ("elements" in Mach's sense). The mental constructions that the two devise for connecting these "elements" can be vastly different. And the two constructions need not agree as regards the "events"; for these surely belong to the conceptual constructions. Certainly only the "elements," but not the "events," are real in the sense of being "given unavoidably in experience."
>
> But if we designate as "real" that which we arrange in the space-time-schema, as you have done in the theory of knowledge, then without doubt the "events," above all, are real.
>
> Now what we designate as "real" in physics is, no doubt, the "spatio-temporally-arranged," not the "immediately-given." The immediately-given can be illusion, the spatio-temporally arranged can be a sterile concept that does not contribute to illuminating the connections between the immediately-given. *I would like to recommend a clean conceptual distinction here.* [EA 21-618]

Savor the implications of Einstein's suggestion. His "two different peoples" story makes it clear that he takes seriously the fact of the empirical underdetermination of theory choice. But his careful delineation of the areas where these two peoples are likely to agree and disagree makes it equally clear that, for Einstein, the differences between two empirically equivalent theories at the level of deep theory are far more significant than one might infer from Schlick's characterization of the difference as merely a difference in "modes of expression." For what Einstein suggests is that, at the level of a theory's deep ontology, the two different peoples can have two different realities. This is surely discomfiting to anyone who wants to interpret the Einstein who applauds Duhemian holistic underdeterminationism for its hospitality toward deep theory as, still, a realist in any of the currently fashionable senses of that term, however content Schlick may have been with that label.

Einstein's desire to avoid being mistaken as a realist seems only to have grown during the years following the completion of general relativity, precisely the time when, according to the Holton scenario, Einstein's conversion to realism was taking place. His exchange with Schlick over the various meanings of "reality" was surely part of the background to his discussion of some of the same issues in a September 25, 1918, letter to the Bonn mathematician, Eduard Study, a letter that also contains the only explicit mention of Duhem that I have ever found in Einstein's writings or correspondence. Einstein's letter is a detailed critical response to Study's book *Die realistische Weltansicht und die Lehre vom Raume* (1914). On the whole, Einstein enjoyed the book, in large part because of its unorthodox, freewheeling style. But Study's main point was to defend scientific realism against a variety of critics, including positivists and conventionalists, and it was principally this defense of scientific realism that Einstein criticized:

> I am supposed to explain to you my doubts? By laying stress on these it will appear that I want to pick holes in you everywhere. But things are not so bad, because I do not feel comfortable and at home in any of the "isms." It always seems to me as though such an ism were strong only so long as it nourishes itself on the weakness of it counterism; but if the latter is struck dead, and it is alone on an open field, then it also turns out to be unsteady on its feet. So, *away with the squabbling.*
>
> "The physical world is real." That is supposed to be the fundamental hypothesis. What does "hypothesis" mean here? For me, a hypothesis is a statement, whose *truth* must be assumed for the moment, *but whose meaning must be raised above all ambiguity.* The above statement appears to me, however, to be, in itself, meaningless, as if one said: "The physical world is cock-a-doodle-doo." It appears to me that the "real" is an intrinsically empty, meaningless category (pigeon hole), whose monstrous importance lies only in the fact that I can do certain things in it and not certain others. This division is, to be sure, not an *arbitrary* one, but instead ......
>
> I concede that the natural sciences concern the "real," but I am still not a realist. [EA 22-307]

Not only does underdetermination imply that two different peoples can produce empirically equivalent theories with different deep ontologies and, hence, different deep realities, but the very concept of the real is an "empty, meaningless ... pigeon hole." Can there be any doubt that interpreting Einstein as a realist misses something essential in his philosophy of science?

That Duhem's holism and his defense of the empirical integrity of deep theory was not far from Einstein's mind at this point is evident from the fact that the above-quoted critique of realism is followed, closely, by this criticism of positivism, with an interlineated endorsement of Duhem (indicated with slashes):

> The positivist or pragmatist is strong as long as he battles against the opinion that there [are] concepts that are anchored in the "A priori." When, in his enthusiasm, [he] forgets that all knowledge consists [in] concepts and judgments, then that is a weakness that lies not in the nature of things but in his personal disposition /just as with the senseless battle against hypotheses, cf. the clear book by Duhem/. In any case, the railing against atoms rests upon this weakness. Oh, how hard things are for man in this world; the path to originality leads through unreason (in the sciences), through ugliness (in the arts)—at least the path that many find passable. [EA 22-307][22]

Einstein is still a champion of hypothesis and deep theory. He still wants to make room for the event ontology of general relativity. But he sees no gain, no point even, in characterizing the elements of such an ontology as real, unless one is ready to accept multiple realities.

