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EINSTEIN AND DUHEM*

ABSTRACT. Pierre Duhem's often unrecognized influence on twentieth-century philosophy of science is illustrated by an analysis of his significant if also largely unrecognized influence on Albert Einstein. Einstein's first acquaintance with Duhem's *La Théorie physique, son objet et sa structure* around 1909 is strongly suggested by his close personal and professional relationship with Duhem's German translator, Friedrich Adler. The central role of a Duhemian holistic, underdeterminationist variety of conventionalism in Einstein's thought is examined at length, with special emphasis on Einstein's deployment of Duhemian arguments in his debates with neo-Kantian interpreters of relativity and in his critique of the empiricist doctrines of theory testing advanced by Schlick, Reichenbach, and Carnap. Most striking is Einstein's 1949 criticism of the verificationist conception of meaning from a holistic point of view, anticipating by two years the rather similar, but more famous criticism advanced independently by Quine in 'Two Dogmas of Empiricism'.

The typical story of the influence of Pierre Duhem's philosophy of science outside of French philosophical circles begins with Otto Neurath and ends with Willard V. O. Quine. There is more than a little truth to this story. Duhem's influence on Neurath was significant, direct, and generously acknowledged.[1] His influence on Quine was equally significant, though indirect (Neurath was the principal intermediary), with Quine himself having been unaware of the parallel between his and Duhem's views until it was pointed out to him by others.[2] And it is primarily through Quine's writings that Duhem's ideas have retained what currency they have in contemporary debates in the philosophy of science.

But the story is far from complete, and it leaves one with the clear impression that Duhem's holistic variety of conventionalism has been far less influential than the views of other thinkers like Mach, Poincaré, Russell, Wittgenstein, Schlick, and Carnap. I think that this impression is misleading. I think that we have, for years, and for a variety of reasons, underestimated Duhem's influence on twentieth-century philosophy of science.[3] And I will defend this thesis by exhibiting what I regard as the pronounced 'Duhemian' features of Albert Einstein's mature philosophy of science. I do not claim that Einstein's philosophy of science is through and through Duhemian; no such simple description can adequately characterize the views of a thinker like Einstein, who

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aimed, deliberately, not to be a systematic philosopher and who took the philosophical turn only when driven that way by problems arising in his scientific work. Instead, I want to argue (1) that recognition of the 'Duhemian' features is essential for understanding Einstein's philosophy of science, and (2) that the central place of the 'Duhemian' elements in Einstein's thinking is evidence of the profound and pervasive, if not always well-recognized influence of Duhem's ideas within twentieth-century philosophy of science.

1. EINSTEIN'S FIRST ACQUAINTANCE WITH DUHEM'S LA THÉORIE PHYSIQUE

Let us begin by exploring the context of Einstein's first acquaintance with Duhem's major work in the philosophy of science, *La Théorie physique, son objet et sa structure* (1906), which appeared in a German edition in 1908 under the title *Ziel und Struktur der physikalischen Theorien*. It was translated by Friedrich Adler, an ardent follower of Ernst Mach, who himself contributed a sympathetic foreword. In at least two respects, the involvement of Adler and Mach is significant for understanding Einstein's acquaintance with the text.

From the point of view of intellectual history, the involvement of Adler and Mach tells us something about the contemporary reception of Duhem's views at least among German-speaking philosophers of science, namely, that many thinkers saw no serious conflict between the views of Mach and Duhem, that both were seen as part of a larger anti-metaphysical movement in the philosophy of science emphasizing the economical and descriptive side of scientific theorizing. Mach's own characterization of Duhem's main thesis is instructive:

> The author shows how physical theory gradually transforms itself from a presumptive explanation on the basis of a vulgar or more or less scientific metaphysics into a system resting on a few principles, a system of mathematical propositions that economically describe and classify our experiences. In this process the explanatory picture changes many times, until finally it falls away entirely, while the descriptive part passes over into the new, more complete theory almost unchanged . . . Duhem regards the model, like the picture, as a parasitic growth. (Mach 1908, pp. iii–iv)

And Adler's characterization echoes Mach's: "The elimination of all metaphysics constitutes the fundamental tendency of the work, and the principle of the economy of thought, which Mach first formulated, is consistently maintained" (Adler 1908b, p. vi). There were, of course,

some issues about which Mach and Duhem disagreed, as Adler dutifully and carefully indicated. Duhem, for example, did not consider any foundation for scientific concept formation like Mach's "elements of sensation" (Adler 1908, p. vi); such crude 'atomistic' reductionism, vesting not only each admissible proposition but each admissible concept with its own, individual empirical content, would be incompatible with Duhem's holism. Nor did Duhem require, as Mach would, that 'hypotheses' have an empirical basis and serve merely as tools for organizing experience (Adler 1908, pp. vi–vii); Duhem rejected such constraints also on the basis of his holistic conception of theories, arguing that entire theories must have empirical content, but that individual hypotheses could not (Duhem 1906, pp. 215–16).[4] With hindsight, *we* might regard these as serious and fundamental disagreements, as they indeed turned out to be, but the point is that *Mach* and contemporary Machians like Adler did not; they minimized the differences and stressed the broad areas of agreement.