Theory is underdetermined by empirical evidence—this was such an important lesson for Einstein in the late 1910s that he used every available opportunity to reinforce the point. Thus, in a 1918 address in honor of Planck's sixtieth birthday, Einstein wrote:

> The supreme task of the physicist is . . . the search for those most general, elementary laws from which the world picture is to be obtained through pure deduction. No logical path leads to these elementary laws; it is instead just the intuition that rests on an empathic understanding of experience. In this state of methodological uncertainty one can think that arbitrarily many, in themselves equally justified systems of theoretical principles were possible; and this opinion is, *in principle*, certainly correct. But

22. As noted above, this is the only mention of Duhem's name that I have found anywhere in Einstein's writings or correspondence. If Einstein read Duhem and was so heavily influenced by his distinctive form of holism and underdeterminationist conventionalism, why is there no other mention of Duhem? Why does Einstein always cite Poincaré in discussions of conventionalism, even where the particular version of conventionalism obviously owes more to Duhem than to Poincaré, as in Einstein's famous essay, "Geometrie und Erfahrung" (Einstein 1921; see also Howard 1990*a*)? One possible explanation is that it is because, for all his brilliance as a philosopher of science, Duhem was also well known to Einstein as the author of a scurrilous piece of wartime, anti-German propaganda, *La Science allemande* (Duhem 1915), a sin for which Einstein—determined proponent of international intellectual cooperation, even in wartime—would never have forgiven Duhem. For more on Einstein's opposition to such propaganda, see Nathan and Norden 1968.

> the development of physics has shown that of all the conceivable theoretical constructions a single one has, at any given time, proved itself unconditionally superior to all the others. No one who has really gone deeply into the subject will deny that, in practice, the world of perceptions determines the theoretical system unambiguously, even though no logical path leads from the perceptions to the basic principles of the theory. [Einstein 1918, p. 31]

How Einstein understood the practical constraints obscuring the underdetermination of theory, in principle, is not clear, but the "two different peoples" story of the May 21, 1917, letter to Schlick suggests that the practical constraints are cultural in nature, that it is our socialization in a single community of scientists—we are one people, not two—that leads us to think that one theory is always obviously superior.[23]

One year later, on December 25, 1919, Einstein returned yet again to the theme of underdetermination, this time in a special supplement to the *Berliner Tageblatt* celebrating the accomplishments of German science during the First World War:

> A theory can thus be recognized as erroneous if there is a logical error in its deductions, or as incorrect if a fact is not in agreement with its consequences. But the *truth* of a theory can never be proven. For one never knows that even in the future no experience will be encountered that contradicts its consequences; and still other systems of thought are always conceivable that are capable of joining together the same given facts. If two theories are available, both of which are compatible with the same given factual material, then there is no other criterion for preferring the one or the other than the intuitive view of the researcher. Thus we may understand how sharp-witted researchers, who have command of theories and facts, can still be passionate supporters of contradictory theories. [Einstein 1919, p. 1]

Especially helpful here is Einstein's careful distinction between Humean inductive uncertainty and Duhemian underdeterminationism, as well as his explicit statement that the empirically equivalent alternative theories may even be mutually contradictory, a degree of difference that is, again, more extreme than Schlick's "different modes of expression."

23. Duhem may also be the source for the distinction of principle and practice; see Howard 1990*a*, p. 370.

Experience alone does not uniquely determine our choice of a theory. Practical constraints may obscure the resulting freedom of choice that the scientist enjoys, but this freedom is nevertheless important from the point of view of the epistemology of science. It was precisely this freedom of choice that Einstein had in mind with his repeated characterization of theories as the "free creations of the human spirit" (see, e.g., Einstein 1921, p. 5). And it was precisely the positivists' neglect of this freedom that Einstein eventually came to regard as their chief failing. The above-quoted passage from Einstein's "Autobiographical Notes" concerning Ostwald and Mach's skepticism about the reality of atoms continues thus:

> The hostility of these scholars toward atomic theory can undoubtedly be traced back to their positivistic philosophical attitude. This is an interesting example of the fact that even scholars of audacious spirit and fine instinct can be hindered in the interpretation of facts by philosophical prejudices. The prejudice—which has by no means disappeared—consists in the belief that facts by themselves can and should yield scientific knowledge without free conceptual construction. Such a misconception is possible only because one does not easily become aware of the free choice of such concepts, which, through success and long usage, appear to be immediately connected with the empirical material. [Einstein 1946, p. 47]

"Success and long usage" are some of the practical constraints, part of the theoretician's cultural heritage, that obscure the theoretician's freedom of choice.

Holism and underdeterminationism were to become two abiding themes in Einstein's writings about scientific methodology in the ensuing years. They were to loom especially large in distinguishing Einstein's philosophy of science from the emerging logical empiricism of the later Schlick and his various friends and allies in Vienna, Prague, Berlin, and elsewhere.[24] By the early 1920s, Einstein, on the one hand, and Schlick and Hans Reichenbach, on the other hand, were beginning to pursue very different approaches to defending the empirical integrity of general relativity against a variety of neo-Kantian critiques. They all agreed that what was needed was a new kind of empiricism that would steer a middle course between Kantian apriorism and radical Machian positivism. They conceded the Kantian

24. For a more detailed account of Einstein's gradual alienation from Schlick, and their different ways of replying to the neo-Kantians, see Howard 1993.

point that we cannot do science without bringing to our inquiry certain organizing principles that are not, individually, dictated by experience, but, being good, fallibilist empiricists, they wanted to deny to such principles any kind of apodicticity. And they agreed that the way to do this was to characterize these principles not as synthetic a priori truths, but as conventions. Where they differed was in locating the place of these conventions in science.

Schlick and Reichenbach came to believe that the propositions constituting any scientific theory could be divided, in a principled way, into two groups: coordinating definitions and empirical hypotheses. Only the former, including such propositions as the ostensive definition of a unit measuring rod, were to be established by conventional stipulation, and, once they were so established, each of the remaining empirical hypotheses was thereby invested with its own, individual empirical content, so that its truth or falsity could be determined unambiguously by experience. By thus focusing the force of an empirical test on individual empirical hypotheses, as opposed to an entire body of theory, Schlick and Reichenbach provided themselves with a powerful argument against neo-Kantian defenders of the a priori status of Euclidean geometry: Once one stipulates, by means of a conventional coordinating definition, what will count as a rigid, unit measuring rod, then the metrical relations of physical space, and so one's geometry, are determined unambiguously by experience. A different geometry might have been obtained if a different coordinating definition had been chosen, designating a different physical object as the rigid, unit measuring rod. But the role of conventions in science is thus carefully circumscribed, for it is the coordinating definition, and not the geometry itself, that has the status of a convention.

Einstein continued to support a more holistic view of the structure of theories, refusing to concede any principled basis for distinguishing coordinating definitions and empirical hypotheses, and emphasizing instead the arbitrariness of any distinction between the conventional and the empirical. For Einstein, like Duhem, every proposition in a scientific theory had both conventional and empirical aspects; if a distinction were to be made, it would be for reasons of passing convenience only. Indeed, the arbitrariness of any such distinction—now between the a priori and the a posteriori—was Einstein's principal argument against the neo-Kantians. Einstein saw in the Schlick-Reichenbach approach a return to the epistemological atomism of Mach, with its more niggardly attitude toward theoretical propositions that could not be grounded, individually, in experience. It was this suspected hostility to deep theory on Schlick's part, and not any sympathy for scientific realism, that led Einstein, in the above-quoted letter of November 28, 1930, the letter that looms so large in Holton's case for Einstein's post-1915 conversion to realism, to condemn Schlick as having become "too positivistic."

Even though the essential elements of Einstein's mature philosophy of science, featuring holism and an underdeterminationist variety of conventionalism, were in place by the late 1910s, there was one issue on which Einstein wavered over the next forty years, and that was the significance of simplicity as a criterion of theory choice. As we saw, Schlick defended a logical criterion of simplicity, according which simple theories are theories with relatively fewer arbitrary elements, such theories being preferable because they more adequately reflect physical reality, rather than the subjective choice of the scientist. And he believed that this criterion would always distinguish one among a set of competing, empirically equivalent theories as the simplest.

Throughout the late 1910s, the 1920s, and the early 1930s, Einstein was also inclined to think that a simplicity criterion would always favor, unambiguously, one theory over its empirically equivalent rivals. It was evidently the success of his search for a general theory of relativity, which he later described as a search for the mathematically simplest set of field equations compatible with the principle of general relativity (see, e.g., Einstein 1946, p. 65), that led him to have such faith in simplicity as a criterion of theory choice. In a letter to Cornel Lanczos on January 24, 1938, he wrote: "Coming from sceptical empiricism of somewhat the kind of Mach's, I was made, by the problem of gravitation, into a believing rationalist, that is, one who seeks the only trustworthy source of truth in mathematical simplicity. The logically simple does not, of course, have to be physically true; but the physically true is logically simple, that is, it has unity at its foundation" (as quoted in Holton 1968, p. 241). This faith in simplicity was never more clearly expressed than in his 1933 Herbert Spencer lecture:

> Our experience hitherto justifies us in believing that nature is the realization of the simplest conceivable mathematical ideas. I am convinced that we can discover by means of purely mathematical constructions the concepts and the laws connecting them with each other, which furnish the key to the understanding of natural phenomena. Experience may suggest the appropriate mathematical concepts, but they most certainly cannot be deduced from it. Experience remains, of course, the sole criterion of the physical utility of a mathematical construction. But the creative principle resides in mathematics. In a certain sense, therefore, I

> hold it true that pure thought can grasp reality, as the ancients dreamed. [Einstein 1933, pp. 12–13]

And three years later he was even more explicit about the degree of determination furnished by simplicity considerations in situations where, logically, we have a free choice among alternative theories: "This freedom is, however, not that great. It is not similar to the freedom of a writer of novels; rather, it is similar to that of a man who is given a well-designed word puzzle. To be sure, he can propose any word as the solution, but there is, indeed, only *one* word that really solves the puzzle in all its parts. That nature—as it is accessible to our senses—has the character of such a well-formulated puzzle is a matter of faith, in which we are to some extent encouraged, it is true, by the successes of science to date" (Einstein 1936, p. 318). One reason why he may have had such a strong faith in simplicity at precisely this time is that Hubble's discovery of the expansion of the universe had allowed Einstein in 1931 to remove the cosmological constant from the field equations for general relativity, a term added, in the first place, in an ad hoc fashion, solely for the purpose of blocking nonstatic solutions. With this offense against mathematical simplicity gone, Einstein's faith in simplicity as the road to truth would surely have been restored.[25]