Further evidence of Duhem's friendly reception by Mach and his followers is easily assembled. Thus, for example, the just-published *La Théorie physique* receives high praise from Mach in the foreword to the second edition (1906) of *Erkenntnis und Irrtum*, Mach's last systematic work on epistemology and the philosophy of science,[5] and it is cited numerous times in the annotations. Especially noteworthy are Mach's seemingly approving mentions of the characteristically 'Duhemian' theses of the theory-ladenness of observation,[6] and the holistic character of all hypothesis testing.[7] There is also Mach's gracious acknowledgment of Duhem's role in "the epistemological discussions" and of Duhem's "valuable" critical remarks in the foreword to the seventh edition of his *Mechanik* (1912, p. ix).[8] And there is, finally, the fact that another one of Duhem's major works, his *L'Évolution de la mécanique* (1903a), was translated in 1912 by Philipp Frank, also an enthusiastic follower of Mach's.[9] As Frank explained some years later, he and many of his contemporaries regarded the views of Duhem and Mach as being quite compatible, both examples of what the philosopher Abel Rey dubbed the "new positivism".[10] Like Adler and Mach himself, Frank stressed the common anti-metaphysical tendency of Mach's and Duhem's theories of scientific method, but he also emphasized theoretical holism as one of the principal features of Duhem's position of interest to him and his contemporaries (Frank 1949, pp. 25–28).[11]

The sympathetic reception by Mach and his followers may not reflect

a wholly accurate reading of Duhem. The one thesis that has proven to be Duhem's major contribution to discussions of scientific method – his theoretical holism – is simply incompatible with the reductionistic and atomistic empiricism in Mach's epistemology. But that does not change the fact crucial for the story I want to tell here, which is that the Machians' public sympathy for Duhem would have prepared the ground for a similarly sympathetic reading by other thinkers, like Einstein, already favorably inclined toward Mach.[12]

Perhaps even more important for my story, however, is specifically the involvement of Friedrich Adler as the translator of *La Théorie physique*. It is important because of the personal relationship between Adler and Einstein.

Einstein and Adler were both students of physics in Zurich around the turn of the century, Einstein at the ETH, Adler at the University of Zurich.[13] Both completed dissertations under Alfred Kleiner at the University of Zurich,[14] and, finally, both were candidates for the Extraordinary Professorship in physics at the University of Zurich to which Einstein was appointed in 1909.[15] They had been acquainted with one another from their student days, Einstein apparently having first sought out Adler upon hearing that he was working on a dissertation on specific heats. They reportedly met often to discuss questions of physics, and, together with their future wives, who were also physics students, they audited lectures at the ETH.[16]

When Einstein returned to Zurich in the fall of 1909 to assume his new position at the university, he and his wife happened to rent an apartment immediately upstairs from the Adlers at Moussonstraße 12. The old relationship between the families, both now with children, was quickly resumed. But more importantly, the renewed personal relationship led to a renewal of the intellectual relationship between Einstein and Adler. In order to escape the noise of the children, they would retire to the attic, where they could work and carry on their discussions in relative quiet.[17] Adler described their relationship in a letter to his parents of 28 October 1909:

> We stand on very good terms with Einstein, who lives above us, and indeed as it happens, among all of the academics, we are on the most intimate terms precisely with him. They have a bohemian household similar to ours, one boy of Assinka's age, who is very often at our place . . . The more I speak with Einstein – and that happens fairly often – the more I see that my favorable opinion of him was justified. Among contemporary physicists he is not only one of the clearest, but also one of the most independent minds, and we are

of one mind about questions whose place is generally not understood by the majority of other physicists. (Ardelt 1984, p. 166)

The Adlers and Einsteins lived as neighbors until the spring of 1911.[18]

Einstein and Adler shared more than an address and a profession. They shared also the interest in the philosophy of science in general, and in Mach's work in particular, that was typical of many students in Zurich at that time, Zurich being as it were the Swiss second home of positivism, owing to the fact of Richard Avenarius's having taught at the University of Zurich from 1877 to his death in 1896. Einstein was introduced to Mach's writings by his friend Michele Besso during their student days at the ETH and quickly developed an appreciation for Mach's style of conceptual criticism.[19] Adler also first read Mach during his student days (Ardelt 1984, p. 98). Having started out as a follower of Friedrich Engels's brand of materialism, he was slow to warm to the anti-metaphysical Mach, but by 1905, when he published a paper criticizing the epistemological assumptions underlying Ostwald's energeticist program from the point of view of Mach and Avenarius (Adler 1905), he had been won over to the cause (Ardelt 1984, pp. 98–99, 125–37).[20] Adler and Mach began corresponding in 1903 (see Blackmore and Hentschel 1985, pp. 30–31), meeting personally for the first time in May 1905 (Ardelt 1984, p. 136), after which Adler went on to become a frequent public spokesman for Mach.[21]