In assessing the larger methodological and epistemological implications of Einstein's attitude toward simplicity as a criterion of theory choice, one must bear in mind, however, his earlier firm assertion that all of the apparent determination in theory choice, beyond that furnished by experience alone, is a determination in practice, not in principle. It is not at all obvious, therefore, that Einstein regarded experience and simplicity as together leading us unambiguously to a uniquely privileged theoretical representation of a unique reality. Our doubts in this matter grow even stronger when we observe that Einstein's faith in a straightforward, objective measure of the simplicity of theories began to wane by the decade of the 1940s. The cause may have been the growing frustration he felt over his failure to discover a satisfactory unified field theory, considerations of mathematical simplicity having no longer worked as the sure guide they had once seemed to him. But, whatever the reason, the shift in his thinking in these years is clear. Here is what he says about simplicity criteria in 1946, in his "Autobiographical Notes":

25. For details, see Ellis 1989, pp. 382–84, and Pais 1982, pp. 281–88. I thank Robert W. Smith for suggesting this connection between Einstein's faith in simplicity and his abandonment of the cosmological constant.

> This point of view, whose exact formulation meets with great difficulties, has played an important role in the selection and evaluation of theories from time immemorial. The problem here is not simply one of a kind of enumeration of the logically independent premises (if anything like this were at all possible without ambiguity), but one of a kind of reciprocal weighing of incommensurable qualities. . . . [This] point of view may briefly be characterized as concerned with the "inner perfection" of the theory. . . . I shall not attempt to excuse the lack of precision of the assertions contained in the last two paragraphs on the grounds of insufficient space at my disposal; I must confess herewith that I cannot at this point, and perhaps not at all, replace these hints by more precise definitions. I believe, however, that a sharper formulation would be possible. In any case, it turns out that among the "oracles" there usually is agreement in judging the "inner perfection" of the theories. [Einstein 1946, pp. 21, 23]

A "reciprocal weighing of incommensurable qualities" can hardly be considered a "method" whereby physicists discover a uniquely correct theoretical representation of a unique reality. If the "oracles" tend always to agree, then it is more likely for the kinds of cultural reasons that lead "one people," as opposed to Einstein's "two different peoples," to agree more often than they disagree.

To the end of his life, Einstein continued to maintain an essentially holistic view of theories. Thus, in his 1946 "Autobiographical Notes," he wrote: "A system has truth-content according to the certainty and completeness of its possibility of coordination with the totality of experience. A correct proposition borrows its 'truth' from the truth-content of the system to which it belongs" (Einstein 1946, p. 13). And as late as 1949, Einstein was still trying to explain the shortcoming in the Schlick-Reichenbach position, which, by making a principled distinction between conventional coordinating definitions and empirical hypotheses, supposedly enables one to identify a determinate empirical content for each individual empirical hypothesis. In a wonderfully lucid reply to an essay by Reichenbach entitled "The Philosophical Significance of the Theory of Relativity" (Reichenbach 1949), Einstein wrote:

> If, under the stated circumstances, you hold distance to be a legitimate concept, how then is it with your basic principle (meaning = verifiability)? Must you not come to the point where you deny the meaning of geometrical statements and concede mean-

ing only to the completely developed theory of relativity (which still does not exist at all as a finished product)? Must you not grant that no "meaning" whatsoever, in your sense, belongs to the individual concepts and statements of a physical theory, such meaning belonging instead to the whole system insofar as it makes "intelligible" what is given in experience? Why do the individual concepts that occur in a theory require any separate justification after all, if they are indispensable only within the framework of the logical structure of the theory, and if it is the theory as a whole that stands the test? [Einstein 1949, p. 678][26]

Note that Einstein wrote this in 1949, two years before Quine's more famous statement of a similar criticism in his "Two Dogmas of Empiricism" (Quine 1951). Is it any surprise that when, in a conversation with Rudolf Carnap and Paul Oppenheim on November 16, 1952, Einstein was first told about Otto Neurath's vivid boat metaphor for Duhemian holism—the task of theory is like rebuilding a boat not in dry dock, but at sea, one plank at a time—Carnap recorded the following reaction: "With that he emphatically agreed" (Rudolf Carnap Papers, document RC 025-80-01).[27]

If experience does not unambiguously determine theory choice, if, consequently, two different peoples pursuing physics independently of one another can develop theories with radically different deep ontologies, theories that are even mutually contradictory, then a "realistic" interpretation of scientific theories, in the sense of that term current in the late twentieth-century philosophical literature, seems not to be justified. And so, Einstein says, he is not a realist. And yet, as Einstein also tells us, he agrees that "the natural sciences concern the 'real.'" What, then, is place of this reality in theoretical science, from Einstein's point of view?

Arthur Fine suggests a very helpful answer to this question. He argues that, for Einstein, realism is not so much a global doctrine about the interpretation of scientific theories as it is a *program* for science. It is Einstein's way of encouraging his fellow scientists to pursue realistic theories as being most likely to lead to progress in science. Thus, Einstein writes to Mario Laserna on January 8, 1955: "It is basic for physics that one assumes a real world existing independently from any act of perception. But this we do not *know*. We take it only as a programme in our scientific endeavors" (EA 15-079; as quoted in Fine

26. The translation has been corrected on the basis of Einstein's original German text, published in Einstein 1954, p. 503.

27. Quoted by permission of the University of Pittsburgh. All rights reserved.

1986, p. 95; see also Howard 1991*a*, p. 139). What counts as realistic theory? Here, too, Fine has a helpful suggestion. It is that, as with other questions, such as determinism, Einstein's way of proceeding is to entheorize a concept like "reality," which is to say that it is physicalized, being turned into a physical principle that can play a role in the foundations of our physical theories alongside any other principle, such as energy conservation or the light principle (Fine 1986, pp. 87–88). The success of theories incorporating such a physicalized or entheorized reality principle is what is meant by the success of the realistic program. Now the only question is, How does Einstein entheorize the real?

"I just want to explain what I mean when I say that we should try to hold on to physical reality": Einstein on Realism and Separability

In December 1947, Max Born sent Einstein a manuscript copy of the Waynflete Lectures, *Natural Philosophy of Cause and Chance* (Born 1949), that he was to deliver the following spring in Oxford, soliciting Einstein's reaction to the account Born gave there of his discussions with Einstein about the quantum theory. Einstein sent the manuscript back on March 18, 1948, with numerous marginal comments, including the following long remark at the end of the manuscript:

> I just want to explain what I mean when I say that we should try to hold on to physical reality. We are, to be sure, all of us aware of the situation regarding what will turn out to be the basic foundational concepts in physics: the point-mass or the particle is surely not among them; the field, in the Faraday-Maxwell sense, might be, but not with certainty. But that which we conceive as existing ("actual") should somehow be localized in time and space. That is, the real in one part of space, A, should (in theory) somehow "exist" independently of that which is thought of as real in another part of space, B. If a physical system stretches over the parts of space A *and* B, then what is present in B should somehow have an existence independent of what is present in A. What is actually present in B should thus not depend upon the type of measurement carried out in the part of space, A; it should also be independent of whether or not, after all, a measurement is made in A.
>
> If one adheres to this program, then one can hardly view the quantum-theoretical description as a *complete* representation of the physically real. If one attempts, nevertheless, so to view it, then one must assume that the physically real in B undergoes a

sudden change because of a measurement in A. My physical instincts bristle at that suggestion.

However, if one renounces the assumption that what is present in different parts of space has an independent, real existence, then I do not at all see what physics is supposed to describe. For what is thought to be a "system" is, after all, just conventional, and I do not see how one is supposed to divide up the world objectively so that one can make statements about the parts. [Born 1969, pp. 223–24]

This is not at all what a late twentieth-century scientific realist might have expected in an explanation of what it means to "hold on to physical reality." It is not a global thesis about truth, reference, or the interpretation of theories. It is not a claim about scientific inquiry ultimately converging to one final theory. It is not a claim about inference to the best explanation. It is not even, at first glance, at least, a claim about objectivity or the independence of observer and observed. What it is, is the claim that we should cling to a certain physical principle, according to which the real in one part of space exists independently of the real in another part of space. And the reason given for adhering to this principle is that, without it, physics would be impossible, because one would have no way to individuate the systems that physics aims to describe.

In order to understand what Einstein means by this principle, which I will call the "separability principle," we must go back a number of years to survey Einstein's steadily growing misgivings about the quantum theory as a framework for a fundamental physics. Einstein was, of course, one of the founders of the quantum theory, with his 1905 paper on the photon hypothesis (Einstein 1905*a*). And, over the years, he made many essential contributions to its development.[28] But by the time of the 1927 Solvay conference, he had emerged as a determined critic, especially of the form the theory was then taking in the hands of Niels Bohr, Erwin Schrödinger, and Werner Heisenberg. And certainly by the time of the famous Einstein-Podolsky-Rosen (EPR) paper (Einstein et al. 1935), in which the quantum theory is criticized as being incomplete, Einstein's nearly total estrangement from the rest of the physics community over the adequacy of quantum mechanics had become a major embarrassment to his colleagues. Many myths have grown up around Einstein's critique of the quantum theory, the most offensive one being that, by the mid-1930s, Einstein had grown

28. For more on Einstein's contributions to the quantum theory, see Pais 1982, pp. 357–469.

too inflexible in his thinking to understand the new quantum theory, that he was an old man, stuck in the past and clinging, dogmatically, to his obsolete ideal of a deterministic fundamental physics. Nothing could be further from the truth.

This is not the place to examine all of the many misconceptions about Einstein's critique of the quantum theory.[29] But one of the most persistent legends must be discussed, namely, the legend that the EPR paper accurately represents Einstein's reasons for thinking the quantum theory incomplete.