So by the time when Einstein became the fellow bohemian Adler's upstairs neighbor and companion in attic conversation in the fall of 1909, Adler was a close colleague and prominent supporter of Mach's. It was that very autumn when Adler wrote his spirited defense of Mach against Planck's widely-discussed criticisms.[22] Just a few months earlier, in July or August, Einstein's own correspondence with Mach had begun, apparently with the help of Adler.[23]

And it had been just one year since the publication of Adler's translation of Duhem's *La Théorie physique* (1908).[24] Under these circumstances, it seems to me highly likely that Einstein's first acquaintance with Duhem's work can be dated to no later than the fall of 1909. There is no documentation of his having read *La Théorie physique* at this time, but given the nature of his relationship with Adler, given their mutual interests in Mach and the philosophy of science, and given Adler's role in translating *La Théorie physique*, it is hard to imagine

that Einstein would not at least have learned about the work through discussion with Adler. And my guess is that he probably also then read it for the first time. But whether he read *La Théorie physique* at this time or merely discussed it with Adler, his acquaintance with Duhem would have been conditioned by the context, a context in which the community of interest between Mach and Duhem as proponents of the 'new positivism' would have been featured, a context that would have predisposed Einstein to sympathy with Duhem.

When exactly Einstein did read *La Théorie physique* for the first time is not clear. That he did read it and had a favorable opinion of it is evident, however, from the one and only apparent reference to the book that I have found in his writings and correspondence. It is in a letter of September 1918 from Einstein to the Bonn mathematician Eduard Study. Einstein had written to Study on 17 September (EA 22–301) praising Study's book, *Die realistische Weltansicht und die Lehre vom Raume* (1914), but suggesting that he did not agree with all of Study's views. In his reply of 23 September (EA 22–304), Study asked Einstein to elaborate on his criticism, and Einstein answered on 25 September (EA 22–307) with a three-page letter setting out his reaction in detail.

The main thrust of Einstein's criticism concerns Study's defense of scientific realism, the principal aim of the book. Einstein says that the proposition "The physical world is real" appears to him "meaningless" (*sinnlos*), as if one were to say "The physical world is cock-a-doodle-doo" (*Die Körperwelt ist kikeriki*); he adds that to him the "real" is "an in itself empty, meaningless category". And he concludes, "I concede that the natural sciences concern the 'real', but I am still not a 'realist'".

But then, as if to balance his criticism of the realist, Einstein offers this criticism of the positivist:

> 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)

That the interlineated remark about Duhem is joined to a criticism of

the positivist attack on hypotheses makes it clear that the book intended is *La Théorie physique*, and that Einstein had in mind specifically sections 8 and 9 of chapter 6, along with the whole of chapter 7.[25] This is where Duhem defends the role of hypotheses against overly restrictive empiricist demands that every admissible scientific proposition possess its own empirical content, arguing, again, that while whole theories must have empirical content, the individual hypotheses constituting those theories cannot.

Given Einstein's relationship with Adler, and my conjecture that he first learned about Duhem through Adler, it is striking that he emphasizes here as a principal virtue of *La Théorie physique* vis à vis the positivism of Mach one of the two differences between Mach and Duhem that Adler had pointed to in his preface to the translation of *La Théorie physique*. But whereas Adler downplayed the significance of such differences between Mach and Duhem, Einstein stresses them. As we shall see later, it was precisely this thesis about the lack of empirical content of individual hypotheses that proved to be Duhem's main legacy to Einstein and that constituted the crucial difference between Einstein's empiricism and that of the Vienna and Berlin empiricists.

2. UNDERDETERMINATION, HOLISM, AND CONVENTIONALISM IN EINSTEIN'S PHILOSOPHY OF SCIENCE

Conventionalism first emerges as an explicit theme in Einstein's thinking in the mid- to late 1910s, partly in response to certain conceptual problems encountered in the development of general relativity, and under the significant influence of Moritz Schlick, himself then still the realist and conventionalist of his pre-Vienna days.[26] Einstein and Schlick most often brought out their conventionalist arguments in reply to attempts by various neo-Kantians to defend Kant against general relativity's threat to the claimed *a priori* character of Euclidean geometry.[27]

In a series of essays and reviews during the early 1920s,[28] Einstein and Schlick agreed with the neo-Kantians that empirical evidence underdetermines theory choice, especially the choice of deep theoretical principles like the axioms of geometry; but whereas the neo-Kantians exploited the fact of underdetermination to insulate cherished principles from empirical refutation, and insisted that our choice among the alter-

native theories equally compatible with experience is determined by *a priori* considerations, Einstein and Schlick argued that no principle is immune to rejection or revision in the light of experience, and insisted that the choice among alternative theories is a matter of convention, guided at most by considerations of simplicity. Indeed, this is one of the roots of Einstein's frequent talk of theories being "free creations of the human intellect" (for example, Einstein 1921, p. 5).

The kind of conventionalism that Schlick and Einstein deployed in response to the neo-Kantians owed at least as much to Poincaré (1902, 1905) as to Duhem. But the distinctively Duhemian themes of holism and underdetermination often came to the fore, especially in Einstein's writings of the period.