Arthur Fine first drew our attention to the fact that Einstein did not write the EPR paper and did not like the form in which the argument for the incompleteness of quantum mechanics is there presented (Fine 1981; see also Howard 1985). In a letter to Schrödinger of June 19, 1935, Einstein explained about the EPR paper: "For reasons of language, this was written by Podolsky after many discussions. But still it has not come out as well as I really wanted; on the contrary, the main point was, so to speak, buried by the erudition" (EA 22-047). What was this "main point"? Einstein goes on to explain his own point of view in this and several later letters to Schrödinger. At the heart of Einstein's own version of the argument is what he terms a "principle of separation" [*Trennungsprinzip*], according to which the real state of affairs in one region of space-time is independent of whatever might be done in another region of space-time separated from the first by a spacelike separation. Applied to a thought experiment involving previously interacting systems, A and B, of the kind employed in the EPR paper, the separation principle implies that the real state of system A, for example, will be unaffected by our choice of a measurement to perform on system B. But quantum mechanics attributes different theoretical states (two different ψ functions) to A, depending on the measurement we choose to perform on B, which is to say that quantum mechanics assigns two different theoretical states to one and the same real state of A. Hence, quantum mechanics is incomplete.

This is certainly a clearer and more elegant argument than the one found in the original EPR paper. Among other things, it makes no essential reference to the Heisenberg indeterminacy principle. And it also makes perfectly clear the reason why the defenders of quantum mechanics would not be moved by the argument: Quantum mechanics does not satisfy the separation principle. It was precisely this aspect of the theory—the necessary role of entangled or nonfactorizable joint states for interacting systems—that Schrödinger went on to develop in

29. Stachel 1986 is a helpful place to start; see also Howard 1985, 1989, and 1990*b*.

detail in a pair of papers inspired by his correspondence with Einstein (Schrödinger 1935, 1936).

We now know that the quantum theory's failure to satisfy the principle of separation was something that had concerned Einstein from the time of his very first paper on the photon hypothesis in 1905.[30] By 1909 he had come to understand that quanta of radiation differ from material particles in that the joint probability for two spatially separated light quanta to occupy specific cells of phase space does not factorize. Since this factorizability is a necessary condition for the derivation of Boltzmann's entropy principle, $S = k \cdot \log(W)$, it follows that quanta will obey a fundamentally different kind of statistics than do material particles. As long as this was a property peculiar to light quanta, it was nothing more than a puzzle. But then in 1924, stimulated by Satyendra Nath Bose's new derivation of the Planck radiation law (Bose 1924), Einstein himself proved that, according to the quantum theory, material particles, such as electrons, must satisfy the same kind of non-Boltzmannian statistics, exhibiting the same kind of strange, long-distance correlations found among quanta of radiation (Einstein 1924, 1925*a*, 1925*b*). This was Einstein's last positive contribution to the development of the quantum theory. He seems to have realized that there was no longer any possibility of a reconciliation between the quantum theory, which violates the separation principle, and field theories, such as general relativity, that satisfy the separation principle. Immediately he turned his attention back to the search for a unified field theory satisfying the separation principle, and he launched his thirty-year campaign to convince his colleagues that the quantum theory was fatally flawed by its failure to satisfy the separation principle. The 1935 EPR paper was only one in a long series of efforts to explain this point.

After 1935, Einstein repeated his own, preferred version of the incompleteness argument, both in correspondence with various of his colleagues and acquaintances, and several times in print.[31] During this time, Einstein's argument underwent one essential refinement. He further analyzed the earlier separation principle into what he now presented as two logically independent principles, which I call the separability and locality principles. The first, the separability principle, says that spatiotemporally separated systems possess their own separate physical states, even if they have previously interacted with one another. The second, the locality principle, says that the physical state

30. For a more detailed version of this story, see Howard 1990*b*.

31. See Howard 1985 for details.

of a system in one region of space-time cannot be influenced by events in another region of space-time separated from the first by a spacelike interval. The locality principle encapsulates the relevant special-relativistic locality constraints on physical interactions, specifically, the assumption that there are no superluminal signals. The separability principle expresses a much more basic ontological assumption about the way in which we individuate physical systems and states. This latter principle, especially, is what Einstein wanted to defend as a necessary part of a fundamental physics.

Perhaps the clearest statement of the distinction between the separability and locality principles, and the clearest statement about the central role that the former—separability—plays in field theories like general relativity, is found in an essay, "Quantenmechanik und Wirklichkeit," that Einstein wrote in 1948 for a special issue of the Swiss journal, *Dialectica*, an issue edited by Wolfgang Pauli and devoted to the interpretation of quantum mechanics. In it, Einstein wrote:

> If one asks what is characteristic of the realm of physical ideas independently of the quantum theory, then above all the following attracts our attention: the concepts of physics refer to a real external world, i.e., ideas are posited of things that claim a "real existence" independent of the perceiving subject (bodies, fields, etc.). . . . Moreover, it is characteristic of these physical things that they are conceived of as being arranged in a space-time continuum. Further, it appears to be essential for this arrangement of the things introduced in physics that, at a specific time, these things claim an existence independent of one another, insofar as these things "lie in different parts of space." Without such an assumption of the mutually-independent existence (the "being-thus") of spatially distant things, an assumption that originates in everyday thought, physical thought in the sense familiar to us would not be possible. Nor does one see how physical laws could be formulated and tested without such a clean separation. Field theory has carried out this principle to the extreme, in that it localizes within infinitely small (four-dimensional) space-elements the elementary things existing independently of one another that it takes as basic, as well as the elementary laws it postulates for them.
>
> For the relative independence of spatially distant things (A and B), this idea is characteristic: an external influence on A has no *immediate* effect on B; this is known as the "principle of local

> action," which is applied consistently only in field theory. The complete suspension of this basic principle would make impossible the idea of the existence of (quasi-) closed systems and, thereby, the establishment of empirically testable laws in the sense familiar to us. [Einstein 1948, pp. 321–22]

What this passage makes clear is that, for Einstein, all talk of physical reality, all talk of a real external world described by physics, rests ultimately on the physical principle of separability, the mutually independent existence (the "being-thus") of spatiotemporally separated systems.

Thus, holding on to physical reality means, for Einstein, holding on to the separability principle. In the final analysis, quantum mechanics offends not so much because it is nondeterministic, but because it denies separability, and thus fails to qualify, by Einstein's standards, as a realistic theory. By contrast, a field theory like general relativity is the epitome of a realistic theory. But why is separability so important?

Einstein says that, without the principle of separability, "physical thought in the sense familiar to us would not be possible." "Nor," he adds, "does one see how physical laws could be formulated and tested without such a clean separation." Why? He explains in the previously quoted comment on Born's Waynflete Lectures: "If one renounces the assumption that what is present in different parts of space has an independent, real existence, then I do not at all see what physics is supposed to describe. For what is thought to be a 'system' is, after all, just conventional, and I do not see how one is supposed to divide up the world objectively so that one can make statements about the parts." The point seems to be that the enterprise of physical theory cannot get started until we first stipulate what will count, by convention, as the physical systems we aim to describe, which is to say that we must have some criterion of individuation for the physical systems constituting a theory's fundamental ontology. The separability condition provides such a criterion of individuation: two systems will be distinguished as two, rather than one, if they occupy spatiotemporally separated regions of space-time, and this no matter what their history of interaction. In other words, the separability principle says that one can draw the lines between the parts of the universe anywhere one wants—there are "joints" everywhere. Field theory does this in the most extreme possible fashion, by regarding every infinitesimal region of space-time (every point-event in the space-time manifold of general relativity) as a separate system characterized by its own, separate state (the magnitude of the metric tensor at that point in the case of general relativity). The locality principle simply adds that the state of one of these separate systems cannot be influenced by events in regions of space-time separated from the given region by a spacelike interval, which is just to say that all influences are local influences.

Is Einstein right when he says that he can imagine no other objective way of dividing the world up into parts? After all, quantum mechanics denies the separability principle; it assigns nonfactorizable, entangled joint states to previously interacting systems regardless of how widely separated they may now be. Does quantum mechanics therefore lack an objective foundation for its basic ontology of systems and states? This is not the place to debate that question, but Einstein certainly thought that the answer was "yes."[32]

Whether or not it has its origins in everyday thought, Einstein's separability principle does have a distinguished heritage, with roots that go back at least to Newton, whose contemporary, John Locke, characterized Newtonian absolute space as a principle of individuation (Locke 1690, bk. 2, chap. 15, secs. 9–10, pp. 264–67), and possibly even to the ancient Democritean conception of the "void" as that which keeps the atoms apart. Newton's commitment to absolute space as a principle of individuation was an important part of the background to the debate over absolute versus relative conceptions of space in the Leibniz-Clarke correspondence, with Leibniz explicitly denying that spatial separation is a sufficient condition for individuating two systems.[33] The antiquity of a principle is, of course, by itself, no argument for the principle's indispensability, for the examples of Leibniz and twentieth-century quantum mechanics show that doubters have not been lacking in the case of the separability principle. But that antiquity might explain Einstein's saying that he can imagine no alternative.

There is still the question why Einstein sees a link specifically between separability and talk of physical reality. There are two answers to this question. The more important answer is that talk of physical reality means talk of a theory's physical ontology, and Einstein can imagine no objective way to individuate the elements of a theory's physical ontology except by the terms of the separability principle. So—no separability, no ontology, no reality.