Consider, first, the theme of underdetermination. In a remarkable letter to Schlick of 21 May 1917, Einstein wrote:

> 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. (EA 21–618)

(By "events" Einstein means the points of the space-time manifold constituting a theory's fundamental ontology.) And in his address on the occasion of Planck's sixtieth birthday (26 April 1918), 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 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)

This passage is especially interesting for the way it contrasts the logical fact of underdetermination with the practical fact of unambiguous determination, exactly the same ironic contrast having been stressed by Duhem.[29]

Perhaps the most definitive statement of the underdeterminationist thesis is found in a little-known newspaper article of Einstein's, entitled 'Induktion und Deduktion in der Physik', that appeared on Christmas day 1919 in the *Berliner Tageblatt*. Einstein wrote:

> 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 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)

Notice here Einstein's careful distinction between normal Humean inductive uncertainty – the standing possibility that new facts will arise that are incompatible with accepted theories – and Duhemian underdetermination – the necessary existence of alternative theories equally capable of explaining the same facts.

Holism is implicitly assumed at every mention of underdetermination – there are empirically equivalent alternative theories precisely because it is theories as a whole, not individual hypotheses, that stand the test of experience. But explicitly holistic arguments are not common in Einstein's writings from the 1910s and 1920s. The only example I have found is in one of Einstein's reviews of a neo-Kantian work on relativity, Alfred Elsbach's *Kant und Einstein* (Elsbach 1924), where, after asserting that relativity theory is incompatible with the Kantian doctrine of the *a priori*, Einstein wrote:

> This does not, at first, preclude one's holding at least to the Kantian *problematic*, as, e.g., Cassirer has done. I am even of the opinion that this standpoint can be rigorously refuted by no development of natural science. For one will always be able to say that critical philosophers have until now erred in the establishment of the a priori elements, and one will always be able to establish a system of a priori elements that does not contradict a given physical system. Let me briefly indicate why I do not find this standpoint natural. A physical theory consists of the parts (elements) A, B, C, D, that together constitute a logical whole which correctly connects the pertinent experiments (sense experiences). Then it tends to be the case that the aggregate of fewer than all four elements, e.g., A, B, D, *without* C, no longer says anything about these experiences, and just as well A, B, C without D. One is then free to regard the aggregate of three of these elements, e.g., A, B, C as a priori, and only D as empirically conditioned. But what remains unsatisfactory in this is always the *arbitrariness in the choice* of those

elements that one designates as a priori, entirely apart from the fact that the theory could one day be replaced by another that replaces certain of these elements (or all four) by others. (Einstein 1924b, pp. 1688–89)

This passage deserves careful attention. For one thing, it anticipates the still more sophisticated holistic arguments we will find Einstein advancing in the late 1940s in opposition to the empiricist theories of meaning. More immediately, however, it points up the reasons for the parting of the ways that was shortly to occur between Einstein and Schlick in their respective understandings of the role of conventions in science.

By 1925, when the second edition of the *Allgemeine Erkenntnislehre* appeared, Schlick had adopted the more refined interpretation of the conventionalist thesis we now associate with the members of the Vienna Circle and its allies like Hans Reichenbach. More clearly than in the first edition of the *Erkenntnislehre*, Schlick now insisted on a fundamental distinction between two types of propositions constituting a theory: analytic *coordinating definitions* and synthetic *empirical propositions*. He argued that only the former are conventional and that once they are fixed by convention the truth or falsity of the individual empirical propositions is unambiguously determined by experience – quite the contrary of Duhem's position (Schlick 1925, pp. 89–101).[30] Schlick and the other defenders of this position, like Reichenbach,[31] seem to have been driven to it by the logic of their argument with the neo-Kantians. As Einstein noted in his review of Elsbach, merely asserting that a theory as a whole possesses empirical import is logically not sufficient to force the hand of the neo-Kantian, who can always then protect cherished, allegedly *a priori* principles from empirical refutation by electing to abandon other elements of the theory when confronted with empirical evidence incompatible with the theory's predictions. Presumably not satisfied with Einstein's subtle criticism that the choice of which propositions to protect is entirely arbitrary, Schlick and Reichenbach seem to have wanted a more decisive reply to the neo-Kantian, one that would logically imply the empirical corrigibility of each individual synthetic proposition the neo-Kantian might want to defend, such as Euclid's fifth postulate. Hence the distinction between coordinating definitions and empirical propositions, and the claim that only the definitions are conventional.

The Schlick–Reichenbach conception of conventionalism stands or falls with the analytic-synthetic distinction, which provides the only

basis for distinguishing between coordinating definitions and empirical propositions. Consistent Duhemians do not endorse the analytic-synthetic distinction, but that was not how Einstein criticized Schlick and Reichenbach. Instead, he used the same argument he used against the neo-Kantians, namely, that such distinctions among the propositions constituting a theory – whether between *a priori* and *a posteriori* propositions or between coordinating definitions and empirical hypotheses – is arbitrary, and hence, presumably, of no fundamental epistemological significance. One finds this argument in Einstein's classic 1936 essay, 'Physik und Realität', where Einstein ever so gently qualifies what might appear to be an endorsement of the Schlick–Reichenbach position:

> We shall call 'primary concepts' such concepts as are directly and intuitively connected with typical complexes of sense experiences. All other concepts are – from the physical point of view – meaningful only insofar as they are brought into connection with the 'primary concepts' through statements. These statements are partly definitions of the concepts (and of the statements logically derivable from them) and partly statements that are not derivable from the definitions, and that express at least indirect relations between the 'primary concepts' and thereby between sense experiences. Statements of the latter kind are 'statements about reality' or 'laws of nature', i.e., statements that have to prove themselves on the sense experiences that are comprehended in the primary concepts. *Which of the statements are to be regarded as definitions and which as laws of nature depends largely upon the chosen representation; in general it is only necessary to carry through such a distinction when one wants to investigate to what extent the whole conceptual system under consideration really possesses content from a physical standpoint.* (Einstein 1936, p. 316; emphasis mine)

See how the holistic viewpoint is insinuated at the end: We need only make a distinction between definitions and empirical propositions when we desire to determine the empirical content of the "whole conceptual system", and even then, where we draw the line "depends largely upon the chosen representation".

3. EINSTEIN'S 'DUHEMIAN' CRITIQUE OF EMPIRICIST CONCEPTIONS OF MEANING AND THEORY TESTING

Over the years, Einstein grew ever more impatient with the failure of Schlick, Reichenbach, Carnap, and their allies to understand his reservations about their view of the structure of theories and the relation between theory and evidence. His summary opinion was stated clearly, and with acid sarcasm, in a letter to Paul Schilpp of 19 May 1953, declining Schilpp's invitation to contribute a paper to the Carnap

volume of the Library of Living Philosophers:

> It is a good idea to devote a volume of your collection to Carnap's life's work. But I cannot comply with your request. That is to say, I have come to terms with this slippery material from time to time only when my own problems made it urgently necessary. But even then I have studied only a little literature, so that I cannot do justice to the swarm of incessantly twittering positivistic little birds . . . . Between you and me, I think that the old positivistic horse, which originally appeared so fresh and frisky, has become a pitiful skeleton following the refinements that it has perforce gone through, and that it has dedicated itself to a rather arid hair-splitting. In its youthful days it nourished itself on the weaknesses of its opponents. Now it has grown respectable and is in the difficult position of having to prolong its existence under its own power, poor thing. (EA 42–534)

Some of the reasons for Einstein's growing disenchantment with positivism emerge in his reply to Reichenbach's contribution to the Library of Living Philosophers volume on Einstein himself. And it is clear from this exchange that the problem concerned precisely the failure of Reichenbach and his colleagues to appreciate the implications of Duhemian holism.

Reichenbach had defended a view of the empirical character of geometry not unlike that which Einstein himself had defended years earlier in his influential *Geometrie und Erfahrung* (Einstein 1921),[32] with the exception that Reichenbach invoked explicitly the distinction between coordinating definitions and empirical hypotheses, interpreting Einstein's identification of the geometer's 'rigid body' with the physicist's 'practically rigid rod' as an instance of a coordinating definition (the definition of 'congruence'):

> The choice of a geometry is arbitrary only so long as no definition of congruence is specified. Once this definition is set up, it becomes an empirical question *which* geometry holds for physical space. . . . The conventionalist overlooks the fact that only the incomplete statement of a geometry, in which a reference to the definition of congruence is omitted, is arbitrary. (Reichenbach 1949, p. 297)

As one might expect, Einstein did not agree. But instead of just saying so, he couched his criticism in the amusing form of an imaginary dialogue between 'Reichenbach' and 'Poincaré'.

A crucial step in the dialogue has 'Reichenbach' grudgingly agreeing with 'Poincaré' that, since there are no perfectly rigid bodies in nature, and since we must therefore employ our physics to correct for deformations resulting from things like changing temperature, we really wind up testing the whole body of theory consisting of geometry plus physics, and not just geometry alone. At this point, Einstein has an 'anonymous

nonpositivist' takeover for Poincaré, out of respect, he says, "for Poincaré's superiority as thinker and author" (Einstein may also have realized that the view attributed to Poincaré was more Duhem's than Poincaré's). The nonpositivist observes that, in agreeing that geometry and physics are tested together, Reichenbach has contravened one of his own fundamental positivist principles – the equation of meaning with verifiability:

Non-Positivist: 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 meaning 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)[33]

Not only is this a strikingly clear statement of the implications of Duhemian holism for our understanding of the empirical content of scientific concepts and theories, it is also a remarkable anticipation of the more famous criticism of the verificationist theory of meaning that Quine advanced independently two years later in his well-known essay, 'Two Dogmas of Empiricism' (Quine 1951).[34]

4. CONCLUSION

A sympathy for Duhemian conventionalism, with its emphasis on underdetermination and theoretical holism, was an abiding and central feature of Einstein's mature philosophy of science. It is one of the keys to an understanding of his attitude toward neo-Kantianism as well as his attitude toward logical empiricism. And it is a measure of the significant (if sometimes almost subterranean) influence that Duhem's philosophy of science has exerted throughout our century.