32. For more on how the quantum theory might meet this objection, see Howard 1989, 1994.

33. A few suggestions about the history are given in Howard 1989, pp. 243–44. See also Torretti 1983, p. 285, n. 7.

But the more interesting answer is that the separability principle gives us a physicalized way of understanding the talk of "observer independence" and "externality" that is so much a part of our traditional conception of physical reality. As Einstein says, "the concepts of physics refer to a real *external* world, i.e., ideas are posited of things that claim a 'real existence' *independent* of the perceiving subject." This talk of externality is no mere Cartesian metaphor for the difference between mind and matter. We should interpret the word "external" in the most literal sense. The real is external to the knower, the observed object is independent of the observer, even when they are interacting with one another, as they must interact for observation to take place, precisely in the sense that they are spatially separated, at least from the moment when the observation interaction ceases. If a comprehensive fundamental physical theory is to explain the observer (in a suitably physicalized form) as well as the observed, then, on Einstein's view, it can hardly qualify as a realistic theory if it cannot treat the observer and the observed as being independent of, or external to, one another, which means treating them as separable systems. Thus Einstein's entheorized or physicalized conception of reality.[34]

Fine quotes some remarks by Einstein linking observer independence with a field theory's employment of a space-time framework, as well as some remarks faulting the quantum theory for its abandonment of a space-time framework (Fine 1986, pp. 97–99). Given what we have learned about how Einstein views field theories as the most extreme embodiment of the separability principle, it should be clear that what is intended by these references to a space-time framework is, again, just the separability principle, for a field theory can assign definite space-time trajectories to systems, especially interacting systems, which is the difficult case, only because of its satisfaction of the separability principle. And quantum mechanics was forced to abandon a space-time framework because the only joint states that one can construct for interacting systems in space-time are nonentangled,

34. Even though I am following Fine's lead here in asking how Einstein entheorizes the concept of the real, I mean to disagree with him about how Einstein does it. Fine argues that determinism is the key to Einstein's entheorized conception of reality (Fine 1986, pp. 97–103). But while there is no denying Einstein's obvious preference for deterministic theories, I believe that, if pressed, Einstein would have sacrificed determinism if that were the price for preserving separability; see Howard 1991*a*, pp. 130–34. Note also that while Fine sees a connection between the separation principle and Einstein's conception of a realistic theory, he maintains that the separation principle "depends upon a prior commitment to a realist description" (Fine 1986, p. 103); it should be clear that I disagree with this point as well.

factorizable states. It was the need to accommodate entangled, nonfactorizable states that forced Schrödinger in the mid-1920s to relocate the state function from physical space into configuration space.[35]

In the last few years of his life, Einstein continued to voice his doubts about the quantum theory and to search for a unified field theory. However unsuccessful he may have been in the latter quest, this was not a silly preoccupation of a senile old man. In many ways, Einstein understood far more clearly than most of his younger colleagues just how profoundly the quantum theory differed from anything that had gone before. His opposition to the theory derived not from a nostalgic attachment to a discredited determinism, but from the belief that the theory violated the only imaginable objective principle of individuation for physical systems, the separability principle. And in identifying separability as the hallmark of a realistic theory, Einstein was not asserting some idiosyncratic conception of reality; he was simply saying that this is the only way in which, in his opinion, one can achieve a coherent conceptual foundation for one's physics.[36]

Conclusion

Einstein wanted to hold on to physical reality. He believed that science concerns itself with physical reality. And yet he said that he was not a realist, that it was meaningless to say that the physical world is real. Are there any lessons to be learned from his example? I believe that there are. The chief lesson is that the philosophy of science is best done not in the library, but in the laboratory. Einstein's own statement of this lesson was quoted near the beginning of this paper: "The reciprocal relationship of epistemology and science is of noteworthy kind. They are dependent upon each other. Epistemology without contact with science becomes an empty scheme. Science without epistemology is—insofar as it is thinkable at all—primitive and muddled" (Einstein 1949, pp. 683–84). Too many philosophers of science think that the kind of contact between epistemology and science recommended here involves the philosopher's searching around in the history of science

35. For more on the shift from physical space to configuration space, and its connection with the abandonment of the space-time framework, see Howard 1990*b*, pp. 81–91.

36. All the same, Einstein was constantly thinking about alternatives. And, thus, he entertained the possibility that a future fundamental physics might have to abandon a space-time framework, with its continuity and separability, taking as basic, instead, an algebraic framework, meaning, presumably, something like matrix mechanics or the Dirac formalism. See Stachel 1986, pp. 380–81.

for thinkers and episodes that serve to validate one's own methodological claims. One uses the scientist's work and words as evidence for one's own methodological theories. And, so, different commentators would have us believe that Einstein is a critical realist, a scientific realist, a metaphysical realist, a convergentist realist, an internal realist, a constructive empiricist, or a passenger on NOA's ark. But the problem with this is obvious. Not only is theory underdetermined by evidence, all evidence is, in the end, theory laden. As the case of Einstein dramatically illustrates, when we do history in this way we inevitably abuse, for our own selfish purposes, the very figures we pretend to honor.

How else can the epistemologist make contact with science? One way is to go into the laboratory and do some science. If Einstein is any example, some of the best philosophy of science has been done by practicing scientists. Another way is to approach the history of science in a different fashion. Instead of turning to history to prove a point, turn to history to learn. Do not ask the Einsteins and the Maxwells, the Newtons and the Aristotles for answers to our own, late twentieth-century questions. Ask them instead what their questions were, and then listen carefully to the answers. We may just find that their questions were more interesting than ours.

Was Einstein really a realist? The question is not well posed.

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