Let me offer now a final anecdote showing clearly where Einstein's sympathies lay. It concerns not Duhem directly, but the wonderful image that Otto Neurath introduced for representing the Duhemian ideas of holism and underdetermination, where he compares theory choice to our having to reconstruct a ship not on firm footing in a dry

dock, but at sea, one plank at a time (Neurath 1932, p. 206). The story is found in Rudolf Carnap's diary. On the 16th of November 1952, Einstein's longtime friend Paul Oppenheim brought him to visit Carnap, who was then staying in Princeton. The conversation touched upon several topics, turning eventually to the subject of reality. Carnap records this exchange:

(2) On reality. I say that only Mach advanced such formulations according to which the sense data are the only reality. He says that the positivists nevertheless want to start from something securely given, and that there is no such starting point. I agree: there is no rock bottom, Neurath's reconstruction of the ship afloat. With that he emphatically agreed. (RC 025-80-01)[35]

NOTES

* I wish to thank the Hebrew University of Jerusalem, which holds the copyright, for permission to quote from the unpublished letters of Einstein. Items in the Einstein Archive are cited by giving their number in the control index after the following format: EA nn-nnn. Similar formats are employed for citing other archival material. Thus 'AA' refers to material in the Adler Archive at the Verein für Geschichte der Arbeiterbewegung, Vienna; and 'RC' refers to material in the Rudolf Carnap collection at the Archive for Scientific Philosophy, Department of Special Collections, Hillman Library, University of Pittsburgh. The research for this paper was supported in part by a grant from the National Science Foundation, No. SES-8420140, as well as by grants from the Deutscher akademischer Austauschdienst, the American Philosophical Society, and the University of Kentucky Research Foundation.

[1] See Neurath 1916, p. 27, 1932, pp. 213–14, and the numerous references to Duhem in Neurath's collected philosophical papers, Neurath (1983).

[2] "When I wrote 'Two Dogmas of Empiricism', I had not read Einstein's reply to Reichenbach, nor did I know of Duhem. My holism there was just my own common sense, plus perhaps some influence from Neurath's congenial figure of the boat. After 'Two Dogmas' appeared, January 1951, both Hempel and Philipp Frank told me about the kinship of my view to Duhem's; so I added the footnote citation of Duhem when 'Two Dogmas' was reprinted in *From a Logical Point of View*, 1953." (Private communication, 9 October 1986.) On the connection to Einstein, see below, note 34.

[3] There are many reasons for the neglect. Foremost among them must be the fact that Neurath, whose thinking most clearly reflected the influence of Duhem and who would have been Duhem's foremost advocate, died immediately after the second world war (1945), and never had the same opportunity as Reichenbach or Carnap to represent Viennese philosophy of science to an English-speaking public. As it turned out, Neurath's views were not often fairly represented, being interpreted to us primarily by the born-again physicalist, Carnap, who never really appreciated how different were his and Neurath's views. English-speaking philosophers – most of whom did not read Poincaré or Duhem in the original – came to know conventionalism only in the form in which it was presented by Schlick, Reichenbach, and Carnap. But as is explained below in Section 2, this version of conventionalism differs significantly from that of Duhem.

[4] Mach also worried that the Catholic Duhem had an ulterior motive: "Given the power and influence that scholasticism and Catholicism still have in France, it is nevertheless possible that Duhem is nurturing some kind of devil in the background; he is after all an admirer of Thomas Aquinas, something of which he makes no secret at all. But of what consequence is that, as long as he does not turn the devil loose? Perhaps he wants to free physics of metaphysics only in order to win elbow-room for the latter over against physics. Philosophers and theologians can do what they will with metaphysics. If by that the physicists, physiologists, and psychologists accustom themselves to making do without metaphysics, then all is won". He concluded: "For the time being I am quite content with the degree of agreement with Duhem". (Mach to Adler, 22 April 1908, in Blackmore and Hentschel 1985, p. 50.)

[5] Mach writes: "I was very pleased by Duhem's work, 'La Théorie physique, son objet et sa structure' (1906). I had not yet hoped to find such thoroughgoing agreement on the part of physicists. Duhem repudiates any metaphysical conception of questions in physics; he views the conceptually-economical determination of the factual as the aim of physics . . . . The agreement between us is all the more precious to me, since Duhem arrived at the same results wholly independently". (1906, p. x.)

[6] "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 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.)

[7] "Duhem (La Théorie physique, pp. 364f) 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.)

[8] What form these 'epistemological discussions' took is not clear. There was a modest correspondence between Duhem and Mach, lasting from 1903 to at least 1909, but as Stanley Jaki puts it, the letters 'contain only generalities' (1984, p. 380). The 'valuable' critical remarks were presumably those contained in Duhem's review (1903b) of the French edition (1904) of the *Mechanik*, remarks for which Mach thanked Duhem in a letter of 15 May 1904 (published, along with the rest of the Mach–Duhem correspondence, in Hentschel 1988, p. 78).

[9] See Frank (1917) for a summary of Frank's views on Mach. For more on the relationship between Mach and Frank, see Frank (1941), pp. 18–30, Blackmore (1972), and Wolters (1987) (which corrects a number of errors in Blackmore).

[10] Rey (1907), pp. 392ff; cited in Frank (1949), p. 21. In one essay Frank called Duhem the "most important representative of the Machian line of thinking in France" (Frank 1917, p. 66).

[11] For more on the relationship between Mach and Duhem, see Paty (1986), Jaki (1984), pp. 319–73, and Blackmore (1972), pp. 196–7. For different reasons, the latter two

discussions should be read with care. Blackmore exaggerates the extent of Mach's influence on Duhem, whereas Jaki too quickly dismisses the "stereotype classification of Duhem as representative of positivism" (p. 358), arguing that it ignores the significant realistic and metaphysical strain in Duhem's thinking. Jaki does not stress sufficiently the distinction – absolutely necessary for understanding the contemporary reception of Duhem's views – between Duhem's broader philosophical, metaphysical, and theological commitments (clearly recognized by Mach, as shown by the letter to Adler quoted above in note 4) and his more restricted views on the methodology of physics. It was the latter that excited the interest of contemporary philosophers of science, and it would be seriously misleading to describe Duhem's views on scientific methodology as a version of scientific realism; the Duhemian thesis of underdetermination is inherently antithetical to a realistic conception of scientific method. Some of the most insightful comments on the relationship between Duhem and Mach and on Duhem's influence on the members of the Vienna Circle are found in the work of Rudolf Haller; see especially Haller (1982, 1985, 1988).

Another implication of Mach's sympathy for Duhem should not be overlooked. The Mach who is so enamored of Duhem cannot be the niggardly positivist often presented to us in the secondary literature. In particular, the Mach who reads *La Théorie physique* with enthusiasm must have a more liberal attitude toward the role of hypotheses in physics than many of his critics grant him. One scholar who has already argued for this more liberal interpretation of Mach on other grounds is Gereon Wolters; see Wolters (1987), pp. 101–20, and (1988).

[12] For more on Einstein's attitude toward Mach, see Blackmore (1972), pp. 247–85 and Wolters (1987), pp. 11–171, which corrects some errors in Blackmore's treatment of these topics.

[13] Einstein studied at the ETH from 1896 to 1900; for documentation on Einstein's years at the ETH, see Einstein (1987). Adler was at the University of Zurich from 1897 to 1901; see Ardelt (1984), pp. 71–111.

[14] Adler's dissertation was completed in 1902 (Adler 1902); for background, see Ardelt 1984, pp. 101–11. Einstein's was completed in 1905 (Einstein 1905) after an abortive earlier attempt at about the same time Adler finished his dissertation; for background on both the 1905 dissertation and the earlier attempt, see Einstein (1989), pp. 170–82.

[15] Einstein, of course, had been working since 1902 as a clerk in the Swiss Federal Patent Office in Bern (see Seelig 1960, pp. 89–160). Adler had been a Privatdozent in physics at the University of Zurich for the previous two and one half years, having received on 13 December 1906 the *Venia legendi* for "experimental and theoretical physics, as well as their history and epistemological foundations" (Ardelt 1984, p. 157–66).

[16] For background on the relationship see Seelig (1960), pp. 162–4. The lectures they attended were either Minkowski's lectures on 'Analytische Mechanik', winter semester 1898/1899, or his lectures on 'Anwendungen der analytischen Mechanik', summer semester 1900 (Einstein 1987, pp. 367, 369). See also Adler to Heinrich Braun, 22 January 1919, EA 6-013: "I have been well acquainted with Einstein from our time together as students in Zurich".

[17] See Seelig (1960), p. 165. Adler himself recalled these conversations in the first letter he wrote to Einstein after his imprisonment for assassinating the Austrian Minister-President, Count Stürgkh; see Adler to Einstein, 9 March 1917, EA 6-001.

[18] In March 1911, Einstein moved to Prague to take up the chair in physics at the Charles

University (Seelig 1960, p. 203). At the end of May, Adler moved to Vienna to take up a position as secretary to the Austrian social democratic party, one of whose founders was Adler's father, Viktor (Ardelt 1984, p. 215).

[19] For more on Einstein's first acquaintance with Mach, see Wolters (1987) and the introduction to Einstein (1989). Einstein frequently expressed his debt to Mach; see for example Einstein (1916).

[20] Ardelt (1984) is also a good source to consult on the interesting role played by Mach's philosophy of science in debates over the interpretation of Marxism that pitted Adler and many of his Austrian colleagues, who were influenced by Mach's anti-metaphysical arguments, against doctrinaire materialist Marxists like Lenin. Adler is one of the targets of criticism in Lenin's *Materialism and Empirio-Criticism* (see, for example, Lenin 1909, p. 46).

[21] See, for example, Adler (1908a). For more on the relationship between Adler and Mach, see Ardelt (1984), Blackmore (1972), and Wolters (1987).

[22] Adler (1909), Planck (1909). Adler's paper appeared on 26 December 1909. Mach himself replied the following year (Mach 1910), eliciting a final rejoinder from Planck, in which Adler's reply is cited (Plank 1910, p. 1188).

[23] Near the end of a letter to Adler of 26 July 1909, Mach asks: "Is Einstein still in Bern? I want to send him a copy also". (Mach was referring to a copy of the new second edition of his *Erhaltung der Arbeit*, 1909.) Einstein's first letter to Mach of 9 August 1909 (EA 17-410) indicates that he received the book sometime during the intervening fourteen days. In that letter, Einstein too expressed sympathy for Mach in the debate with Planck (Einstein to Mach, 9 August 1909, EA 17-410). The mentioned letters are reprinted in Blackmore and Hentschel 1985, pp. 58–59.

[24] It was Adler who first suggested the project to Mach in the fall of 1906 after reading the second edition of Mach's *Erkenntnis und Irrtum* (1906), in which, as noted above, Mach had praised the book. As it turned out, Mach had already recommended translation of the work to the publisher Barth, who then recruited Adler for the task. See Adler to Mach, 19 October 1906 (AA 130), Mach to Adler, 20 October 1906 (AA 130), and Adler to Mach, 10 November 1906 (AA 130). These details are provided in Ardelt (1984), p. 293, n. 16.

[25] The only other serious candidate for 'the clear book by Duhem' is *L'Évolution de la mécanique* (Duhem 1903a), which appeared in the German translation by Philipp Frank in (1912). But not much in this book would bear directly on the positivist critique of hypotheses, whereas the latter is an important theme in *La Théorie physique*.

[26] See Schlick (1915, 1917, 1918). For further discussion of these issues, see Howard (1982, 1984, 1987, 1988).

[27] Between 1919 and 1925 there was a floor of books and articles of this kind coming both from critical realists in the tradition of Oswald Külpe and Alois Riehl and from critical idealists in the Marburg tradition of Hermann Cohen and Paul Natorp. See, for example, Sellien (1919), Cassirer (1921), Schneider (1921), Winternitz (1923), and Elsbach (1924). For a helpful survey of the neo-Kantian reaction to relativity theory, see Hentschel (1987).

[28] See especially Schlick (1921, 1922), and Einstein (1924a, 1924b).

[29] Duhem wrote: "Contemplation of a set of experimental laws does not, therefore, suffice to suggest to the physicist what hypotheses he should choose in order to give a theoretical representation of these laws; it is also necessary that the thoughts habitual

with those among whom he lives and the tendencies impressed on his own mind by his previous studies come and guide him, and restrict the excessively great latitude left to his choice by the rules of logic. . . . On the other hand, when the processes of universal science have prepared minds sufficiently to receive a theory, it arises in a nearly inevitable manner and, very often, physicists not knowing each other and pursuing their reflections at a great distance from each other generate the theory at the same time. One would say that the idea is in the air, carried from one country to another by a gust of wind, and is ready to fertilize any genius who is disposed to welcome it and develop it, as with pollen giving birth to a fruit wherever it meets a ripe calyx. . . . Logic leaves the physicist who would like to make a choice of a hypothesis with a freedom that is almost absolute; but this absence of any guide or rule cannot embarrass him, for, in fact, the physicist does not choose the hypothesis on which he will base a theory; he does not choose it any more than a flower chooses the grain of pollen which will fertilize it; the flower contents itself with keeping its corolla wide open to the breeze or to the insect carrying the generative dust of the fruit; in like manner, the physicist is limited to opening his thought through attention and reflection to the idea which is to take seed in him without him". (Duhem 1906, pp. 255–56)

[30] See also Schlick (1936).

[31] Reichenbach's books on relativity are even better known sources for essentially the same conception of the role of conventions; see especially Reichenbach (1924, 1928).

[32] This essay is often misread as a repudiation of conventionalism, since Einstein's principal aim was to criticize Poincaré's conventionalist defense of Euclidean geometry, arguing that when geometrical primitives ('rigid body') are given physical interpretations ('practically rigid rod') geometry becomes an empirical science. But all Einstein denies is that one would always choose to save Euclidean geometry owing to its simplicity relative to alternative geometries. He still asserts that our choice of a total theory – geometry plus physics – is conventional, determined primarily by considerations of simplicity, and he concludes: "In my opinion, Poincaré is correct, *sub specie aeterni*, in this conception" (Einstein 1921, p. 8).

[33] I have corrected the translation on the basis of Einstein's original German text, which was published in Einstein (1954), p. 503, the German edition of Schilpp (1949).

[34] Quine was unaware of Einstein's criticism when he wrote 'Two Dogmas': 'I never met Einstein, and I saw him only once – fifty years ago [1936], when he addressed the Harvard tercentenary. . . . When I wrote 'Two dogmas of empiricism'. I had not read Einstein's reply to Reichenbach". (Private communication, 9 October 1986). Quine does acknowledge his possibly having been influenced by Neurath (see above, note 2), who may also have been a source for Einstein's ideas, though I have found no reference to Neurath by Einstein. It is more likely that Paul Oppenheim discussed these questions with Einstein during the 1940s. See below, section 4.

[35] Quoted here from the transcription made by Richard Nollan from Carnap's original, which is in Stolze–Schrey shorthand. Quoted by permission of the University of Pittsburgh. All rights reserved.

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